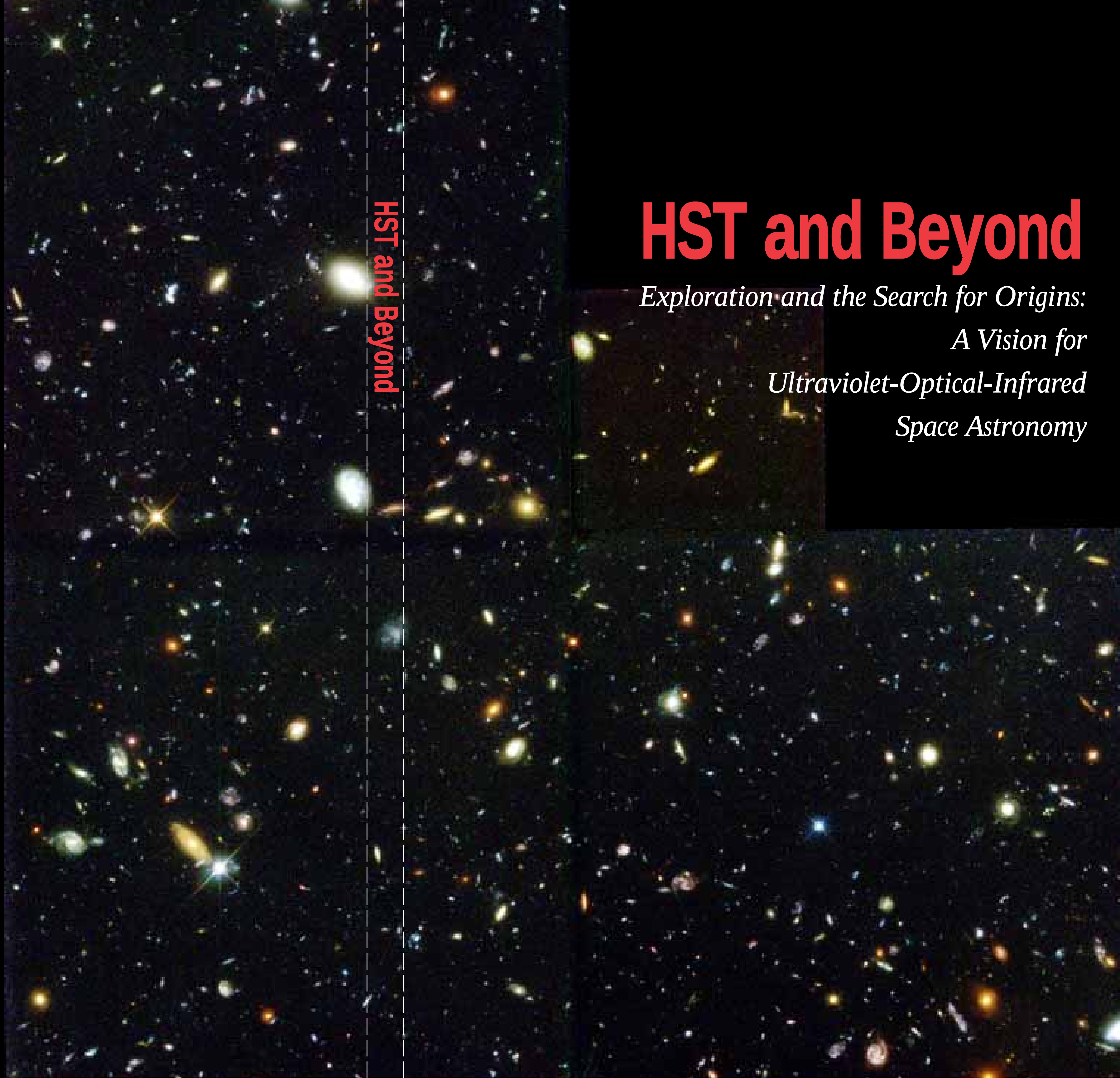

HST and Beyond

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Exploration and the Search for Origins:
A Vision for
Ultraviolet-Optical-Infrared
Space Astronomy

AURA

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Exploration and the Search for Origins: A Vision for Ultraviolet-Optical-Infrared Space Astronomy

REPORT OF THE "HST & BEYOND" COMMITTEE

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MAY 15, 1996

ASSOCIATION OF UNIVERSITIES FOR RESEARCH IN ASTRONOMY
Washington, D.C.

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Preface and Acknowledgements

In September 1993, AURA appointed, at the behest of the Space Telescope Institute Council, and with support from NASA, the HST & Beyond Committee "to study possible missions and programs for UVOIR astronomy in space for the first decades of the twenty-first century" and to "initiate a process that will produce a new consensus vision of the long term goals of this scientific enterprise." The eighteen committee members were primarily chosen for their interest in and experience with UVOIR observations from space, including the Hubble Space Telescope (HST), ASTRO, IRAS, and IUE.

The focus of the Committee was to be science opportunities, with less emphasis on technical capabilities. In accordance with our charge, the Committee assumed that NASA's currently operating and planned major programs in UVOIR astronomy, including HST, SIRTF, and SOFIA, will have been implemented. We have therefore undertaken to find the scientifically compelling next steps.

Committee meetings were held at Goddard Space Flight Center on April 25-26, 1994, at the University of Michigan on August 31 and September 1, 1994, and at the Carnegie Observatories on May 8-9, 1995. A subset of the Committee who attended a public discussion of the Committee's work at the American Astronomical Society (AAS) meeting in Tucson met for further discussions on January 11, 1995. In preparation for these meetings, the committee members reviewed documents, such as *The Next Generation Space Telescope*, proceedings of a 1989 STScI Workshop chaired by Garth Illingworth, the NAS Astronomy and Astrophysics Survey (Bahcall) Report and its *Working Papers*, and *TOPS: Toward Other Planetary Systems*, a report chaired by Bernie Burke for NASA's Solar System Exploration Division. These and other documents, which we took as broadly representative of the thinking of the astronomical community, became the foundation for our extensive discussions and further investigations. We received valuable input through personal presentations from those in the community with special expertise, from e-mail messages following a solicitation of input through the AAS, and from the public meeting on January 10, 1995, at the AAS meeting in Tucson, Arizona.

We concluded that UVOIR astronomy from space has been remarkably successful in advancing the frontiers of astronomical knowledge, and that the promise for future space observatories is equally encouraging. We also believe that our community needs to better articulate and express its long-term scientific goals to the general public, which supports our research. The

report that follows is our attempt to provide both inspiration and direction for that effort.

We expect the readers of this report to represent many interests, with a correspondingly large range of technical and scientific experience and expertise. We recognize this diversity by presenting first, in Parts I and II, what are primarily non-technical discussions of some of the intellectual and societal issues involved in our work as astronomers. In this spirit, we present two scientific goals that we believe could serve as rallying points for UVOIR space astronomy in the next century, present recommendations for facilities that will pursue these goals, and discuss some policy considerations. In Part III we raise the level of technical detail in order to explain the scientific rationale and requirements for our recommendations. Finally, in Part IV, we broaden our scientific discussion to produce a more comprehensive, but still far from exhaustive, description of the implications of our proposals for astrophysical research. Non-specialists may find this part the most challenging, but we believe it to be important to include in this report examples of the breadth of scientific programs that can be carried out with the UVOIR space missions that we recommend—programs that both aid the pursuit of our two scientific goals and maintain the tradition of bold exploration that has evolved our modern view of the universe.

The Committee expresses its thanks to Goetz Oertel, Lorraine Reams, and the AURA staff for their excellent support. Dick Malow sat in on most of our meetings and provided an important perspective and wise counsel. We are very grateful to Mike A'Hearn, Roger Angel, Garth Illingworth, Mike Kaplan, Deanne Peterson, Mike Shao, and Sam Williams for their time and effort in coming to speak to us. We also express our sincere appreciation to Radford Byerly, Bruce Murray, and Scott Tremaine for serving as readers of a preliminary version of this report; their comments have been extremely valuable in improving the effectiveness of our presentation.

Alan Dressler

Executive Summary

Public support and enthusiasm for astronomy have been strong in the final decades of the twentieth century. Nowhere is this better demonstrated than with the Hubble Space Telescope (HST), a grand endeavor, which is enabling astronomers to make giant strides in understanding our universe, our place in it, and our relation to it. The HST continues to provide spectacular pictures and measurements of the exotic phenomena in space that engage our curiosity. It will, we think, answer fundamental questions about the size and age of the universe, and the evolution of stars and galaxies over the last 5 billion years or more. NASA's first infrared observatory, the Space Infrared Telescope Facility (SIRTF), promises to take the crucial next steps towards understanding the formation of stars and galaxies.

These results encourage us to believe that someday we will be able to describe and understand in detail the cosmic events that led to the conditions suitable for our own existence. However, two crucial chapters are missing from our story. Toward their completion, the HST & Beyond Committee identifies two major goals, whose accomplishment will justify a commitment well into the next century: (1) *the detailed study of the birth and evolution of normal galaxies such as the Milky Way*, and (2) *the detection of Earth-like planets around other stars and the search for evidence of life on them*. Despite substantial progress in both areas in recent years, we have not achieved, nor will existing or already planned missions achieve these two ambitious and crucial goals.

To further these two central scientific endeavors, and simultaneously to provide broad capabilities in ultraviolet-optical-infrared (UVOIR) astronomy from space that are needed to advance the field on its many fronts, the Committee recommends the following program for the years 2005 and beyond:

(1) *The HST should be operated beyond its currently-scheduled termination date of 2005.* An emphasis on ultraviolet imaging and spectroscopy, and wide-field, high-resolution optical-light imaging makes the HST an essential astronomical tool through the first decade of the next century. Present budgeting shows that this premier scientific tool could be operated in a "no repair, no upgrade" mode at approximately 20 percent of the current cost of operation and maintenance, which would yield a highly cost-effective, continuing return on the investment in HST.

(2) *NASA should develop a space observatory of aperture 4m or larger, optimized for imaging and spectroscopy over the wavelength range 1-5 μm.* Like the HST and SIRTF, it will be an essential tool in an ambitious program of study in many areas of astronomy; it will be especially powerful in studying the

origin and evolution of galaxies. The Hubble Deep Field, pictured on the front of this report, points the way to the remote, early universe where galaxies formed. By making detailed studies of these distant galaxies, whose light is shifted into the infrared part of the spectrum, we will be able to look back in time to study the process of galaxy formation as it happened. A powerful Infrared (IR)-optimized space telescope, in concert with the new generation of large-aperture ground-based telescopes, will together make giant strides in understanding this crucial step in the origin of our universe.

Extension of this telescope's wavelength range shortward to about 0.5 μm and longward to at least 20 μm would greatly increase its versatility and productivity. The Committee strongly recommends this course, if it can be done without a substantial increase in cost. We believe that a 4m or larger version of this facility could be built for well under $1 billion, and have set an approximate cost of $500 million as a desirable goal. Key to achieving this goal will be shorter development times by smaller development teams, lightweight and compact components, and designs that take advantage of the weightless, thermally stable environment of a high-earth or solar orbit. These changes will be a substantial departure from the HST model, which was in many respects a ground-based telescope modified for space operations. Equally important will be the development of space- and ground-based design elements that will greatly simplify the operation of this observatory, so that resources available for scientific analysis will be increased proportionately.

As the first major astronomical "facility class" instrument in space to follow the Advanced X-ray Astrophysics Facility (AXAF) and SIRTF programs, the development of this space observatory will require many technological advances, such as ultra-lightweight, precision mirrors and structures, advanced cooling systems, and "smart" controls. These will be important for a variety of concurrent and follow-on programs, such as a space telescope for the detection and study of Earth-like planets around other stars.

(3) *NASA should develop the capability for space interferometry.* The first step should be a mission capable of astrometric observations in visible light at the 10 microarsec or better level, which will produce fundamental distance measurements for the Milky Way galaxy and beyond. This mission will lead eventually to the construction of an imaging interferometer that will map the surfaces of nearby stars, and, for other galaxies, study their stellar populations and reveal the structure of active nuclei such as quasars. The Committee recognizes interferometry as a vital next step in pursuit of such fundamental astrophysical questions and, specifically, sees IR interferometry from space as es-

sential to one of our primary goals: the detection and study of Earth-like planets around neighboring stars.

To accomplish these ambitious goals within the resources that are likely to be available in future years will require new space-technology hardware as well as innovations in the management of large projects. These activities are already advancing within NASA. The Committee acknowledges that proposing missions costing billions of dollars is neither realistic nor desirable at this time. This means that the realization of our recommendations requires a break from the approach employed by NASA in building the Great Observatories, of which the enormously successful HST is a prime example. Substantial changes, such as smaller scientific and engineering teams, and greatly simplified management and operation systems, will be necessary to reduce costs to the level where such projects can feasibly be undertaken. Reducing the cost of major space facilities will involve the assumption of greater risk, but a multistep approach will reduce the effect of any one failure on the long term progress of the program. Reduction of risk through association with the human space flight program, though an absolutely crucial factor in HST's success, is unlikely to be practical or affordable for the next generation of space telescopes, unless the ease with which astronauts can retrieve and repair spacecraft improves substantially. On the other hand, the human space flight program might be utilized effectively to provide low-cost experiments of new technologies, which might, for example, be flown on the Space Shuttle.

NASA is presently revising many aspects of its approach to space science; in describing its new initiatives, words like "cheaper, faster, better" are often used. There has been a tendency to include "smaller" in the package, but the former need not imply the latter. We note that the space instruments we are recommending can be "miniaturized" to only a limited extent—large light-gathering surfaces of high precision are an irreducible component in forefront astrophysical research. Nevertheless, we believe that the technological breakthroughs and management reforms represented in these initiatives can also revolutionize the construction of large space telescopes, thereby reducing costs to the point that they can be built. By increasing their direct involvement in the planning, manufacture, and operation of these facilities, astronomers can take a positive, active role in achieving these objectives.

Because the lead time for such challenging missions is long, two different kinds of activities must begin soon. First, NASA should set up study teams to investigate the technical issues involved in building an economical, large-aperture, near-IR-optimized space telescope. We believe that development of a "roadmap" for this effort, as was recently done by NASA for the Exploration of Neighboring Planetary Systems (ExNPS) program, would be a very worthwhile step. These efforts will support and parallel the activities already underway within NASA to explore the possibilities for space interferometry. Improved information regarding capabilities, costs, and tradeoffs will be crucial for the deliberations of the next Astronomy and Astrophysics Survey Committee of the National Academy of Sciences as it plans the national astronomy program for the first decade of the twenty-first century.

Second, we recognize that it is increasingly important for scientists to explain their motivations, goals, and results to the society that supports their research. The necessity of doing so is especially acute because, as we draw nearer to answering some of humanity's ancient questions, it would appear that competition for the resources available for scientific research will be stronger than at any time in the last half century. Sections 1 and 2 of this report are an attempt to proceed in that spirit.

Acronyms

ACS	Advanced Camera for Surveys
AGNs	Active Galactic Nuclei
AIM	Astrometric Interferometry Mission
AU	Astronomical Unit
AURA	Association of Universities for Research in Astronomy
AXAF	Advanced X-ray Astrophysics Facility
BLR	Broad Emission-Line Region
BMDO	Ballistic Missile Defense Organization
BOA	Big Orbital Array
CAA	Committee for Astronomy and Astrophysics
CCD	Charge-Coupled Device
CHARA	Center for High Angular Resolution Astronomy
COAST	Cambridge Optical Aperture Synthesis Telescope
COBE	Cosmic Background Explorer
ESA	European Space Agency
ESO	European Space Observatory
EUVE	Extreme Ultraviolet Explorer
ExNPS	Exploration of Neighboring Planetary Systems
FUSE	Far Ultraviolet Spectroscopic Explorer
GI	Guest Investigator
HST	Hubble Space Telescope
IMF	Initial Mass Function
IOTA	Infrared-Optical Telescope Array
IR	Infrared
IRAS	Infrared Astronomical Satellite
ISM	Interstellar Medium
ISO	Infrared Space Observatory
IUE	International Ultraviolet Explorer
JPL	Jet Propulsion Laboratory
LMC	Large Magellanic Cloud
MidEX	Mid-sized Explorer
MO&DA	Mission Operations and Data Analysis
NAS	National Academy of Sciences
NASA	National Aeronautics & Space Administration
NGST	Next Generation Space Telescope
NICMOS	Near Infrared Camera
NTOT	New Technology Orbiting Telescope

OSI	Optical Stellar Interferometer
PI	Principal Investigator
POINTS	Precision Optical Interferometer in Space
QSO	Quasi-Stellar Object
SIM	Space Interferometry Mission
SIRTF	Space Infrared Telescope Facility
SISWG	Space Interferometry Science Working Group
SmEX	Small Explorer
SOFIA	Stratospheric Observatory for Infrared Astronomy
SSB	Space Studies Board
STIS	Space Telescope Imaging Spectrograph
ST ScI	Space Telescope Science Institute
SUSI	Sydney University Stellar Interferometer
TOPS	Towards Other Planetary Systems
UV	Ultraviolet
UVOIR	Ultraviolet-Optical-Infrared
VLA	Very Large Array
VLBI	Very-Long-Baseline Interferometry
VLT	Very Large Telescope
VLTI	Very Large Telescope Interferometer
WFPC2	Wide Field Planetary Camera 2

I. A Vision for the Future of Ultraviolet-Optical-Infrared Astronomy from Space

1. Astronomy: Its Rewards for Science and Society

In the second half of the twentieth century, public funding for science in the United States has soared, reaching levels that are without precedent. The return on this investment—in biology, geology, geophysics, paleontology, physics, chemistry, as well as astronomy—has been substantial. In fact, in those fields in which fundamental questions remain, there has been stunning progress in understanding nature. To our society, to those individuals whose labor and sacrifice supports these endeavors, the benefits have been a more comfortable, healthy life, and an inspirational series of accomplishments in the human pursuit of understanding the world and our place in it.

U.S. astronomy has been a fortunate beneficiary of public support. There is no better example of this patronage than the support for the Hubble Space Telescope (HST), a monumental enterprise that required the dedicated efforts of thousands of scientists and engineers for the prime years of their careers. The HST is one of the most costly projects ever undertaken for the purposes of basic science: the capital investment plus the costs of operations and periodic refurbishment over HST's lifetime is approximately equivalent to that of an aircraft carrier or a few large hospitals. Over the same period, and by no coincidence, public and private investment in other aspects of astronomy, including advanced ground-based optical and radio telescopes and other orbiting observatories, has skyrocketed. The end of the twentieth century has truly become a golden age for astronomical research.

Unlike many other sciences, astronomy's principal contribution to society cannot be reckoned or justified in terms of increases in the Gross

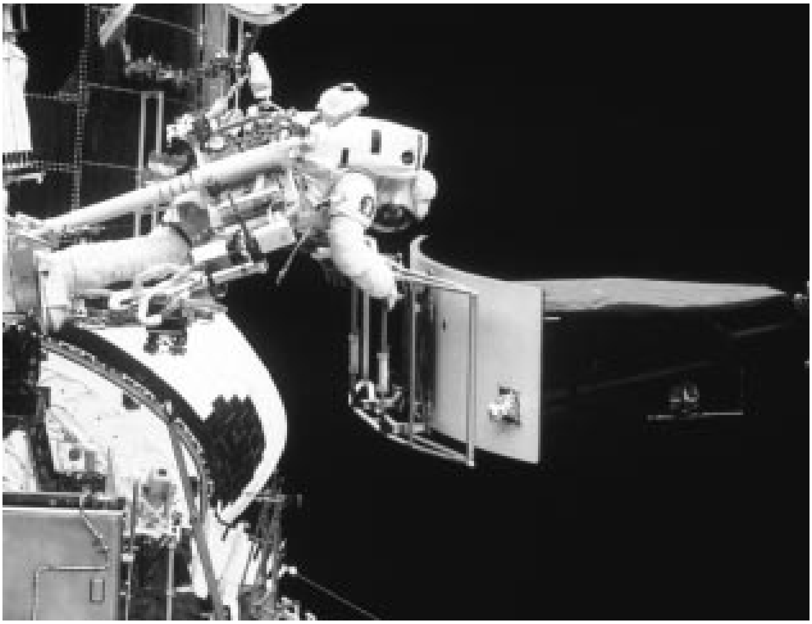
Domestic Product or improvements in the health of average citizens. While it may come as a pleasant surprise to learn that even in these areas astronomy makes important contributions, such as the recent application of pattern-recognition techniques developed by astronomers to the problem of detecting early-stage tumors in mammograms, such benefits are not the principal factor in our nation's support of astronomical research. Astronomy's real contribution and its remarkable appeal owe to its resonance with a basic human preoccupation from time immemorial—to look into the sky and ask "What is this?" and, further, to seek to know "What is my relation to this?" It is in the nature of humans to be curious; astronomy nourishes that curiosity about some of the basic questions of human existence.

Astronomy is inspirational. Of all the sciences, it remains the most accessible and approachable, capturing the imagination and engaging the sense of wonder of people whose daily activities may be far removed from academic or intellectual pursuits. Within our society are calls for a reinvigoration of respect for learning and love of knowledge, aspects of our culture that have been crucial to our success as a nation. In this effort astronomy has a vital role to play: by introducing young people to the intellectual and spiritual rewards of science, astronomy produces an effect that far exceeds its share of the scientific enterprise.

These are early years for the HST, but not too early to access its impact, anticipate its finite lifetime, and plan for the mission's scientific progeny. The scientific payoff of HST for astronomers and astrophysicists is proving to be profound. Great strides are being made on long-standing problems of a fundamental nature: the size, age, and history of the universe. and the physics of exotic events, such as black holes, supernova, and quasars. Pictures taken with the HST, such as the Hubble Deep Field on the cover of this report, have provided direct, unambiguous evidence for the central idea of modern astronomy—that the universe has evolved from a very different state, a hot and dense plasma left by the Big Bang, to the much cooler world of galaxies and stars we see today. By looking far out into space, and thus back in cosmic time, the HST has demonstrated with the immediacy of a simple picture how different the universe appeared when it was half its present age. The HST has made it possible for anyone to see and understand the concept of an evolving universe.

Recently, the HST has produced a remarkable picture of the Eagle Nebula, a region where new stars, and probably new "solar systems" are being born. The picture captures in clear three-dimension relief towering columns of gas and dust driven by the energy of stellar birth. The resemblance of the picture to an Earth thunderstorm has alone been sufficient to capture the public's imagination; even greater astonishment is registered when people are told that the towers are a light-year high.

But the HST is more than a camera that takes dazzling pictures. It is a powerful scientific instrument that produces solid, quantitative results.



The Hubble Space Telescope demonstrates the importance of observations from space, with a clarity of its wide-field images which cannot be matched from within the Earth's atmosphere. Moreover, the HST and other telescopes in space are required for observation of wavelengths that are absorbed by the terrestrial atmosphere.

Arguably the most important mission for the HST is a final resolution of the cosmic distance scale: how big, and how old, is the universe? By isolating and studying individual stars in galaxies 50 million light years away, the HST is providing the first convincing evidence that the age of the universe deduced from the conventional cosmological model is measurably younger than the oldest stars in the galaxies themselves. This may prove to be the most important result in cosmology in the last half century. If made compelling by further HST observations, this result will lead to a major revision of cosmological theory, including a fuller understanding of the Big Bang model. It is the kind of startling, unanticipated result that drives forward our understanding of the universe.

We believe that such scientific bounties confirm the value of the HST. But precisely because of the considerable cost of building, operating, and maintaining such a facility, and the difficult financial constraints facing us for the foreseeable future, scientists must address squarely the worth of the HST to the public. The Committee was charged with anticipating the major astronomical missions that will follow the HST. Therefore, we took it as part of our task to reflect on the question of what the appeal has been, and is likely to be, for possible future missions. We recognize that we are entering a period when public resources for research may become more scarce than at any time since the beginning of the post-World War II "science boom." This, we think, is an opportune and appropriate moment to review the social contract between science and its patrons, the public. The public must be heard and attended. The commitment to science education, in particular, has to find new expression through the work that scientists do with front-line tools such as the HST. We have a responsibility to do more than report our findings through the media and in college classrooms. Our new goal must be to cultivate a wider audience with which to share the unfolding marvels of the universe. We believe this is not as difficult a task as one might think, as it requires changing the attitudes and behavior of a small group—astronomers—rather than a huge, already receptive group—the public.

