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2020 Vision

The universe has always beckoned us. Over the course of human civilization, the night sky has provided a calendar for the farmer, a guide for the sailor, and a home for the gods. Astronomy led the scientific revolution, which continues to this day and has revealed that the sky visible to the naked eye is really just a hint of a vast and complex cosmos, within which our home planet is but a pale blue dot. Astronomers continue to explore the universe, learning its amazing history, discovering the richness of its contents, and understanding the physical processes that take place in its astoundingly diverse environments. Today, astronomy expands knowledge and understanding, inspiring new generations to ask, How did the universe form and the stars first come into being? Is there life beyond Earth? What natural forces control our universal destiny?

Because of the remarkable scientific progress in recent decades, in particular the explosion over the last decade of interest in and urgency to understand several key areas in astronomy and astrophysics, scientists are now poised to address these and many other equally profound questions in substantive ways. These dramatic discoveries came about through the application of modern technology and human ingenuity to the ancient craft of observing the sky. We have explored the cosmos, not just by observing through the tiny visible window used by our eyes, but also by exploiting the entire electromagnetic spectrum, from radio waves with wavelengths larger than a house to gamma rays with wavelengths 1,000 times smaller than a proton. The universe has also been studied by using samples returned to Earth from comets and meteorites, and by detecting and analyzing high-energy particles

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that permeate space. The opportunities for the future fill us with awe, enrich our culture, and frame our view of the human condition.

This report is the result of the National Research Council's (NRC's) survey of astronomy and astrophysics for the decade of the 2010s—Astro2010. The survey covers what has been learned, what could be learned, and what it will take to sustain the current revolution in understanding. As requested, the report outlines a plan to realize the scientific promise of the decade to come. The recommended major new elements must be combined with ongoing support for and augmentation of the foundational core of the federally supported research program to ensure a balanced program in astronomy and astrophysics that optimizes overall scientific return.

Below and in subsequent chapters of this report the Committee for a Decadal Survey of Astronomy and Astrophysics presents a compelling science program (Chapter 2), outlines the relationship of the federal program to the larger astronomy and astrophysics enterprise (Chapters 3 and 4), discusses workforce development and other core activities (Chapters 5 and 6), and describes in detail the integrated program it recommends for the decade ahead (Chapter 7). The process that was followed in carrying out Astro2010 is recounted in this report's preface and reviewed again in Chapter 7.

SCIENCE OBJECTIVES

The exciting program of activities proposed here will help to advance understanding of how the first galaxies formed and started to shine. It will direct the discovery of the closest habitable planets beyond our solar system. It will use astronomical measurements to try to unravel the mysteries of gravity and will probe fundamental physics beyond the reach of Earth-based experiments. The committee found that the way to optimize the science return for the decade 2012-2021 within the anticipated resources was to focus on these three science objectives while also considering the discovery potential of a much broader research program. To achieve these objectives, a complementary effort of space-based, ground-based, and foundational, core research is required.

Cosmic Dawn: Searching for the First Stars, Galaxies, and Black Holes

We have learned much in recent years about the history of the universe, from the big bang to the present day. A great mystery now confronts us: When and how did the first galaxies form out of cold clumps of hydrogen gas and start to shine—when was our "cosmic dawn"? Observations and calculations suggest that this phenomenon occurred when the universe was roughly half a billion years old, when light from the first stars was able to ionize the hydrogen gas in the universe

from atoms into electrons and protons—a period known as the epoch of reionization. Scientists think that the first stars were massive and short-lived. They quickly exploded as supernovae, creating and dispersing the first elements with nuclei heavier than those of hydrogen, helium, and lithium, and leaving behind the first black holes. Astronomers must now search the sky for these infant galaxies and find out how they behaved and interacted with their surroundings.

