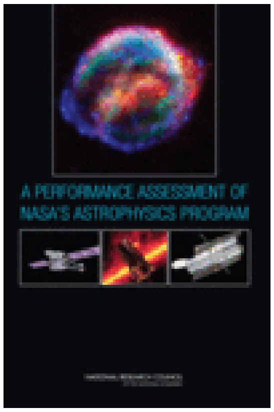

A Performance Assessment of NASA's Astrophysics Program

NASA Astrophysics Performance Assessment Committee, National Research Council

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THE NATIONAL ACADEMIES
Advisers to the Nation on Science, Engineering, and Medicine

A PERFORMANCE ASSESSMENT OF NASA'S ASTROPHYSICS PROGRAM

NASA Astrophysics Performance Assessment Committee

Space Studies Board

Board on Physics and Astronomy

Division on Engineering and Physical Sciences

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Cover—Top image: Remnants of a supernova (SN 1604, also known as Kepler's Supernova) discovered by Johannes Kepler in 1604. The image is a composite of data from the Spitzer Space Telescope (color-coded red in the image), the Hubble Space Telescope (yellow), and the Chandra X-ray Telescope (blue and green). SOURCE: NASA.
Bottom, left: Artist's rendition of the Chandra X-ray Telescope. SOURCE: CXC/TRW.
Bottom, center: Artist's rendition of the Spitzer Space Telescope. SOURCE: NASA.
Bottom, right: The Hubble Space Telescope. SOURCE: NASA.

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Preface

The past 20 years have been a time of remarkable discovery in the field of astronomy and astrophysics. In conjunction with advances in other fields such as elementary particle physics, understanding of the physical laws governing the universe has grown and changed in substantial ways. These developments would not have happened without the missions and programs supported by NASA, and opportunities for future breakthroughs will require NASA's continuing leadership in the scientific exploration of the 21st century. The scientific community recognizes these facts, as does the American public. Scientific discovery has been central to NASA's ability to capture the imagination of the public and to inspire new generations of scientists and engineers.

In Section 301(a) of the NASA Authorization Act of 2005, the Congress directed NASA to have "[t]he performance of each division in the Science directorate . . . reviewed and assessed by the National Academy of Sciences at 5-year intervals." In early 2006 NASA asked the National Research Council (NRC) to conduct such an assessment for the agency's Astrophysics Division. The committee's statement of task was to

> study the alignment of NASA's Astronomy and Physics Division (the Division) with previous NRC advice—primarily from the reports *Astronomy and Astrophysics in the New Millennium* (NRC, 2001) and *Connecting Quarks with the Cosmos* (NRC, 2003). More specifically, the committee will address the following:
>
> 1. How well NASA's current program addresses the strategies, goals, and priorities outlined in Academy reports;

> 2. Progress toward realizing these strategies, goals and priorities; and
>
> 3. Any actions that could be taken to optimize the scientific value of the program in the context of current and forecasted resources available to it.
>
> The study will not revisit or alter the scientific priorities or mission recommendations provided in the cited reports, but may provide guidance about implementing the recommended mission portfolio leading toward the next decadal survey.

The NASA Astrophysics Performance Assessment Committee met three times in the course of its deliberations. In its first meeting on June 19-21, 2006, at the Keck Center of the National Academies in Washington, D.C., the committee focused on understanding the programmatic status of the Astrophysics program at NASA and the context in which this report was requested. The committee's second meeting was held August 14-16, 2006, at the Science Museum of Minnesota in St. Paul. Representatives of each of the projects recommended in the two NRC reports *Astronomy and Astrophysics in the New Millennium* (2001) and *Connecting Quarks with the Cosmos* (2003) were invited to discuss the progress made thus far. After the second meeting, subsets of the committee met with Mary Cleave, NASA associate administrator for science, Charles Elachi, director of the Jet Propulsion Laboratory, and Edward Weiler, director of the Goddard Space Flight Center. The committee's final meeting on October 20-22, 2006, at the Keck Center of the National Academies in Washington, D.C., was devoted to work on the committee's report.

The committee thanks those who made formal presentations at its meetings and expresses its deep appreciation to the hosts and facilitators of its site visits; the hospitality was impeccable and the conversations candid, enlightening, and invaluable.

Two other recent NRC reports have addressed related subject matter concerning NASA's entire science program: *An Assessment of Balance in NASA's Science Programs*, which was released on May 4, 2006, and "Review of NASA's 2006 Draft Science Plan," a letter report sent on September 15, 2006, to Mary Cleave, associate administrator, Science Mission Directorate, NASA Headquarters. Although the reviews and deliberations leading to the present report were conducted independently of those two studies and dealt with questions pertaining only to astrophysics, the findings and recommendations presented here are consistent with and complement those provided in the two 2006 reports.

Kenneth H. Keller, *Chair*
Martha P. Haynes, *Vice Chair*
NASA Astrophysics Performance Assessment Committee

Acknowledgment of Reviewers

This report has been reviewed in draft form by individuals chosen for their diverse perspectives and technical expertise, in accordance with procedures approved by the National Research Council's Report Review Committee. The purpose of this independent review is to provide candid and critical comments that will assist the institution in making its published report as sound as possible and to ensure that the report meets institutional standards for objectivity, evidence, and responsiveness to the study charge. The review comments and draft manuscript remain confidential to protect the integrity of the deliberative process. We wish to thank the following individuals for their review of this report:

Jonathan Bagger, Johns Hopkins University,
Roger Blandford, Stanford University,
Neal J. Evans II, University of Texas at Austin,
Richard McCray, University of Colorado,
Christopher F. McKee, University of California at Berkeley,
George A. Paulikas, Aerospace Corporation (retired), and
Anneila L. Sargent, California Institute of Technology.

Although the reviewers listed above have provided many constructive comments and suggestions, they were not asked to endorse the conclusions or recommendations, nor did they see the final draft of the report before its release. The review of this report was overseen by Robert Frosch, Harvard University. Appointed by the National Research Council, he was responsible for making certain that an independent examination of this report was carried out in accordance with institutional procedures and that all review comments were carefully considered. Responsibility for the final content of this report rests entirely with the authoring committee and the institution.

Contents

Summary

Astronomy and astrophysics is in the midst of a period of unprecedented discovery, yielding new understanding of phenomena ranging from dark energy and extrasolar planets to supermassive black holes, as well as insights about the birth of the universe. Revolutionary discoveries in the field have been recognized by the awarding of four Nobel Prizes, the most recent being the 2006 Nobel Prize in Physics for the discovery, by NASA's Cosmic Background Explorer satellite, of the seed inhomogeneities in the matter density which ultimately led to all structure in the universe. Some of the breakthroughs were made by NASA missions, and some by ground-based instruments and telescopes. Always, the coordination of ground- and space-based resources has been critical to the rapid advances made in understanding the universe.

Today the field of astronomy and astrophysics is poised for more breakthroughs. Stunning opportunities for the decade of 2000-2010 were identified in the 2001 National Research Council (NRC) decadal survey *Astronomy and Astrophysics in the New Millennium* (AANM; National Academy Press, Washington, D.C., 2001) and expanded on in the subsequent NRC report *Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century* (Q2C; The National Academies Press, Washington, D.C., 2003). Together the two reports laid out an ambitious program of ground- and space-based projects to turn these grand opportunities into exciting discoveries, and NASA's 2003 Astrophysics Program plan responded effectively. In particular, the 2003 plan properly addressed the stated priorities and was well optimized among mission goals, mission types, and mission sizes.

However, the implementation of the 2003 plan has been severely limited by circumstances and events both internal and external to NASA, including the Columbia disaster, mission cost overruns, smaller budgets than anticipated by the astrophysics community, and significant changes at NASA in both management and mission focus. As a result, the 2003 plan was dramatically descoped, and NASA's 2006 program plan addresses the goals of the NRC reports much less effectively than did the 2003 plan.

The sudden slowing of progress toward the exciting science goals laid out by the AANM survey and the Q2C report led to the creation of the NASA Astrophysics Performance Assessment Committee, which in this report considers how well NASA's current Astrophysics program addresses the strategies, goals, and priorities outlined in the two NRC reports; evaluates progress toward realization of those goals; and suggests mechanisms by which the scientific value of the implemented program can be optimized.

At present, NASA's Astrophysics Division does not have the resources to pursue the priorities, goals, and opportunities described in the NRC reports and has chosen to concentrate its resources on the highest-priority large and medium missions recommended by the AANM survey and those in development from the previous survey, to the detriment of the Explorer line and other small initiatives. As a result, NASA's current Astrophysics program is no longer well balanced across a desirable range of scientific areas, mission sizes, and mission-enabling activities, thus falling short of the AANM survey's specific recommendation that NASA maintain a diverse mission portfolio.

The Astrophysics Division's adopted strategy comes at a steep scientific cost, substantially reducing the prospects for future contributions to astrophysics by NASA missions. Moreover, because there will be fewer space missions, there will be fewer opportunities for coordination between space- and ground-based initiatives, and as a result the entire astronomy and astrophysics enterprise has been and will continue to be negatively impacted.

The committee recommends that NASA take a series of steps aimed at optimizing the scientific return from its Astrophysics program in the near term and laying the groundwork for continuing progress even in a restricted budget climate.

Recommendation 1: NASA should optimize the projected science return from its Astrophysics program by (a) ensuring a diversified portfolio of large and small missions that reflect the science priorities articulated in the 2001 decadal survey *Astronomy and Astrophysics in the New Millennium* and (b) investing in the work required to bring science missions to their full potential: e.g., technology development, data analysis, data archiving, and theory.

The most important step in implementing this recommendation is a reevaluation by the Astrophysics Division of the program's mission balance, with the goal of restoring the Explorer line to the launch rate achieved in the early part of this decade. The division should also identify structural mechanisms (e.g., firewalls, cost caps, constraints on the concentration of resources in single programs) to protect small programs and mission-enabling activities such as technology development that are critical for optimizing the science return from missions and are particularly vulnerable to cost growth in large missions, changes in accounting systems, or project budget instability.

NASA should also seek to limit cost growth in all missions by exploring ways to provide less expensive launch services (particularly for smaller missions), re-examining mission safety and assurance requirements to achieve an appropriate match with mission size, relaxing deorbit requirements for smaller spacecraft in low-cost missions, and finding improved ways to establish and maintain effective international collaborations on missions of all sizes.

Recommendation 2: NASA should consider changes in its advisory structure to shorten the path between advisory groups and relevant managers so as to maximize the relevance, utility, and timeliness of advice as well as the quality of the dialogue with advice givers. Clear communication between stakeholders and the agency is critical to a strong partnership for successfully implementing national priorities and realizing community science aspirations.

Recommendation 3: NASA should recognize that ambitious missions could require significantly more than 10 years to complete, from conception through technology readiness and launch. NASA should insist that future decadal surveys specifically include in their prioritizing deliberations those projects carried over from previous surveys that have not yet entered development (NASA Phase C/D or equivalent). To enable an accurate assessment of science success and overall life-cycle costs, NASA should, in presenting potential missions to future survey committees, also distinguish between projects that are ready for implementation and those that require significant concept design or technology investment.

1

Introduction

The second half of the 20th century brought breakthroughs in astronomy and astrophysics made possible by a confluence of increasingly sophisticated instruments, advances in computing and information technology, new numerical tools for data processing and analysis, and powerful new theoretical insights. The limits of the observable universe now range from unimaginably large intergalactic distances down to subatomic particles, and unexpected and remarkable connections have been established between natural laws pertaining to the largest and the smallest scales.

The recent breakthroughs and potential for future advances are nothing short of revolutionary. Ten years ago the first extrasolar planets were discovered; today more than 200 are known. In our solar system, hundreds of Kuiper belt objects, a handful of trans-neptunian objects larger than Pluto, and comets with unique volatile content and mineral composition together are leading to a new understanding of our own cosmic origins. Dust and gas disks around intermediate- and low-mass stars akin to the Sun with gaps, warps, and knots detected by the Hubble Space Telescope (HST), the Spitzer Space Telescope, and large ground-based telescopes are perhaps telltale signs of active planetary system formation. Driven by these advances, in the coming years the discovery and imaging of Earth-like planets and perhaps the first evidence for life elsewhere in the universe may be realized.