Despite its broad utility for the astronomical community, the HST is still a highly specialized instrument utilized by a minuscule segment of society. No such enterprise can expect unanimous endorsement in a population as diverse as that of the United States. It is, then, particularly gratifying and significant that such a large fraction of the public has received with enthusiasm reports of the HST's successes in advancing knowledge on the frontiers of astronomy and astrophysics. This great appeal of astronomy may have several sensible explanations. We have found it particularly useful to consider two overall themes in research that have broad appeal to the public: (1) the quest for the exotic, and (2) the quest for place and origins.

Fascination with the exotic, things that appear to be, based on our Earthly experiences, bizarre aspects of nature, is a well developed human trait. Tales of the fantastic fuel our curiosity and fortify the search for novelty and adventure. We see evidence in all types of media—television, films, print, even in the gross excesses of tabloid journalism—that the quest for the exotic, a signature of a creative society, still thrives. A fortunate manifestation of this interest, as far as astronomers are concerned, is the way the public devours accounts of quasars and neutron stars, of massive black holes in other galaxies, of a supernova exploding 170,000 years ago whose light only now reaches us, and of the search for the strange dark matter dominating the universe. These unearthly happenings lift people's spirits above the mundane. They revive our spirits by recalling our lives of discovery as children, when we saw the world as new and full of promise.

Our other quest is as old as civilization, perhaps as old as our species. Comparing the ancient myths of many peoples tells us that the question "Where did we come from?" is one as cogent and profound as we can pose. For an answer, we have continually looked to the sky—the great unknown that has always figured prominently in human ideas of origins and destinies. A remarkable triumph of twentieth-century astronomy is the demonstration that this notion is true: that our origin, and perhaps our destiny, lies among the stars. The idea has already captured the imagination of perhaps a billion people. Many have not found it easy to follow our scientific findings, but very few have missed the point. This realization is perhaps why we find so many modern tales linked to space: a trip to the bookstore, or a glance at a movie listing or television schedule, is enough to convince any of us that, increasingly, great themes of human existence are being projected into space. Our physical journeys into the cosmos may be generations in the future, but our minds already live in the Space Age.

It may be decades before the discoveries of our time are absorbed by the majority of Earth's people, but many are following our journey closely. Many more will join in the years to come. What inspires them, as it inspires so many scientists, is the possibility of retelling the story of our origins, as we now read it from the sky, a story told in the relatively new language of science. The Big Bang is a concept that now has currency with many people in our society, and, for the first time, the word "galaxy" evokes an image in the minds of many who hear it. That the Sun was born when the Milky Way galaxy was already old, that new stars are yet to be born, that life itself appeared and evolved on Earth as a part of an longer-evolving universe—these are concepts that are gradually becoming part of our cultural vocabulary. Such concepts may appear at odds with certain traditional versions of the origins story, but most people find little conflict between the goals of religion and science in searching for our origins and satisfying the human need to understand the world and our place in it.

As the HST enters its prime years of scientific productivity, it can already lay claim to exciting discoveries that have captured the public imagination, in arenas of both “origins” and “exotica.” Crisp pictures of the planets and their moons, the other members of our solar system, forge a mental image of our tiny corner of the universe. Jupiter is ripped by cometary bombs and people watch, told that the same kind of event on Earth, sixty million years ago, appears to have dispatched the dinosaurs and thus made possible the development of our own species. With these Jovian fireworks scarcely over, the HST has only to swing to the left, it seems, to take pictures of galaxies “at the edge of the universe,” seen as they were at a time before the Earth itself was born. Feeding our appetite for the exotic, the HST unmasks massive black holes buried in the centers of galaxies, watches stars explode and quasars blast, and digs for clues to the identify of the stuff that holds the universe together, but cannot be seen.

These two fascinations with exploration and origins also motivate astronomers and astrophysicists. Many of our colleagues are driven by a passionate curiosity to understand the laws of physics, particularly as they are manifest in the extreme environments of the cosmos. To a surprising extent, astrophysics has become a major frontier within the larger discipline of physics. For many, the desire to understand a piece of nature, no matter how small, how specific in its behavior, or how limited in its influence, is the fuel of their creative engines. Taking a different perspective, many astronomers consciously identify their investigations as part of a larger endeavor, one that we acknowledge as a fundamental human ambition. We seek, as did our ancestors, to tell the story of our origins, to describe our place in the universe and to understand how events transpired to lead to it. Scientists are as likely as anyone to find it remarkable that most of the atoms in our body once resided in a star gone supernova, while their colleagues revel in the details of the microphysics that made such an event possible. Scientists are motivated to some extent by both desires, of course, but we believe it is helpful to acknowledge these two aspects of why we are excited by what we do.

It is the goal, then, of the HST & Beyond Committee, to broaden the discussion of what the HST is all about and to explain why we ask for the public’s continuing support for missions that follow: not one, but many, missions and not just for the coming decade, but for a campaign of astronomical exploration stretching well into the twenty-first century. We seek to articulate what are the long-term goals of our science, and to explain how the HST has been just one step, albeit a giant one, in reaching those goals. We know that the HST will answer some questions and greatly inform our understanding of others. However, for our long range goals, even with our limited vision of the present, we recognize that we have a long way to go beyond the HST. This reasoning is *not* simply that, as it is often phrased, 'HST observations will lead to more new questions than it will answer.' Indeed, we expect to discover with the HST genuinely new phenomena about whose nature we cannot even guess. However, we base our case for extending the scientific mission of the HST, and for ambitious follow-on missions perhaps stretching into the middle of the next century, on those problems that we can already define and conceive of studying to the point of solution. These are the questions of our origins: *How did the cosmos and our galactic neighborhood turn out this way, and are there other places like Earth where there is life?*

We select these goals for the future of space astronomy: the observation of remote *galaxies in formation*—the assembly of matter and the first generation of stars in systems that will have evolved into galaxies like our own Milky Way, and the detection of *Earth-like planets* around other stars and the search for *evidence of life* on those planets. These are goals worthy of a civilization.

As we will describe, pursuit of these goals will not end in a decade, or perhaps even in two or three. It will require tools that we can foresee but not yet construct, and success will demand cleverness and economy. The advanced technology to be developed will serve our society in its pursuit of a higher standard of living. But, most importantly, the steps to be taken will allow future astronomers and astrophysicists to satisfy the shared human fascination with novel, exotic manifestations of nature's variety. They will lead to a better understanding of our place in the universe of time and space, and further our quest to understand our origins as the result of the universe's own evolution.

2. Goals for the Twenty-First Century: Seeking Our Origins and Exploring the Exotic in Nature

2.1 Visiting a Time When Galaxies Were Young

Our modern ideas concerning the birth and evolution of the universe began with the fundamental observation by Edwin Hubble in 1929 that galaxies are receding from each other with speeds that scale in direct proportion to their separation. Physicists were quick to grasp the startling implication of this result—that the universe was much denser, and possibly much hotter, in the past. The notion of an evolving universe that, instead of being "eternal" had an origin and a destiny was one of the major revolutions in scientific thought.

Today the "Big Bang'' model has become much more: it is a paradigm whose extraordinary power became apparent when it anticipated the discovery of the *cosmic microwave background* — the dominant, pervasive, radiation field in the universe. The Big Bang model also predicts that the expansion of the universe should be incrementally older than the oldest stars (whose ages are determined by using computer models of stars to interpret observations of star clusters) and the oldest chemical elements (whose ages are ascertained through the physics of radioactive decay). Although precise measurements of these ages remain a challenge, the approximate agreement of the three, of which we are certain, is an extraordinary result that only the Big Bang model accommodates naturally. Furthermore, in impressive detail, the Big Bang model explains the abundances of the light elements deuterium, helium, and lithium (and the lack of production of heavier elements) as the result of nuclear reactions in the primeval plasma during the first few minutes of the expansion.

In the last decade attention has focussed on understanding how structure formed in the universe, how a smooth sea of hot particles and light expanded and cooled into the clumpy, wispy structure of galaxies and their stars. Here, the Big Bang model predicted that sensitive measurements of the cosmic microwave background would reveal slight undulations in mass-energy density, which grew through the concentrating action of gravity into the galaxies and clusters of galaxies we see today. In one of the most remarkable observations in the history of science, NASA's Cosmic Background Explorer (COBE) satellite has detected the largest of these embryonic ripples. In the next decade, a major activity for astronomy from space will be detailed measurement and analyses of these earliest observable structures in the universe. From these studies we expect to learn much about the physics of the Big Bang—for example, a possible test of the idea of an epoch of inflationary expansion that links cosmology to the realm of elementary particle physics. And it is in this epoch that the seeds are sown for the formation of galaxies, the principal building blocks of today's universe.

Some tens or hundreds of millions of years after these faint waves appeared in the sea of hot matter, the first generations of stars formed. The universe became enriched with the heavy chemical elements produced by these stars—carbon, oxygen, nitrogen, calcium, silicon, magnesium, iron. Early construction began of what would become the giant galaxies: bright orbs and vast pinwheels a hundred thousand light years across, populated by hundreds of billions of stars. Sophisticated models of the mechanics of galaxy formation, carried out with supercomputers, suggest that galaxies were built through the agglomeration of many subunits. However, the physics of stellar birth has proven sufficiently difficult to model, in particular, the dynamic collapse from, and the return of energy to, the turbulent medium of gas and dust from which stars form. Insufficient understanding of this and the other complex physical processes that ruled during this early time are likely to continue to frustrate our attempts to predict from first principles the process of galaxy formation. On the contrary, it is much more probable that observations of galaxy birth will teach us a great deal about the fundamental physical processes involved. For this reason, observations of this critical epoch in the history of the universe, when the seeds were sown for all subsequent steps, is essential for the advancement of the field and our understanding of how we came to be. With empirical data and vigorous support for the theoretical astrophysics research that is required for its interpretation, the question of how galaxies formed and evolved in the early universe is likely to be finally answered.

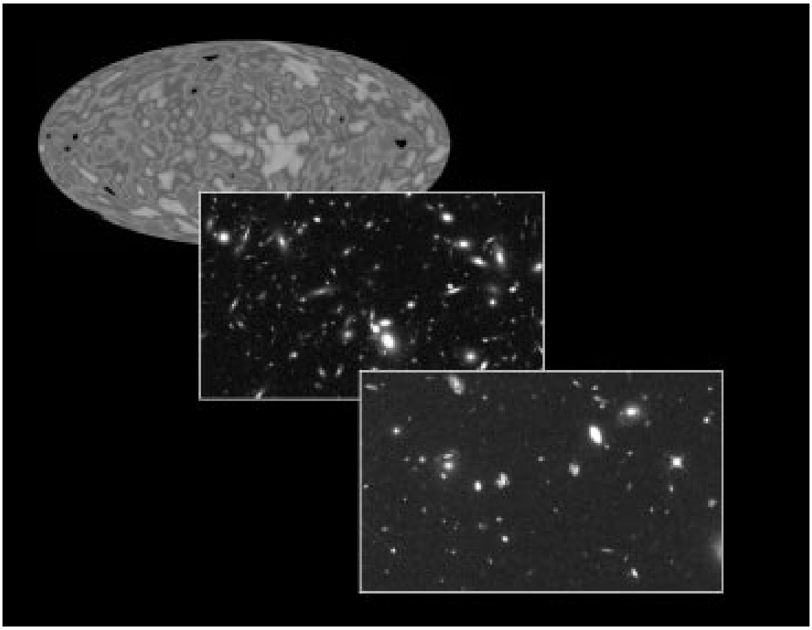
Identifying and studying galaxies in the act of formation is the culmination of much of twentieth century astronomy. It was only during this century that astronomers learned that the Milky Way is a galaxy of a hundred billion stars, and that myriad other galaxies occupy the universe beyond the Milky Way. Applying twentieth-century physics, astrophysicists answered age-old questions of what stars are and why they shine. They assembled the basic facts of how stars are born, how they die, and how they build, through the energy-releasing process of nuclear fusion, the heavy elements that make up our world. Astronomers learned that the Milky Way galaxy is a complex system of generations of such stars, their distribution (galactic structure) and motions (galactic dynamics) offering evidence of its complicated history. The chemical enrichment of our galaxy has proceeded through its Interstellar Medium (ISM), the gas and dust floating among the stars, and astronomers have studied the composition and physical properties of this component. Such processes as stellar birth in dense “molecular clouds,” and the death of stars in violent supernova explosions, have been found to be rich in hydrodynamical phenomena. Just as with our attempts to understand “weather” on Earth and on other planets in the Solar System, these processes of energy exchange between stars and the ISM, and the complex behavior of the often chaotic systems that result, challenge our understanding of nature’s basic physical laws.

In the course of these investigations, astronomers have discovered dark matter, the majority constituent of the universe, still of unknown character. The role of dark matter in galaxy formation is poorly understood, but undeniably crucial. Also found are quasars, those galaxy centers where prodigious energy release results from the accretion of stars and gas onto massive black holes. The connection of quasars to galaxy formation is almost certainly a strong one, but it remains basically unexplained.

In summary, even with considerable understanding and volumes of specific information of these and the many other elements that go into the making of a galaxy, the complexity of this process has prevented us from describing how galaxy formation took place. By looking back in time and actually witnessing the event—by studying the coalescence of all these elements as they occurred—we will integrate and begin to perfect our knowledge of a crucial step in our origins.

The HST has shown us examples of galaxies as they were in the remote past, within a few billion years of the Big Bang itself. Yet, we need to look back even further. Distances in the universe are so vast that even light takes billions of years to cross it. Thus, by looking far into space, we can look back in time to witness directly the events that astronomers collect under the term “galaxy formation.” Even before the HST, work had begun using ground-based telescopes to find, among the billions of incredibly faint smudges of light from distant galaxies that cover the sky, those that might be early ancestors of common galaxies like the Milky Way. The HST has already made a crucial contribution by providing pictures with ten-times greater spatial resolution than is possible with ground-based telescopes, revealing for the first time the structure and form of these nascent galaxies. The Hubble Deep Field, reproduced on the cover of this report, is a stunning illustration of the depths to which space can be probed. With the future Advanced Camera for Surveys (ACS), the HST will gain in both resolution and sensitivity, allowing it to sample even fainter and younger galaxies.

However, for the most distant (and, therefore, youngest) objects that



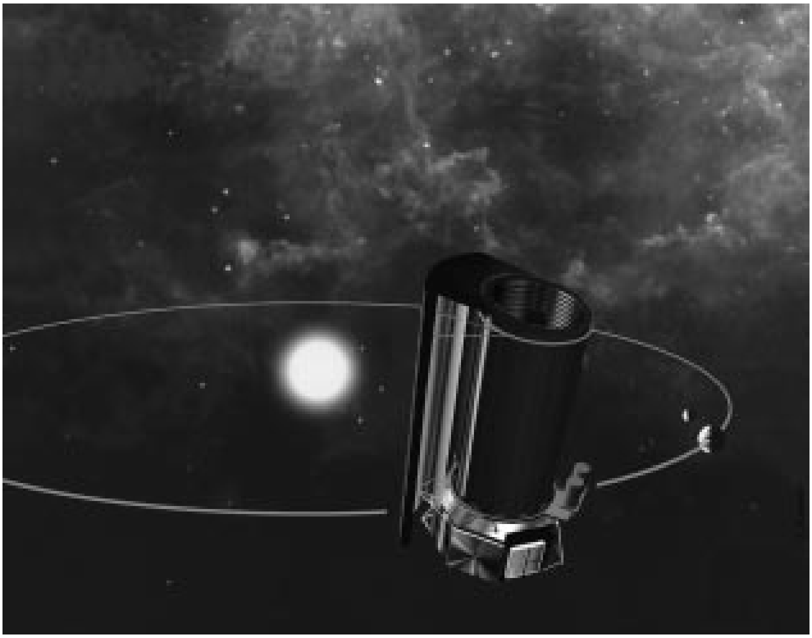
The figure depicts our view of the universe on three very different epochs scales. The background image is a view of the entire sky at microwave wavelengths obtained by the COBE satellite. All contributions due to foreground sources have been removed to show the small undulating signal thought to be due to intrinsic anisotropy in the universe at very early times (z=2000). (Courtesy Goddard Space Flight Center and the COBE Science Working Group.) Variations such as these were the seeds to current day galaxies and clusters of galaxies as seen in the foreground HST optical image (z=0-3) with 20,000 times higher magnification. (Courtesy Rogier Windhorst of the HST Medium Deep Survey (MDS) team.) The middle image simulates a comparable portion of sky as seen at 2.2 microns with a passively cooled 4m telescope. An open universe is assumed, as well as no mergers and an initial epoch of formation at z=10. The simulation includes different star formation rates, initial mass functions, and morphologies for elliptical, spiral, and irregular galaxies.The brightest elliptical galaxies can be detected at redshifts z > 8 corresponding to the onset of star and galaxy formation. (Courtesy Myungshin Im also of the MDS team.)

we have been able to identify, the present HST can provide only a rather crude pixelation of what is likely to be a very complex structure. Furthermore, it is generally acknowledged that the objects studied to date are the most luminous examples of galaxies at early times, so that studies of more typical objects, like the ones that would evolve into galaxies like our own Milky Way, will be harder still. Even ACS will not provide the detailed pictures and spectroscopy that will be needed to understand the earliest steps in assembling a galaxy. This limitation is primarily due to the fact that the HST is an optical rather than an Infrared (IR) telescope. We are looking in the wrong place. As the story goes, we are searching for our keys under the lamppost because the light is better there, even though the keys were lost in the dimly lit park. Because primeval galaxies are extremely far away, the expansion of the universe gives light from them a very large redshift, which means that the visible light of their stars has been shifted to near- and mid-IR wavelengths ($\lambda \approx$ 1-10 μm) when it reaches us.

The HST will acquire a near-IR capability with the planned installation of the Near Infrared Camera (NICMOS), but, because of the high background level produced by a "warm" (room temperature) telescope, NICMOS has inadequate sensitivity in the critical region beyond 2 μm. The high thermal background from telescope and atmosphere is also the reason that ground-based telescopes will be of limited effectiveness for this research. NASA is also planning the construction of the Space Infrared Telescope Facility (SIRTF), a cooled telescope with very low thermal background. SIRTF is likely to revolutionize the study of star and brown dwarf formation, make substantial contributions to the study of planetary systems, and provide the first systematic look at the birth and early evolution of Milky Way-like galaxies. However, the relatively small aperture (0.85 m) of this telescope will severely limit the ability to image nascent galaxies. We conclude that new instruments will be needed to gain detailed understanding of the ancestors of galaxies like our own Milky Way that HST and SIRTF will glimpse in the young universe.

What, then, will be needed to reach our goal of studying the process of galaxy formation as it occurred for galaxies like our own? We believe that a giant step will be a radiatively-cooled space telescope of 4m or larger filled aperture. This new telescope would be extremely capable in the near- to mid-IR, particularly at 1-5 μm, where much of the light of these forming galaxies is expected. In this regime it would be far more powerful than any other extant or planned facility on the ground or in space. Furthermore, it appears technically feasible to extend, with modest cost, the operating range of this telescope to visible wavelengths shortward to at least 0.5 μm, which would provide a powerful and broadly-useful capability for maintaining and in some cases extending the most important aspects of the HST mission.

A telescope with a 4m filled aperture would have a spatial resolution of about 0.16 arcsec at 2.5 μm, corresponding in a very distant galaxy to approximately 500 pc, the size of a very large complex of star formation.



The Space Infrared Telescope Facility (SIRTF), to be launched early in the next decade, will be a cornerstone in the understanding of the evolution of galaxies, stellar birth, and planetary material around stars. The 0.85 m telescope will be cooled via a mixture of radiative and passive cooling, while the instruments will use a new generation of infrared array detectors. This will permit an overall performance orders of magnitude superior to previous space- or ground-based facilities.

From what we have learned both from theory and HST observations, it is likely that the images of such early stages of a galaxy's life will show a chaotic and blobby pattern. It will be difficult to decide, based on pictures alone, what are the relations between various parts of the image, especially which regions exemplify the galaxy formation process, and how the pieces are coming together. A more complete understanding will require determinations of the rates of star formation in the various pieces, estimates of the abundances of heavy elements that have been synthesized, and measurements of the internal and relative motions of the gas and star clouds that will eventually combine to form the galaxy. Such data can only come from a sophisticated imaging spectrograph taking full advantage of the spatial resolution of the telescope to obtain the spectra of various galaxy components with high sensitivity and a precision of tens of kilometers per second. Carrying out such difficult measurements with efficiency will require a multiobject or "integral" format where many spectra can be obtained simultaneously.

As we describe in more detail in section 7, the Committee believes that the kind of observations just described will not be accomplished with ground-based telescopes. Ground-based telescopes will continue to be essential tools in the study of galaxy evolution; our programs could not succeed without them. However, we believe that even with the anticipated "adaptive optics" packages that will bring image size in the near-IR down to the 0.1 arcsec regime, ground-based facilities cannot do the job alone. Ground-based observations suffer from the bright emission from the Earth's atmosphere, from substantial atmospheric opacity at certain IR wavelengths, and, as for the HST, from thermal emission of the optical system at wavelengths greater than 2.5 μm. These are sources of noise that overwhelm the faint signals from extremely distant galaxies. A cooled space telescope of substantial aperture with a sophisticated imaging spectrograph will improve, by orders of magnitude, our ability to obtain redshifts for the faintest, most distant galaxies, and to provide the wealth of spectroscopic information that will address the physics of the nascent galaxies that are identified. Our judgment is that four meters is the minimum aperture diameter required for the task of solving the riddles of galaxy formation—the higher resolution and additional light afforded by a larger aperture would pay substantial dividends in sensitivity and resolution.

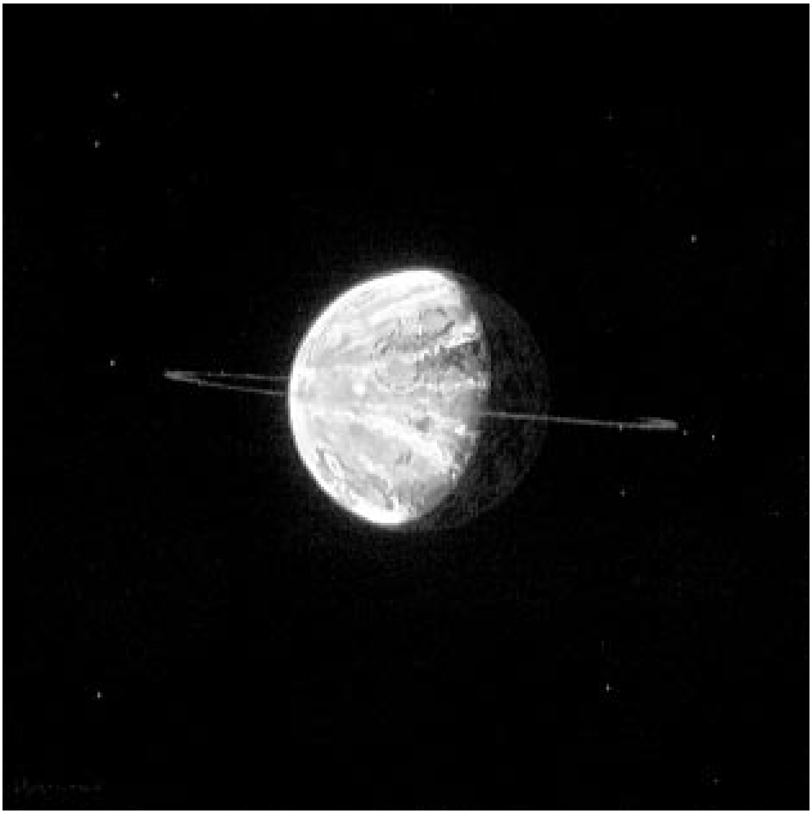
2.2 The Search for Earth-Like Planets and Life

In the last few decades, scientists have been able to explain major parts of the sequence of events leading to life on Earth. An extraordinary range of scientific disciplines has contributed to the list of accomplishments:

(1) Physicists and astronomers have learned that the chemical elements necessary to build a planet and assemble the building blocks of life—carbon, nitrogen, oxygen, calcium, silicon, magnesium, and iron—originated not in the Big Bang, but in the cores of stars through nuclear fusion, a process that has continued over the life of the universe. Astronomers have also made the first observations that indicate the existence of planetary systems around other stars. The likelihood of planets around other stars has captured the imagination of scientists and the lay public alike.