After the cosmic dawn, more and more galaxies formed, merged, and evolved as their gas turned into stars and those stars aged. Many of the faintest images from current telescopes are of these growing infant galaxies. Their properties are just starting to be revealed. In particular, it is now known that such galaxies quickly grow black holes in their nuclei with masses that can exceed a billion times the mass of the Sun and become extraordinarily luminous quasars. How this happens is a mystery.

We also know that the giant galaxies we see around us today were built up from the mergers of smaller galaxies and the accretion of cold gas. Not only do the stars and gas commingle, but the central black holes also merge. Amazingly, it should be possible to detect waves in the fabric of space-time—gravitational waves—that result from the dramatic unions when galaxies and black holes are young and relatively small.

Another approach to understanding our cosmic dawn is to carry out “cosmic paleontology” by finding the rare stars that have the lowest concentrations of heavy elements and were formed at the earliest times. Today, we can scrutinize only stars in our galaxy; in the future, we will be able to explore other nearby galaxies to uncover stellar fossils and use them to reconstruct the assembly of young galaxies.

Exploring the first stars, galaxies, and quasars is a tremendous challenge, but one astronomers and astrophysicists are ready to tackle and overcome, thereby continuing the story of how our universe came to be.

New Worlds: Seeking Nearby, Habitable Planets

On Christmas Eve, 1968, Apollo 8 astronaut William Anders took an iconic photograph of the rising Earth from his vantage point orbiting the Moon. It highlighted, to more people than ever before, that we humans share a common home that is both small and fragile. It also brought into focus the question, What does Earth look like from much farther away? Remarkable discoveries over the past 15 years have led us to the point that we can ask and hope to answer the question, Can we find another planet like Earth orbiting a nearby star? To find such a planet would complete the revolution, started by Copernicus nearly 500 years ago, that displaced Earth as the center of the universe.

Almost two decades ago, astronomers found evidence for planets around neutron stars, and then, in 1995, a star just like the Sun in the constellation Pegasus

was shown to vary regularly in its radial velocity—resulting from motion toward or away from us here on Earth—in response to the gravitational pull of an orbiting planet. This planet was roughly as massive as Jupiter but orbited its star every 4 days, far more quickly than any of our Sun’s planets. So, in a single set of observations we solved an age-old puzzle: yes, there are other planetary systems around stars like our Sun. However, they do not necessarily look like our solar system. Today, in mid-2010, we know of almost 500 extrasolar planets with masses ranging from a few to a few thousand times the mass of Earth.

We have greatly expanded our discovery techniques since 1995. Radial velocity detection of planets is much more sensitive, reaching down below 10 Earth masses. We can detect tiny changes in the combined light of a star and planet as they transit in front of one other, a technique currently being exploited very successfully by the Kepler space telescope. We can also probe planetary systems by measuring microlensing as their gravitational fields bend rays of light from a more distant star. Telescopes on the ground and in space have even directly imaged as distinct point sources a few large planets. In other cases, we can learn about planetary systems by measuring infrared and radio emission from giant disks of gas out of which planets can form. Finally, in a most important development, the Hubble Space Telescope and the Spitzer Space Telescope have found the spectral lines of carbon dioxide, water, and the first organic molecule, methane, in the atmospheres of orbiting planets. This is extraordinarily rapid progress.

Astronomers are now ready to embark on the next stage in the quest for life beyond the solar system—to search for nearby, habitable, rocky or terrestrial planets with liquid water and oxygen. The host star of such a planet may be one like our Sun, or it could be one of the more plentiful but less hospitable cooler red stars. Cooler red stars are attractive targets for planet searches because light from a planet will be more easily detected above the stellar background. Making the search harder, terrestrial planets are relatively small and dim, and are easily lost in the exozodiacal light that is scattered by the dusty disks that typically orbit stars. The observational challenge is great, but armed with new technologies and advances in understanding of the architectures of nearby planetary systems, astronomers are poised to rise to it.