Discoveries relating to the origin, evolution, and destiny of the universe have been at least as revolutionary as the discoveries made on planetary scales. The all-sky measurement of the cosmic microwave background by the Cosmic Background Explorer (COBE) satellite made an overwhelmingly compelling case for a big-bang cosmology and opened the door to studying the earliest moments of creation. The significance of the COBE breakthroughs was recognized by the

2006 Nobel Prize in Physics, awarded to two COBE scientists, and the Gruber Prize in Cosmology, awarded to the entire COBE team.

Results from the Wilkinson Microwave Anisotropy Probe (WMAP) Explorer mission and HST, and data from the Sloan Digital Sky Survey, a project funded in part by NASA, have pinned down the expansion rate, composition, and age of the universe and provided strong evidence that the big bang included a burst of rapid expansion known as inflation, driven by an unknown mechanism. In combination with important data from ground-based observations, these missions have enabled exploration of two profound mysteries: the attractive gravity of dark matter that holds together galaxies and larger structures in the universe, and the repulsive gravity of a new form of dark energy that causes the expansion of the universe to accelerate, rather than slow down. Together, dark matter and dark energy account for some 96 percent of the "stuff" in the universe. The nature of dark matter and dark energy and the physical cause of inflation appear to involve deep connections between the submicroscopic world and the universe at its largest scales and earliest times. Present opportunities for deepening understanding of the universe and the laws that govern it are profound.

For half a century, black holes have captured the attention of scientists, science fiction writers, and the public alike. A combination of ground- and space-based observations have now firmly established the existence of hundreds of stellar-sized black holes in our galaxy, the Milky Way, as well as a correlation between the mass of black holes at the center of a galaxy and the mass of the galaxy's central bulge. NASA missions—including the Compton Gamma Ray Observatory (CGRO), Swift, and High Energy Transient Explorer-2 (HETE-2)—played key roles in establishing that a substantial fraction of the mysterious gamma-ray bursts, discovered 40 years ago by military satellites monitoring the nuclear test-ban treaty, are associated with the birth of black holes throughout the universe, and they occur at a rate exceeding one per day. A proposed new generation of probes will test whether or not black holes conform with the predictions of Einstein's theory of general relativity. If there are discrepancies, analysis and interpretation of results from the probes may provide clues as to how Einstein's theory can be modified.

A large proportion of the discoveries summarized here were made using tools provided to the scientific community (and, through it, to the larger world) by U.S. taxpayers. The U.S. Congress, through the National Science Foundation (NSF), the Department of Energy (DOE), and NASA, has funded the development, construction, and use of telescopes, particle accelerators, supercomputers, and other necessary equipment. Because the financial investment was well matched to the scientific opportunities, the gains in knowledge have been enormous. This extraordinary outcome has not been merely serendipitous. In astronomy and astrophysics a system has been developed to ensure that government resources are utilized in the most effective way. Since 1964 the astronomy and astrophysics community, through the National Research Council, has produced a series of reports, known collectively as the decadal surveys, laying out priorities for federal investment in the field. The latest such volume, *Astronomy and Astrophysics in the New Millennium* (AANM; National Academy Press, Washington, D.C.), was published in 2001.

Comprehensive, collaborative, and broadly consultative efforts, the decadal surveys have proven highly effective in setting the agenda for scientific activities in astronomy and astrophysics in each of the last five decades. Guided largely by those surveys, NASA, through its Astrophysics Division (and precursor organizational structures), has implemented a science program whose contributions to advances in the field have been enormous. The most widely known example is the Hubble Space Telescope, which has returned unprecedented images of objects ranging from the Moon and Mars to the galaxies in the Hubble Ultra Deep Field—a collection of the most distant objects ever detected. HST, the first of NASA's Great Observatories to be launched, was followed by the CGRO, the Chandra X-ray Observatory, and the Spitzer Space Telescope (a sensitive infrared facility). In addition to these flagship missions, NASA has built and operated numerous successful smaller missions, such as Swift, HETE-2, and WMAP, which have helped to determine the content, age, shape, and history of the universe. The current portfolio of operating missions has revolutionized the substance and the practice of astronomy and astrophysics, and NASA deserves substantial credit for their successes. The advice and priorities set out in the decadal surveys have been sound, and NASA's implementation has been effective.

In this time of continuing extraordinary discovery and stunning opportunities in astrophysics, however, NASA's ability to forge ahead is constrained. As described in more detail in subsequent chapters, demand for increased funding for existing astrophysics programs, coupled with a less robust outlook for future budgets, is hindering the agency's ability to achieve the goals identified in the AANM decadal survey.

As is apparent in the discussion that follows, the committee believes that NASA's effective management of its Astrophysics program is critical to realizing the great opportunities ahead to advance understanding of the universe and the place of humans within it. Chapter 2 summarizes the recommendations made in the AANM survey and the Q2C report. Chapter 3 assesses how well NASA's plans address the goals laid out by the AANM survey and the Q2C report (addressing point one of the charge) and what progress has been made toward realizing those goals (addressing point two). In Chapter 4 the committee analyzes the factors that have led to the increased strain on the Astrophysics Division, and in Chapter 5 it recommends steps that NASA can take to optimize the science value of its Astrophysics program and its central role in enabling the discoveries and breakthroughs ahead (addressing point three). The committee concluded that the long-term structural issues in NASA's Astrophysics program should be addressed by the next decadal survey and, in response to the final section of the charge, recommends changes to the decadal survey process intended to prevent some of the issues identified in this report from recurring in the future.

2

Summary of AANM Survey and Q2C Report Recommendations

Following a tradition extending back to the 1960s, *Astronomy and Astrophysics in the New Millennium* (AANM; National Academy Press, Washington, D.C., 2001) surveyed recent progress in astronomy and astrophysics and made recommendations for the most important new initiatives in the decade 2000-2010. The survey process overlapped with a period of remarkably rapid advances in the field. Planets orbiting stars other than the Sun were being discovered; observations revealed density variations in the very early universe, a few hundred thousand years after the big bang, and clearly identified them as the seeds of galaxy formation; new objects were discovered in the outer part of the solar system, beyond the orbits of Neptune and Pluto; extremely distant galaxies were found near the edge of the observable universe; massive black holes were discovered in the centers of many galaxies; and, near the end of the 1990s, the existence of a new form of energy that pervades the entire universe and has repulsive gravity—called dark energy—was inferred from the discovery that the expansion of the universe is speeding up, not slowing down.

Building on these and other advances, the AANM survey committee looked forward to identify several key problems as particularly ripe for progress in the years 2000-2010, including further elucidation and detailed measurements to better characterize the large-scale properties of the universe, including matter, energy, age, and expansion history; the dawn of the modern era of the universe, when the first stars and galaxies formed; the formation and evolution of black holes of all sizes; the formation of stars and planetary systems, including both giant and terrestrial-type planets; and the relationship of Earth to its astronomical environment.

The survey committee went on to lay out a plan under which the most exciting problems could be addressed, setting explicit priorities to guide NASA and the NSF in their allocation of available resources to optimize the return on the nation's investment in astrophysics.

The opening pages of the AANM survey emphasized the importance of balancing new initiatives with research efforts already underway, and ensuring adequate funding for unrestricted grants providing broad support for research—especially research programs involving students and young scientists just entering the field. These priorities were described as prerequisites for a vigorous scientific program extending over the next decade and beyond. The survey committee emphasized the critical need for NASA to maintain a diverse range of mission sizes including major, moderate, and small missions, in order to ensure the most cost-effective returns from space astrophysics. In addition, recognizing the important synergies between experimentation and theory, the survey committee spelled out the essential importance of integrating targeted theoretical efforts into plans for moderate and major new initiatives. The survey committee also emphasized the need for long-term investment in technology development to enable future advances not directly related to ongoing missions.

The AANM survey prioritized recommendations for new initiatives into three categories—the major, moderate, and small classes mentioned above—on the basis of projected costs, including costs for the first 5 years of operation. The dividing lines between categories for space-based missions were set at $140 million and $500 million.[1]

In the major category, the clear top priority was a follow-on to the Hubble Space Telescope, the Next Generation Space Telescope (now known as the James Webb Space Telescope, or JWST). This proposed facility was to be an 8-meter-class infrared telescope with 100 times the sensitivity and 10 times the image sharpness of HST, and would involve cooperative participation by the European and Canadian space agencies. The cost to the U.S. government was estimated by NASA at $1,000 million, excluding some necessary technology development funded in a separate line. The second highly recommended major spaced-based initiative for the decade was Constellation X (Con-X), a suite of four powerful x-ray telescopes designed to study the formation and evolution of black holes. NASA's estimated cost for Con-X was $800 million. In addition, the AANM survey strongly recommended NASA funding for the technology development required to develop the Terrestrial Planet Finder (TPF) and Single-Aperture Far Infrared Observatory (SAFIR). It was estimated that with suitable planning and development, these missions could be started in earnest near the end of the 2000-2010 decade.

Top priority for space-based missions in the moderate category was assigned to the Gamma-ray Large Area Space Telescope (GLAST), with a NASA-estimated

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[1] Costs were provided in FY 2000 dollars.

cost of $300 million. GLAST was to be a joint NASA and DOE mission; it would provide observations of gamma rays from 10 MeV to 300 GeV with six times the effective area, six times the instantaneous field of view, and much better angular resolution than the Energetic Gamma Ray Experiment on NASA's CGRO. Next in the ranking was the Laser Interferometer Space Antenna (LISA), designed to detect gravitational waves from merging supermassive black holes anywhere in the universe and from binary stars throughout the galaxy. International cooperation was assumed for each of these projects; NASA's estimated U.S. cost for LISA was $250 million. Three additional space missions were also ranked: the Solar Dynamics Observer (SDO), designed to study the Sun's outer convective zone and corona; the Energetic X-ray Imaging Survey Telescope (EXIST), which was to be attached to the International Space Station; and a facility known as Advanced Radio Interferometry between Space and Earth (ARISE), which would extend ground-based radio interferometry so as to provide a 10-fold increase in resolution for the study of regions near supermassive black holes and active galactic nuclei.

In the small-mission category, the top NASA-related priority was funding to help create a National Virtual Observatory—a Web-based data-mining facility that would provide wide access to the huge digital collections of astronomical data then being acquired from many sources, and the even larger ones being proposed for the future. Augmentation of NASA's Astrophysics Theory Program was deemed essential to help restore balance between the acquisition of data and the theoretical research needed to interpret it. Ultralong-duration balloon flights were identified as an important and cost-effective way of carrying out small near-space experiments at a fraction of the cost of satellites, and an Advanced Cosmic-ray Composition Experiment on the International Space Station was identified as a high priority.

In late 1999, as the AANM decadal survey was nearing completion, the NRC's Board on Physics and Astronomy hosted a science meeting on the frontiers of research at the intersection of physics and astronomy. Then-Administrator of NASA Daniel Goldin attended the meeting and at its conclusion asked the NRC to assess the science opportunities in this frontier area of interdisciplinary science and devise a plan for realizing those opportunities. NSF's Assistant Director for Mathematical and Physical Sciences Robert Eisenstein and DOE's Associate Director for High-energy and Nuclear Physics S. Peter Rosen joined Administrator Goldin in calling for such a study. The Committee on the Physics of the Universe (CPU) established as a result of the request issued an interim report in January 2001 and released its full report, *Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century* (Q2C; The National Academies Press, Washington, D.C., 2003), in 2002.

Motivations for the Q2C report included a rapidly expanding level of scientific activity at the interface of physics and astronomy, coupled with a concern that this research near the boundary between the disciplines of physics and astronomy might somehow be overlooked in agency planning processes due to its interagency nature.[2] The relevant science includes some of the most exciting areas of recent times: dark matter, dark energy and cosmic acceleration, inflationary cosmology, ultrahigh-energy cosmic rays, solar neutrinos and neutrino astronomy, the cosmic microwave background, and gravitational waves.