(2) It is now well accepted that the presence of oxygen in our atmosphere at a concentration that allows the development of complex life forms is itself a product of early life forms on Earth, rather than the result

of geological action alone. That is, free oxygen would virtually disappear from the Earth's atmosphere without the presence of life. Even the temperature of the Earth's surface may be regulated by life processes: oceanographers have discovered vast stores of limestone, formed by tiny animals with calcium carbonate shells, which sequester carbon and oxygen that otherwise would dominate our atmosphere as carbon dioxide. Without this reduction of carbon dioxide, the greenhouse effect would raise the Earth's surface to a scorching temperature far too hot to support life, as it has on Venus. Some geologists have further suggested that subduction of sea bed deposits of carbon



This planet resembles Earth in many ways. It has a temperature at which water is liquid, and forms oceans and clouds. Impacts by countless large meteorites have have contributed to its formation, leaving craters, circular continents, and arcs of islands in the oceans. Left-over debris forms a ring of fine particles and small moonlets in orbit around the planet, which will dissipate in time. This world possesses many—and perhaps all—the necessary conditions for life. Painting by William K. Hartmann. Copyright ©1994 by AURA, Inc.

and oxygen, through the action of plate tectonics, a process that may be unique to Earth within the Solar System, could also play a key role in the habitability of our planet.

(3) Paleontologists have recorded periods of mass extinction on the Earth that cleared the way for sudden bursts of biological evolution: there is now considerable evidence that the impact of a comet or asteroid 60 million years ago was directly responsible for the extinction of the dinosaurs and the ascendence of mammals, including, eventually, ourselves. Recent evidence suggests that the presence of the giant planet Jupiter played a principal role in reducing the number of such impacts to the point where life could indeed evolve on Earth without too-frequent catastrophic interruption.

Astronomy has a central role in this burgeoning area of exploration, starting with studies of the Big Bang itself, continuing through the formation of the chemical elements, through the building of galaxies and their star-forming gas clouds, and to the detailed physics of the births and deaths of the stars themselves, which release into space the heavy atomic elements they produce. In seeking to learn how a galaxy, like a vast ecosystem, mixes, distributes, and regulates the production of the material from generations of stars, astronomers are establishing the basis for producing a planet like Earth around a later-generation star like the Sun. Progress is encouraging and steady in understanding the background and framework for the formation of planets themselves.

However, about one vital subject we remain largely ignorant: there has been, up until recently, only one known example of a planetary system around a normal star, and there remains but one example of a life-bearing planet. Many astronomers believe, based on deductive reasoning from these sole examples, that planetary systems, and even life, are common in the universe. Nevertheless, there remains a disappointing and frustrating lack of real data with which to inform what are, at this point, little more than educated guesses and opinions.

The HST & Beyond Committee believes that the time is right to make the search for other worlds like Earth a priority program, a goal of our profession. There has been rising interest within the astronomical community; a number of different approaches are being advanced for the detection and study of other planetary systems. Already, progress has been made. The discovery and study of protoplanetary material in disks around neighboring stars reveals key processes in planet formation. By measurements of the reflex motion caused by orbiting planets, nearby stars are being searched for companions with the mass of Jupiter or larger. The first dedicated searches have produced the unanticipated result that such giant planets may be relatively rare, at least those with orbits comparable to that of Jupiter in our own solar system. Nevertheless, three excellent examples have already been found, including the odd case of a Jupiter-sized planet so close to the star 51 Peg that it could not have formed there, but must have migrated from further out. No doubt there will be a rapid increase in the data on such planets in the next few years. Several groups plan to employ near-IR interferometry and adaptive optics with the new generations of large-aperture earthbound telescopes to study protoplanetary disks and search for large planets. Observations of microlensing events, which can reveal planets by their gravitational influence on the light of a background star, are a promising new way to determine the statistical occurrence of planets, including small planets like Earth.

However, it seems that the detection of "other Earths" around neighboring stars—those close enough to allow further study of planets that might be found—is possible only through direct imaging observations with interferometers in space. As we describe in more detail elsewhere in this report, an early concept of such an instrument envisions four or more telescopes mounted on a rigid frame with minimum separations of about 75 meters. The telescopes would have apertures at least 1.5 meter in diameter and would be optimized for the mid-IR (~5-20 μm), and thus would require radiative cooling in space. Launching this interferometer-type telescope into an orbit that extends nearly as far as Jupiter would provide an enormous gain in sensitivity, by placing it beyond the "glare" of radiating dust (the zodiacal light) in the inner solar system. As described in section 8.6, NASA is in the process of defining a program to accomplish this next step, the Exploration of Neighboring Planetary Systems (ExNPS) program. We encourage and support this effort.

The HST & Beyond Committee finds one aspect of the ExNPS program particularly exciting: the possibility of obtaining spectra of Earth-like planets with a mid-IR, multi-element interferometer. These spectra can be used to search for evidence of life, a unique benefit of direct imaging. Indirect imaging methods might demonstrate the existence of such worlds, but we believe that the ability to take low resolution spectra and search for free oxygen and other "life signatures" in the atmosphere of such a planet is an essential part of the science that needs to be done. We believe that searching for signs of extraterrestrial life, in addition to identifying places where it might exist, is the real goal of this extraordinary research effort.

2.3 Tools for the Jobs

In proposing these two long-term themes for the search for our origins—the birth and early evolution of galaxies and the search for Earth-like planets, as well as continuing astronomy's mission of exploration—the HST & Beyond Committee recognizes the necessity for new capabilities for Ultraviolet-Optical-Infrared (UVOIR) observations from space beyond those of the SIRTF and extended-life HST. Studying the early universe and the process of galaxy formation calls for the construction of a large filled-aperture (4m or greater) telescope operating with low thermal background in the near- to mid-IR. We describe this instrument in more detail below. It is clear that the design and manufacture of this instrument will be very challenging. We are encouraged to believe, however, from consultations with engineers at NASA and at two experienced industrial manufacturers of space hardware, that these steps are within the state of the art, or nearly so, because of what has been learned from the HST, SIRTF, and other orbiting observatories. This is a more favorable situation than occurred in the actual building of HST itself, whose development required steps at or even beyond the state of the art at that time. An emphasis on new technological solutions for both the observatory and its operations is expected to lower the cost of such a mission substantially relative to the HST and, furthermore, to provide greater reliability through increased simplicity. Though this cost-savings remains to be demonstrated, it is very important: the Committee thinks it is imperative that, in addition to developing this new capability, it be developed and operated at a substantially lower cost than the HST. This makes the challenge all the more relevant to the future technological advance of the nation.

The second required capability for meeting our scientific goals in UVOIR space astronomy is interferometry. Space interferometry is crucial for the detection and study of Earth-like planets, as we describe below, but it also is of vital importance to the future of space astrophysics. NASA has initiated the SIM (Space Interferometry Mission, previously known as Astrometric Interferometry Mission, or AIM), which was identified as a high priority in the 1991 National Academy of Sciences (NAS) Astronomy and Astrophysics Survey "Bahcall" report. This program seeks to develop a space-based visible-light interferometer capable of astrometric measurements with an accuracy of better than $\lesssim 10$ microarcsec. As described later in this report, such data would have broad applications to many central problems in astrophysics, such as galactic structure, stellar astrophysics, and the cosmic distance scale. There is also considerable enthusiasm for some imaging capability in this first instrument. The Committee regards the development of full imaging capability at optical wavelengths as an essential goal of space interferometry. It is in this arena that the capability of space telescopes is unique[1], and ultra-high resolution images are required for the most interesting problems, such as viewing the central emitting regions of Active Galactic Nuclei (AGN), accretion and outflow phenomena of Galactic stars, and stellar populations of other galaxies.

Though the specific mission of detecting and studying Earth-like planets may require a relatively specialized instrument, such as a nulling interferometer[2] operating at 10 μm, the technologies needed will have much in common with the development of SIM and other space missions, including the large IR-optimized telescope we propose here. Therefore, the Committee supports a broad development program of interferometric techniques, starting with a simple test-bed interferometer, and perhaps leading to a suite of station-keeping, large aperture telescopes. Eventually, the technology of low-cost, large-aperture telescopes will combine with the development of space interferometry in the development of extremely powerful space observatories with capabilities far beyond anything we now possess.

As we discuss in section 3, there are other areas of unique capability in UVOIR space astronomy that are not supported by our three core recommendations, such as additional access to the ultraviolet below the atmospheric cutoff of 3000 Å, which is crucial for studies of star formation and the ISM. We believe that such important goals can be met, for the foreseeable future, by the upgrade and continued operation of the HST, and by smaller, specialized missions. For this and other reasons, we believe it to be very important that the HST continue in operation past the year 2005.

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[1] Adaptive optics techniques for ground-based telescopes, which can compensate for the affects of Earth's atmosphere in the IR, are relatively ineffective at optical wavelengths.

[2] A system that combines the light of the target star in such a way as to cancel its contribution, so that nearby, fainter objects can be detected.

In summary, it is our judgment that the promise of UVOIR space astronomy in the twenty-first century will best be met by extending the operating lifetime of the HST beyond 2005, by the construction of a passively cooled, 4m or larger filled-aperture space telescope capable of imaging and spectroscopy, and by the development of space interferometers with astrometric and imaging capabilities at visual and IR wavelengths. The HST can continue to make unique, valuable contributions to astrophysical research, particularly in the ultraviolet, for the foreseeable future. The astrometric observations that can be carried out with even a modest-size interferometer will provide revolutionary improvements in measurements of distances to stars and star systems and thus have a major impact on a wide range of astronomical problems. An IR-interferometer could detect Earth-like planets around neighboring stars, which would have profound impact on both the scientific community and the general public. The cooled 4m space observatory will reveal the early evolution of galaxies and serve the general astronomical community as a general-purpose observatory, in the tradition of the HST.

II. The Proposed Program

3. Ultraviolet-Optical-Infrared Space Observatories for the New Millennium

3.1 Summary of Recommendations

Central to the Committee's recommendations were a handful of key considerations: (1) consistent with our charge, we considered only Ultraviolet-Optical-Infrared (UVOIR) missions, and only those that would be built after 2005, a time frame beyond NASA's plans (when this committee was set up) for facilities for space astrophysics; (2) the proposed missions were to take advantage of unique capabilities afforded only by space observations; (3) the wavelength regimes emphasized would be among the richest for astrophysical investigations; (4) proposed missions would be major advances over current or planned capabilities and could not be duplicated by more modest, specialized satellites; (5) where appropriate, missions would be operated as general-purpose observatories, accessible to the broad community of astronomers; (6) major technologies required to carry out these missions should be within or at the state of the art, with any technologies presently well beyond the state of the art approached by a series of technically innovative, but less ambitious steps; and (7) new projects should also be evaluated with regard to their contribution to enabling subsequent generations of space observatories with enhanced capabilities. We point out that the HST & Beyond Committee was not charged, nor was it constituted, to make technical judgments about the specific configurations that might best meet our scientific goals. The following recommendations should serve as a framework for other panels better suited to develop the best engineering strategies.

Our two grand scientific themes, and continuation of a world-class program in astrophysics, lead to the following recommendations to NASA:

(1) *Extend the lifetime of the Hubble Space Telescope (HST).* Under the circumstances described below, extending the operation of the HST be-

yond 2005 would provide unique ultraviolet and wide-field imaging capabilities, and provide continuity of high scientific capability through the time that a successor can be launched. A plan for capable, though efficient and low cost, operation of the HST through this period, presumably without planned repairs or upgrades, should be developed and implemented.

(2) *Build a large, filled-aperture IR-optimized observatory.* The near- to mid-Infrared (IR) will be the next major wavelength regime to be investigated with observatory-class missions. A cooled telescope optimized for the wavelengths $\lambda \approx 1$ - $5\ \mu$m, with 4m or larger aperture, is the key tool for studying the very high redshift universe. In particular, it will enable the Committee's science goal of studying galaxies like the Milky Way in the process of formation.

(3) *Develop and demonstrate space interferometry.* Interferometry from space is the next major technological step for a variety of important astrophysical problems, such as astrometric measurements capable of fixing distances throughout our Milky Way galaxy and the possibility of imaging the emitting regions of Active Galactic Nuclei (AGN.) The Committee's science goal of detecting Earth-like planets around other stars, and searching for evidence of life on these, may best be implemented via high-angular-resolution observations at mid-IR wavelengths.

3.2 A Sustained Hubble Space Telescope

At this writing, the HST has been operating for two years since the successful refurbishment, and has a planned further lifetime of ten years. The HST is an enormously versatile observatory due to its multiple instruments and on-orbit servicing capability. We foresee no significant space capability beyond 2005 for UVOIR observation, even assuming a very optimistic response to our report. Hence it is likely that the HST will remain a unique resource in 2005.

Our other recommendations do not include Ultraviolet (UV) capability, so the HST would remain unique in this wavelength region. The HST, equipped with Space Telescope Imaging Spectrograph (STIS) and Advanced Camera for Surveys (ACS), and possible 2002 mission instruments, should have excellent, unprecedented UV capability for imaging and spectroscopy, capabilities completely unavailable from the ground or from space with such a large collecting area. Equipped in this fashion, the telescope seems likely to satisfy the projected needs of the UV community for a general purpose instrument, and it is, at any rate, premature to plan a follow-up mission until these substantial enhancements are exploited. As the extended life of International Ultraviolet Explorer (IUE) has shown, the longevity of this instrument should lead to genuinely new science in its later years. We discuss some of these science goals below.

Also extremely valuable is the HST's ability to respond to transient or unforeseen developments. In the 2005 – 2010 window it is likely to be the only UVOIR space instrument able to respond to opportunities to study comet impacts on planets or supernovae, for example.

In order to promote the operation of HST beyond its currently planned lifetime, we recommend a much more economical style of operation beyond 2005. With no servicing or instrument replacements after 2005, a final boost into a higher, long-lived orbit, and a possibly reduced instrument complement and limited modes of operation, the cost of operating the HST could be greatly reduced below its current level. [Approximately 80 percent of the present Mission Operations and Data Analysis (MO&DA) budget for HST is for preparation for upcoming servicing missions and the development of new instrumentation. The remaining annual expenditure for HST operations is approximately $50 million, exclusive of the budget for scientific program support.]

The Hubble Space Telescope is a young observatory, but already, in the 1994 Senior Review, it was ranked first in impact on a science-per-dollar basis. Ground-based telescopes have useful lifetimes of several decades and the HST, with the opportunity for upgrade and repair, is more like a ground-based telescope than a typical NASA mission. We recommend that, within a few years, careful evaluation be made regarding the cost and benefits of an extended HST lifetime by an appropriate scientific and technical review committee. It seems likely to us that the operating budget can be substantially reduced while preserving vital scientific opportunities with the HST in the years beyond 2005. In that case, the lifetime of the telescope should be extended.

3.3 A Large Infrared-Optimized Space Telescope

Extremely sensitive observations over the wavelength range $\lambda \approx 1 - 5\ \mu$m will be essential elements of the "origins" theme, which we propose to guide space astronomy in the next decade: star formation and the Interstellar Medium (ISM), planetary material around neighboring stars, and, especially, the birth and early evolution of normal galaxies. Even including advances currently on the drawing boards, ground and space observatories will reach an impasse as the light from more distant and more rapidly receding galaxies shifts into the thermal IR beyond $\lambda \sim 2.3\ \mu m$. At these wavelengths, both ground-based telescopes and the HST will suffer from the strong thermal emission from their optical systems. Likewise, Earth's atmosphere becomes more emissive and, in common molecular bands, more opaque. Even at wavelengths shortward of $2.3\ \mu m$, there are several bands of high opacity, which reduce the light from faint, distant sources to invisibility. We anticipate that by the year 2000, 10m-class telescopes in both hemispheres, as well as the HST, will be fitted with optical and near-IR cameras, which will extend our studies of the early universe to a redshift set by the $2.3\ \mu m$ thermal limit, or $z = 2 – 2.5$. The angular resolution of a 10m telescope at $2.3\ \mu m$ will be comparable to that of the HST in the optical, assuming the full development of adaptive optics techniques to correct for atmospheric distortion. However, it now seems clear that to reach the epoch of formation of normal galaxies, extremely sensitive observations beyond about $2\ \mu m$ will be necessary.

To achieve extremely high sensitivity beyond $2.3\ \mu m$ and achieve a major improvement in sensitivity, a space observatory is required, as demonstrated by ESA's Infrared Space Observatory (ISO) and, shortly after the turn of the century, by Space Infrared Telescope Facility (SIRTF). Space operation uniquely allows celestial-background-limited observations through significant cooling—to $T \sim 30 – 50$ K—and unimpeded access to the entire near- to mid-IR spectral regime. At temperatures this low, observations with a space observatory to $5\ \mu m$ and beyond will be limited in sensitivity primarily by the telescope's aperture and the reflection and emission from zodiacal dust located primarily interior to the asteroid belt. For this reason, an orbit beyond that of the zodiacal dust is well worth considering. At a minimum, a high-Earth orbit is a practical necessity in order to achieve sufficiently low telescope temperatures through passive cooling.

We emphasize that this wavelength regime covers the continuum emission and many key diagnostic spectral features from a vast number of astronomical objects that exhibit a wide variety of physical processes (see section 10). *As there are presently no U.S. plans for a major space facility optimized for this wavelength region after about 2005, the Committee recommends a large IR-optimized observatory as the highest priority post-HST facility-class mission for NASA.*

We further recommend, and emphasize the importance of, extending the wavelength coverage shortward to about 0.5 μm and longward to about 20 μm, as far as is technically possible and cost effective. (The ability of passive cooling to reach 50 K or below may be the determining factor.) Quasars have been observed up to redshifts of $z \sim 5$; studies of galaxy formation at this epoch would require crucial observations with a short wavelength cutoff of $(1 + z) * 912$Å or approximately 0.5 μm. The performance gain with a 4m space telescope over ground-based telescopes with adaptive optics in the 0.5 - 0.8 μm wavelength regime is enormous. Together with a prolonged operational lifetime for the HST, such a facility would allow very sensitive measurements of spectral analysis over the wavelength range $\lambda \approx 0.2$ - 20 μm, permitting measurements of key diagnostic features over the complete redshift range $1 < z < 10$. Moreover, the technology required for this observatory is important to smaller, more focussed projects in this wavelength range and will be relevant, perhaps even crucial, for construction of a nulling IR interferometer to detect Earth-like planets around neighboring stars, which we discuss below.

The Committee did not evaluate the detailed technical aspects of the numerous post-SIRTF mission concepts for a future cooled space observatory proposed over the past few years. We, therefore, consider our recommendations for this initiative to be only starting points for a detailed study, available to, or part of, the next National Academy of Sciences (NAS) decennial review of our profession. We assume that the appropriate study team will consider both “conventional” technical solutions, as well as more speculative designs, while giving due consideration to cost savings in all areas. A 4m aperture would have a light-collecting area 25 times greater than that of SIRTF and will reach the same point source flux level almost a thousand times faster (section 2.5), and will resolve morphological features at cosmological distances which would not be discernible with the smaller apertures of ISO or SIRTF. Such a mission will truly be a major scientific advance. Furthermore, the scientific capability and technological challenge of this program is a natural opportunity for collaboration between nations, which we include as part of our “policy” recommendations.

The SIRTF project has led the way in indentifying new approaches to reduce the cost of IR space telescopes while maintaining high performance. Several groups, such as those studying the New Technology Orbiting Telescope (NTOT), the *Edison* concept, and the HIGH-Z proposal, have considered the construction of even larger IR-optimized space telescopes.

We are encouraged about the feasibility of such an undertaking by the results of the recent study by the Space Studies Board of the NAS, *A Scientific Study of a New Technology Orbital Telescope* (discussed in the addendum to this section). In this study, U.S. industry showed the scientific community that it is technically feasible to develop and launch a passively-cooled, filled aperture 4m space telescope within the next decade, for a cost well below $1 billion, and possibly as low as $500 million. Segmented or deployable technologies may offer significant technological advantages which should be explored, but the Committee emphasizes the primary scientific requirements: high-quality, celestial background-limited, wide-field imaging and spectroscopy over a wide wavelength range.

We expect that the technical studies and development essential to this mission include: (1) large, very lightweight optical systems and associated structures; (2) active and passive cooling techniques; (3) precision structures and controls; (4) ultra-smooth optical surfaces and materials; (5) advanced, large-format detectors and instruments to take advantage of very low backgrounds; and (6) sophisticated, modest-cost sub-systems, such as pointing controls, telemetry and onboard computing, and power generation. In addition, it is crucial that choices of design elements lead to an operation style that is efficient and economical, in order to maximize funding for science analysis and minimize funding for routine functions. This includes the development of "thinking spacecraft" that make decisions and adjustments on orbit, onboard systems that reduce data reduction tasks, and advanced, open architecture for control systems that is user friendly and easily upgradable. Because the Committee recognizes the scientific capabilities and potentially high science per dollar of long-life operation, as demonstrated by IUE and HST, we recommend a lifetime of ten years as a mission goal.

Addendum: The BMDO/NTOT 4m Telescope

Both ground- and space-based astronomy are benefitting from technology developed primarily for military purposes, notably as part of various 'Star Wars' programs. Indeed, it is difficult to imagine that purely-civilian R & D resources would have been sufficient to support development of monolithic optics, large IR detector arrays, very low noise circuitry, high-capacity launch vehicles, and affordable pointing/tracking systems.

One of the most prominent of these efforts has been the Ballistic Missile Defense Organization (BMDO) NTOT developed by Lockheed and

Itek. The original goal of this program was to produce a 4m-class, optically-precise orbiting telescope in the cost range of approximately $500 million or less. The primary mission of this observatory was technology demonstration in support of space-based laser weapons, with little or no adaptation for astronomical research in the original program. To our knowledge, no mission of this type has been launched, but the basic optical components exist and have been offered for consideration to the scientific community, should financing become available from some source. This system was recently evaluated by a task group of the Space Studies Board of the NAS, chaired by Michael A'Hearn. The design and technology evaluated by the Space Studies Board are now some years old and such a mission was concluded to have important—albeit limited—value for astronomical purposes, as currently configured.

Some of the limitations identified in the Space Studies Board report include:

(1) little or no systems analysis to assure that individual components could be combined into an operational astronomical observatory;
(2) possible scattered-light problems with the optical design and support system;
(3) a pointing system that may not be able to track sufficiently faint stars;
(4) possible limitations in the active control of the primary mirror;
(5) possible limitations in the onboard software; and
(6) a program of 'building to cost,' which for NASA and the space science community is a significant departure.

Finally, from the point of view of the Committee, the BMDO/NTOT is optimized for wavelengths shortward of that which we consider necessary to carry out the central scientific programs we have highlighted. Therefore, the design would have to be significantly altered to permit significant radiative and/or active cooling.

Nevertheless, in many respects, the BMDO/NTOT program, technology, and design have impressed a number of people and review committees, including ours. At the very least, this concept suggests that precision telescopes substantially larger than the HST can be built and launched for budgets well below those estimated not long ago for a large successor to the HST. We note that the cost of the BMDO/NTOT and its operation has been a subject of debate. However, there is no doubt that this mission would cost well below the few billions of dollars which once was commonly estimated as the cost of a large post-HST observatory. That is, the 'cost curve' has apparently been broken. The scientific and engineering communities should continue aggressive efforts to reduce the cost of future space missions.

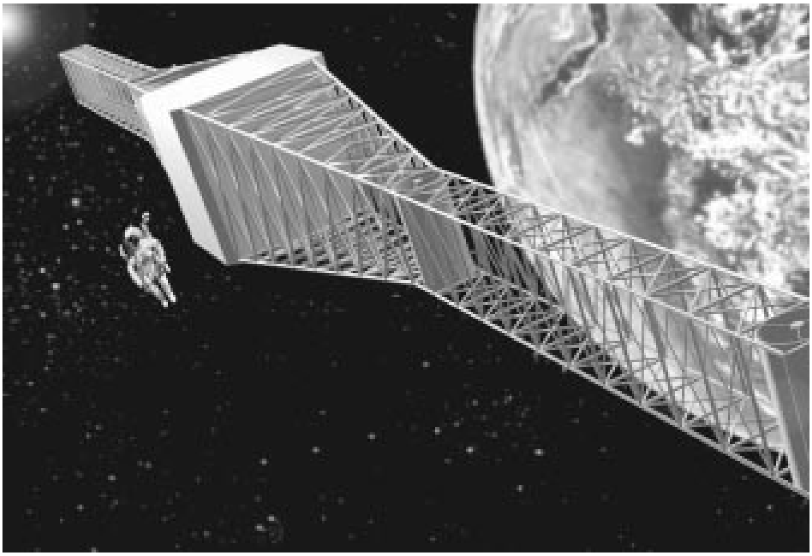
3.4 Development and Demonstration of Space Interferometry

The 1991 NAS Astronomy and Astrophysics (Bahcall) Survey report recognized the importance of interferometry in the future of astrophysical research, and, in particular, the role of space interferometry in providing astrometric measurements of stellar positions to an accuracy of 10 microarcsec or better. Such a capability would enable true parallax distance measurements to virtually any locale in the Milky Way galaxy, for any sufficiently bright star, which for the first generation of astrometric telescopes would be about 20th magnitude.

A recent NASA review of New Astrophysics Missions Concepts selected two proposals, Optical Stellar Interferometer (OSI) and Precision Optical Interferometer in Space (POINTS), as two priority programs selected for further study. These instruments are proposed to have the kind of capability described above. A significant difference is that the OSI configuration could develop a modest imaging capability, which we feel is important for future generations of space interferometers. The HST & Beyond Committee recognizes the importance of this program, previously known as Astrometric Interferometry Mission (AIM) and now renamed Space Interferometry Mission (SIM), and has consulted with the Space Interferometry Science Working Group about these programs. The HST & Beyond Committee supports this effort as an important goal for both science and technology.

The Committee supports the search for Earth-like planets and evidence for life on such planets as a primary theme for future astrophysical research. The testimony we have heard and the documents we have reviewed suggest that high-angular-resolution observations at mid-IR wavelengths are the most direct road to those goals. The choice of wavelength at about 10 μm is driven by the enhanced contrast between an Earth-like planet and its parent star, and the presence of many molecular features in this region of the spectrum.

A multi-element nulling interferometer would appear to have the capability to find such planets and collect spectra with sufficient resolutions to classify each planet's atmosphere. It is suggested that the presence of carbon dioxide signals the presence of a terrestrial planet, the presence of



Long-baseline spatial interferometry will be required for the search for terrestrial planets, to separate the light of the target planet from that of the central star. Such an interferometer will have to work at thermal infrared wavelengths, where absorption bands of abundant molecules are found.

water vapor indicates that the planet resides in a "habitable zone," and the presence of free oxygen (e.g., ozone) may actually indicate biological activity. The HST & Beyond Committee was very impressed by the prospects of such a study.

We describe in this report a particular configuration that has been proposed to make such observations. It is one that may have limited application to other areas of astrophysics, because of its configuration, the size of its apertures, its operating mode in an orbit at several astronomical units from Earth, but there is much in its development that would be in common with more general interferometric instruments as well as larger filled-aperture telescopes.

4. Policy Considerations

4.1 Background and Motivation

The HST and Beyond Committee has as its membership the first generation of scientists to have experience with space observatories. This perspective permits us to consider some of the policy issues that will affect major facilities in the post-HST era. Our primary motivation for the following discussion is to increase the accessibility of space astronomy to researchers in many fields, while considering carefully mechanisms by which future space missions may be carried out more efficiently, and to enhance public access to the space science enterprise.

4.2 A Balanced Space Astronomy Program: Observatories, "PI Only" Satellites, and Dedicated Missions

As a result of the success to date of the Great Observatories program, astronomers are becoming adept at exploiting the research opportunities offered by NASA's fleet of broadly capable telescopes. These missions span a wide range of wavelengths with general-purpose instrumentation suitable for a wide range of observational programs, as reflected in the breadth and popularity of the Guest Investigator (GI) programs, notably for HST. We recognize that the Great Observatories have in part been justified by a handful of important, relatively specific research programs, but both the knowledgeable public and the research community appreciate the exciting results from the diverse program of GI observations. There is every reason to expect that future NASA astronomy programs will be most productive if they provide access to space observations for a very broad community, resulting in high visibility of this research to the public. This probably can be achieved only via general-purpose missions. We believe that future premier missions, such as those as we have discussed earlier in this report, will be essential to major revelations about the universe, and will benefit from wide applicability to astronomical research.

The Committee also acknowledges the importance of facilities with greater focus, such as "PI class" satellites developed and operated by modest groups, and "dedicated missions"—facilities of any size that have a narrow scientific goal and capability. Such missions are important to a balanced space astronomy program, as significant improvements in our understanding of the cosmos have often been realized with a comparatively modest expenditure of human and financial resources. The Cosmic Background Explorer (COBE) mission is an excellent example of the success of this kind of program, and the Committee commends NASA on its attempts to increase the number of more modest missions, while improving cost control and devolving management and operations responsibilities to the lowest feasible level.

The HST & Beyond Committee, therefore, encourages NASA to consider 'access to space by a wide scientific community' as a serious "figure of merit" for future funding. It is unrealistic to expect significant community involvement in lower-cost programs, but the Committee is concerned that excessive concentration on smaller and more focused programs may be undertaken at the expense of broadly relevant scientific research. To some degree, accessibility to archival data bases will satisfy this need. However, this alone is insufficient to pursue understanding of cosmic processes in their diverse settings.

It is primarily for this reason that the Committee recommends future UVOIR programs that will operate as observatories, with active GI opportunities.

4.3 Breaking the Cost Curve: Major Science Within a Budget

The exploration of the Solar System, via sophisticated studies by spacecraft traveling to the planets, and of the cosmos, via observatories orbiting the Earth, will historically be viewed as one of the great periods of human exploration. Our understanding of the fundamental processes of nature, the structure and composition of the universe, and of humanity's place within it has advanced more rapidly than at any other time in history. We expect that our depth of understanding of nature will continue to increase as a consequence of the missions recommended to NASA in this report.

However, for the foreseeable future, ambitious space science missions will have to be undertaken within a declining budget. This does not mean that powerful, general-purpose space observatories must be relics of the past. On the contrary, the Committee believes that powerful space astronomy missions can be achieved in this climate through vigorous pursuit of cost savings in development and operations. However, we stress that such economies should not be achieved by cutting essential components of a mission, such as collecting area, lifetime, or key instrumentation. The Committee is impressed with NASA's progress in maintaining key capabilities of missions while reducing costs by application of new technologies, novel approaches, simpler operations, reductions in duplicated work, and assignment of responsibility to smaller groups and individuals. In astronomy programs, improvement is probably most apparent in recent SIRTF and Far Ultraviolet Spectroscopic Explorer (FUSE) designs and in operations of HST and Extreme Ultraviolet Explorer (EUVE). This is the direction that must be followed in order to carry out the post-HST missions we propose here.

Our Committee was told that a major cost driver for the HST was its link to the Space Shuttle and the human space flight program. It is clear that servicing missions and replacement instruments have been crucial for the brilliant success of the HST. However, it may not be practical for future space telescopes, such as the ones which we are proposing, to be launched, maintained, and upgraded in this way. Foregoing these capabilities offers the possibility of reduced costs, but substantially increased risk. This further highlights the necessity for greater simplicity and reliability of future space telescopes. When scientific and technical requirements are incompatible with low-Earth orbit, servicing and upgrading are at any rate impractical. Relating specifically to matters of economy and present feasibility, we note that a fixed 4m aperture is the largest size that will fit within the shroud of a presently available, expendable launch vehicle. However, the possibilities of deployable apertures and future launch vehicles with larger shrouds mean that larger apertures can and should be considered.

4.4 Space Astronomy as an International Adventure

The Committee shares the view expressed in numerous other advisory committee reports and NASA policy statements, that international collaboration should be a high priority for the agency. The potential benefits from such collaborations are significant: (1) expertise from a broader resource base; (2) cost savings to an individual country; and (3) a contribution to a more humane world via a deepening of international cooperation in shared discovery. In any case, increasing economic, cultural, and technical interdependence makes international cooperation virtually inevitable. Consequently, there is much to be gained, especially for programs on the scale recommended in this report, by exploring new opportunities and innovative administration of international programs. The Committee encourages NASA to continue seeking partnership at all levels outside the US.

Strong, autonomous national programs have a central role to play in future NASA astrophysics programs. The Committee identified some key activities for which a purely national approach may be the most attractive: (1) small or moderate “PI class” or dedicated projects, which generally have a limited scope; (2) programs for which national security is an issue; (3) opportunistic missions, for which the particular talents or technology is uniquely available within the United States; or (4) projects in which there is a large political uncertainty, making the program vulnerable to funding reversals.

We are obliged to recognize that the U.S. budgetary process, which allows for frequent review and consequent changes in direction for NASA programs, has discouraged some of our international partners from future collaboration. Opinions vary about the necessity and wisdom of Congressional intercession, but at any rate the process is unlikely to change in the foreseeable future. Therefore, if international participation on new major space missions is to be realized, it will be important to mitigate the negative effects of our process. Working to fixed budgets is one step that can minimize repeated Congressional intrusion. We also believe that, in order to reassure partners and to lay the groundwork for more fruitful collaborations, NASA must be more willing to debate and negotiate requirements, rather than simply offering opportunities for international partners to join in on set missions.

The Committee was not able to compile generic rules for international programs; such opportunities will perhaps need to be considered on a case-by-case basis. However, for the three central recommended missions in our report—extension of the life of the HST, a large, filled-aperture IR/optical observatory, and the development of space interferometry—opportunities for international partnership are extensive. We believe such opportunities should be aggressively pursued.

4.5 Sharing the Adventure: Inviting the Public Along

Space astronomy is in the unique position of satisfying essential national needs: exploration and creativity, technological innovation, and educational excellence. NASA programs have high visibility and credibility in classrooms around the world; where possible, this exposure should be sustained and enlarged. Scientists and engineers are essential to this effort, as we are the people who carry the message of space exploration to a curious—and tax-paying—public. The Committee supports the growing awareness within NASA and among our colleagues concerning the importance of communicating the excitement of science to the public.

We further encourage NASA and our peers in the astronomical community to involve the public even more actively in astronomy missions. It was not the charge to this Committee to address the issue of public participation in the exploration of the universe. However, as beneficiaries of government spending, we believe we owe our fellow citizens the opportunity to be more involved in this adventure. The Committee heard engaging ideas for NASA-sponsored art competitions, for active participation by teachers in airborne astronomy, for 'young engineers' competitions to design new astronomical observatories, and for increasing participation by lay people in electronic communication on space science issues, just to cite a few examples. Opportunities for a fuller participation in space exploration by a larger portion of society is highly regarded by this Committee.

In the same vein, the Committee supports NASA's commitment to broadening its relevance to segments of American society that have historically been underrepresented in space science: the poorer inner cities and rural, small-town America. Unfortunately, it is not difficult to find large areas of the country where citizens, although taxed to support one of humanity's great adventures, are severely limited in their access to the results of their generosity—with out-of-date educational materials, poor access to technology and research facilities, and a sense of the irrelevance of higher learning to their lives. The Committee consequently commends NASA on its growing recognition of its mission to involve all of American society.

4.6 Maintaining America's Technological Base

The end of the Cold War, coupled with other constraints on the Federal budget, has led to a decline in available resources in support of technologies that are likely to be required for future generations of space science missions, such as ultra-lightweight optics, detector systems, precision pointing and control systems, and so on. The Committee is concerned that, as a consequence, some crucial engineering capabilities from the mid-1990s might not be available a decade hence, when two of our three recommendations may begin development. Of course, our concern is more broad than simply NASA's space science program, as decline in the country's engineering talent will no doubt have a deleterious ripple effect throughout the nation. However, other institutions and organizations have made the economic case elsewhere for robust support of high technology. Here we merely note the fact that space science has benefited greatly from governmental support for both military and civilian technology. Should the support of these activities continue to decline, space science missions of all sizes are likely to become more expensive and/or less capable than otherwise would be the case.

NASA makes a vital contribution to U.S. technological research, on a number of frontiers that few U.S. industries will encounter. It will be important to our technological advance as a nation that it continues to do so, even in an era of declining budgets. In this regard, we believe that NASA might compensate partially for declining technology funding by focusing its remaining resources more narrowly on likely future space missions. 'Curiosity-driven' basic technological research has an important role to fill, as speculative ideas are as important to engineering as they are to science. The Committee was not constituted to evaluate the efficacy of NASA's continuing engineering effort. However, we encourage the space agency to judge this activity regularly in the light of the science missions that it expects to support in future years.

5. Coda: First Steps Toward a Next Generation Space Telescope and the Searches for Extra-Solar Terrestrial Planets

As we finished this report, NASA began a preliminary study of a large single-aperture optical-IR telescope dubbed the Next Generation Space Telescope (NGST). In its baseline description, this mission is very similar to that which we have recommended as the next workhorse space astrophysics mission after the HST. We consider the first major 'programmatic' milestone for all major new space astronomy initiatives will be the NAS decennial review, to begin in a few years. Therefore, we urge the NGST teams to work vigorously to produce a mission description in time for consideration by the NAS. We commend the NGST activity for seeking innovative working relationships between NASA centers, academia, and industry, in order to take advantage of the best capabilities of each. At present, the NGST studies emphasize segmented optical systems of various kinds. The Committee does not have the expertise to evaluate this technological choice, although it seems to be the case that such designs have the distinct advantage of 'growth potential,' to allow even larger apertures than we have here considered plausible. This obviously should be vigorously pursued, while at the same time a careful eye should be kept on the near-future goal of a feasible, cost-effective design to be presented to the scientific and engineering communities. We reiterate that a central goal of our recommended large-aperture mission is background-limited mid-IR sensitivity with moderate (0.2 arcsec or better at a few micrometers) angular resolution. This requirement may be difficult to meet with some proposed optical designs and orbits. Similarly, although the Committee emphasizes the compelling justification of high-redshift science for a telescope optimized at $\lambda \approx 1 - 5\ \mu$m, we also urge general-purpose capabilities for both shorter and longer wavelengths. The NGST team should at all times be encouraged to think of their designs as satisfying a very broad and active scientific community.

Also, in 1995, NASA began, through the office of Charles Elachi at the Jet Propulsion Laboratory, an investigation of techniques that might be used to find and study Earth-like planets around neighboring stars. The program, ExNPS, began with the selection of a Jet Propulsion Laboratory (JPL)/Space Telescope Science Institute (STScI) team led by Charles Beichman, and two competitively selected teams led by Roger Angel and Robert Reasenberg. These three teams developed independent "roadmaps" toward the goal of identifying and studying Earth-like planets, which were presented to an "Integration Team" composed of twenty-one scientists and engineers. The Integration Team met in July to hear presentations of the three approaches and to synthesize a single roadmap for presentation to a "Blue Ribbon Panel" chaired by Charles Townes in October. The Townes committee has subsequently reported its recommendations to NASA. The proposed program is wide ranging, including a realignment of the SIM that would accomplish many of the goals for astrometry and the first steps in imaging interferometry that are discussed in our report. Moreover, chief in its plans is the development of a four-telescope nulling interferometer with 1.5m apertures and a 75m baseline, operating in the mid-IR. We provide a brief discussion of some of the scientific considerations that have motivated this design strategy in section 8.6 of this report. The challenges to constructing and operating such an instrument are formidable, but the HST & and Beyond Committee welcomes NASA's bold initiative of investigating the technological and scientific challenges in this mission that we regard as fundamental and essential.

The HST & Beyond Committee is gratified and encouraged that NASA has already begun to explore the programs we have advocated. However, the Committee also recognizes the importance of a wider involvement of the astronomy and astrophysics community in the discussion and debate of goals and priorities. A step in this process will be a preliminary review of all space astrophysics missions by the Committee for Astronomy and Astrophysics (CAA) of the NAS, in preparation for the upcoming NAS decennial survey. The CAA will consider UVOIR space astronomy within the broader context of astrophysics missions over the entire wavelength spectrum. Broad participation by the community in these NAS and NASA activities is essential for the realization of our scientific aspirations for space astronomy in the first decades of the twenty-first century.

III. The Scientific Case for the "Origins" Program

In Part III, we develop in more detail the scientific foundation and program for the two grand goals introduced above. We begin in section 6 with a discussion of the probable state of Ultraviolet-Optical-Infrared (UVOIR) space astronomy in the year 2005. In section 7 we present a program to study galaxies in the early universe and discuss the requirements for a 4m cooled space telescope that can advance this field, and address why the required observations cannot be done as effectively, if at all, from ground-based facilities. In section 8 we discuss scientific issues relating to the search for other Earth-like planets, including technical matters such as the role of interferometry and a representative mission.

6. Defining the Future: The Landscape of U.S. Space Astronomy in 2005

It is useful to place this report in the context of the foreseeable UVOIR astronomical facilities and interests in the first decade of the next century. Since the lead-time for major astronomical instruments is long, one can project with some confidence the tools that will be available in the year 2005.

In ground-based activities, the suite of U.S. telescopes will include the northern and southern Gemini 8m telescopes, available to all U.S. astronomers, and a host of independently-financed facilities, such as the two 10m Keck Telescopes. From an international perspective, there are the Subaru 8m (Japan) and the 4 x 8m Very Large Telescope (VLT, Europe). We expect that U.S. astronomers will be able to compete for time on well-instrumented telescopes with collecting areas ranging from 30 to 75 square meters. A list of the U.S. facilities may be found in the McCray Report.

Two key features of these telescopes, in addition to their impressive apertures, are their low Infrared (IR) emissivities and high angular resolutions. In particular, the goal of the Gemini project is to produce, with adaptive optics, images of 0.1 arcsec 50 percent encircled energy at 2 μm over about a one arcminute field. (Impressive as this is, it is important to remember that this is a factor-of-two poorer resolution, over a comparatively small field, of a 4m aperture in space observing in visible light, a wavelength regime for which ground-based adaptive optics systems are likely to be ineffective.)

We are encouraged that at some times, under good conditions, Gemini and other large telescopes will reach this target. However, the difficulty of achieving comparable image quality increases markedly for wavelengths less than 1 μm, and the size of the correctable field shrinks to a few arcsec. For these reasons, we are discouraged from believing that ground-based telescopes will routinely compete in resolution with space-based instruments of 4m or larger aperture at wavelengths less than 1 μm. The future for space telescopes is less assured. The Hubble Space Telescope (HST) design lifetime carries it to 2005. Reaching that "advanced age" will require at least three servicing missions: in 1997 to install the new science instruments Space Telescope Imaging Spectrograph (STIS) and Near Infrared Camera (NICMOS), and solid-state recorders; in 1999 to install advanced gyros and Advanced Camera for Surveys (ACS), and to boost HST to a higher orbit; and in 2002 to install at least one science instrument and replace the batteries and solar arrays. ACS will include wide-field capability in the form of two 2048×4096 charge-coupled devices (CCDs) optimized for the I-band (roughly 200×200 arcsec), and one 1024×1024 CCD high-resolution camera critically sampled for visible wavelength imaging. The spectrograph of choice for most problems will be STIS, which will have a 52 arcsec long slit capability [in the visible and near Ultraviolet (UV)] and spectral resolution $\lambda/\Delta\lambda = 10^3$, 10^4 and, for the UV, $\lambda/\Delta\lambda = 10^5$, modes. STIS will cover the wavelength range 1175 Å to 1 μm. NICMOS will provide near-IR imaging capability at 0.8 – 2.5 μm over fields of 10 – 50 arcsec, depending on sampling, and low-resolution ($\lambda/\Delta\lambda = 100$), slitless spectroscopy using a grism. The European Space Agency (ESA) has indicated some interest in providing an HST instrument for the 2002 refurbishment mission. An integral field (or imaging-slicing) spectrograph has been suggested as a valuable way to use HST's superb spatial resolution to produce spectral maps of small, complex objects. Another promising candidate is a coronographic camera capable of exploiting the HST's nearly perfect optical system, in order to image regions requiring extremely high dynamic range, such as searching for planets of Neptune-size or larger around neighboring stars, or probing the distribution of gas and stars in the central regions of a quasar.

In the area of IR space astronomy, two very important missions are ESA's Infrared Space Observatory (ISO), just beginning its planned 1-1/2 year mission, and NASA's Space Infrared Telescope Facility (SIRTF), now planned as a 2-1/2 year mission for launch in 2001. These missions provide the first IR observatory capabilities in space, including imagers, spectrometers, and photometers. ISO includes two small arrays among many other capabilities and is well matched to following up the pioneering survey work done with the Infrared Astronomical Satellite (IRAS). SIRTF uses high performance large format arrays across the entire IR region, both to follow up on ISO and to enable new breakthroughs such as study of: (1) galaxies like the Milky Way to redshifts of $z = 3$ to 5; (2) IR ultraluminous galaxies to $z > 10$ if they exist there; (3) planetary debris disks down to systems similar in scale to the Solar System; and (4) brown dwarfs, including determination of the brown dwarf component of the Galactic halo.

In the area of ultraviolet space astronomy, the flight of the Far Ultraviolet Spectroscopic Explorer (FUSE) will be an important mission. According to the current schedule, FUSE's flight phase will have been completed before 2005. The scientific drivers for the FUSE mission are: (1) the measurement of the Deuterium isotopic fraction (D/H) along many lines of sight in our galaxy and in other galaxies; (2) the characterization of the hot "coronal" gas in the disk and halo of our galaxy; and (3) the determination of the intensity of the diffuse extreme UV background intensity through the measurement of the "Lyman-α" 304 Å transition of twice-ionized helium in the Lyman-alpha absorption line systems for $2 < z < 3$. FUSE was designed to cover the key spectral range 912 Å to 1216 Å with resolution $\lambda/\Delta\lambda \sim 30{,}000$ and an effective collecting area of 50-100 cm^2. It is limited, for practical purposes, to objects brighter than $m_V = 17$. The combination of limited bandpass and high resolution make this a rather special purpose instrument. There is reasonable expectation that FUSE can accomplish the first two goals, and perhaps even the third. Although the measurement of the ionized helium absorption is the most challenging and may be better suited to a lower dispersion facility, many on this committee also regard it as the most interesting.

Space Interferometry Mission (SIM, [formerly Astrometric Interferometry Mission, or AIM]), a program for space-based interferometry , has had as its primary goal to make visible-light astrometric measurements of $V < 20$ mag point sources at the 10 microarcsec or better level. This program is entering a phase of intense study and appears to be evolving rapidly at the time of this writing; an increased emphasis on imaging capability seems to be emerging. A reasonable forecast is that the SIM spacecraft will be launched around 2005. More detail about this program is given in section 7.