To ensure that any overlap between recommendations in the Q2C report and the AANM survey would be constructive, the CPU was directed to focus on the science at the intersection of astrophysics and elementary particle physics and not to reprioritize the projects in the AANM survey report. Although it was unusual that two NRC reports touching on scientific opportunities in astronomy and astrophysics were issued in little more than 2 years, the two studies appear to have meshed very constructively[3] and have helped build cooperative relationships between NASA, NSF, and DOE in supporting research in an important and rapidly moving field.

The Q2C report identified 11 compelling science questions at the intersection of astronomy and physics:

- What is dark matter?
- What is the nature of dark energy?
- How did the universe begin?
- Did Einstein have the last word on gravity?
- What are the masses of the neutrinos, and how have neutrinos shaped the evolution of the universe?
- How do cosmic accelerators work and what are they accelerating?
- Are protons unstable?
- What are the new states of matter and energy at exceedingly high density and temperature?
- Are there additional space-time dimensions?
- How were the elements from iron to uranium made?
- Is a new theory of matter and light needed at the highest energies?

The report made seven unranked recommendations for addressing these scientific questions, calling particular attention to the desirability of interagency cooperation in responding to them. In particular, the Q2C report recommended measuring the polarization of the cosmic microwave background (CMB), determining the properties of dark energy and other physical elements such as protons and neutrinos, and taking other steps pertaining to an increased understanding

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[2] Although the charge directed CPU to assess the intersection of physics and astronomy, NASA's Astrophysics program focuses primarily on the intersection of the subfields of astrophysics and elementary particle physics, and the present report thus uses these more specific terms throughout.

[3] For example, the two reports led to the development of NASA's Beyond Einstein program, which provides a framework for investigating the physics of the universe.

of the astrophysical universe. In addition to these efforts in specified science areas, the Q2C report recommended that to realize the scientific opportunities at the intersection of physics and astronomy, the government should establish an interagency initiative on the physics of the universe, with the participation of DOE, NASA, and NSF, providing structures for joint planning and mechanisms for joint implementation of cross-agency projects.

Two of the Q2C report's recommendations provided additional support for three of the projects and missions recommended by the AANM survey: Con-X, LISA, and the ground-based Large-aperture Synoptic Survey Telescope (LSST). The support was based on the ability of these planned facilities to do science beyond that envisioned in the AANM survey that would be especially relevant to questions at the intersection of astrophysics and elementary particle physics. The Q2C report's other recommendations did not overlap with those of the AANM decadal survey, and at least some did involve new projects for NASA, including a wide-field space telescope to probe dark energy (now called the Joint Dark Energy Mission, or JDEM) and a mission dedicated to searching for the polarization signature of inflation in the cosmic microwave background.

3

Astrophysics Program Plans and Progress

The AANM survey report and the Q2C report together recommended a coordinated program designed to address major astrophysical questions. The program included goals, strategies, and relative priorities, and it emphasized that optimizing scientific return would require, in addition to recommended NASA missions over a range of sizes, a correspondingly balanced program of astrophysical theory, data archiving, and data mining coordinated with continued development of the necessary scientific and technical workforce. The set of recommended missions for space-based science was highly diverse: Some were large, some moderate, and some small; and some required substantially more technology development than others.

In assessing the progress of NASA's Astrophysics program toward achieving the goals outlined in NRC reports, it is essential to distinguish between accomplishments based on goals and priorities established in decadal reports that preceded the AANM survey, and those based on the more recent goals set in the AANM survey itself.

RECENT ASTROPHYSICS ACHIEVEMENTS

Recent NASA missions have delivered a scientific program in astrophysics that can only be described as spectacular.[1] If anything, scientific progress has

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[1] All of the missions mentioned in this section were recommended in previous NRC decadal surveys of astronomy and astrophysics: the Hubble Space Telescope in *Astronomy and Astrophysics for the 1970's*, and the Chandra X-Ray Observatory and Spitzer Space Telescope in *Astronomy and Astrophysics for the 1980's*; the rest of the missions discussed are part of the Explorer line. Although

been even more rapid than anticipated at the time of the AANM survey. Missions in operation at that time, such as HST, have continued to deliver essential data concerning the nature and origins of planets, stars, galaxies, and the universe. The pace of NASA-led launches from 1999 through 2004 was impressive, including Chandra (1999), FUSE (1999), HETE-2 (2000), WMAP (2001), GALEX (2003), CHIPS (2003), Spitzer (2003), and Swift (2004). These successful missions have given rise to an enormous breadth of scientific discoveries ranging from new information for studies of protoplanetary disks around nearby stars to measurements of supermassive black holes in active galactic nuclei and images of vast reservoirs of hot gas in clusters of galaxies. Gamma-ray bursts continue to be discovered and monitored through synoptic missions like Swift, and results from the WMAP mission have set entirely new standards of precision for the measurement of fundamental cosmological parameters. In all respects, these missions have delivered on their scientific promise in the best traditions of the NASA programs that proposed and executed them.

Origin of the Universe: Geometry, Structure, and Contents

NASA missions have provided key insights leading to validation or disproof of cosmological models. Understanding the nature of dark energy and dark matter requires conducting a census of basic constituents of the universe: dark energy, dark matter, baryons, heavy elements, stars, and so on, all as functions of cosmological time (Figure 3.1). The history of the universe is revealed in some detail by the density fluctuations traced by WMAP and the measurement of the present expansion rate (the Hubble constant) by HST. Large samples of Type Ia supernovas traced over a significant span of the history of the universe have become an important tool for measuring the properties of dark energy. Studies carried out with HST have been critical for rapid, more detailed observations of the most distant Type Ia supernovas discovered by automated ground-based searches and studied spectroscopically with the largest telescopes from the ground. HST has been essential for the detection and measurement of gravitationally induced cosmic shear, the apparent distortion in the elliptical shapes of galaxies caused by the gravitational bending of light as it travels through space, on small angular scales (Figure 3.2).

Groundbreaking spectroscopic observations of deuterium, interstellar molecular hydrogen, and multiply ionized carbon and oxygen have been produced by the Far Ultraviolet Spectroscopic Explorer (FUSE) satellite and by the Space Telescope Imaging Spectrograph (STIS) on HST, and have contributed to understanding of the content, distribution, and physical conditions of baryonic matter throughout the universe. NASA programs have been critical to understanding the

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not prioritized directly in *The Decade of Discovery in Astronomy and Astrophysics* (1991), the Small Explorer line was recommended for acceleration in that decadal survey report.

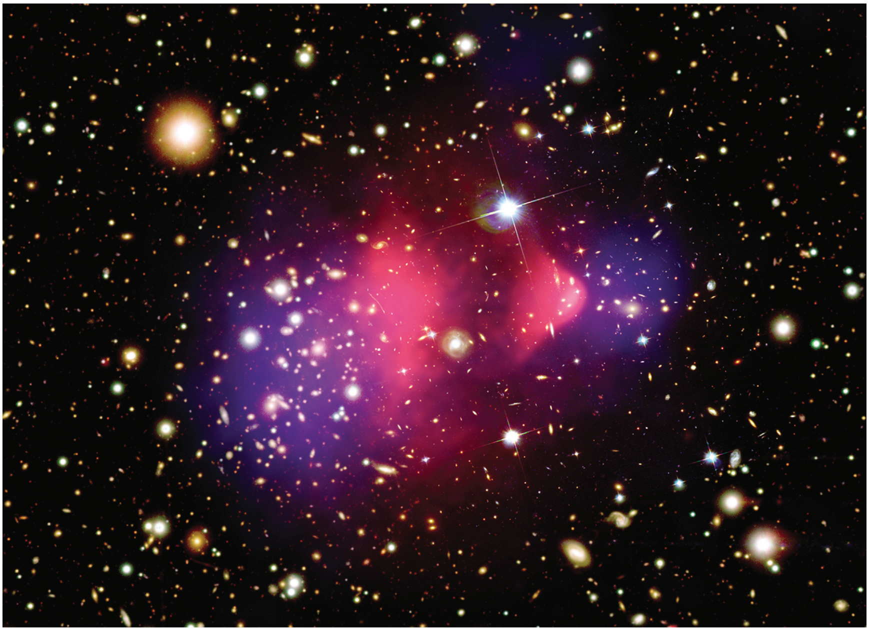

FIGURE 3.1 A composite with an x-ray image that reveals the location of the majority of the baryonic matter in the cluster (pink) and a gravitational lensing (shear) image that shows where the gravitating matter is. The blue color indicates the location of most of the mass. This experiment showed definitively that the gravitating matter is indeed dark, and the results ruled out theories that postulate that gravitating matter is hot and baryonic. SOURCE: *X-ray image:* NASA/Chandra X-Ray Center/Harvard-Smithsonian Center for Astrophysics/M. Markevitch et al. *Optical image:* NASA/Space Telescope Science Institute; Magellan/University of Arizona/D. Clowe et al. *Lensing map:* NASA/Space Telescope Science Institute; European Southern Observatory Wide Field Imager; Magellan/University of Arizona/D. Clowe et al.

chemical history of the universe, including the creation of materials essential to the origins of life, through better documentation of the processes of nucleosynthesis inherent in stellar evolution.

Origin and Evolution of Galaxies

Following the 2002 servicing mission to Hubble, the multiwavelength combination of the Advanced Camera for Surveys (ACS) and Near Infrared Camera and Multi-Object Spectrometer (NICMOS) has delivered views of distant galaxies with unprecedented clarity and depth. These observations have revealed

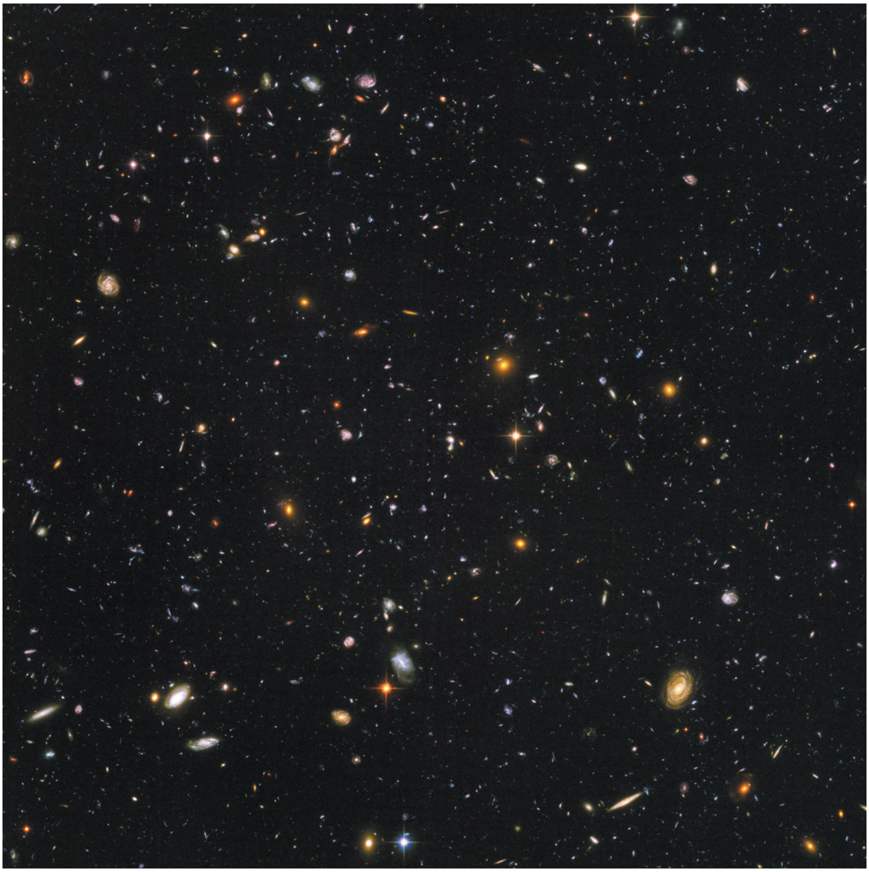

FIGURE 3.2 The deepest image of the visible universe ever obtained, dubbed the “Hubble Ultra Deep Field,” is a 1 million-second-long exposure obtained by the Hubble Space Telescope. The image is a composite of one made in the optical portion of the spectrum with Hubble’s Advanced Camera for Surveys (ACS) and another obtained in the infrared with its Near Infrared Camera and Multi-Object Spectrometer (NICMOS). About 10,000 galaxies are evident, many of which are too faint to be seen in images obtained with ground-based telescopes. The combined image provides unprecedented detail on galaxies across cosmic time, especially the first generation of galaxies that formed within the first billion years after the big bang. SOURCE: NASA, European Space Agency, S. Beckwith (Space Telescope Science Institute), and the Hubble Ultra Deep Field team.

galaxies too faint to be seen by ground-based telescopes. Studies of the earliest epochs of star and galaxy formation are critical to an understanding of the era of cosmic reionization, and of the initial growth of cosmic structure on all scales. Studies with the Galaxy Evolution Explorer (GALEX) are revealing details of star formation in the local universe and its coupling to more global phenomena on galactic and intergalactic scales (Figure 3.3).