The Stratospheric Observatory for Infrared Astronomy (SOFIA) will be a 2.5m aperture IR-optimized telescope flown at stratospheric altitude in an airplane. As such, it is not strictly a space mission, though it will achieve many of the advantages of one by rising above much of Earth's atmosphere and opening up many absorbed spectral bands in the mid- and far-IR. This will make SOFIA a powerful tool for studying high brightness temperature sources, like forming stars and active galactic nuclei. The relatively high thermal background from its optics and relatively modest angular resolution will, however, limit its sensitivity and therefore its usefulness for detailed study of fainter sources like distant galaxies.

With the exception of SIM there are no other major UVOIR astrophysics space missions planned to extend past 2005. It is hoped, of course, that during this period Small Explorer (SmEX) and Mid-sized Explorer (MidEX) missions will provide a steady, albeit punctuated, flow of data from specific, small-scale space missions.

It is not easy to predict what fields of scientific activity will be exciting in 10 years, but such an attempt is required. We begin by considering the status of the HST in 2005, by providing a brief status report on the three "key programs" undertaken with the HST. As part of the time allocation process for the HST, three ambitious programs were specially selected to receive large allocations of observing time over several scheduling cycles in order to optimize the chances for success of these highly regarded programs.

One key program is the investigation of the low redshift analog of the unseen clouds of gas that produce Lyman-α absorption-line systems along the lines of sight to high redshift quasars. One of the most surprising early findings of the HST has been the detection of several such systems toward the relatively nearby quasar 3C 273. The analysis of ground-based data for many such sightlines to high redshift quasars has shown that the number density per unit redshift can be approximately described by the relation $dn/dz \propto (1 + z)^{\gamma}$, with $\gamma \sim 2$ for $z > 1.7$. An extrapolation of this relation to $z < 1.7$ would predict that such systems would be very rare on sightlines to nearby quasars; nevertheless, the HST results show that these clouds or similar absorbers persist to the present day. Efforts to identify galaxies with absorption systems at low z have been often unsuccessful: the relationship of Lyman-α clouds at low z to galaxies or otherwise observable structures remains unknown. It would seem that sufficiently bright targets for these kind of observations will be exhausted relatively soon, and the main goals of this program will have been accomplished. At this time, we think it is difficult to make a case for a much larger aperture UV telescope to push these observations to many more (fainter) nearby quasars. On the other hand, this key project marks only the beginning of metallicity and dynamics studies of the stronger absorbers, for which high resolution spectroscopy on the extended mission for the HST will be a minimum requirement.

A second key project is the establishment of the cosmological distance scale. The work with the HST's Wide Field Planetary Camera 2 (WFPC2) to find Cepheid variables in spiral members of the Local Supercluster promises to revolutionize this subject. Cepheid distances to particular galaxies that have been the sites of type Ia supernova are providing an alternative route to the Tully-Fisher relation (and similar methods) for tying local distance measures to the far-field Hubble expansion. Considering the progress to date and the added power that ACS will provide, the Committee is cautiously optimistic that the value of the Hubble constant (H_0) will have been measured to better than 10 percent by 2005. However, direct measurement of other important cosmological parameters, such as the deceleration parameter q_o or the cosmological constant Λ_o, are likely to be topics of continuing observations that may indeed require an advanced space telescope. Should the current studies of H_0 lead to an expansion age substantially less than that of the oldest Milky Way stars (about 14 Gyr), a shift in focus to other cosmological parameters and stellar age determinations may well occur.

The third of the initial key projects for the HST is the Medium Deep Survey, whose main focus is the investigation of galaxy evolution at redshifts up to $z \sim 1$. The deep images of this survey, along with HST observations of galaxies in distant clusters, indicate a significant change since $z \sim 1$ in structure and stellar populations, and in distributions of luminosity and surface brightness. It is our judgment that a combination of ground-based near-IR large aperture imaging and spectroscopy, and imaging with the HST, will be effective in defining the geography of the universeat $z \sim 1$, and the spectral and morphological evolution of galaxies from that epoch. We think, however, that for redshifts $z > 1$, investigations with the HST will continue to be limited to very luminous, relatively rare objects, and that intensive studies of the universe at the likely epoch of galaxy formation, $z > 2$, will require new space missions.

We conclude, then, that these initial key projects of the HST are likely to have been executed to the limit of HST's ability by the end of its scheduled lifetime in 2005. There will be many important science programs and, new key programs now being defined that will continue to make the HST an essential tool of astronomical research. However, technical limitations will eventually blunt efforts to make further major steps in our study of the universe. This suggests that it will be important beyond 2005 to reduce the costs of HST operation substantially, in order to maintain its unique capabilities for as long as its productivity remains high, while at the same time freeing resources for investment in future facilities. Because the current operating costs of the HST are dominated by preparation for repair missions and new instrumentation, it is practical to consider a "no further service" future for HST, and a simplified observing program. Present budgeting suggests that this could reduce operation and maintenance costs to approximately 20 percent of their current level, exclusive of funding for science analysis. In order to avoid the additional cost of subsequent Shuttle missions, the final servicing mission would need to boost the HST into a sufficiently high orbit that a long orbital lifetime is achieved.

The exploitation of the HST for UV observations has only just begun. The new instruments, STIS and ACS, will offer vast improvements in imaging and spectroscopy, which will support a variety of programs. New instruments, now being discussed for possible installation in 2002, would add further capabilities. It is in this manner, we believe, that HST can best be utilized in this streamlined, lower-cost mode. The prolonged life of the International Ultraviolet Explorer (IUE), with its high ratio of scientific productivity per dollar, is an appealing precedent.

With regard to the search for and study of other planetary systems, particularly the discovery and investigation of other planets like Earth, we expect that the ISO and SIRTF will make significant contributions to the study of "protoplanetary disks" in star-forming regions, and that the early stages of stellar birth—protostellar objects, bi-polar flows, the interaction of stellar winds with protoplanetary disks, etc.—will be well investigated by 2005.

We further expect that ground-based techniques, such as near-IR adaptive optics, visible interferometry, and precision radial velocity monitoring, will have conducted a deep search for Jupiter-like planets around nearby stars. A helpful step in this search might be the outfitting of HST with a coronograph during the 2002 repair mission. While such investigations are crucial, the techniques employed to accomplish them fall far short of the goal of detecting and studying Earth-like planets. We think that this goal will be largely unaccomplished by 2005, awaiting the construction of a multi-aperture infrared interferometric space observatory.

It is more difficult to speculate about the degree to which many other HST programs, such as the history of chemical enrichment, the physics of star formation, and the evolution stellar remnants like neutron stars will have reached their present goals. We expect that dozens of important, presently approved programs will have led to new questions about basic physical phenomena, and it is difficult to predict to what extent the hardware of 2005 will be able to make the required follow-on observations. It seems clear, however, that the search for a planet like the Earth, and the study of the formation and early evolution of galaxies like the Milky Way, will not be far advanced by 2005. Moreover, the post-2005 future will not include a major facility for high resolution (≤ 0.1 arcsec) imaging at optical and near-IR wavelengths over large fields. Such images will be of tremendous potential value in the search for proto-solar systems and young galaxies, and, the Committee believes, are needed to provide crucial and unique data for the numerous specific investigations that today account for the present vibrancy of astrophysical research.

7. Galaxy Formation in the High-Redshift Universe

7.1 Current Capabilities

Observations and analysis of data using current and near-future ground and space-based observatories is likely to increase significantly our understanding of the local and recent ($z \leq 1$) universe. From space, the HST is able to investigate galaxy morphology to redshifts of about 1 for nearly-normal objects. Within a few years, the near-infrared NICMOS camera on the HST should permit morphological studies of brighter galaxies to redshifts possibly as great as 3. On the ground, the new generation of 8 – 10m telescopes will obtain essential spectroscopic and redshift information, perhaps to B = 25 – 26 ($z \approx 2$). Furthermore, deep broadband visual photometric surveys might reach B ≈ 30 in several regions of the sky. Such work will prepare the way for the next generation of observatories to push back the frontier of our understanding of the cosmos to redshifts $z > 2$, into the era of formation of the material universe.

Although no galaxy being born has been unambiguously identified, there are good circumstantial arguments to believe that spheroidal formation takes place at redshifts in the range of $z \approx 2 - 5$. SIRTF is likely to confirm this. In addition, it is possible that there was at least detectable stellar birth as early as $z \sim 10$, which must have been a critical period in a universe of proto-galaxies. Finally, spectroscopic observations reveal that at least some heavy elements have been injected in the Interstellar Medium (ISM) at the earliest time for which suitable observations exist.

Unfortunately, nature seems to have conspired to make increasingly difficult the study of highly-redshifted, very young galaxies from the ground at visual ($\lambda \approx 0.35 - 0.8\ \mu$m) and near-infrared ($\lambda \approx 0.8 - 2.5\ \mu$m) wavelengths. In the first place, the peak in the spectral energy distribution for galaxies with a wide range in ages occurs at $\lambda = 0.4 - 1\ \mu$m in the rest frame. This maximum appears at wavelengths that suffer dramatically larger backgrounds due to airglow for $z \gtrsim 0.5$ and to telescope/atmospheric thermal emission for $z \gtrsim 1.5$. Even worse, at wavelengths about where the thermal background is becoming severe ($\lambda \approx 2.3\ \mu$m), the atmosphere begins to become increasingly opaque. As a consequence, our view from the Earth's surface of the birth and early evolution of galaxies is murky under the best of circumstances. Overcoming airglow emission has been a major justification for the NICMOS instrument, while reducing the thermal background has justified cooled space missions to survey the infrared sky [Infrared Astronomical Satellite (IRAS), Cosmic Background Explorer (COBE) and, to provide the first space observatory capabilities in the infrared, ISO and SIRTF. These missions will furnish critical information to guide the studies with the larger telescope recommended here.

7.2 Fundamental Questions in High-Redshift Astrophysics

Over the past decade, with improving observations of the local universe and a preliminary understanding of conditions in the early universe, a consensus has emerged on the fundamental questions concerning the formation and early evolution of galaxies.

(1) *What was the sequence of mass accumulation (including dark matter) in the central regions of galaxies?* Was this a sudden event or the result of steady accretion? Over what z range did this occur? Did very early

mergers play a dominant role in shaping the masses of galaxies? Were there triggering events?

(2) *What was the sequence for disk formation?* Was this also a sudden event or was the accretion of matter into disks a prolonged process? How did this depend upon the type of (pre-existing?) central core in the galaxy?

(3) *When and where were the first heavy elements formed?* What was the mass spectrum of the first generation of stars, and how did the chemical enrichment process proceed through generations of type I and type II supernovae? What was the nature of the early interstellar medium?

(4) *What was the role of Active Galactic Nuclei (AGNs) in galaxy formation and early evolution?* What conditions in galaxies led to the dramatic peak in comoving density of AGNs at $2 < z < 3$ and what, if any, role did AGNs have in changing the evolution of their host galaxies?

(5) *How do the answers to Questions #1 – #4 depend on environment, galaxian mass, state of the primordial gas, and dynamics?*

(6) *What were the conditions (e.g., temperature, density spectrum, elemental composition) in the universe between $z \approx 1000$ and $z \approx 5$?*

(7) *Were there any "precursor events" (e.g., supernovae) that preceded full-blown galaxy formation?*

7.3 Generic Capabilities That Are Required

Study of the early universe is complex and is usefully thought of in four phases that pose distinct observational and interpretational challenges. This sequence bears directly upon the types of astronomical missions that will be most useful to the scientific community.

(1) *Detection*. Sources in the early universe are typically extremely faint, highly redshifted, and may be obscured by dust. Merely detecting light from the most abundant objects is technically challenging, and we are hampered by not knowing exactly for what we are searching.

(2) *Identification.* Because the spectral energy distribution of radiation from primitive objects may not to any significant degree differ qualitatively from the radiation of normal processes occurring in much nearer galaxies, objects at very high redshift must be identified from amongst a potentially much larger number of superficially similar objects at lower redshifts. It may reduce ambiguity to search for specific selection criteria, such as strong radio emission or diagnostic emission lines. This will in turn require the greatest sensitivities.

(3) *Characterization.* Once objects are detected and identified, they must be characterized as to their constituents (e.g., stellar populations, gas, metallicity, dust, "dark matter") and their dynamical state (e.g., relaxed or in pre-virialized condensations, interactions/mergers). *Although some information can be gleaned from morphology at a single wavelength, spectroscopy of multiple spectral features provides key diagnostics as to the physical conditions of the gas (chemical abundances, temperature, density) and the stellar populations.* Spatially resolved spectroscopy can be used to analyze the dynamics of the system, while continuum photometry over the widest possible range of wavelengths can be used to determine the overall energetics of the system, and can lessen the effect of dust obscuration on the appearance of objects.

(4) *Placement in context.* The primary aim of studying the universe at high redshifts is to understand the origin and evolution of typical galaxies such as our own. Given that many observable properties of distant galaxies are transient, it is essential that ways be found to relate the population seen at one epoch to the population seen at some later epoch and different physical location within the universe. This analysis requires at the outset large statistical samples so that a fair sample of the universe can be taken at each epoch. In addition, those properties of the galaxies that are the least transient and therefore provide the best accounting from epoch to epoch must be measured. Examples of these less transient properties are the masses of galactic halos and chemical abundances in the gas.

From the above, a requirement emerges for a number of generic observational capabilities.

(1) *Wide wavelength coverage.* Ideally, individual objects will be observable from rest-frame Lyman-a to rest-frame 100 μm, equivalent to 5000 Å to 1 mm for $3 < z < 10$.

(2) *Deep imaging at high resolution.* To determine morphologies and to avoid confusion between adjacent (but possibly unrelated) objects, a broad-band (i.e., $\lambda/\Delta\lambda \sim 5$) imaging capability with kiloparsec-scale resolution (i.e., $\theta \lesssim 1$ arcsec) is required, in the near-infrared ($\lambda < 10$ μm) where stellar emission will dominate even at high redshift, and where morphology can be most closely related to present-day objects. High resolution is also likely to have a major impact on sensitivity as most objects have structure on all scales.

(3) *Spectroscopy at $\lambda/\Delta\lambda > 10^3$.* This is required to detect spectroscopic features in integrated light optimally. Spatially resolved spectroscopy at the same spatial resolution as the imaging facilitates the dynamical interpretation and is likely also to lead to sensitivity gains. Full 2-dimensional (integral field) spectroscopy is particularly well-suited to the chaotic morphologies and dynamics that are anticipated in very young galaxies.

(4) *Wide-field and multiplexing efficiency.* The study of the early universe is likely to proceed in a survey mode. The objects of most interest will probably be initially detected and subsequently studied by a single mission, and so an efficient survey mode will be required. Although the progenitors of typical galaxies must, by nature, have a high projected surface density on the sky (i.e., $n > 1$ arcmin^{-2}), large samples of comprehensively studied objects will be required if the conclusions are to be more than suggestive.

These requirements will be at least partially met via the new generation of 10m-class ground-based observatories operating at $\lambda \lesssim 2.5\ \mu$m and incorporating active/adaptive optical systems. Effective angular resolutions approaching the diffraction limit over small fields appear to be possible on a reasonably regular basis with these systems, especially at near-infrared wavelengths. Furthermore, interferometric systems will become available on the timescale appropriate to our considerations in this report, which will further increase available angular resolution, although probably not for significant numbers of high-z (that is, extremely faint) objects.

At longer wavelengths (i.e., $\lambda \gtrsim 2.5\ \mu$m), the thermal background from both telescope and atmosphere starts to rise by many orders of magnitude. At the same time, groundbased observations begin to lose the continuity of wavelength coverage that characterizes optical observations. Some sensitivity and wavelength coverage can be retained by airborne or Antarctic observatories, but these telescopes still suffer the high backgrounds resulting from ambient temperature. Therefore, by far the most sensitive and most extensive observations at near- and mid-IR wavelengths must be undertaken from large cooled telescopes in space.

7.4 A Baseline Science Program for Studying Galaxies with $z > 2$

The projected baseline science program for studying galaxies with $z > 2$ is as follows:

(1) *Identification of "primeval spheroids."* The unambiguous identification

of nearly-normal galaxies in the process of formation has been one of the major efforts in extragalactic research. To date, no one has succeeded, perhaps in part because sufficiently sensitive observations are not yet possible at the long wavelengths where the bulk of the stellar radiation will be found. Such a study would require extremely high broadband ($\lambda/\Delta\lambda \approx 5$) sensitivity to wavelengths as long as about 10 μm (about the peak in the galaxian energy distribution for $z \sim 10$). To achieve an angular resolution of at least 1 kpc in cosmologically distant objects, an angular resolution of about 0.2 arcsec will be required, which in turn requires an aperture (or separation between elements of an interferometer) of several meters. Celestial background-limited sensitivity at 10 μm inside the zodiacal dust cloud requires a telescope temperature lower than about 50 K.

(2) *Birth and early evolution of disks.* The disk is often the most prominent and characteristic component of galaxies. In the modern universe, this is also the location of most stellar formation and, consequently, the most actively evolving component. Disks are believed to have formed more slowly and/or later than the spheroid, but appear to be well-established by $z = 1$. A detailed study of disk structure and early evolution would possess baseline observatory requirements similar to those for the study of spheroids.

(3) *Early evolution of galaxies: effects of environment, mass, and dynamics.* The current condition and appearance of galaxies is determined by their birth and early evolution, including the effects of environment (crowding, merging), mass, available ISM, and dynamics. A detailed study of the early evolution of galaxies would have baseline observatory requirements similar to those for the study of spheroids, except that velocity structure in the young systems will be very important. For an estimate, we assume that we must resolve velocity structure comparable to large-scale motions in disks in the local universe: a few hundred kilometers per second. These observations will require moderate-resolution spectrographs with $\lambda/\Delta\lambda \approx 10^3$.

(4) *Early evolution of dark matter in the first galaxies.* The distribution and concentration of the dominant component of galaxies will affect significantly every aspect of our understanding of galaxies, e.g., birth of both the spheroid and disk components, evolution, dynamics, collisions, etc. A quantitative study of the distribution of dark matter might be undertaken by investigating the kinematics of the visible matter

using the prominent near-IR photospheric features (e.g., H_2O, CO). These strong photospheric features are found at 4.6, 2.3, 1.8, and 1.6 μm, so are shifted well into the mid-infrared at even moderate redshift. However, they may easily be the best features for determining large-scale motion in galaxies, and may be important indicators of the stellar populations of the youngest galaxies. As a baseline, we will assume that the CO first-overtone band (2.3 μm) will be a prime target, which will mean extremely sensitive operation of an observatory at $\lambda \gtrsim 10$ μm for $z \gtrsim 3$. A spectral resolution of about $\lambda/\Delta\lambda \approx 10^3$ should be adequate for investigating and mapping galaxian velocity structure.

(5) *The material universe before the birth of galaxies.* It is possible that galaxy formation extends over a relatively long time, with important "precursor" events at $z \gg 5$. Such events may include the birth of the first generations of massive stars in the proto-spheroid, followed by extensive supernova eruptions and the injection of heavy elements into the erstwhile pristine ISM. These precursor events of heating and enriching the gas no doubt play an essential role in the subsequent birth and evolution of galaxies. Such events should be able to be identified via very-high-z supernova explosions and/or very luminous "halo" structure produced by massive main-sequence stars. An investigation into "precursor" stellar systems would require very sensitive broadband imaging of stellar light (including, perhaps, such things as individual supernova events) at $\lambda \gtrsim 10$ μm, where the redshifted stellar light would be found.

(6) *The first heavy elements: origin of the modern ISM.* One of the most important events in the evolution of the universe was the transformation of the star-forming gas from the nearly pure H and He of the Big Bang to the heavy-element-bearing material of the modern ISM. Diagnostic absorption lines from heavy elements are found in the UV and visual, emission lines are found at visual and infrared wavelengths, and broad solid-state features are found at many wavelengths beyond about 3 μm. Consequently, an inventory of the heavy element composition of the high-z universe will require a space observatory (or observatories) with wavelength coverage extending from the visible to as far as possible into the mid- or even far-infrared.

(7) *A complete spectral inventory.* All the dominant components of galaxies (save dark matter) emit distinctive spectral lines, solid-state

features, or a broad continuum. These features are essential diagnostics of such fundamental parameters as abundance, temperature, density, mass motion, and star formation rate. Because location of the emission features within a galaxy is important, imaging spectroscopy over a wide range of wavelengths will be a vital capability for a future observatory. As a guide, a spatial resolution of less than about 1 kpc at cosmological distances will provide useful discrimination among different components/locations in a galaxy (i.e., roughly 0.2 arcsec). The diagnostic lines and bands of interest in the rest frame are spread over all wavelengths, from the UV to centimeter radio lines. Simply stated, this program requires as wide a wavelength coverage as technically feasible.

(8) *Birth and early evolution of active nuclei.* Quasars and related systems are among the most luminous objects in the universe. They are apparently powered by massive, centrally condensed objects, probably black holes. Such engines may play a critical role in the early evolution of galaxies, but their birth and evolution is poorly known. The best diagnostic lines for study of AGNs are found from the UV to the mid-IR ($\lambda \approx 20\ \mu$m) in the rest frame. Consequently, this program requires as wide a wavelength coverage as technically feasible, with an angular resolution sufficient to isolate the bright nuclear core from surrounding "contaminating" emission unambiguously. Because it is possible that many of the earliest quasars are obscured by gas and dust, the near- to mid-IR lines are particularly valuable indicators of their nuclear activity.

7.5 The Advantages of Space: Estimated Sensitivities for Future Ground-Based, Airborne, and Space-Based Observatories

There are three compelling reasons for optical-IR observations from space: (1) vastly improved clarity over a wide field, due to the absence of refraction in the turbulent atmosphere; (2) unimpeded access to a wide wavelength region; and (3) enhanced sensitivity due to reduction of the background emission from the atmosphere and telescope optical system. Technological advances over the past decade have permitted ground-based telescopes to approach their diffraction limit over small fields at visual and near-IR wavelengths, which has somewhat reduced the advantage that space has over the ground in clarity. Furthermore, significant reduction in atmospheric obscuration can be achieved by observations at high altitude in the Earth's atmosphere, which is a primary justification for the SOFIA and proposed balloon observatories. However, the atmospheric and telescopic backgrounds remain an overwhelming limitation for Earth-bound observatories, especially at IR wavelengths beyond about 2 μm. In this section, we discuss the advantages of space operation in the infrared via conservative estimates of point-source sensitivities. (Although galaxies in formation are expected to be resolved with the large IR-optimized telescope recommended here, many of the sites of star formation, such as young globular clusters, are small enough to be considered as point sources.)

We show in two accompanying figures estimated point source flux levels (S/N = 5) achievable with a 4m space telescope, compared with identical instruments on SOFIA and on Gemini. The space observatory was assumed to have an equilibrium temperature of 35 K, an overall optical system emissivity of 0.05, and a throughput of 0.5. The detectors were assumed to have a dark current of 10 e s^{-1}, a read noise of 50 e rms, and a quantum efficiency of 0.40. These values are intended to approximate current/near-future state-of-the-art mid-IR (3 μm $\lesssim \lambda \lesssim$ 30 μm) silicon-based detectors. Substantially better performance is already being realized for InSb and HgCdTe detectors, which operate at the short wavelength end of the regime that we are modeling here. Such detectors would, at present, be the obvious choice for deep cosmological observations at wavelengths of a few micrometers, one of the premier research programs which our committee has highlighted. However, broadband imaging and low-resolution spectroscopy at $\lambda \gtrsim 3$ μm, except for the shortest integration times, are dominated in space by the thermal background of the zodiacal dust. For Earth-based observatories, the atmosphere and optical system emission overwhelmingly dominate all other noise sources. All in all, we consider our estimates here to be very conservative and feel they probably underestimate the advantages of space-based observatories.

For these figures, we have assumed that the space observatory operates at 1 Astronomical Unit (AU), within the zodiacal dust cloud, taken to be emitting as an optically-thin blackbody with T= 280 K and $\tau = 10^{-7}$. Our committee considers a trans-asteroid orbit to be an attractive alternative for future space missions.

The following figures show the 1 σ IRAS survey values (first figure only) and the estimated SOFIA and Gemini performance after an integration time of 10,000 seconds (about 3 hours). The integration time(s) for the 4m space telescope are shown next to each curve. Two spectral resolutions are included: broadband ($\lambda/\Delta\lambda = 5$) and moderate-resolution spectroscopy ($\lambda/\Delta\lambda = 1000$). The shading in the first figure shows the atmospheric obscuration at a high mountain site.

The conclusion is clear: with a much reduced atmospheric/optical system background, infrared space observatories are orders of magnitude more

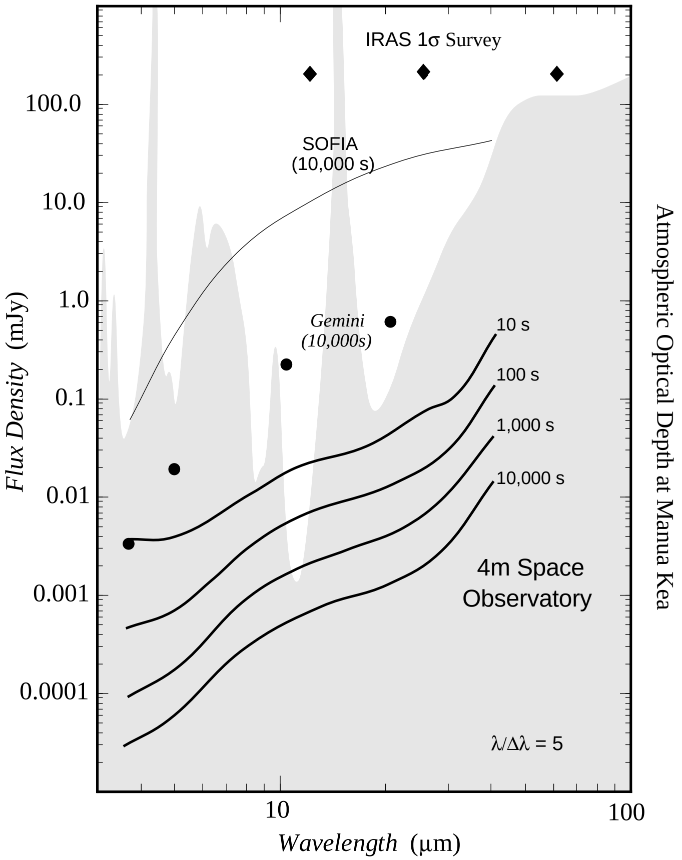


Figure 1 — Shown here are estimated broadband point-source sensitivities (S/N = 5) for three future observatories which will operate at infrared wavelengths. The proposed 4m telescope is assumed to have instruments using current detector technologies and an optical system equilibrium temperature of 35 K and emissivity of 0.05. Integration times are shown beside each curve. All other things being equal, estimated SIRTF point-source sensitivities would be about a factor of 22 (the square of the collecting area) less than that for the 4m telescope, but comparable for extended sources. The shaded area shows the relative optical depth from Mauna Kea.

sensitive than even the largest planned future ground-based telescope. Given that the required integration time to achieve a certain signal level goes linearly with the thermal background, cold space observatories such as ISO and SIRTF can easily outperform much larger ground-based telescopes, which have $10^{5 \rightarrow 6}$ higher backgrounds in those few IR windows accessible from the ground.

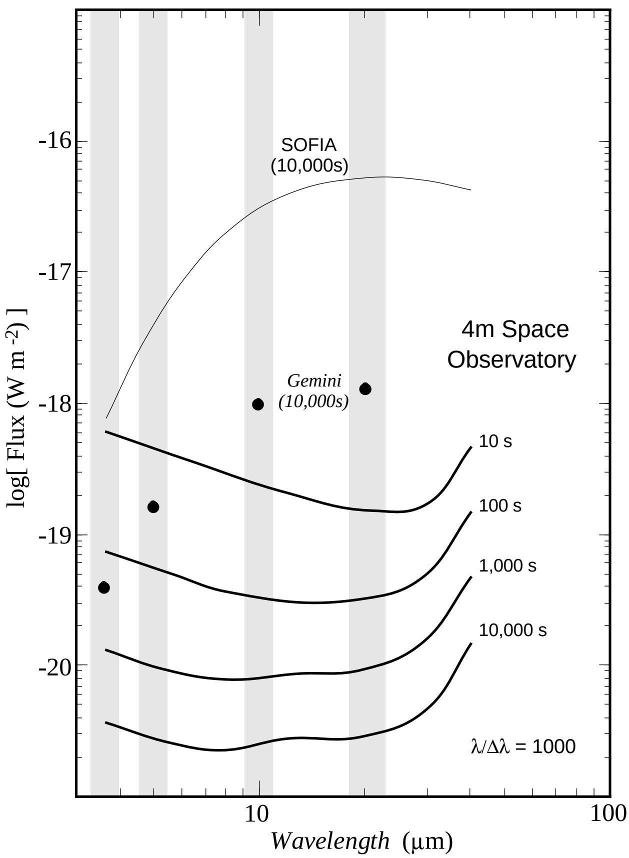


Figure 2 — Shown here are estimated spectroscopic point-source sensitivities (S/N = 5) for three future observatories which will operate at infrared wavelengths. The proposed 4m telescope is assumed to have instruments using current detector technologies and an optical system equilibrium temperature of 35 K and emissivity of 0.05. Integration times are shown beside each curve. All other things being equal, estimated SIRTF point-source sensitivities would be about a factor of 22 (the square of the collecting area) less than that for the 4m telescope, but comparable for extended sources. The four shaded bands indicate the approximate regions of good transparency from Mauna Kea.

As an illustrative example of the relative performance of space-based observatories, consider a simple broadband survey at 10 μm photometry of, say, AGNs or circumstellar Vega-type “debris disks.” Assuming a 25 percent net efficiency for all facilities, the 4m space telescope could survey 100 sources to about 2 μJy (S/N = 5) in about 5 days of satellite operation. During the same hypothetical 5 day period, a Gemini-class telescope could observe a single source to a flux level of about 50 μJy, more than an order of magnitude brighter than the limit of the satellite survey. Alternatively, let us assume that the pair of observatories had the same length of time to produce a volume-limited broadband survey of uniformly-distributed sources. The satellite survey would contain roughly 30,000 times more sources than the ground-based survey.

As the spectral resolution increases, the thermal background on the warm Earth-bound telescopes decreases. The advantage of space observatories declines, although more impressively at shorter wavelengths. For example, assume a moderate-resolution ($\lambda/\Delta\lambda$ = 1000) spectral survey of the ISM at about 20 μm. This wavelength regime contains numerous atomic and forbidden ionic lines, as well as the ground-state H_2 lines, so it is a major target for astronomers. Again assuming net system efficiencies of 25 percent, the 4m space observatory could survey 100 different locations to a limiting, S/N = 5 line strength of 10^{-20} W m^{-2} in about 5 days. During the same total length of time, the ground-based telescope could reach a flux level roughly 30 times less sensitive and for only a single source. A ground-based telescope would require an impossible 1000 years of observation to duplicate this 5-day survey by the space-based 4m telescope.

In time, even larger ground-based telescopes than the current 10m-class systems may be built. However, to equal the performance of the 4m cooled space observatory in the relatively good 10 μm window, such a “super Gemini” would have to possess a light-collecting capability equivalent to a diameter of about 200 m. Estimated facility costs are not a major emphasis of our committee’s report, but a conservative popular cost-scaling law (i.e., $\$ \propto D^2$) suggests that a ground-based optical system this large would cost roughly two orders of magnitude more than a 4m space observatory. Hence, using metrics such as those above, space observations are substantially more cost-effective than are ground-based facilities when comparable observations are evaluated. This may be an unprecedented situation for space astronomy.

8. The Search for Earth-Like Planets

8.1 Introduction

In the 1960s and '70s, Apollo astronauts looked back from the Moon and took now-famous photographs depicting the Earth as an island of life in space. Besides representing a great scientific and technical accomplishment, these pictures symbolized the growing realization that our common inheritance and destiny supersedes the divisions and distinctions between the people of the world. In the not too distant future, we could take similar pictures of 'other worlds' revolving around other stars, worlds that may also be habitats of life.

Discovering other worlds will require advanced optics and the proposed new generation of space observatories. We have in hand the technology today for finding big planets like Jupiter around other stars, and we can envision the future technology to discover, study, and obtain the portraits of small planets like Earth. When we have achieved this, when we find these other worlds, it will mean knowing better how our Earth, our solar system, and we ourselves came about. We will better understand our origins.

The search for planets around other stars is a quest on the cutting edge of science and technology. It draws upon all we know of Earth, the solar system, the formation of stars, and the remarkable phenomenon of life here on Earth. In addition, the search for other worlds has importance for our culture, because in the process of searching and studying we learn whether the Earth and the life it supports are a rare or common occurrence in the universe. Since ancient times, philosophers have speculated on the uniqueness of Earth and the life it bears. People from all walks of life continue to appreciate the deep significance of the question of whether there are planets around other stars.

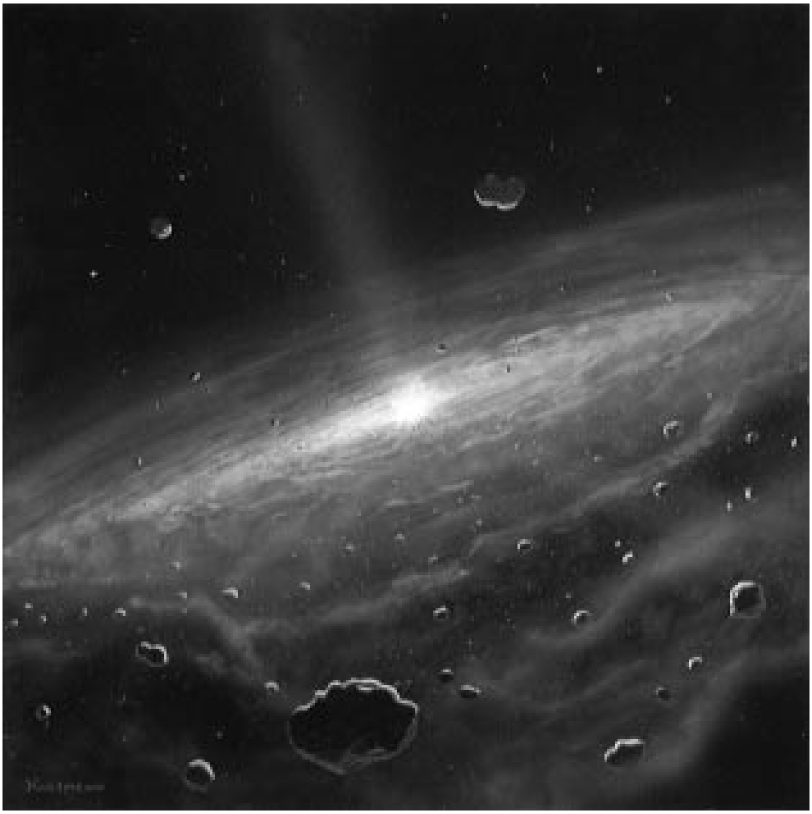
8.2 Current Scientific Theory of Planet Formation

Two classes of astrophysical objects are unique because each has only one known example: planetary systems like the solar system and life-bearing planets like Earth. As a result, the sciences pertaining to the origin of planets and conditions for life have not developed in a standard progression. That is, they have not passed through successive stages of discovery, classification, inference of basic principles, and continuing interplay of observation and theory, which are processes of inductive reasoning. Instead, our understanding of planetary systems and life habitats is based on deductive reasoning, that is, on basic physics and investigations of the sole examples. Astronomers are keenly aware that observations of a variety of planetary systems, and especially studies of Earth-like planets, are needed to establish planetary science and life science in their proper universal context.

Today's dominant theory on solar system formation is one of the oldest surviving scientific theories. In the 18th century, Immanuel Kant (1724-1804) and Marquis Pierre Simon de Laplace (1749-1827) independently suggested that the solar system planets accumulated from material in a revolving disk of gas and dust around the young Sun. This hypothesis was inspired by two sets of observations: the mapping of the orbits of the planets in three-dimensional space, (which were found to be circular and lying in a common plane, an apparent vestige of an earlier disk structure) and the discovery and cataloguing the shapes of astrophysical nebulae by Charles Messier (1730-1817), which objects seemed to be disks around stars in which new planets might be forming. Even though these nebulae turned out to be galaxies—that is, whole hosts of stars—the Kant-Laplace Hypothesis they inspired still survives as the basic paradigm of planet formation. Furthermore, the coplanarity and co-revolution of planetary orbits is still regarded as the definitive signature of planet formation by accretion in a circumstellar disk.

Planet formation is an aspect of star formation. We believe the process begins with the gravitational collapse of a vast cloud of gas and dust. The central condensation accretes infalling material, shrinks, and eventually becomes a star. Material that does not fall on the star forms a disk. The rotation of the original cloud is preserved in the revolution of the disk and the rotation of the star. After a few tens of thousands of years, the dust sinks to the midplane of the disk, forming a dense sheet. Within this sheet, the dust will clump together and agglomerate into larger objects, called planetesimals, which themselves collide and build up to Earth-size planets and the cores of giant planets like Jupiter, Saturn, Neptune, and Uranus. Next, these bodies can accrete gases. Finally, the disk's residual gas and dust will dissipate by being either blown away by stellar winds or accreted onto the planets.

In the 1980s and '90s, new astronomical discoveries have documented stages in this scenario of how planetary systems form. These supporting examples include newborn stars surrounded by embryonic material, youthful stars with flattened disks in which planets may be forming, and older stars with diffuse dusty envelopes that may harbor mature planets. How-



In this image, bodies in a circumstellar disk aggregate from dust particles, forming planetesimals ranging in size from boulders and small asteroids. The process will continue for about 50 million years, until the disk-shaped nebula of gas and dust and planetesimals is transformed into a family of asteroids and planetary bodies orbiting the central star, like our own solar system. Painting by William K. Hartmann. Copyright ©1994 by AURA, Inc.

ever, only a small number of planets have yet been found around other normal stars like the Sun. This dearth of evidence does not yet prove that planets are rare, because we have only recently begun to possess the tools capable of finding them. Because of the wealth of circumstantial evidence, however, most astronomers today believe that planetary systems are common occurrences around other stars.

8.3 Direct and Indirect Discovery

There are two general discovery strategies: detecting planets indirectly, usually by measuring the planet's effect on the star's position, velocity, or brightness, and observing them directly, by isolating the planet's light from the star's light, which can then be studied by obtaining pictures or spectra of the planet.

Indirect detection can determine the orbit, mass, or size of the planet, but nothing beyond these attributes. This information may be enough to settle whether the unseen object is indeed a planet, but direct detection is required to measure the planetary orbit and tell us about the chemistry and physical properties of the planet itself. If a planet has an atmosphere, features in its spectrum can tell us what kind of atmosphere it is, whether it might be compatible with life, and even if life is present. If we could get a picture of the planet, we might see clouds, continents, and oceans.

Detecting a planet with size and orbit similar to Earth, determining its physical and chemical similarities to Earth, and actually getting a picture of the face of such an Earth-like planet calls for the use of successively more powerful telescopes.

8.4 Indirect Detection of Earth-Like Planets

For most forms of indirect detection, the small radius and mass of a planet like Earth—respectively about 10^{-2} and 10^{-6} those of the Sun—mean that the signal is very small. This is because the planet affects the star very little as viewed from afar. For example, the motion of the star around the center of mass of the star-planet system could be detected using astrometry or spectroscopy to measure the Doppler effect. However, these techniques do not appear feasible in the case of an Earth-like planet. In the case of astrometry, which involves observing the star's position on the sky, the center of mass of the Jupiter-Sun system lies just outside the surface of the Sun, but for the Earth-Sun system it lies only 1/1000 solar radius from the Sun's center. This displacement of the star's center of light is so small that a false signal could be produced by sunspots. In the case of Doppler spectroscopy, which involves measuring the star's velocity toward or away from us, the Sun revolves around the Jupiter-Sun center of mass with an orbital speed of some 13 meters per second. This signal could be detected. However, the Sun's speed in response to the Earth is only 9 centimeters per second, which is too small to be measured using any known astronomical technique.

Occultations—passages of the planet in front of the star—are another possible indirect detection technique. For a distant observer, the Earth in transit across the solar disk would cut off 0.01 percent of the total light from the Sun. Even though this is a very small effect, it could be detected with current technology, but to avoid the noisy scintillation caused by the Earth's atmosphere it would require an observatory in space. However, such a large investment in might not be justifiable. The correct orientation of the planet orbit with respect to the line of sight has a very low probability, only about 0.2 percent in the case of the Earth-Sun system. Therefore, a very large sample of stars would need to be studied to discover a planet even if all stars had them. This means it is very unlikely that a terrestrial planet could be discovered by occultation around a star close enough to permit follow-on spectroscopic studies and picture taking, both of which rely on direct detection.

If occultations could be measured with sufficient accuracy from ground-based observatories, the information would be valuable and cost effective, even though the findings could not be followed up with physical studies of the discovered planets. Occultations could tell us the statistical occurrence of planets, that is, what fraction of stars of various sizes have Earth-like planets. Recently, a new way has been discovered to provide this statistical information using ground based telescopes: gravitational microlensing.

Microlensing refers to the gravitational focusing of light, in this case, from a distant background star by a foreground star with a possible planet. These line-up events are rare, except when the density of background stars is great, such as when we look toward the center of our Milky Way galaxy. Monitoring the light from many distant stars in this direction, astronomers are now detecting an occasional lineup when a foreground star wanders in front of a distant background star. When this happens, the background star appears to brighten and then returns to its original state. If the foreground star has a properly placed planet, then a short, pronounced spike of brightness can be observed, which is superimposed on the smooth and gradual signal of the star-star encounter. Even planets as small as the Earth can produce easily detectable microlensing signals.

Over the next several years, microlensing will probably be the most important indirect method for understanding Earth-like planets around other stars. We should learn how frequently they occur by observing many microlensing events.

Not only professional astronomers and large telescopes will be involved in this exciting search: many amateur astronomers have the skills and equipment to make important observations of events in progress. Because there are thousands of avid amateurs around the world, the coverage they provide on microlensing events in progress could prove valuable.

8.5 Direct Detection of Earth-Like Planets

Because indirect detection is not a certain way to detect Earth-like planets and, given that indirect detections cannot be followed up by physical studies of the planets that may be found that way, direct detection is required. This entails optically separating the planet light from the star light and imaging the planet light on a detector. Direct detection is made difficult by the small amount of light, in both relative and absolute terms, emitted by an Earth-like planet.

The Earth, shining by light reflected from the Sun, is a very faint source as viewed from a distance of 10 pc (about 30 light years). In the visible part of the spectrum (reflected sunlight at 0.5 μm wavelength), the Earth would be about 29th magnitude, close to the detection limit of the HST. The HST is the most sensitive instrument yet available to observers looking for faint companions to stars. In the infrared (thermal radiation at 10 μm wavelength), the brightness of Earth would be about 21st magnitude, which could not be detected by any ground-based telescope due to the thermal emission from warm telescope optical elements. Telescopes in space can be cooled down to reduce their emission, but even then the general sky brightness due to emission from the zodiacal light—warm dust in the solar system—must be taken into account.

Comparing the planet and star brightness, the Earth is 10^{-10} times fainter than the Sun at visible wavelengths; the fraction is a thousand times larger, 10^{-7}, in the thermal infrared. Removing the unwanted starlight, which threatens to overwhelm the weak planet signal, is the premier problem of direct planet detection. Both absolute and relative brightness factors favor infrared wavelengths for finding and studying Earth-like planets around other stars.

The overwhelming amount of starlight received from a star compared with an Earth-like planet means that the telescope system must separate the planet and star images with exquisite precision. Using a conventional telescope, the image of a star consists of two parts: the core, where most of the light is concentrated, and the wings or halo, where light has been scattered. Detecting a planet next to the star requires both separating the image cores of the planet and star, and strongly suppressing the scattered star light at the planet's location on the detector. Diffraction limited conventional telescopes, like the HST, and interferometers, like the Very Large Array (VLA), routinely apply these principles to astronomical problems less challenging than detecting planets. However, the techniques for strongly suppressing the light of a nearby star is not an established practice, though a cadre of respected scientists and engineers are currently making important advances on how to do it.

In a conventional, single aperture telescope, the angular resolving power and light-collecting area are coupled mathematically, being proportional to the diameter and the diameter squared, respectively. For some astronomical problems—the direct detection of terrestrial planets is one—high resolution is required but large collecting area is not. In this case, an interferometer configuration can be used to save the cost, and avoid the impracticality, of a single large optical element. The separation of the interferometer's collecting telescopes must be sufficient to achieve the required resolution, and their light-collecting area must be sufficient to provide adequate signal from the planet in the presence of background radiation.

8.6 A Space Infrared Interferometer

As noted earlier in this report, NASA is currently studying an infrared interferometer mission to find and study Earth-like planets around other stars through the ExNPS program. Much effort is still needed to settle on the best design, but the basic concepts, key trade-offs, and technical issues have been identified.

The interferometer would consist of separated telescopes pointing accurately at the target star, with a field of view that includes the surrounding region in which a planet might be orbiting. For a star like the Sun, this would be the radius of the Earth's orbit or 1 Astronomical Unit (AU) from the star, where water could exist in a liquid state. For a cooler star, this 'life zone' would be closer to the star, and for a hotter star, the zone to be searched would be farther out.

For a star at a distance of 10 pc, and for an interferometer operating at 10 μm wavelength, the minimum separation of the light-collecting telescopes is 10m to separate a planet 1 AU from the star. (The reason for choosing 10 pc is that there are several hundred stars within that distance of the Sun, including more than 50 'solar type' stars.) The needed telescope separation would be proportionally larger to detect cooler planets at longer wavelengths, to search closer to the star, or to probe more distant stars. The size range 10-100m is sufficiently small that a space structure could be designed to connect and initially align the collecting telescopes.

The minimum number of telescopes in the interferometer will be two. More telescopes would reduce the ambiguities that always arise in interferometric images and more effectively remove the emission from the central star. Whereas a conventional telescope (or, for that matter, the human eye) produces an image with uniform sensitivity over the field of view, an interferometer produces patches of sensitivity over the field of view, which must be rotated around the star to map the distribution of light intensity. In the case of a two-telescope interferometer, these zones of sensitivity are long stripes; a four-element, square array produces a checkerboard pattern. A symmetric, sparsely filled array will have trouble distinguishing a planet signal from the signal of an elongated, diffuse source, such as the edge-on dust disk of zodiacal light in the other stellar system. Furthermore, such a disk might have bright spots in it due to concentrations of dust, which might mock planets.

To suppress the starlight, it is essential that a null (a zero-sensitivity patch) be placed on the star, which is centered in the interferometer's field of view. This can be achieved by introducing a 180° phase shift in the light collected by paired telescopes in the array—this configuration is called a "nulling interferometer." Because the angular size of the stellar disk is not zero, a little starlight will leak through, but can be made negligible if the interferometer is very accurately aligned. The optical elements must also be very smooth and accurately shaped to avoid scattering the starlight.

The telescopes must be cooled below 40 K to remove the background radiation from the telescope optics. This will be achieved either by active cooling using a cryogen or a heat engine, or by passive cooling, using the fact that well-shaded and well-insulated optics will cool by emitting thermal radiation to space.

The zodiacal light in our solar system produces uniform foreground radiation, which is not modulated by the interferometer as it rotates to gather the data for an image. The zodiacal light is the dominant source of unwanted radiation after the telescope optics are cooled and the star is nulled. There are two possible approaches to eliminating this problem. The first is to use larger telescopes in the interferometer, which would increase the contrast of the planet relative to the uniform foreground, and the second is to move the interferometer outside beyond most the solar system dust which produces the zodiacal light, out to 3 – 5 AU. Based on preliminary estimates, a telescope of 4m diameter could do the job at 1 AU, or 1m-class telescopes would work at 3 – 5 AU, where the zodiacal light should be reduced one or two orders of magnitude. More studies of the zodiacal light, both in our solar system and the target systems, will be needed before the size-location issues can be settled. ISO and SIRTF missions will be especially valuable for such studies.