Recent observations have provided critical clues that the growth of the central engines of active galactic nuclei and more normal galaxies are intimately linked. As a consequence, active galactic nuclei are no longer viewed as peripheral oddities but are understood within a more unified picture of galactic evolution. The growth of supermassive black holes in the centers of galaxies has been found to be tightly correlated with the masses of the stellar spheroids in which they are found, raising the question, Which came first, the galaxy or the supermassive black hole? Understanding the evolutionary relationship between supermassive black holes and their host galaxies, and characterizing the conditions under which galactic nuclei are active, are fundamental objectives discussed in both the AANM survey and the Q2C report. Exploring these related questions requires diverse information gleaned from both ground- and space-based observations,

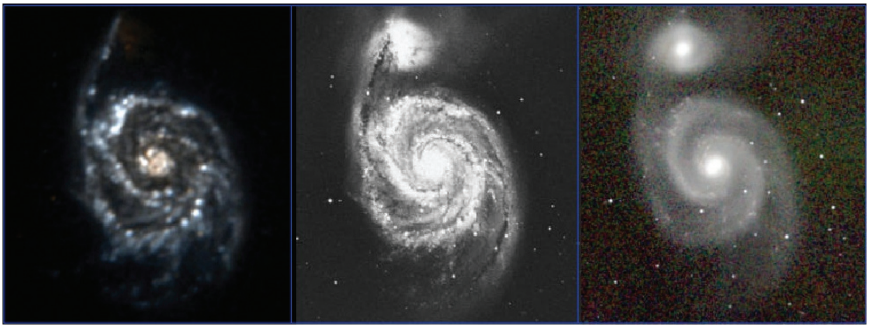

FIGURE 3.3 The "grand design" spiral galaxy pair NGC 5194 and NGC 5195, also known as M51, viewed in ultraviolet light by the Galaxy Evolution Explorer (GALEX) (left), in optical light from the Palomar Digitized Sky Survey (center), and in infrared light by the Two Micron All Sky Survey (right). The regularity and prominence of the spiral structure in the bigger galaxy NGC 5194 are believed to result from the gravitational effects of the passage of the smaller one NGC 5195 seen at the tip of its northern spiral arm. The ultraviolet image illustrates sites of ongoing high-mass star formation. In contrast, the long wavelengths trace the older long-lived stellar populations. Note that the small perturbing companion NGC 5195 is devoid of the hot, young, massive stars highlighted by GALEX. SOURCES: NASA/California Institute of Technology (left); Digitized Sky Survey (center); University of Massachusetts/Infrared Processing and Analysis Center/California Institute of Technology/NASA/NSF (right).

as well as theory and simulations. There is now a much better census of active galactic nuclei, and Chandra observations have resolved the x-ray background into a set of sources that nearly all correspond to active galactic nuclei at the center of normal galaxies. In the cores of galaxy clusters the echoes of multiple and powerful active galactic nuclei outbursts can be observed, and their effect on the formation of other galaxies can be explored. Observations with the Spitzer Space Telescope reveal distant, massive clusters containing some of the most massive galaxies in the universe, seen as they are still in early stages of formation.

Origins of Black Holes and the Gamma-Ray Burst Mystery

Great progress has been made in solving the mystery of gamma-ray bursts (GRBs), now thought to be the birthing signals of stellar-mass black holes. HST and the largest ground-based telescopes have captured such events as they occurred, observations that have helped to characterize the host galaxies. From this work long-duration GRBs were discovered to be associated with distant galaxies, and a few have been clearly associated with supernovas. Swift is now providing large statistical samples of GRBs, and the short-duration GRBs have been localized using the capabilities of HETE-2. The synergistic observations possible with three Great Observatories and large ground-based telescopes have provided unprecedented opportunities to explore multiwavelength, time-varying phenomena to obtain a more complete picture of the physical processes in pulsars and neutron stars, star-formation regions, active galactic nuclei, and the most distant and youngest galaxies known (Figure 3.4 and 3.5).

Origins of Stars and Planets

With its unprecedented capability to examine dusty clouds that are opaque at optical wavelengths, the Spitzer Space telescope has delivered exquisite images of disks around forming stars: the birthplaces of planets. Complementary to studies of the coldest, darkest clouds at millimeter and submillimeter wavelengths, Spitzer observations at mid- and far-infrared wavelengths provide unique and vital probes of clouds at somewhat warmer temperatures. Spitzer detections of debris and transition disks enable studies of the mechanisms by which planetary systems might form, while Spitzer and HST both provide insight into the physical conditions characteristic of extrasolar planets. For the first time, Spitzer has detected the warm infrared glow of two previously detected “hot Jupiter” planets, massive gaseous extrasolar planets that orbit very close to and rapidly around their parent stars. Spectral observations with HST yield clues to the chemical composition of the extrasolar planetary atmospheres and may even reveal how hot Jupiters, because of their proximity to the central star, may lose a substantial fraction of their atmospheres, leaving behind planets with hydrogen deficiencies or no atmospheres at all.

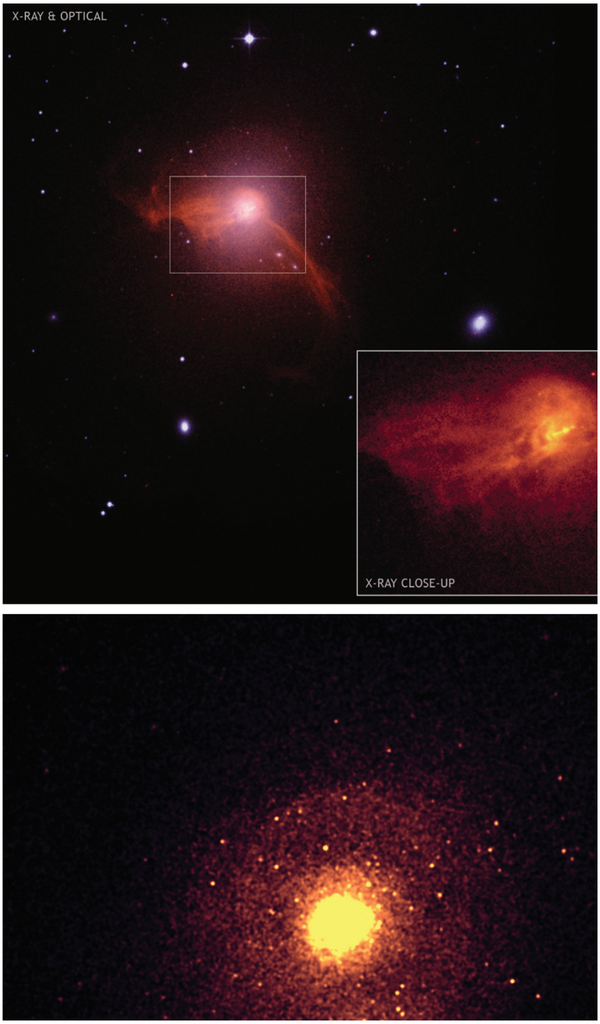

FIGURE 3.4 The giant elliptical galaxy M87. The top image is a composite showing x-ray (red) and optical (blue) emission from M87 at the heart of the Virgo cluster. The optical light arises from stars in the galaxy, and the x-rays trace hot gas, fed by the supermassive black hole at the galaxy's center. The hot gas outlined by the x-rays shows a series of loops and bubbles that can be traced to outbursts emanating from close to the black hole. The bottom image shows a close-up of high-energy x-rays in the very hot central region, showing the ring-like signature of an outward propagating shock wave (rather like a sonic boom) as expected from an outburst near the black hole. Such images allow researchers to understand the nature and behavior of supermassive black holes in galaxies. SOURCE: *X-ray image:* NASA/Chandra X-Ray Center/Harvard-Smithsonian Center for Astrophysics/W. Forman et al. *Optical image:* Digitized Sky Survey.

FIGURE 3.5 Composite image of the optical (red) and x-ray (blue) emission from the Crab Nebula, the remnant of an exploding supernova event that could be seen from Earth with the naked eye in 1054 A.D. The core of the star remains today as a rapidly rotating neutron star and is observed as a radio and x-ray pulsar; the star rotates 33 times per second. The star's outer layers, still expanding away from the core, make up the extended glowing nebula seen in this image. The extent of the x-ray emission is smaller than that of the optical light because the higher-energy x-ray-emitting electrons radiate energy more rapidly as they move than do the electrons associated with the optical emission. SOURCE: *Optical image:* HST (NASA/HST/Arizona State University/J. Hester et al.). *X-ray image:* Chandra X-ray Observatory (NASA/HST/Arizona State University/J. Hester et al.).

PLANS AND PROGRESS TOWARD RECOMMENDED GOALS

Nearly all of the exciting scientific progress summarized above was accomplished with instruments planned and developed in the 1990s or earlier. In this section the committee focuses on NASA's planning and implementation for the achievement of goals recommended for astrophysics for the remainder of the present decade and beyond.

Decadal surveys articulate and prioritize the science goals identified by the community for the upcoming decade and recommend ground- and space-based projects and missions for achieving those goals. Implementation is guided by the roadmaps that NASA generates every 3 years. As laid out in the 2003 roadmaps, the 2003 program plan for the Astrophysics Division provided a logical progression of missions to properly address the recommendations of both the AANM survey and the Q2C report,[2] including recommendations for science priorities as well as those for program balance in terms of mission goals, types, and sizes; technology development; infrastructure support; and other non-mission-related activities.

In 2003, what is now the Astrophysics Division was made up of two program units: Structure and Evolution of the Universe (SEU), and Origins. Each developed its own roadmap. The Origins roadmap[3] covered primarily those missions designed to observe in the optical and infrared portions of the spectrum, in order to address questions about the formation of galaxies, stars, and planets. The SEU roadmap,[4] a plan for the implementation of several missions in high-energy astrophysics and cosmology, was able to integrate the AANM survey's astrophysical goals with the scientific opportunities identified in the Q2C report, creating a coherent program now called the Beyond Einstein program. The Beyond Einstein program was also an integral component of the interagency plan led by the Office of Science and Technology Policy for responding to the science opportunities identified in the Q2C report.[5]

In the 2006 draft science plan produced by NASA's Science Mission Directorate[6] and reviewed by the NRC,[7] the outlook for astrophysics missions differs

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[2] The Solar Dynamics Observatory mission recommended in the AANM survey is part of NASA's Heliophysics Division and therefore not discussed in the present report.

[3] National Aeronautics and Space Administration, *Origins Roadmap*, Washington, D.C., January 2003.

[4] National Aeronautics and Space Administration, *Beyond Einstein: From the Big Bang to Black Holes*, Washington, D.C., January 2003.

[5] National Science and Technology Council Committee on Science, *A 21st Century Frontier for Discovery, The Physics of the Universe: A Strategic Plan for Federal Research at the Intersection of Physics and Astronomy*, Office of Science and Technology Policy, Washington, D.C., February 2004.

[6] National Aeronautics and Space Administration, NASA Science Plan, Draft 3.0, Washington, D.C., June 23, 2006.

[7] National Research Council, "Review of NASA's 2006 Draft Science Plan," letter report, The National Academies Press, Washington, D.C., 2006.

considerably from that in the 2003 plan. As Table 3.1 shows, the 2006 plan forecasts delays in the 2003 plan's projections—changes that will delay progress toward the achievement of established scientific goals.