8.7 Spectra of Earth-Like Planets

Once an Earth-like planet has been directly detected, obtaining a spectrum is an essentially straightforward, albeit lengthy next step. High resolution is not required for the important spectral features, which are both strong and broad; it is feasible for the nulling interferometer that makes the first detections of Earth-like planets to also take the first spectra that will reveal conditions of the planetary atmosphere and perhaps even the presence of life.

The spectral lines of carbon dioxide, which is abundant on Earth and predominant on Venus and Mars, would indicate that the planet has an atmosphere. This, in effect, indicates that the planet is similar in size to Earth. As is found in our own solar system, planets much more massive than Earth are likely to have an atmosphere dominated by hydrogen-rich compounds, such as methane and ammonia, and planets much less massive than Earth are not likely to have retained any atmosphere at all.

Evidence of water vapor in a concentration similar to that in the Earth's atmosphere would indicate that liquid water is present; in terms of its position in the planetary system, it is in a "habitable zone." Liquid water is a prerequisite for life as we know it. If, in addition to water, there is a substantial concentration of oxygen in an atmosphere, this may be a strong indication of biological activity on the planet. Oxygen is a highly reactive gas, which in Earth's atmosphere is maintained only through photosynthesis. It is probable that, on other worlds as well, free oxygen would not persist without the influence of some complex chemistry like life on Earth.

Carbon dioxide, water, and oxygen (in the form of ozone) all have distinctive absorption bands in the thermal infrared wavelength region: another excellent reason for choosing the infrared over the visible region for searching for Earth-like planets.

To obtain a useful spectrum, the planet light must be subdivided into segments according to wavelength, each containing 5-10 percent of the available light. If the same telescope system used for detection were employed to obtain a spectrum, the integration times will be longer, but could be reduced with more telescope collecting area.

8.8 Resolved Pictures of Earth-Like Planets

The direct detection of an Earth-like planet, as well as the subsequent spectroscopic study of its atmosphere, can be done with a telescope system designed simply to isolate the planet as no more than a speck of light next to the star. With much higher resolution, we could obtain the picture of the face of such a planet and see cloud cover, oceans, and continents if they exist. Such large scale features could be identified if the picture contained about 10 pixels across the diameter of the planet. However, the resolution required for such a picture is much greater than that required for simple detection—about 100,000 times greater—and the optical system required is much more challenging.

Whereas a 10- to 100m infrared interferometer can detect and study the spectrum of an Earth-like planet, a 1000km interferometer would be needed to obtain a picture with 10 by 10 pixels. While it is understood how an interferometer of this size would resolve the face of a terrestrial planet, the suppression of the light from the central star is more complicated. For example, each element in the interferometer might have to be a nulling interferometer itself or be a large filled aperture operating in a coronographic mode. Also, the interferometer's collecting area would certainly need to be considerably greater than the area of the detection and spectroscopic systems.

In engineering terms, such a picture-taking optical system is truly visionary. It would combine the beams of a constellation of free-flying telescopes separated by very large distances. Although such an instrument is not beyond the range of possibility, it remains a daunting prospect until the interferometers designed for direct detection and spectroscopy have been developed, and until the limitations and possibilities of the instrumentation are thoroughly understood.

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IV. A Broad Scientific Program for Future Space Facilities

9. A General Astrophysics Program for a Large Filled-Aperture, Infrared-Optimized Space Telescope and for the Post-2005 HST

9.1 Introduction

During the two years of the HST & Beyond Committee's work, we discussed extensively future scientific goals and priorities for Ultraviolet-Optical-Infrared (UVOIR) astronomy. Although our report focusses primarily on two central scientific themes, we recognize that these could not be accomplished, and, indeed, our profession would not advance, without diverse astronomical programs that address a variety of scientific questions. We abstracted these discussions, which may be thought of as the "working papers" of our committee, into the sections that follow.

Our purpose in presenting this material is twofold. First, we believe that it provides context for the conclusions of the report by exhibiting the scientific priorities of the Committee. Second, as our committee recommends operation of our three priority initiatives as general-purpose observatories, we called upon our scientific expertise to provide examples of the variety of important astrophysical programs that can be undertaken with the three missions.

Because the Illingworth subcommittee, Optical and Ultraviolet Astronomy from Space, of the 1991 National Academy of Sciences (NAS) Survey Committee conducted a comprehensive review of the scientific case for a future space telescope, the HST and Beyond Committee chose not to undertake a similar exercise. Instead, we drew on the experience of members of our committee to provide representative scientific projects which could be carried out with our three recommended initiatives: (1) extension of the life of the Hubble Space Telescope (HST) with emphasis on Ultraviolet (UV) spectroscopy and UV/optical wide-field imaging;

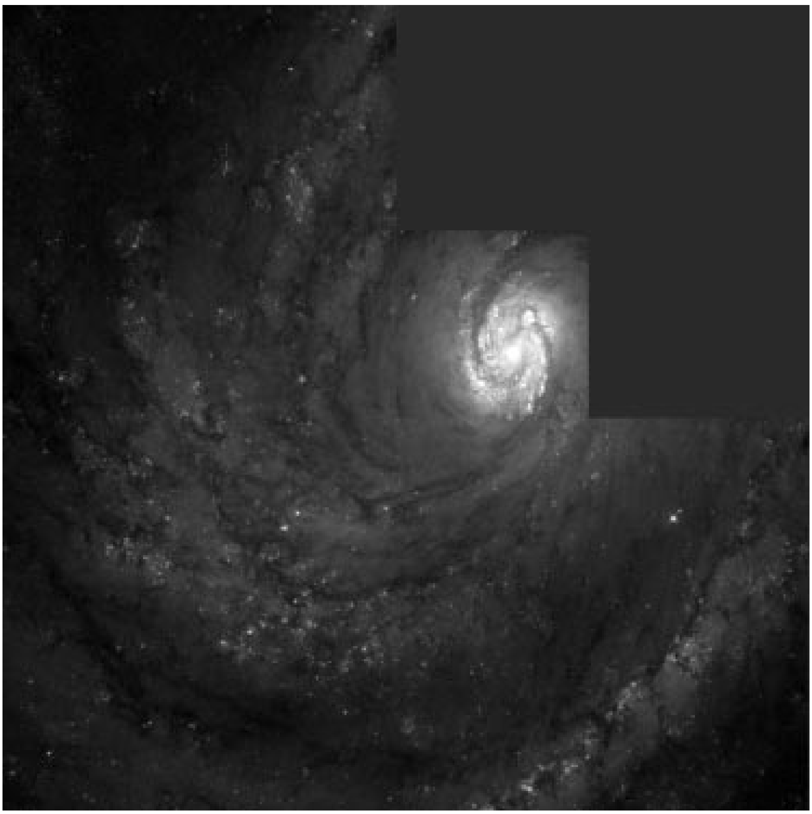
(2) construction of an Infrared (IR)-optimized space telescope of at least 4m aperture; and (3) the development of space interferometry.

Although we consider operation of the HST beyond 2005 to be an essential component of the exploration of the cosmos, the scientific case for UV/optical imaging and spectroscopy has been made extensively elsewhere. In this section, we emphasize the IR ($\lambda \sim 1 - 100\ \mu$m), which is rich in diagnostic information about physical processes and composition, from the closest Solar System sources to the most distant objects at the birth of the galaxies. Objects with temperatures between about 20 and 2000 K emit most of their light at these wavelengths, and, because of redshift, we receive the light from distant, hotter objects such, as solar-type stars in primeval galaxies, in the near- to mid-IR as well. This means that sensitive IR observation from space is an essential diagnostic tool for many of the future scientific programs which the Committee considered.

For point sources, a 4m cooled space observatory will have roughly an order of magnitude greater sensitivity than currently planned IR space missions and several orders of magnitude greater sensitivity than the largest proposed groundbased telescope. For programs requiring high angular resolution, the relevant comparison is with ambient temperature telescopes such as Gemini and Stratospheric Observatory for Infrared Astronomy (SOFIA). Section 7.5 discusses point-source sensitivities for a 4m telescope for broadband imaging ($\lambda/\Delta\lambda = 5$) and moderate-resolution spectroscopy ($\lambda/\Delta\lambda = 1000$) compared with these telescopes, using similar detector systems. For extended sources, as discussed in section 7.5, cooled telescopes in space have an immense advantage over ambient temperature telescopes; the 4m telescope and Space Infrared Telescope Facility (SIRTF) will have performance similar to each other if both are in orbits at 1 Astronomical Unit (AU).

For point sources, a 4m cooled space observatory will have roughly an order of magnitude greater sensitivity than currently planned IR space missions and several orders of magnitude greater sensitivity than the largest proposed groundbased telescope. For programs requiring high angular resolution, the relevant comparison is with ambient temperature telescopes such as Gemini and SOFIA. Section 7.5 discusses point-source sensitivities for a 4m cooled space telescope for four integration times, compared with achievable flux levels for the airborne and ground-based telescopes using comparable detector systems. For extended sources, as discussed elsewhere, cooled space telescopes will have an immense advantage over observatories in the atmosphere. However, the 4m telescope and SIRTF will have comparable sensitivities, if both are working inside the zodiacal dust at about 1 AU.

Our recommended large-aperture, IR-optimized space telescope will be essential for the detailed studies of the early universe at $\lambda \approx 1 - 5\ \mu$m. However, we also recommend that it be operated as a powerful general-purpose observatory, serving a broad range of scientific programs over the wavelength range $\lambda \approx 0.5 - 20\ \mu$m, the exact coverage to be determined on the basis of future technical evaluation. In the following subsections, we outline some additional scientific programs that require large aperture



The wide-field clarity of space observatories, as demonstrated by this Hubble Space Telescope (HST) visual image of M100, is a primary justification for the infrared-optimized space telescope recommended in this study. The angular resolution of this proposed observatory will be comparable to the HST, with a sensitivity orders of magnitude greater than that which can be achieved from within the Earth's atmosphere.

in space operating at wavelengths shortward of about 100 μm. In many cases, these hypothetical programs are directly related to our general "origins" themes: the early evolution of the material universe, the birth of stars and planets, and organic matter in space.

9.2 Our Solar System

The prominent solar system objects are bright and capable of being resolved over limited IR wavelengths with the new generation of 10m-class ground-based facilities and are targets for future *in situ* missions. However, a celestial background-limited space observatory will be critical for spectroscopic study of small and distant objects, including petrology and organic mineralogy of asteroids, comets at large distances from the Sun, satellites, Kuiper Belt objects, and their relatives. The vast majority of Solar System sources are simply far too faint for detailed observations by observatories suffering the thermal background and obscuration of the Earth's atmosphere.

For both bright and faint Solar System objects, a space observatory will provide unimpeded access throughout the scientifically-crucial spectral regions, such as the 3 – 4 μm and 6 – 8 μm regions of carbonate signatures; 2.3, 4.5, and 9 μm features of sulfates/bisulfates; as well as the hydrated minerals with bands at 3 and 10. In the case of Mars, for example, such observations could indicate an earlier, wetter period and/or a sequestering of a past, thicker atmosphere. Sensitive mid-IR band emission over $\lambda \approx 8 - 30$ μm from solid surfaces can also indicate surface mineralogy, weathering, particulate structure, and the effects of photochemistry, all of which would qualitatively improve our understanding of the evolution of our own planetary system.

Kuiper Belt and distant cometary objects are barely understood today, but are expected to be storehouses of information, as they are thought to be remnants of the early formation of the planets. Dark, ostensibly organic-rich material is found on the surfaces of a large number of small bodies in the outer Solar System. Dark, C-type asteroids are sometimes considered to be remnants of cometary nuclei which have lost most of their volatiles. We know that comets collide with the Earth, bringing biogenic material with them. We do not know what organic materials these objects contained, from where in the Solar System they came, where they are now located, nor the mass of organic materials within these objects. However, these faint objects resist analysis, as organic compounds have diagnostic features over $\lambda \approx 2 - 50\ \mu$m, a wavelength regime mostly inaccessible from the ground.

9.3 Extra-Solar Planetary Material and Circumstellar Gas

A complete understanding of the formation of planets requires extensive study of the birth of stars and their surrounding material. This study is best undertaken at IR wavelengths, which are rich in diagnostic atomic and molecular lines, solid state bands, and the dust continuum. A large-aperture IR-optimized mission could undertake an extremely deep survey thousands of times more sensitive than Infrared Astronomical Satellite (IRAS), searching for circumstellar dust emission at $\lambda \approx 5 - 50\ \mu$m – the "Vega phenomenon"—around main sequence stars similar to the Sun. Detection of hundreds of such objects would produce a powerful data base for follow-up studies, but a large next-generation observatory would also possess sufficient sensitivity to undertake spectroscopic mineralogy and petrology of the dusty material. This undertaking would include study of the composition as a function of position in the target system to determine whether the material is ice or rock, crystalline or amorphous, and includes organics.

9.4 Transition Objects: Brown Dwarfs

Brown dwarfs and related objects are key "transition objects" between planets and normal stars, which have sufficient mass to ignite hydrogen fusion in their cores. Within a decade, it is likely that several dozen examples of young brown dwarfs will have been discovered by Infrared Space Observatory (ISO), SIRTF, and large ground-based observatories. A cooled 4m telescope would be able to about 25 times fainter than SIRTF for the same integration time. This would permit a program of high resolution spectroscopic analysis of these objects: the photospheric features predicted for these objects are sensitive to age, mass, and chemical composition. High-resolution spectroscopy would further permit determinations of rotation, low-amplitude pulsations, and the presence of fainter—possibly planetary—companions.

9.5 The Interstellar Medium and the Birth of Stars

A 4m telescope would be a powerful tool for general studies related to important problems in stellar formation. Spectroscopy over the wavelength range of about $10 - 30\ \mu$m includes the pure-rotational lines of H_2, emission from Fe, Si and other elements near regions of mass outflow, and the forbidden transitions of Ne and S, which are sensitive to the ambient UV radiation field. Observational programs would include studies of embedded stars and proto-stars, shock chemistry and excitation, massive outflows, and the state of the surrounding Interstellar Medium (ISM). Unobscured IR diagnostic lines will allow determination of density, excitation and stellar temperature, and elemental abundances. Oxygen-bearing molecules such as H_2O and O_2 are essentially unobservable from the ground. Finally, polarization studies over a wide wavelength range will reveal the grain and magnetic field orientation.

The solid state component of the ISM—organic and Si-bearing grains; PAHs and their relatives—commonly possess the strongest emission features at any wavelength from many regions in the Milky Way. It now appears to be the case that in the cool ISM, small grains/large molecules dominate the energetics and chemistry, which makes them prime targets for sensitive space observatories with an unbroken wavelength coverage throughout the mid-IR.

9.6 Stellar Populations

A major theme of this report is the quest to understand the origins and evolution of galaxies. A fundamental and very complementary aspect of studying the evolution of galaxies at high redshift, is the study of the stellar populations in galaxies that are nearby. Only in the nearest galaxies can individual stars be resolved and the star formation history be mapped directly.

Interpreting the colors and spectra of high-redshift galaxies requires both a knowledge of the distribution of stellar masses (the Initial Mass Function or IMF), and the history of star formation in galaxies. It requires a knowledge of whether the IMF is universal, or whether it varies with time, chemical composition and/or environment. From the ground, the IMF has been measured for our own galaxy and the very nearby (irregular) Magellanic Clouds for stars below the main-sequence turnoff. A major extension of this work will come with the HST, allowing stars in M31 to be resolved down to V~28 mag. At these magnitudes, the horizontal branch and red giant branch stars will be well-studied, but a telescope of aperture 4m or larger will be required to reach below the older main sequence turnoff (V~30 mag) in these next nearest galaxies located a factor of 10 more distant than the Magellanic Clouds. For these galaxies where the (old) main-sequence turnoff populations can be detected, the star formation histories can be measured directly as a function of time. Even so, the Local Group itself contains no giant elliptical galaxies with which to make a direct comparison with elliptical galaxies found at high redshift.

For the nearest giant elliptical galaxies, for example, at 10 Mpc in the Leo I group, the brightest giant stars have been resolved for the first time with the HST, but a 4m-class telescope will be required to reach the horizontal branches in these systems. A sustained HST with enhanced UV capability will be able to address directly the problem of what contributes to the ultraviolet excess in giant elliptical galaxies. Red giant stars emit the bulk of their radiation at near-infrared wavelengths. Hence, with a large filled aperture IR-optimized telescope, the old red giant branches, and any intermediate-age extended asymptotic giant branches (if present) could be measured directly in the near-infrared out to, and perhaps beyond, the distance of the Virgo cluster. Resolution of individual stars in these systems will again be critical to the interpretation of the integrated-light properties of distant unresolved galaxies.

The dominant light of very high-redshift galaxies comes from the youngest, most massive stars in regions of active star formation, which emit the bulk of their radiation at ultraviolet wavelengths. UV surveys of the massive star distributions in nearby galaxies will be essential for comparing the morphologies and the star formation rates of galaxies in the early universe with those today.

As a basis for comparison, a complete empirical stellar population library is critical for the interpretation of spectra of high-redshift galaxies. Required are spectra of individual stars spanning the range of ages and heavy element abundances found in galaxies, and covering a wavelength region from the ultraviolet through the visual and into the infrared. Existing empirical stellar libraries have been based on stars located nearby [in the solar neighborhood, and more recently, with stars and clusters from the nearest galaxies: the Milky Way Bulge, the Large Magellanic Cloud (LMC), and M31]. An enhanced UV sensitivity HST and large aperture cooled telescope will be crucial to these efforts, particularly for stars of high metal abundance.

Currently, the most accurate means astronomers have available for measuring extragalactic distances is the application of the Cepheid period-luminosity relation. With the HST + Advanced Camera for Surveys (ACS), the detection of individual Cepheid variables should be feasible out to the distance of the Coma Cluster. With a 4m-class telescope observations at this distance will be routine, and even larger distances will be feasible, allowing the peculiar velocity flow to be mapped directly using Cepheid distances. However, accurate Cepheid distances to galaxies require that a correction for interstellar dust be obtained. The most accurate means of correcting for reddening, once Cepheids have been discovered at optical wavelengths, is to obtain observations at near-infrared wavelengths. With HST and Near Infrared Camera (NICMOS), these data can be obtained for nearer galaxies, perhaps for the brighter Cepheids out to the distance of the Virgo cluster, but a large aperture telescope will be required for greater distances where the Hubble flow can be probed directly.

9.7 Stellar Death and Transfiguration

Dying stars return the products of nuclear burning to enrich the ISM in the form of ejected gas and dust. In the complex cycle of birth and death in the cosmos, these objects create the rich "soil" necessary for future generations of stars and planets. Near the end of their lives, moderate-mass stars enshroud themselves in large, dusty envelopes. Many of their key spectral diagnostics, as well as most of their energy, shifts to the near- and mid-IR. Space observatories will be able to obtain unbroken detailed IR spectra of objects inaccessible from any Earth-bound site, permitting mineralogy as a function of location and time in rapidly-evolving post-main sequence stars.

Since the explosion of SN1987A, mid-IR spectroscopy has become extremely important in the study of the deaths of high-mass stars. This wavelength regime contains the important diagnostic lines of H, Fe, Mg, Ni, Ar, N, CO, among many others. A 4m space telescope will be able to obtain a spectrum equivalent to the best published of SN1987A at the distance of the Virgo cluster after an integration time of only 1 hour, and will be able to explore the totally unknown spectral region of exploding stars beyond about 20 μm.

9.8 Infrared Emission from Normal Galaxies

Normal galaxies, such as the Milky Way, emit about 1/3 of their total light at IR wavelengths that would be accessible to the next generation space observatory. Mid- and far-IR lines from the cool ISM permit study of the structure, energetics, chemistry, composition, and dynamics in all types of galaxies throughout the local universe. ISO and SIRTF will undertake the essential opening studies of normal galaxies in the IR, but a 4m telescope will be able to study structure with a clarity at least 25 times greater than these precursors. It will also have a substantially longer lifetime to undertake more extensive survey programs of fainter objects.

A particularly informative program for a future IR space observatory will be extragalactic mineralogy: the dust composition in a variety of systems in the local universe. It is likely that the mid-IR solid state bands are the strongest spectral features in more-or-less normal galaxies, and the strength and structure of these bands indicate grain composition, nature of the radiation field, life-cycle and history of the dust, and thermophysics of the cool ISM.

9.9 Active Galaxies

Advances in the understanding of AGNs depend upon three advantages of space-borne facilities: access to UV and IR wavelengths obscured by the atmosphere; wide-field, high-resolution imaging; and orders of magnitude gains in sensitivity in the mid-IR, even at wavelengths accessible from within the atmosphere. Decades of concentrated effort from the ground has left a vast number of unsolved problems, even for the nearest sources. For example, the origin of the continuum emission has not been determined, which might be best revealed via detailed UV spectroscopy of the Broad Emission-Line Region (BLR). This spectral region includes ionization states from neutrals up to five-times ionized oxygen and sulfur, which allows analysis of the stratification of the BLR cloud systems. Furthermore, high-resolution imaging of nearby Seyfert galaxies strongly suggests that the ionizing radiation pattern is anisotropic. Physical conditions within individual clouds and filaments can be derived from emission-line ratios, which also act as kinematic probes of the bulk gas motion. Continued operation of HST in the ultraviolet is essential for complete understanding of these objects. Furthermore, with the sensitivity possible only in space, along with access to obscured UV diagnostic features, new classes of active nuclei may be discovered and understood, such as active cores of dwarf and irregular systems.

Despite decades of ground-based IR observations, many properties of even nearby active galaxies, revealed only at these wavelengths, remain unknown. This is likely to be one of the most important results of the ISO and SIRTF programs: high-sensitivity IR spectroscopy throughout the mid-IR will significantly clarify the structure, energetics, elemental abundance, and evolution of several classes of active galaxy, taking advantage of the key diagnostic lines of Ne, Fe, Si, and Ca at these wavelengths.

A larger, longer-lived space observatory optimized for $\lambda \approx 3 - 20\ \mu$m will be the single most important facility for the study of Active Galactic Nuclei (AGNs) and their cousins. Not only are essential spectral features scattered throughout this regime, but shorter-wavelength features from cosmologically-distant sources are redshifted into the mid-IR. For example, shock diagnostics include [O III] at a rest wavelength of 500 nm and [S II] at 660 nm, which will be found at 2 and 2.6 μm, respectively, for $z = 3$ systems. At longer wavelengths, the [Fe II] lines around 1.6 μm and the strong 1-0 S(1) transition of H_2 at 2.1 μm, in $z = 3$ objects will be found at 6.4 and 8.4 μm, respectively, which is completely unobservable at high sensitivity from within the Earth's atmosphere. Finally, the Brackett and Paschen hydrogen lines, diagnostics of high-mass star formation, will be shifted to throughout the most sensitive regime of our recommended large-aperture IR-optimized telescope.

9.10 Chemical Evolution of the Interstellar Medium as a Function of Redshift

The history of the Milky Way is written in the elemental composition of its stellar, gaseous, and solid state components. Similarly, as we look to greater redshifts—that is, further back in time—we expect to observe the evolution in composition, star formation rate and location, heating/cooling of the ISM, and dynamics of the gas and stars. Because of the central role that gas and dust play in galaxian processes, this evolution with time is one of the most fundamental in all of astrophysics. Indeed, it might be said that the universe became "modern" only when the ISM became enriched with heavy elements at some early epoch.