Obviously many constraints within and outside NASA can lead to program delays and deferrals, and NASA must formulate plans for its Astrophysics program, together with its many other programs, within the framework of the agency's overall budget and mission. But the fact that GLAST (2007) and JWST (2013) are the only NASA-led moderate or major new space astrophysics missions slated for completion or even for substantial progress in the 2000-2010 decade is disappointing. *Nevertheless, the committee believes that NASA has generally done well in crafting program plans that are responsive to the science goals and opportunities outlined in the AANM survey and the Q2C report.*

Providing Missions for New Science

Although NASA's Astrophysics program plans address the NRC's recommendations, progress toward achieving the recommended missions has not matched the anticipated pace.[8] As mentioned above, NASA's currently operating missions have begun to address the scientific goals of the AANM survey report, but to continue to make progress (as well as to address the scientific opportunities identified in the Q2C report), new missions are necessary.

JWST

The top-priority major space mission for implementation in the present decade is the James Webb Space Telescope. NASA has taken its high priority seriously. The technology development effort has been substantial and successful, leading to significant risk reduction, particularly in the areas of detectors, scientific instruments, and flight mirror blanks. The flight system design, currently in Phase B, is occurring on the baseline schedule with the preliminary design review and subsequent transition to Phase C/D expected in 2008. Launch is planned in the 2013 timeframe.

The development of JWST has not been without problems. The estimated cost for design and development of the mission, including the launch vehicle, is \$3.3 billion, roughly \$2 billion (in FY 2006 dollars) more than the AANM decadal survey anticipated. This cost increase can be traced to a number of factors. The cost estimate provided by NASA to the AANM decadal survey committee was unrealistically low. The agency rebaselined the project in 2004, providing a more realistic cost estimate of \$2.5 billion through to launch. Since that baseline was established, the cost of the project has increased by nearly another billion dollars.

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[8] This section does not discuss the ARISE mission, which has not been funded and does not appear in any NASA roadmap.

TABLE 3.1 Summary of NASA Plans for Recommended Large and Moderate Astrophysics Missions

		Launch Date[b]	
Mission	Recommended by[a]	2003 Plan	2006 Plan
Hubble Space Telescope Servicing Mission-4	AA1980, DDAA, AANM	2004	2008
Space Infrared Telescope Facility (SIRTF)	AA1980, DDAA	2003	Launched August 2003
Stratospheric Observatory for Infrared Astronomy	DDAA, AANM	2005	Canceled[c]
Space Interferometry Mission	AA1980, DDAA, AANM	2005-2010	NET 2015
Keck Telescope Outriggers		2003	Canceled
Herschel/Planck	ESA	2007	2008
Gamma-ray Large Area Space Telescope	AANM	2007	2007
Kepler (Discovery)	AANM[d]	2007	2008
James Webb Space Telescope	AANM	2005-2010	2013
Constellation-X[e]	AANM, Q2C	NET 2011	NET 2016
Terrestrial Planet Finder	AANM (td)	2010-2015	NET 2018
Laser Interferometer Space Antenna[e]	AANM, Q2C	NET 2011	NET 2016
Black Hole Finder Probe[e]	AANM	NET 2012	Deferred
Single Aperture Far Infra-Red Observatory	AANM (td)	Deferred	Deferred
Inflation Probe[e]	Q2C	NET 2012	Deferred
Joint Dark Energy Mission[e]	Q2C	NET 2012	Deferred
LBTI (Large Binocular Telescope Interferometer)	AANM	2005	2009

[a] AA1980, *Astronomy and Astrophysics for the 1980's* (1982); DDAA, *The Decade of Discovery in Astronomy and Astrophysics* (1991); ESA, European Space Agency; AANM, *Astronomy and Astrophysics in the New Millennium* (2001); Q2C, *Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century* (2003); (td), missions recommended for technology development in this decade.

[b] NET, no earlier than.

[c] SOFIA has since been reinstated by NASA, with a goal of beginning science operations in 2012.

[d] AANM recommended a diverse range of NASA mission sizes, but did not identify particular Discovery- or Explorer-class missions.

[e] Denotes a Beyond Einstein program mission.

NASA had attributed half of that increase ($530 million) to a 22-month slip in the projected launch date, a slip that the program traces to a limitation of funds for the project in FY 2005 and FY 2006 and a delay in receiving approval to use a foreign launch vehicle.[9] Another third of the increase ($386 million) is due to growth in the cost of the mission. The remaining increase ($125 million) is attributable to additional contingency budget reserves being added to the project.[10]

The JWST project has had two descopes to date, including a change from an 8-m-class to a 6-m-class mirror and a reduction of short-wavelength capability. Nevertheless, this large and challenging program appears to be healthy and on a path to being capable of accomplishing most of its stated scientific requirements. It has met all its cost, schedule, and technical milestones since being replanned in September 2005. At the same time, its past cost growth and schedule slippage cause the committee to be concerned about its continued success in meeting technical milestones and cost estimates.

As the highest-priority large mission, JWST remains critical to realization of the goals set forth in the AANM survey report.

GLAST

The AANM survey's highest-priority recommendation in the moderate space mission category was the Gamma-ray Large Area Space Telescope (GLAST), on which NASA and DOE have worked together as the developing agencies. The GLAST team and NASA officials told the committee that GLAST was a model for interagency cooperation, and that although there were problems (such as a cost overrun on the Large Area Telescope, the primary instrument on the spacecraft), the agencies were able to resolve them successfully. The mission is planned for launch in 2007.

TPF

The AANM decadal survey committee broke new ground by recommending that NASA invest in technology for missions that would not be ready to begin development until the decade beyond that addressed by the report. One of these recommendations was that NASA commit $200 million for technology development for an interferometric Terrestrial Planet Finder (TPF) mission. Following rapid advances in coronagraph technology, NASA chose in 2003 to divide the project into two missions, an interferometer (TPF-I) and a coronagraph (TPF-

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[9] The magnitude of the impact of the delayed Ariane launch decision is unclear; it is possible that the delay caused by the decision gave the project time to make progress in areas that potentially would have caused a similar slip.

[10] Presentation from Phil Sabelhaus, JWST project manager, at the June 2006 meeting of the Space Studies Board.

C) version, and began to invest in technology for both missions with a goal of launching TPF-C near the end of the decade. This change in strategy was assessed in an NRC letter report that made the interim recommendation that NASA return TPF to the originally recommended spending level, in part to preserve balance with other projects.[11] Since that time, TPF funding has been reduced such that the projected launch date for either version is now no earlier than 2018.

Einstein Great Observatories

NASA's 2003 *Beyond Einstein* roadmap identified the Constellation X-ray Observatory (Con-X) and the Laser Interferometer Space Antenna (LISA) as Einstein Great Observatories. Con-X was the second-priority large-category mission recommended by the AANM survey, and LISA was the second-priority moderate mission. Both missions were also recommended in the Q2C report. Since the release of the AANM survey report, cost estimates for both Con-X and LISA have grown, and as a result LISA is now also classified as a flagship mission—an Einstein Great Observatory. To this point both missions have received technology development support from NASA, but the investment has been unsteady and far less than that envisioned in the AANM decadal survey ($1,050 million combined). LISA is a collaboration with the European Space Agency (ESA), and both ESA and NASA have funded the Space Technology-7 (ST-7) technology demonstration mission, which will validate a number of technologies critical to the project. ST-7 is planned for launch in 2010 or 2011.

Einstein Probes

NASA's *Beyond Einstein* roadmap highlighted three missions recommended by the NRC as Einstein Probes, and it recommended that NASA conduct these missions as competitively selected principal-investigator-led missions with a cost cap of $600 million. Although a number of mission concept studies have been supported, no Announcement of Opportunity is expected for these missions until at least 2009.

The Q2C report recommended that NASA collaborate with the Department of Energy on the Joint Dark Energy Mission, a wide-field telescope in space that would explore the acceleration of the expansion of the universe. NASA has competitively selected three proposals for mission concept studies, and the results of those studies are due in 2008.

Also explicitly recommended in the Q2C report was the Inflation Probe, which would aim to detect the signature of inflation in the infant universe by measuring the polarization of the cosmic microwave background (CMB). NASA

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[11] National Research Council, "Review of Science Requirements for the Terrestrial Planet Finder: Letter Report," letter report, The National Academies Press, Washington, D.C., 2004.

has funded a number of mission concept studies designed to address the goals of this mission and has cooperated with NSF and DOE to support the CMB roadmap activity called for in the *Physics of the Universe*[12] report.

The Black Hole Finder Probe is the roadmap's response to the AANM survey's third-priority moderate mission, the Energetic X-ray Imaging Survey Telescope (EXIST). The Black Hole Finder Probe would conduct a census of accreting black holes, from supermassive black holes in the nuclei of galaxies, to intermediate-mass (about 100 to 1000 solar-mass) holes produced by the very first stars, to stellar-mass holes in the Milky Way Galaxy.

In August 2006, NASA requested that the NRC conduct a study to identify which of the five Beyond Einstein mission concepts (the two Einstein Great Observatories and the three Einstein Probes) should be started first, based on both scientific priority and technology readiness. The report is due in September 2007, to help the agencies prepare for a FY 2009 start.

Unprioritized Recommended Missions

The AANM survey report recommended that NASA implement or participate in a number of missions that were not included in the report's priority list. These missions were either missions recommended in previous NRC decadal surveys (HST, SOFIA, SIM) or missions led by foreign partners (such as the Herschel/Planck mission).

HST continues to produce exceptional science despite the effects of aging and the loss of the Space Telescope Imaging Spectrograph (STIS). NASA's long-delayed SM-4 servicing mission, recommended by the AANM survey, will install the Wide Field Camera 3 and Cosmic Origins Spectrograph instruments, recover the STIS capability, and install replacement components that will prolong its lifetime for 5 years or more. However, the delay in the servicing mission (discussed in Chapter 4) has cost the Astrophysics Division more than $600 million.

The Stratospheric Observatory for Infrared Astronomy (SOFIA) is a general-purpose suborbital observatory designed to operate in the lower stratosphere above 99.8 percent of the obscuring atmospheric water vapor. The mission's scientific goals have not changed since the mission was recommended in the NRC's 1991 decadal survey,[13] and the AANM survey recommended that NASA complete the project. The first flights for SOFIA are planned for 2009, and the observatory is expected to move to operational status in 2012.

The Space Interferometry Mission (SIM; now SIM PlanetQuest) was origi-

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[12] National Science and Technology Council Committee on Science, *A 21st Century Frontier for Discovery, The Physics of the Universe: A Strategic Plan for Federal Research at the Intersection of Physics and Astronomy*, Office of Science and Technology Policy, Washington, D.C., February 2004.

[13] National Research Council, *The Decade of Discovery in Astronomy and Astrophysics*, National Academy Press, Washington, D.C., 1991.

nally designed to measure the distances to stars throughout the Milky Way Galaxy with significantly more accuracy than is currently possible. The prospect now of the mission's capability to detect planets around nearby stars has enhanced its scientific value. The AANM survey endorsed this expanded science case and recommended that the mission be completed. SIM PlanetQuest's technology development has been completed successfully, but the mission is being held in the formulation phase due to budgetary constraints.

The long tradition of NASA cooperation on foreign astrophysics missions continues with Herschel/Planck. These missions are being successfully implemented with a substantial science return relative to the money invested.

Herschel will make observations in the full far-infrared and submillimeter waveband and will study dust-obscured and cold objects, such as clouds of gas and dust in areas of new star formation, planetary disks, and the first galaxies. Planck will map the cosmic microwave background anisotropies with improved sensitivity and angular resolution, testing inflationary models of the early universe, among other investigations.

Explorers

The Explorer program specializes in the development of small (SMEX) to medium-class (MIDEX) missions using available technology to provide a low-cost quick response to targeted opportunities for scientific discoveries. For example, when gamma-ray bursts were discovered, the Swift mission was quickly conceived, proposed, built, and launched to address the mystery. When the COBE mission discovered anisotropy in the cosmic microwave background radiation from the infant universe, it was possible to deploy the WMAP mission quickly to exploit the discovery.

As noted above, small-scale missions in the current decade have been very productive, with HETE-2, WMAP, RHESSI, CHIPS, GALEX, and Swift launched in the years 2000 to 2004. Despite unstable funding, WISE is now in Phase C/D and scheduled for launch in late 2009. NASA had also selected the NuSTAR SMEX proposal for detailed study, but lack of funds resulted in termination just before the project's confirmation review. According to NASA's 2006 Astrophysics program plan, the next competition is scheduled for 2008, which would lead to a 2013 launch.

Mission Support Activities

NASA's agency culture is centered on flight missions, but supporting activities such as general technology development and grant support for research, data analysis, and theory are necessary to make NASA's astrophysics missions successful.

Technology development is clearly identified in the AANM survey report as essential to efficient and cost-effective preparation for future missions not yet slated for development, such as SAFIR. Traditionally development funds were provided by both the Astrophysics Division and the Office of Aerospace Technology (OAT). Before its elimination, OAT provided roughly $40 million per year in technology development that was applicable to astrophysics missions (some of those funds were captured by the Astrophysics Division when OAT was eliminated).

The AANM survey report also recommended that NASA tie support for theory research to flight missions, particularly in the form of theory challenges, in order to encourage theorists to contribute to the planning of missions and to the interpretation and understanding of the scientific results. The support for theory connected to operating missions, particularly the Great Observatories, has been adequate but tends not to support the kind of open-ended thinking that is essential to generating ideas that can drive next-generation missions. NASA included support for theory in the TPF and Beyond Einstein Foundation Science programs (although there has been a virtual elimination of the TPF Foundation Science line in FY 2006). The Astrophysics Division attempted to add a Theory Challenge line to the JWST program in the early part of the current decade, but that line was eliminated in the administration's budget formulation process, and the division has not attempted to recreate it or to provide a Theory Challenge for the GLAST mission.

Ensuring the Diversity of NASA Missions

The AANM survey report stated that both flagship and Explorer missions are important, noting that, at the time the report was written, opportunities for moderate-scale missions were less readily available. The AANM report recommended that NASA encourage a diverse range of mission sizes in order to produce the most effective science return from the program. Although six astrophysics Explorer missions have been launched in the current decade, those launches are the result of development work performed mostly in the 1990s. Now it appears that only one Explorer mission will be developed and launched in this decade, and at most one Explorer will begin development in this decade for launch in the next. *The comparison between this decade and the previous is stark, leading the committee to conclude that NASA has chosen to concentrate its resources on the highest-priority large and moderate missions, to the detriment of the Explorer line and other small initiatives. In so doing, the Astrophysics Division has failed to adequately respond to the AANM survey report's recommendation that NASA maintain a diverse mission portfolio.*

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Slowdown of Progress—Analysis and Appraisal

The NRC reports *Astronomy and Astrophysics in the New Millennium* and *Connecting Quarks with the Cosmos* identified a compelling set of scientific goals and opportunities that the field is in a position to address. NASA's 2003 roadmaps presented a set of plans to address many of these goals. However, between 2003 and 2006 the fiscal posture of the Astrophysics Division changed considerably, and the resulting 2006 plan is substantially different from the 2003 plan in two key ways. First, suites of missions designed to address goals and opportunities in cosmology, high-energy astrophysics, and searches for Earth-like planets have been delayed or deferred. Second, the division's program portfolio has become heavily skewed toward support of the largest missions, with a resultant loss of balance in terms of mission size, type, and goal and with respect to long-term investment in technology and infrastructure development. In the committee's estimation, both of these issues, if unaddressed, will reduce the scientific productivity of NASA's Astrophysics Division for at least the next 15 years. In addition, a number of intangible concerns about the stability of the agency and its interactions with the community have established the perception that NASA is not responding to the guidance provided in the NRC reports.

The dramatic slowing, at the middle of the current decade, of progress toward realizing the scientific opportunities laid out in the Q2C report and starting the missions recommended in the AANM survey report has been due to no one single factor. Rather, the slowdown reflects the confluence of external and internal factors and events that together had the effect of a perfect storm hitting the Astrophysics program at NASA. Moreover, all of this occurred in a larger context: At the beginning of the decade 2000-2010, the ambitions and expectations of the scientific community were rising, buoyed by the stunning discoveries of the late 1990s; the fact that by the mid-1990s the survey list of projects for that decade was exhausted (at NASA); and the prospects for rising science budgets in the larger fiscal environment of a federal budget deficit that had then turned to a surplus. In addition to budgetary issues, there were other important changes, including management reorganizations; changes in mission philosophy, from faster, better, cheaper back to a more traditional approach; new accounting systems; and increasing mission complexity leading to cost escalation. This chapter addresses the elements that came together to slow progress toward realizing the goals articulated in the AANM survey and the Q2C report, and it offers the committee's appraisal of some of the likely effects.

INTERNAL AND EXTERNAL PRESSURE ON NASA'S ASTROPHYSICS BUDGET

An important and quantifiable part of the story of how progress toward AANM and Q2C goals has slowed involves budgetary expectations versus budgetary reality for astrophysics. Estimates of the dollar value of the mismatch between the assumptions of the AANM survey and the budget reality for the period from 2001 through 2010 are summarized below:[1,2]

- At least $2 billion: Higher than expected costs for the recommended projects, dominated by cost escalation for JWST;
- At least $0.6 billion: Cost of delay in the HST fourth servicing mission due to the Columbia accident, from the planned 2003 servicing to the projected mid-2008 servicing;
- Approximately $0.5 billion: Carryover projects from previous decades, largely SIM and SOFIA, which were not accounted for in the AANM survey;
- Approximately $0.5 billion: Approximate cost of NASA's share of the Joint Dark Energy Mission, which was not included in the AANM survey but was recommended by the Q2C report; and
- $0.383 billion: Projected decreases (as given in the president's FY 2006 budget request) in the astrophysics budget for budget years FY 2006 through

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[1] Between FY 2001 and FY 2005, NASA spent $5.7 billion on astrophysics (in FY 2000 dollars). In FY 2006, the astrophysics budget is at historic highs, even when inflation and the shift to full-cost accounting are considered. The committee also notes that the AANM-recommended suite of missions was estimated to have a total cost ($3.7 billion in FY 2000 dollars) equivalent in inflation-adjusted dollars to the costs estimated for implementing the 1990 decadal survey's recommendations. Therefore, the committee concluded that the rate of increase in the overall astrophysics budget is not a major contributing factor in this discussion.

[2] Figures in the list were provided (in real-year dollars) to the committee by NASA Astrophysics Division staff.

TABLE 4.1 Astrophysics Division Budget Changes (in FY 2006 $ millions)

	FY06	FY07	FY08	FY09	FY10	Total
FY 2006 Request	1512	1532	1539	1495	1407	7485
FY 2007 Request	1508	1509	1501	1308	1276	7102
Change	(–4)	(–22)	(–39)	(–187)	(–131)	(–383)

FY 2010 (Table 4.1), redirected toward the president's new vision for space exploration.

For comparison, the NASA budget for astrophysics for the period from 2001 through 2010 has averaged (and is projected to be) about $1.5 billion per year, for a total of about $15 billion for the 10-year period. Thus, the more than $4 billion gap relative to the expectations of the AANM survey is currently projected to amount by the end of the decade to about 25 percent of NASA's astrophysics budget. It is no wonder that progress has slowed.

Moreover, this analysis does not include the past expectation that, in the wake of the federal budgetary surplus in the mid-1990s, science budgets would be rising. The stark reality now is that the nation faces a budget deficit that will persist for some years to come.

In the wake of the Columbia accident, the effort to return the remaining shuttles to flight, completion of the International Space Station, and the new exploration activities associated with the vision for space exploration, NASA's focus has moved to the human spaceflight program. This situation and the resulting squeeze on NASA's science portfolio are unlikely to change in the foreseeable future unless special action is taken. As quickly as circumstances have changed in this decade, it is certainly possible that future changes could return the astrophysics budget to the levels planned in FY 2005-FY 2006. However, in the estimation of the committee such changes cannot be taken for granted, and NASA's Astrophysics Division should take steps to resolve the imbalance in the budget currently projected in the FY 2007 request (see Table 4.1).

The dominant reason for the delay or deferral of large sections of the Astrophysics Division's program (as well as for the imbalance in the program) is the growth in the cost of the projects that the division is implementing. The higher-than-anticipated costs to the division for missions currently in development will total roughly $2 billion more over the course of the decade than the AANM decadal survey anticipated based on the cost data the survey committee was given by NASA. In addition, the cost estimates for the missions still in the formulation phase (such as LISA and Con-X) are also significantly greater than anticipated at the time of the AANM survey.

MISSION COST ESCALATION

Many factors have contributed to the lack of cost realism for these missions. Some causes are imposed from the top down by institutional factors. Others are driven from the bottom up due to mission complexity, engineering capabilities, infrastructure limitations, and other elements. In the committee's judgment, the following factors have combined to limit NASA's ability to implement AANM survey objectives at the resource level anticipated in the survey report:

- The AANM survey recommendations were developed in the "faster, better, cheaper" era, using NASA budget estimates that were not rigorously assessed and have since proven to be systematically underestimated.
- Changes in accounting methods, including the move to full-cost accounting, have affected NASA cost estimates in ways that currently result in unplanned cost increases for many missions.
- The effect of increases in mission complexity was not taken into account during the earliest stages of mission formulation.
- New agency policies, processes, and mission assurance requirements have disproportionately driven up the cost of small missions.
- Access to launch vehicles appropriate for smaller missions has been reduced.

NASA has already improved its budgetary and planning processes through the use of independent cost-estimating models, maintaining adequate budget and schedule reserves, and development of suitable tracking metrics. The importance of these techniques is increasingly understood in the scientific community as well.

Since the agency's shift to full-cost accounting, astrophysics missions must budget individually for staff support, launch services, mission operations, and NASA infrastructure overhead. While this accounting change may be neutral from an overall agency perspective (the agency transferred funds to the Science Mission Directorate to cover the cost of the new responsibilities), programs now bear a greater percentage of the cost burden than in the past. The result is that funds budgeted for Astrophysics program missions now buy less astrophysics. Moreover, from the perspective of maintaining balance, small and moderate missions are affected to a greater degree than large missions, because the work on small and medium missions is often done at universities or other institutions that lack the technical infrastructure to deal with special problems. And whereas NASA's support was formerly essentially free, full-cost accounting now means that such costs are directly assessed to the program. Since this support must be accounted for up front, the net result is either less mission than what was achievable in the past or higher cost.

A trend toward increasing mission complexity is to be expected for flagship missions as science goals extend beyond first-generation survey objectives to more difficult second-generation objectives. In the committee's judgment, the risks associated with increasing mission complexity have been significantly underestimated in the past, leading to a corresponding underestimation of costs and schedules. Complexity and risk are key discriminators between mission categories. Small and medium missions are generally cost-capped either directly or indirectly by being limited to using only available technology. In such cases, risk can be looked at as mostly programmatic and can be mitigated by allocating sufficient project reserves.

Larger missions including flagship missions employ advanced technologies that require substantial development before the risks can be mitigated and associated mission costs thoroughly understood. Therefore, controlling risk not only is important but also requires a rigorous process operated at two levels. First, the process must correctly identify the technology areas requiring study and early development. Second, the management process must effectively plan the effort and then execute it according to a roadmap with specific metrics that are critically reviewed.

The end product of such efforts should be technology development in mission-critical areas so as to achieve a technology readiness level of 6 before a project is fully defined, planned, and costed. The committee credits NASA for implementing management procedures for risk and technology development for larger missions such as SIM and JWST. These missions now appear to have been accurately defined and planned with respect to both cost and schedule.

However, the application of similar risk management processes to smaller missions, although it helps to reduce mission risk, has done so at relatively great expense in both time and money. Given the projected paucity of smaller missions like Explorers and the consequent imbalance in its portfolio, NASA should consider whether cost savings can be realized by scaling down the risk management and mission assurance approaches applied to smaller missions. Such a change in approach would not so much increase risk in smaller missions as recognize the opportunity costs entailed by fewer small missions and, in a sense, the consequent reduction in science return to the overall Astrophysics program.