Gas enriched by nucleosynthesis has been detected to $z \approx 4$ via metal absorption lines in the spectra of background Quasi-Stellar Objects (QSOs). Analysis of the spectra allow determination of key physical phenomena: temperature, column density, elemental abundances, grain and molecular content, average supernova rate, and so on. A key discriminant in abundance patterns associated with dominant stellar populations is the ratio of iron-peak elements (Fe, Cr, Mn, Ni...) to "α-produced" elements (O, Mg, Si, S...). Both iron peak and α elements are produced in the supernovae that arise from high-mass, short-lived (~10^7 years) progenitors (i.e., type II SNe), but *only* iron peak elements are produced by the low-mass, long-lived type I SNe. This lifetime difference results in a time delay between the first type II SNe and the first type I SNe, which, in turn, leads to a change in the α/Fe ratio over time. The value of the time delay is not precisely known, but is somewhere between 1 and 4 Gyr, and may depend sensitively upon local conditions and characteristics of different galaxies. By looking at high redshift systems, we can observe the gas in very young galaxies and attempt to measure the time delay directly by determining the redshift where low α/Fe ratios first appear.

This process may be observed directly at red and infrared wavelengths with a large space observatory. At rest wavelengths, the UVOIR regime is dense with diagnostic features of the state of the ISM. They range from the N I triplet at 120 nm, O I at 130 nm, Mg I at 285 nm, out to the Fe II lines around 1.6 μm. At the redshifts that we consider to be central to the scientific justification of this mission, $z \geq 3$, even the UV lines will be shifted well into the visible, which is one justification for extending operation of the 4m telescope to as short wavelengths as feasible.

9.11 Galaxy Dynamics in the Early Universe

A combination of very high sensitivity and good angular resolution will be necessary for one of the most important programs in our understanding of the early material universe: determination of the dynamics of normal galaxies and their precursors. The strongest stellar photospheric features are the CO bands at 4.6 μm (fundamental) and 2.3 μm (first overtone). The latter has been used extensively in recent years to study the stellar mass motion in a number of nearby, obscured systems, including M82. These bands appear early in the history of star formation in galaxies, as they are prominent in both giants and, especially, massive supergiant stars. Consequently, high-resolution spectroscopy with an extremely sensitive 4m space telescope in the 10 – 15 μm regime will allow determination of large-scale mass motion on few-kpc-scale sizes for $z \approx 3 – 5$ for the first-overtone CO band.

Such a study will elucidate the dynamics of formation—collapse or collision?—of both the spheroidal and disk systems in galaxies. Furthermore, the influence of a “dark halo” will be apparent, as the stellar component responds to its presence. Finally, the evolution of galaxy kinematics can also be studied, as sensitive observations of the CO bands will be possible throughout the known universe.

9.12 Cosmology

Our previous scientific discussion has concentrated on the problem of understanding how galaxies form, and has illustrated how the plan for HST & Beyond will illuminate those questions. The growth of galaxies in the universe depends on the important details of how gas turns into stars, as described there, but it also depends on the background of the cosmic expansion and the legacy of primeval density fluctuations that grow through the action of gravity into the structures we observe.

Understanding the history of the universe on the large scale is one of the great frontiers of modern science. Even the simplest questions about the age of the universe and its geometry have proved very difficult to answer directly from observation. Extending the lifetime of the HST, constructing a large IR-optimized telescope in space, and the development of interferometry in space will be extremely effective ways to help make progress on these cosmic riddles. For example, an accurate estimate of the expansion age of the universe requires observations that can tell us how fast the universe is expanding today and measurements of its expansion at significantly earlier cosmic times.

Because the objects we want to study emit most of their energy at visible or near-IR wavelengths, the most distant, highest redshift objects will have their emission shifted into the infrared, where sensitivity limits due to the atmosphere and the high temperature of terrestrial telescopes make observations difficult. Even for nearby objects, the obscuring effects of dust can distort the measurements. IR wavelengths, which are much less vulnerable to reddening and absorption by dust, will provide a clearer picture of the situation. Although it still lacks an IR capability, the HST has already begun to narrow the range of values for the present rate of expansion (the Hubble constant) that can be measured locally. While there is still a lively debate, it is clear that the age of the universe derived from the present rate of expansion is perilously close to, or even less than, the ages of the oldest stars observed in globular clusters in our own galaxy. Age estimates for globular cluster stars rest on the measurement of stellar distances in our own Galaxy, whose precision is limited by the Earth's atmosphere. Interferometric measures of stellar distances will place this subject on a much firmer foundation. If the universe was expanding more rapidly in the past, and has decelerated through the action of gravitation, then the age problem is much worse. In the case of a universe which has just enough matter to balance the expansion, the age of the universe could be as low as 8 billion years, much too young to be in concordance with the ages of stars. To resolve this problem, we would need refined knowledge of the stars in globular clusters, more precise measures of the present-day Hubble constant and an accurate measurement of the deceleration.

The potential difficulty in reconciling present observations has led to active speculation that the underlying geometry of time and space is not dominated by matter (even including a full measure of dark matter), but by a "cosmological constant," which in modern terms would be a vacuum energy that acts to speed up the expansion of the universe. Direct measurements of the geometry of the universe are the only way to test whether these ideas are correct, and space-based observations are the surest path to solid progress on these fundamental problems.

Our present-day toolkit for measuring cosmic distances is limited by the collecting area, resolution, and wavelength response of instruments. For example, the measurement of galaxy surface brightness fluctuations is limited to nearby galaxies by limitations imposed by Earth's turbulent atmosphere. HST observations will allow this method to be extended to a larger range, and an extended mission will show how far this method can be pushed. We are certain, though, to run up against the limit imposed by HST's modest collecting area, and this will come about at the distance to the Coma cluster, where it could be of great help in understanding the details of mass distribution on important scales. Providing it performs at the diffraction limit at optical wavelengths, a 4m or larger telescope will be much more effective in extending this precise, but demanding, measurement into unexplored regions.

One of the most promising tools for measuring cosmic distances is the brilliant emission from a Type Ia supernova (SNe Ia). These events, which mark the thermonuclear explosion of a white dwarf star, are one million times more luminous than the Cepheid variable stars that have been used to map distances in the nearby universe. Because these are events whose properties are determined by local physics, they may be less vulnerable to evolutionary effects than complex objects like galaxies. Recent improvements in understanding the properties of SNe Ia have led to hopes that they might be useful to assess the deceleration of the cosmic expansion and to measure directly the global geometry of space. The HST has already been used to study nearby SNe Ia as a target of opportunity. In an extended HST mission, one interesting innovation in operations might be to extend this quick response to more distant targets, where the cosmological effects are larger. Ground-based work with 4m telescopes shows that SNe Ia can be detected out to $z = 0.5$ with present methods at the rate of one per observing night. With the superior imaging of the HST, and especially with the sensitivity of 4m telescope we envision, this direct exploration of the geometry of the universe could be extended to redshift of 1 and beyond. For these objects, resolving a new point source from the galaxy in which it occurs is the key observational problem. Since the flux from the supernova will have been redshifted into the infrared, this points naturally to an instrument of high sensitivity and excellent angular resolution.

Most of the matter in the universe is invisible, but we learn of its presence through gravitational effects. For nearby galaxies, we can measure the effects of dark matter by observing the rotational velocity of a spiral galaxy with a spectrograph. Another way to measure the presence of invisible matter is by observing the effect that gravity produces on light from a distant object that passes near a concentration of mass. Gravitational lensing produces remarkable phenomena, including giant arcs that are the images of background galaxies that are imaged by the lens action of dark matter in clusters of galaxies, and multiple images of a single quasar far behind a lensing galaxy. The properties of arcs help determine the dark matter distribution in galaxy clusters, the statistics of lenses constrain the possible geometric effects of the cosmological constant, and the time delay between observed changes in the separate images of a multiply imaged quasar can be used to make measurements of extragalactic distances that depend only on the lens geometry. All of these measurements demand high spatial resolution, high sensitivity, and for high redshift, they require infrared capability. If we wish to trace the formation of clusters, observe the effects of cosmic expansion on the statistics of lensed objects, and make an independent measurement of the cosmic distance scale we need to be able to image over large areas with a resolution of 0.2 arcsec or better in the near infrared.

10. Space Interferometry: A Powerful New Tool for Astrophysics

10.1 Introduction

The Committee's third recommendation is for the development and demonstration of space interferometry. The motivating factors for space interferometry include the following:

(1) *High resolution imaging.* High angular resolution has consistently been a priority goal for astronomical telescopes across the entire electromagnetic spectrum. The advantages of very high angular resolution are demonstrated by the success of radio interferometers such as the Very Large Array (VLA) and Very Long Baseline Interferometry (VLBI). Optical astronomers are implementing adaptive optics to sharpen images obtained with ground-based telescopes. Space-based interferometry will enable us to obtain high resolution images of almost every type of celestial object, including the surfaces of nearby stars, the ejecta of novae and supernovae, and accretion disks around young stars, white dwarfs, neutron stars, and both stellar-mass and super-massive, extragalactic black holes. This is well summarized in the Bahcall Committee *Working Papers* (page V-6, figures 3a and 3b).

(2) *High accuracy astrometry.* The scientific goals consist of understanding the geometry of the Universe (the distance scale, the curvature of the universe), the dynamics of our galaxy, including understanding the distribution of matter (visible and invisible), and the *quantitative* comparison of stellar structure and evolution models with observations. We purposely chose the adjective "accuracy" rather than precision to emphasize that space interferometry will allow us to obtain distance measures that are truly fundamental (i.e., parallax). A more complete discussion and demonstration of the astrophysical gains as a function of angular resolution can be found in *Science Objectives and Architectures for Optical Interferometry in Space* (Astrotech 21, JPLD-8540, Vol. 1, page ES-3).

(3) *Detection and characterization of extra-solar planets.* In section 10.2 we argue that IR interferometry is essential to the *direct* detection and spectroscopic characterization of nearby Earth-like planets. Moderate or large apertures (diameter $\gtrsim$ 1.5 m) are required to obtain a detectable signal from the faint planet, but this size also depends on our solar system's zodiacal background, and thus on the observatory's orbit. Optimization for wavelengths longer than 3 μm is required, as the contrast of star to planet is much more favorable, but also because absorption bands of key molecules such as CO_2, H_2O, and O_3 are found in this region. Very long baselines (tens to hundreds of meters) are needed to overcome zodiacal dust which may be present around the target star.

Below we discuss in more detail the unique gains obtained by space-based interferometers and the important science goals that can be achieved with the technique of space interferometry. Each of the three goals listed above are best served with optimized interferometers. However, common areas of technological development which they share are listed in section 10.5. We conclude with a list of recommendations in section 10.7. We have made liberal use of the Bahcall Committee's *Working Papers* in crafting section 10.3 and section 10.4.

10.2 Why Space-Based Interferometers?

It is important to understand the unique capabilities offered by space-based interferometers relative to ground-based systems. The primary limitation of ground-based interferometers arise from the turbulence generated in the atmosphere. The turbulence generates spatial distortion of the planar wavefront (which leads to scattering and the formation of the seeing disk) and radial variations in the effective path lengths to the source. The spatial distortion is common to both interferometry and single telescopes. The second is of particular concern to interferometry, as it is necessary to know the path length to a fraction of wavelength in order to obtain white light fringes.

A number of ground-based interferometers are now being constructed or are in operation: Big Orbital Array (BOA, Anderson Mesa), SUSI (Australia), Center for High Angular Resolution Astronomy (CHARA, Mt. Wilson), Palomar Test Interferometer (Mt. Palomar), Cambridge Optical aperture Synthesis Telescope (COAST, United Kindgom), Infrared Optical Telescope Array (IOTA, Mt. Hopkins). These are essentially single- r_0 instruments and have apertures of less than 1 m. The principal purpose of these instruments is to study stellar angular diameters, as well as their masses and distances. The Palomar Test Interferometer is a dual-beam interferometer specifically dedicated to indirect detection (via angular perturbation) of Jupiter-mass planets orbiting nearby stars. Two ambitious next-generation instruments are being planned [Keck interferometers and the European Space Observatory's (ESO) Very Large Telescope Interferometer(VLTI)]. These employ a mixture of 10m (or 8m), with 2m telescopes as outriggers, and will require adaptive optics to rectify the wavefront for each detector. We assume that the cost of outfitting telescopes with adaptive optics will decrease in the near future, and that each element will be equipped with a laser that will then enable us to remove the spatial perturbations. However, determination of the unknown radial perturbation will require a true celestial source of sufficient brightness to fringe-track over the temporal decoherence timescale, $\tau_0 \sim 20(\lambda/0.55\ \mu\mathrm{m})^{1.2}$ ms. Moreover, this celestial source has to be within the isoplanatic angle $\theta_0 = 20(\lambda/0.55\ \mu\mathrm{m})^{1.2}$ arcsec. (We assume 0.5 arcsec seeing at 0.55 μm, under typical conditions.)

The performance of ground based interferometers, taking all these considerations into account, has been estimated by M. Colavita and M. Shao of JPL in *Science Objectives and Architectures for Optical Interferometry in Space* (Astrotech 21 Workshop, Series 1, JPL D-8540, 143-155). Their conclusions are presented on page 152 of their report. The key conclusions from the Colavita-Shao study are as follows:

(1) *Optical.* The primary limitation for ground-based interferometers is sky coverage: $\lesssim$ 2 percent of the sky can be observed from a single location. Ths limit refers only to (point source) detection. Full imaging requires the use of closure phase and the limiting magnitude of ~15 is not impressive. Clearly, high resolution imaging of many varieties of celestial objects *requires* space-based interferometers. With 2m apertures and a stable structure, a limiting point source sensitivity of 29 mag is achievable with perfect detectors.

(2) *Near-IR ($\lambda \approx 1 - 2\ \mu m$).* Provided our assumptions, especially about the isoplanatic angle, are not too optimistic, it appears to be possible to observe a large fraction of the sky from the ground. The case for a space-based near-IR system is not as compelling as that for a visible interferometer.

(3) *Mid-IR ($\lambda \geq 2\ \mu m$).* A space-based interferometer offers an enormous gain, of at least two orders of magnitude, provided the telescope is cooled. Thus, there is a compelling case for a space-based mid-IR system.

As summarized above, a number of ground based optical interferometer projects are in various stages of completion. Due to their low sensitivity, these arrays are limited to the study of bright stars. Despite this limitation, these pioneering instruments will produce fundamental advances in stellar astrophysics, yielding precise values for fundamental parameters, such as size, mass and effective temperature. These can then be compared to the values predicted by our best stellar models. Discrepancies will expose our gaps in stellar theory and enable us to improve our understanding of the interior structure of stars. Over the next decade, we anticipate the construction of sensitive ground based interferometers optimized for the near-IR. Examples include the Keck interferometer and VLTI.

Apart from increased sensitivity, there are two other advantages of space-based systems that are particularly important for astrometry and planet detection. First, the limitations with ground-based systems due to scattering by the reference star and scattering within the instrument (the laser-guide star does not result in a fully corrected planar wavefront and, thus, subsequent development of scattering of each beam will lead to some limitations) is not well understood. This will certainly lead to restrictions in the ability to image in the vicinity of either the bright target star or the reference star. Second, absolute astrometry requires the measurement of absolute position on the sky. Ground-based interferometers will do an excellent job of measuring relative positions, but only within the isoplanatic patch. Absolute astrometry is possible through the use of group delay. But, as this measure is considerably inferior to phase, the limiting accuracy is expected to be ~1 milliarcsec. This level of accuracy limits the use of ground-based interferometers to measuring parallaxes of nearby objects. Thus, high-accuracy and high-precision astrometry can be done only from space.

We conclude that high-resolution and high-sensitivity imaging at optical wavelengths requires a space-based interferometer. Similarly, accurate and precise astrometry requires a space-based system. Finally, direct detection of planets (and their characterization) requires a cooled IR interferometer. Because only modest special requirements are necessary for near-IR operation, it would be advantageous to include near-IR detectors in the optical interferometer system.

10.3 High Resolution Imaging

The need for higher angular resolution has been one of the three paramount historical imperatives in observational astronomy, along with increased overall sensitivity and new spectral bands. In radio astronomy, this led to the rapid development and deployment of interferometers, with the VLA representing the state-of-art in connected element interferometry and the VLBI the state-of-art in long baseline interferometry.

A variety of phenomena can be resolved at different levels of angular resolution. The most dramatic increase in the range of accessible phenomena occurs at resolution better than 1 milliarcsec, corresponding to the resolution now available with radio VLBI. With such resolution, one could not only resolve a nearby main sequence star, but could also make detailed images of giant stars and of stellar winds. In the near-IR, the structure of disks and incipient bipolar outflow in star-forming regions, and the early evolution of novae and planetary nebulae could be studied. Milliarcsec resolution will give crude structural information about the broad emission-line regions in active galactic nuclei, and detailed information about the narrow-line regions that are thought to represent the transitional zone between the active nucleus and the ISM of the host galaxy.

Resolution of 10-100 microarcsec would yield a new set of breakthroughs. Not only could the broad-emission line regions of AGNs and quasars be mapped, but it would become possible to investigate the accretion disks themselves (in the optical and UV). Accretion disks in close binaries could be imaged along with the mass transferring streams that feed them. Supernovae out to the Virgo cluster could be studied as early as three months after the explosion, and surface phenomena of main sequence and nearby white dwarfs could be mapped. Combining ground-based spectroscopy with detailed imaging of supernovae is one of the best methods to obtain distances to distant galaxies, a very promising technique for our quest to determine the geometry of space on the largest scale. However, to be useful as an imager, a high resolution instrument must also have adequate sensitivity and dynamic range, which must be incorporated into the baseline mission requirements.

10.4 High Accuracy Astrometry

Although imaging will probably prove to be the most compelling long-term motivation for interferometry, the most profound product of the the early years of interferometry may well be ultra-precise (and accurate) astrometry made possible in space by freedom from terrestrial disturbances. See the comprehensive review by Reasenberg et al. (AJ 96, 1731-1745, 1988).

The RR Lyrae and Cepheid "standard candles" are critical to the determination of the Hubble constant. At present, the realistic uncertainty in the Cepheid distance calibration is about 15 percent. Microarcsec parallax measurements would reduce this uncertainty by at least an order of magnitude. Quasars are generally assumed to be at "cosmological distances" and therefore exhibit no proper motion of their centers of mass. An instrument with a few microarcsec precision could measure the relative motions of quasars, not only testing the cosmological-distance hypothesis, but also investigating the large-scale motions in the early universe.

In the area of Galactic structure, there are several applications of precision astrometry. The measurement of fundamental distances to stars that form the basis for determining the extragalactic distance scale, such as Cepheid and RR Lyrae variables, would be invaluable.

We could also measure the position, parallax, and proper motion of many of the massive young stars that mark the spiral arms. These data would map the arms without the distance uncertainty that now degrades such maps and would even show the motion associated with the density waves that are believed to be responsible for the existence of the arms. Measurements of stars within a few kiloparsecs of the Sun would yield a portion of the rotation curve for the Galaxy and, thus, constrain the mass distribution in the Milky Way. Proper motions of the Large and Small Magellanic Clouds would make possible independent determinations of the total mass of the Galaxy and thus would tell us the amount of “dark matter” it contains.

Other applications include the determination of the three dimensional orbits of globular clusters. The cluster orbital parameters could be used to investigate correlations between metal abundance, perigalactic distance, cluster radius, and orbital eccentricity, all having strong consequences for theories of the formation of the Galaxy.

Ground-based interferometers will make significant contributions to stellar astronomy by observing suitable binary systems. The observations will yield precise masses, radii, and distances. However, observations of special binary systems, such as black-hole/main-sequence binary systems, can be done only with space interferometers (unless the target star has a suitable reference star within the isoplanatic patch). Wobble of the secondary main sequence star can lead to direct determination of the mass of the black hole primary.

10.5 Common Requirements for Space-Based Visual and Infrared Interferometers

Successful implementation of space-based interferometers requires successful demonstration of multiple sub-systems. At present, we assume that desirable space-based systems will be of the Michelson rather than the Fizeau type primarily because this type appears to be most favored by the active practitioners in this field. Second, we see the desire to image as being of great interest to a broad community. In coming to this conclusion, we note that at radio wavelengths, both astrometry and imaging are done with the VLA and VLBI. We consider it of great strategic advantage to combine imaging with astrometry and to have a development path that unifies all themes. While we remain neutral as to which is the best architecture (Michelson versus Fizeau), we strongly favor a development path that is common to high angular resolution imaging, precision astrometry, and detection of extra-solar planets. We suspect that such an approach is necessary in the current tight fiscal climate.

10.6 Current Baseline Description of Space-Based Visual and Infrared Interferometers

In response to recommendations of the NAS "Bahcall Report" in 1991, NASA has been funding technology development programs in support of a visual-wavelength astrometric spatial interferometer. This project, Space Interferometry Mission (SIM), is intended to be launched in the early part of the next decade. At present, the baseline design is derived from the JPL Optical Stellar Interferometer (OSI), with the following characteristics: (1) a 7m baseline; (2) an accuracy of 5 microarcsec for wide field astrometry for sufficiently bright objects; (3) an accuracy of 10 microarcsec on stars with $m_V \approx 20$ after an integration time of 1000 s; and (4) limited, but real, imaging capability. As part of the initiative to include the SIM program within the broader Exploration of Neighboring Planetary Systems (ExNPS) activity, serious consideration is being given to increasing the capability of the visual interferometer in two specific areas: (1) inclusion of a nulling focal plane system; and (2) extending the baseline to, perhaps, 20 m. Current estimates are that the former addition is a minor perturbation on the system's basic design. However, the latter is a more serious effort and will increase the cost and complexity of SIM by an as-yet uncertain factor. During recent deliberations on the requirements for the new SIM, longer-wavelength operation was also considered. The strong consensus was that the technical requirements for operation longward of about 2 μm—instrument and observatory cooling, cryogenic operation of active structures, alternative orbits, etc—meant a *substantial* increase in mission cost. Moreover, the primary scientific justifications for SIM would be sacrificed. A more detailed, current discussion of this program can be found in the 1996 report of the Space Interferometry Science Working Group (SISWG; Deane Peterson, Chairman).

An interferometer with the goal of detecting and characterizing nearby planetary systems has substantially different baseline characteristics from that of SIM, particularly as the IR mission must operate at substantially longer wavelengths as a nulling system, with a much longer baseline. A number of basic designs have been proposed by European and American teams. At present, the most popular design appears to be a linear array on a baseline of 75 ± 25 m, consisting of at least five ~1.5m apertures. Operation over the wavelength range $\lambda \approx 5 - 15$ μm suggests equilibrium temperatures less than about 50 K for key optical elements. Such temperatures should be easily achieved in orbits beyond the asteroid belt, which appear to be required to sufficiently reduce the contaminating "background" zodiacal dust emission.

10.7 Conclusions and Recommendations

In summary, the development of space interferometry consists of two elements: developing imaging and astrometry at optical wavelengths for a large number of astrophysical targets, and devisinging an IR interferometer for the specific purpose of detecting and characterizing extra-solar planets. Common to both goals are: (1) development of relatively inexpensive telescope elements with minimum apertures of about 1 m; (2) demonstration of very precise, stable structures in space; and (3) successful demonstration of beam combination. Item (1) is being pursued as part of the SIRTF program and will be extended as a central goal of the large, filled aperture IR observatory. Item (2) requires the demonstration of precision laser metrology at the level appropriate of fraction of a wavelength over the entire baseline. We are aware that considerable progress in this direction has been made at NASA labs. Demonstration that such precision can be achieved in space, as well as at low temperatures, is vital. As a precursor, this might be best done in one of the Space Shuttle programs.

Item (3) requires the development of delay lines in the case of a Michelson interferometer. Such delay lines have been demonstrated to work in many ground-based systems. Again, demonstration in a space environment, notably at low temperatures, is necessary. Furthermore, suitably precise optics and alignment technology must be developed.

Demonstrations will have to take place well before undertaking the ambitious planet-detection system. It seems reasonable, then, to explore a suitable intermediate-level interferometric mission to demonstrate items (2) and (3). The precise choice of wavelength may not be critical, although we note that considerable returns can be expected at optical wavelengths.

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