NASA's mission assurance requirements have grown increasingly process-oriented and restrictive in an attempt to use external regulation to prevent mission failures. Current mission assurance requirements for small and medium unmanned missions have become restrictive in ways that are counterproductive, given that the time and money associated with such efforts are necessarily subtracted from essential engineering activities. A balanced risk approach with respect to mission assurance, such as that used on WMAP, is described in the NASA Integrated Action Team report[3] and should be appropriately applied to smaller missions.

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[3] National Aeronautics and Space Administration, *Enhancing Mission Success—A Framework for the Future: A Report by the NASA Chief Engineer and the NASA Integrated Action Team*, Washington, D.C., December 21, 2000.

An additional barrier to successful small and medium missions is the cost growth of smaller launchers as the launch industry moves toward larger and more expensive vehicles. For example, the U.S. Air Force has announced its intention to stop supporting Delta II launches after 2009, meaning that the WISE mission planned for launch in 2009 may be the last NASA mission to be launched on a Delta II. Therefore, unless the Delta II line can be retained or an alternative launcher of comparable capability developed, small and moderate astrophysics missions will be affected either by being restricted to small launchers or by becoming much more costly.

In conclusion, the central reason for the difference between the 2003 and 2006 NASA strategies for astrophysics is that, for many reasons, there has been a substantial increase in the cost for nearly all astrophysics missions.

SCIENCE OPPORTUNITIES LOST

Past NASA successes have encouraged ever more ambitious planning, but at the same time missions are taking longer to complete. It is now clear that four of the space-based missions recommended in the AANM survey, namely JWST, Con-X, LISA, and TPF, are sufficiently complex and challenging that even if additional funds had been available, the missions could not have been completed within the current decade. (In the case of TPF, the AANM survey realized this constraint and recommended only design and development funds.) At the beginning of the decade, the ambitions and expectations of the science community were rising, buoyed by the stunning discoveries of the late 1990s and the prospects for rising science budgets in the larger fiscal environment of a federal budget deficit that had turned to a surplus. And the new missions recommended by the Q2C report made NASA's plate even fuller. Clearly the real impact of the current situation at NASA has been magnified by the high expectations of the astrophysics community for the decade 2000-2010.

Confronted with more realistic mission costs, the Astrophysics Division appears to have chosen to concentrate its resources on the highest-priority items listed in the AANM survey report and to maintain other missions in development at their current level. The committee believes that when the comparison is between flagship missions, this strategy is consistent with the decadal survey priorities.[4]

The division's strategy comes at a steep scientific cost, however. By choosing to defer the entire Beyond Einstein suite of missions until 2009 at the earliest, the

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[4] This strategy is not the only strategy that would be consistent with survey priorities. One possibility would be to develop more missions simultaneously, while still giving some priority to the highest-ranked missions. The committee recognizes that such a strategy would have significant drawbacks as well. That said, this strategy and others are advocated by some members of the community and are mentioned to complete the discussion.

agency is also deferring the ability to address the scientific opportunities at the intersection of astrophysics and elementary particle physics, as presented in the Q2C report. By choosing to keep SIM PlanetQuest in stasis until after the launch of JWST and to defer the TPF mission until the next decade, NASA is foregoing the opportunity to take the lead in a field with broad resonance in the general public and around the world. These areas of scientific inquiry are quite exciting to both the astrophysics community and the general public. The committee believes that these choices are one key reason for the science community's perception that the agency is not making the expected progress in addressing the science goals and opportunities in astrophysics. However, the committee does not believe that this situation can be remedied without either a dramatic change in the fiscal outlook for the Astrophysics Division, or a significant reduction in the capabilities of many of the missions that have been proposed or are in development.

IMPACT OF PERSISTENT IMBALANCE IN THE PROGRAM

The decision to concentrate resources on the highest-priority items listed in the AANM survey report and to maintain the carryover missions in development at their current level has also caused an imbalance in the Astrophysics program's mix of mission sizes, as described above in Chapter 3. The committee believes that the current imbalance, if not addressed, will have a significant long-term impact on NASA's Astrophysics program and the field as a whole.

As displayed in Figure 4.1, three flagship Great Observatory missions were launched in the 1990s, although as noted above, HST and CGRO were developed in the 1980s. Chandra is the only flagship mission both started and launched in the 1990s, although the three HST servicing missions (SM1, SM2, and SM3A) can also be considered large missions executed within that decade.

The Explorer program in the early 1990s launched the Extreme Ultraviolet Explorer (EUVE) and the X-Ray Timing Explorer (XTE), both of which were in the moderate-class category, which is above the typical Explorer mission cost baseline. The "faster, better, cheaper" (FBC) initiative started in the early 1990s was set in motion, in part, to develop smaller and less costly moderate missions in astrophysics as well as the other NASA program areas.

Although the impact of the FBC initiative began to appear in the late 1990s with the reduction in cost and complexity of the SWAS and FUSE missions, the major impact has come in the current decade. Seven Explorer-class missions have been launched or are projected to be launched in the decade 2000-2010, with six of them—HETE-2, WMAP, RHESSI, CHIPS, GALEX, and Swift—being carryover missions from the FBC era, launched through 2004. A significant secondary result of the FBC era is that no traditional flagship missions will be launched in the 2000-2010 decade. Spitzer, although considered a Great Observatory, was really a medium-class mission owing to simplifications implemented under FBC principles.

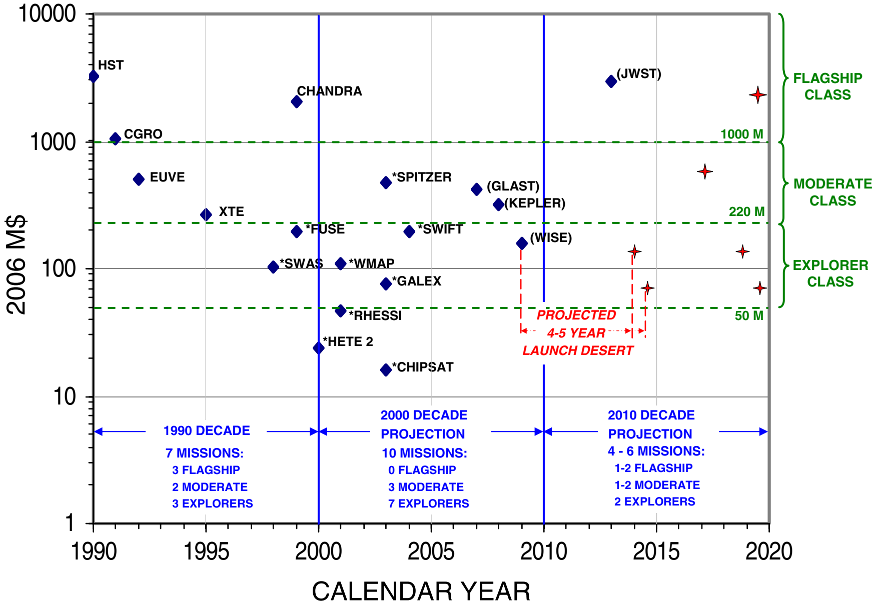

FIGURE 4.1 Run-out cost normalized to 2006 dollars (excluding launch) for NASA astrophysics missions launched or projected to be launched since 1990. Missions with an asterisk were developed under a general strategy of "faster, better, cheaper." Missions represented in parentheses are shown at their currently planned launch date. Mission classes designated on the right-hand side are approximate based on plotted missions as well as currently baselined mission cost assumptions. Projections into the 2010 decade (represented with "+" symbols) are estimates based on the current understanding of mission status and expected mission starts.

The committee believes that the current era of discovery in astronomy and astrophysics owes much to the combination of flagship missions and Explorer missions that are now operational. Smaller missions using available technology provide opportunities for rapid turnaround when new discoveries are made. For example, when the COBE mission discovered anisotropy in the CMB radiation from the infant universe, the WMAP mission could be quickly deployed to follow up. Smaller missions have the important side benefit of developing spaceflight experience and expertise in young scientists and engineers who then apply their experience to other missions, including flagship missions. However, the highly targeted nature of the Explorer missions leads them to have relatively short scientifically productive lifetimes. Without a steady stream of new Explorer missions to build on the discoveries made by the flagship missions, the productivity of the flagships and of the Astrophysics program as a whole will suffer.

Currently just three astrophysics missions are projected for launch before 2013: GLAST in 2007, Kepler in 2008, and WISE in 2009. With no medium- or Explorer-class missions now in early stages of development, the picture looks particularly bleak for maintaining the current balance in NASA's astrophysics portfolio. Indeed, the *only* mission currently manifested for launch in the next decade is JWST, with a projected date of 2013. The elimination of the Small Explorer mission NuSTAR combined with the delays in issuing announcements of opportunity for Explorer missions will lead to a 4- to 5-year "launch desert" in the first half of the next decade. Without new Explorer mission proposal opportunities in the immediate future, this launch desert will extend even further into the next decade.

It is important to note that all of the remaining astrophysics missions in the division's plans, with the possible exception of the Einstein Probes, fall into the flagship mission category with respect to complexity, cost, and schedule. JWST is still early enough in its development that its cost, schedule, and associated launch date must be viewed as preliminary; considering that the spacecraft has not yet begun the integration phase, historically a phase that has led to cost growth, it is not impossible that further delays and cost increases could occur. Such an event would lead to an even greater impact on overall Astrophysics program content and balance. *A decade limited to one new start on a flagship mission plus two to four small and medium missions would constitute a major reduction in the NASA Astrophysics program.*

ORGANIZATIONAL INSTABILITY

NASA's Astrophysics Division will always be subject to a variety of budgetary, managerial, and political forces that it can neither predict nor control. But the environment in which the division has operated in recent years has been extraordinarily unstable. During the committee's data-gathering sessions, the following sources of instability were identified as having a negative effect on the productivity of the division. While the magnitude of the effect caused by these factors is difficult to quantify, the committee includes this discussion as a reflection of concerns within the astronomy and astrophysics community that contribute to the perception that the agency's program is not progressing.

- *New focus for NASA.* The new vision for space exploration has changed the budgetary priorities at NASA and, as discussed above, this has led to a projected decrease of $383 million in funding for astrophysics for 2006 through 2010 (see Table 4.1). More importantly, if the NASA budget for science remains constant in the years thereafter (or only grows with inflation), the pressures to devote some significant fraction of the NASA science budget to other priorities is likely to further erode the dollars available for the priorities in astrophysics identified by the AANM, Q2C, and future decadal surveys.

- *Leadership changes and reorganizations.* Over the past 5 years, NASA has had three administrators. In the same period, the Science Mission Directorate (SMD) has had four reorganizations and three changes in basic administrative guiding documents. Also, the agency continues to suffer from a large number of vacancies in key positions in SMD. For example, in June 2006, 6 of the 10 directorships and deputy directorships of the five SMD divisions were either vacant or occupied by acting personnel.
- *Programmatic instability*. Numerous decisions have been made over protracted periods. For example, the administration's failure, for well over a year, to approve an Ariane launch for JWST led to a costly delay in the mission. In the wake of the Columbia accident, a number of internal NASA decisions were made and then reversed on whether to proceed with the next Hubble servicing mission. At a programmatic level, the committee heard of numerous instances of changes made to program budgets late in the fiscal year that hindered the ability of managers to manage their project teams. Such changes were said to be even more damaging to the program than budget levels that are lower than requested.

COMMUNITY INPUT AND ADVICE

In addition to the instability within the agency, NASA's advisory structure underwent a number of changes during the decade. An important feature of the U.S. scientific enterprise is effective communication between the scientists who are making discoveries and the managers within the federal science agencies who are responsible for making decisions. Science managers need expert advice to inform their decisions; scientists need to understand how the science agencies function as well as the basis for the decisions that managers are making. The effectiveness of two-way communication between scientists and science managers is unique to the U.S. scientific enterprise and a key to its success over the past 50 years.

The engagement of the expert science community with federal science managers takes various forms, ranging from high-level policy and strategic advice provided by agency councils (such as the NASA Advisory Council) and NRC committees and studies to more tactical and ad hoc advice from NASA Advisory Council subcommittees and ad hoc committees of experts (e.g., management operations working groups and review panels). Several features appear to be key to a robust and effective advice structure: the openness and transparency of the process; recognition by all that advice is advice, but that managers must finally make the decisions; and a short path between managers and experts that allows for efficient and rapid exchange of information.

The current advisory structure at NASA, which consists of the NASA Advisory Council and its committees and subcommittees, is very vertical. That is, advice at all levels, from tactical and ad hoc expert advice to high-level policy and strategic advice, flows through the NASA Advisory Council to the NASA administrator and then down through the NASA organization.[5] This architecture lacks the short path between the relevant manager and outside experts that is needed for efficient communication and dialogue. Furthermore, it puts the high-level NASA Advisory Council, whose primary function is advising the NASA administrator on broad strategic planning questions, in the position of having to digest and transmit more mission- and division-specific advice, which can only distract the council from addressing the global issues. Inevitably, there is a loss of valuable information, and necessarily the critical dialogue between expert advisers and relevant managers becomes difficult if not impossible.

The current vertical structure has deprived the science community of insight into the goals and objectives of the agency, just as it has deprived NASA of needed tactical advice in making critical decisions. A more effective structure would be more horizontal, separating the different advice functions and providing more direct connections between the experts and the relevant managers. The NASA Advisory Council would continue to advise the administrator on policy and strategic matters, with NRC science committees and studies providing advice to the associate administrator for the Science Mission Directorate and the NASA Advisory Council. At the critical tactical level, NASA Advisory Council subcommittees and ad hoc panels would provide advice to SMD's associate administrator and science managers (including the Astrophysics Division director).

The committee concluded that the following are key principles for an effective advice structure:

- A hierarchy of advice where input is provided to the appropriate level of manager;
- A short path connecting the advising body to the relevant manager; and
- The ability for the manager to engage the advising body directly.

The committee notes that the previous advice structure had these attributes.

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[5] NASA officials state that because the appropriate NASA employees are in the meetings with the subcommittees, they can act on what they hear from the committee regardless of whether comments make it through the long reporting chain.

5

Findings and Recommendations

Astronomy and astrophysics is in a golden age of discovery and understanding. Breakthroughs are being made throughout the field, and the associated dramatic leaps in understanding are creating new opportunities, attracting new scientists to astronomy and other fields of science and technology, and inspiring people of all ages.

Finding: NASA's Astrophysics program has played a central role in creating the current era of revolutionary discovery in astrophysics and is key to further progress now clearly within reach.

As discussed in Chapter 3, NASA missions have given rise to an enormous breadth of scientific discoveries ranging from characterization of protoplanetary disks around nearby stars to measurements of supermassive black holes in active galactic nuclei to finding the seeds of cosmic structure in the cosmic microwave background. In all respects, these missions have delivered on their scientific promise in the best traditions of the NASA programs that proposed and executed them. These missions have positioned the field to capitalize on further scientific opportunities, as presented in *Astronomy and Astrophysics in the New Millennium* and *Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century.*

Finding: NASA's 2003 Astrophysics program plan responded effectively to the recommendations made in the National Research Council reports *Astronomy and Astrophysics in the New Millennium* and *Connecting Quarks*

***with the Cosmos*. In particular, the 2003 plan properly addressed the stated priorities and was well optimized across mission goals, types, and sizes.**

Finding: Implementation of NASA's 2003 Astrophysics program plan has been curtailed, limited by circumstances and events both internal and external to the agency. The 2006 plan further erodes NASA's ability to efficiently address the diverse goals of the AANM survey and the Q2C report with the vigor needed to produce transformational science return.

The 2003 NASA roadmaps for the Astrophysics program laid out plans to implement the majority of the priorities from the AANM survey and the Q2C report, in terms of both science objectives and the development of a diverse and well-balanced mission portfolio. However, as discussed in Chapter 4, the Astrophysics program's fiscal posture changed considerably between 2003 and 2006. The science plan produced in 2006 retained most of the elements of the 2003 plan, but with a significantly lengthened timeline for implementation of the projects. As a result, NASA is no longer in a position to achieve the goals of the AANM decadal survey within the timeframe envisioned in that report.

Finding: In a time of extraordinary potential for scientific discovery, the prospects have been substantially reduced for NASA's contributing in the future to astrophysics over a diverse range of enterprises, and with the agility necessary to rapidly respond to opportunity.

Finding: NASA's Astrophysics Division does not have the resources to pursue the priorities, goals, and opportunities outlined in the AANM and Q2C reports.

Furthermore, according to the FY 2007 budget request, funds for NASA's Astrophysics program will be declining for the foreseeable future. The division has therefore chosen to concentrate its resources in two areas: the highest-priority missions in the AANM survey, and those missions that are still in development from the previous survey. The result is that the present program is no longer well balanced across a desirable range of scientific areas, mission sizes, and mission-enabling activities. The committee believes that these changes diminish the nation's near-term ability to achieve the balance of science expectations articulated in the AANM and Q2C reports. The committee is also concerned that if a significant imbalance persists, deleterious effects on development of the workforce needed to sustain NASA and the astrophysics community will result. The committee believes that an optimal strategy would maintain a diverse portfolio that includes smaller programs and mission-enabling investment as well as the flagship missions.

Based on its analysis of NASA's current Astrophysics program, the committee recommends that the agency rebalance its mission and mission activity portfolio with the goals of (1) increasing the science return from the program in the near term; (2) establishing an advisory structure that communicates more effectively with the astrophysics community as the current situation is resolved; and (3) preventing a similar situation in the next decade by setting more realistic and practical ground rules for the characterization of future projects and for the carryover of legacy projects in the next decadal survey in order to ensure progress in short- as well as long-term projects.

Recommendation 1: NASA should optimize the projected science return from its Astrophysics program by (a) ensuring a diversified portfolio of large and small missions that reflect the science priorities articulated in the 2001 decadal survey *Astronomy and Astrophysics in the New Millennium* and (b) investing in the work required to bring science missions to their full potential: e.g., technology development, data analysis, data archiving, and theory.

The most important step in implementing this recommendation is a reevaluation by the Astrophysics Division of the program's mission balance, with the goal of restoring the Explorer line to the launch rate achieved in the early part of this decade. The division should also identify structural mechanisms (e.g., firewalls, cost caps, constraints on the concentration of resources in single programs) to protect small programs and mission-enabling activities such as technology development that will lay the groundwork for future missions and research support which are critical for optimizing the science return. The smaller missions and programs are particularly vulnerable to perturbations such as cost growth in large missions, changes in accounting systems, or project budget instability.

NASA should also to seek to limit cost growth in missions by exploring ways to provide less expensive launch services (particularly for smaller missions), re-examining mission safety and assurance requirements to match them more appropriately to mission size, relaxing deorbit requirements for smaller spacecraft involved in low-cost missions, and finding improved ways to establish and maintain effective international collaborations on missions of all sizes.

The committee realizes that implementing this recommendation may require the division to scale back larger programs that are currently in development. However, the committee concluded that the Explorer line is of the same priority as the top-ranked priorities in the moderate and large categories and should be implemented accordingly. It is essential that NASA find ways to accommodate a balance in its investment in large and small programs.

Recommendation 2: NASA should consider changes in its advisory structure to shorten the path between advisory groups and relevant managers so

as to maximize the relevance, utility, and timeliness of advice as well as the quality of the dialogue with advice givers. Clear communication between stakeholders and the agency is critical to a strong partnership for successfully implementing national priorities and realizing community science aspirations.

Currently advice of all kinds—from the high-level policy and strategic advice needed by NASA's administrator and senior management to the more tactical expert advice needed by science managers—is transmitted vertically through the NASA Advisory Council to the administrator and then down to the relevant managers. Direct two-way connections between advisory committees and managers would foster several important goals, including timely provision of and access to input tailored to the needs of the managers at each level, strengthened communication between NASA and the scientific user community, and greater flexibility for the NASA Advisory Council to focus on issues of policy and high-level agency strategy. NASA might also wish to reconstitute informal management operations working groups to enable science managers to quickly and effectively obtain expert advice on specific issues. The committee suggests that a continual dialogue between vested parties will produce the most effective outcome, especially in circumstances when difficult choices may be required.

Recommendation 3: NASA should recognize that ambitious missions could require significantly more than 10 years to complete, from conception through technology readiness and launch. NASA should insist that future decadal surveys specifically include in their prioritizing deliberations those projects carried over from previous surveys that have not yet entered development (NASA Phase C/D or equivalent). To enable an accurate assessment of science success and overall life-cycle costs, NASA should, in presenting potential missions to future survey committees, also distinguish between projects that are ready for implementation and those that require significant concept design or technology investment.

One factor contributing to the science community's current perception that NASA's Astrophysics program is not making steady progress is the confusion over the status of missions recommended in previous decadal surveys. Division officials stated that because missions recommended for the previous decade were not prioritized with the new missions, they are unclear as to the priority of those missions. NASA should seek to prevent similar confusion in future decades by requesting that each successive decadal survey committee's prioritization process include consideration of previously recommended missions that have not made significant progress within the proposed decade. Similarly, the agency should recognize the increasing complexity of its astrophysics missions and the likelihood that they will take more than a decade to complete. In particular, for future decadal surveys the agency should identify which missions are ready for development within a decade and which need significant preparatory work. An optimal strategy would ensure progress in both categories. The committee concluded that it is critical to establish uniform criteria for the accurate estimation of program costs, risks, and contingencies, and to understand the uncertainties in each at each stage of a mission.

Appendix

Acronyms

AANM	*Astronomy and Astrophysics in the New Millennium*
ACS	Advanced Camera for Surveys
ARISE	Advanced Radio Interferometry between Space and Earth
CGRO	Compton Gamma Ray Observatory
CHIPS	Cosmic Hot Interstellar Plasma Spectrometer
CMB	Cosmic microwave background
COBE	Cosmic Background Explorer
Con-X	Constellation-X-ray Observatory
COS	Cosmic Origins Spectrograph
CPU	Committee on the Physics of the Universe
DOE	U.S. Department of Energy
ESA	European Space Agency
EUVE	Extreme Ultraviolet Explorer
EXIST	Energetic X-ray Imaging Survey Telescope
FBC	Faster, better, cheaper
FUSE	Far Ultraviolet Spectroscopic Explorer
GALEX	Galaxy Evolution Explorer
GLAST	Gamma-ray Large Area Space Telescope
GRB	Gamma-ray burst
HETE-2	High Energy Transient Explorer mission
HST	Hubble Space Telescope

JDEM	Joint Dark Energy Mission
JWST	Next Generation Space Telescope, now known as the James Webb Space Telescope
LISA	Laser Interferometer Space Antenna
LSST	Large-aperture Synoptic Survey Telescope
MIDEX	Medium-class Explorer
NASA	National Aeronautics and Space Administration
NICMOS	Near Infrared Camera and Multi-Object Spectrometer
NRC	National Research Council
NSF	National Science Foundation
NuSTAR	Nuclear Spectroscopic Telescope Array
OAT	Office of Aerospace Technology
Q2C	*Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century*
RHESSI	Reuven Ramaty High Energy Solar Spectroscopic Imager
SAFIR	Single-aperture Far Infrared Observatory
SDO	Solar Dynamics Observer
SEU	Structure and Evolution of the Universe
SIM	SIM PlanetQuest (formerly called the Space Interferometry Mission)
SMD	Science Mission Directorate
SMEX	Small Explorer Program
SOFIA	Stratospheric Observatory for Infrared Astronomy
Spitzer	Spitzer Space Telescope (formerly SIRTF, the Space Infrared Telescope Facility)
STIS	Space Telescope Imaging Spectrograph
SWAS	Submillimeter Wave Astronomy Satellite
Swift	Multi-wavelength observatory dedicated to the study of gamma-ray burst science
TPF	Terrestrial Planet Finder
WISE	Wide-field Infrared Survey Explorer
WFC3	Wide Field Camera 3 (HST)
WMAP	Wilkinson Microwave Anisotropy Probe
XTE	X-ray Timing Explorer