Physics of the Universe: Understanding Scientific Principles

Astronomy and physics have always been closely related. Observations of orbiting planets furnished verifications of Newton’s law of gravitation and Einstein’s theory of gravity—general relativity. In more recent years, observations of solar system objects and radio pulsars have provided exquisitely sensitive proof that general relativity is, indeed, correct when gravity is weak. The universe continues

to be a laboratory that offers access to regimes not available on Earth, helping us to both understand and discover new elements of the basic laws of nature.

Scientists can study the universe on the largest observable scales—more than 10 trillion, trillion times larger than the size of a person. The past decade has seen the confirmation from measurements of the truly remarkable discovery that the expansion of the universe is accelerating. In modern language, this acceleration is attributed to the effect of a mysterious substance called dark energy that accounts for 75 percent of the mass-energy of the universe today causing galaxies to separate at ever faster speeds. The remainder of the mass-energy is 4.6 percent regular matter and 20 percent a new type of matter, dubbed dark matter, that is believed to comprise new types of elementary particles not yet found in terrestrial laboratories. The effects of dark energy are undetectable on the scale of an experiment on Earth. The only way forward is to use the universe at large to infer the properties of dark energy by measuring its effects on the expansion rate and the growth of structure.

Amazingly, we can ask and hope to answer questions about the universe as it was very soon after the big bang. Recent observations of the microwave background are consistent with the theory that the universe underwent a burst of inflation when the expansion also accelerated and the scale of the universe that we see today grew from its infinitesimally small beginnings to about the size of a fist. Gravitational waves created at the end of the epoch of inflation can propagate all the way to us and carry information about the behavior of gravity and other forces during the first moments after the big bang. These waves can be detected through the distinctive polarization pattern[1] that they impose on the relic cosmic microwave background radiation. Detection of this imprint would both probe fundamental physics at very high energies and bear witness to the birth of the universe.

Yet another opportunity to study fundamental principles comes from precisely observing the behavior of black holes. Black holes are commonly found in the nuclei of normal galaxies and are born when very massive stars end their stellar lives. Scientists have an exact theoretical description of space-time around black holes but do not know if this description is correct. One way to find out is to observe X-ray-emitting gas and stars as they spiral toward a black hole's event horizon beyond which nothing, not even light, can escape. Another is to observe the jets that escape black holes with speeds close to that of light. However, the best test of all will come from measuring the gravitational radiation that is observed when moderate-mass black holes merge. We now have the software and the computing power to calculate the signals that should be seen and the technology to test the theory.

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What excites astronomers and physicists alike is that the tools are now at hand to greatly expand current understanding of fundamental physics in new and important ways.

OPTIMIZING THE SCIENCE PROGRAM

Astronomy is a rich and diverse science that encompasses much more than the grand challenges described above. There are great opportunities to be seized over a broad research program, as described in Chapter 2. Astronomy is still driven by discovery, and when the programs described in past decadal surveys were successfully executed, many of the most important results were largely unanticipated. The new facilities contained in this survey's recommended program are highly versatile. In addition to carrying out the observational program described, they will advance the broad research program and are also able to both make and respond to fresh breakthroughs.

This report is written at a time when the nation's finances are severely stressed. The committee was charged to consider alternative budget scenarios. It chose to adopt for each agency the agency-projected budget and a second, optimistic budget that reflects modest relative growth. In the case of the National Aeronautics and Space Administration (NASA), the agency-projected budget is flat in real-year dollars and allows very little new activity until the James Webb Space Telescope (JWST) is launched, presumably in mid-decade. The optimistic budget used by the committee is flat in FY2010 dollars. In the case of the National Science Foundation (NSF), the agency-projected budget is flat in FY2010 dollars, which allows little to no opportunity for new activity over the entire decade, given the obligations to support existing facilities. The optimistic budget used by the committee supposes growth in purchasing power at a rate of 4 percent per year, the so-called doubling scenario that is being applied to the overall NSF budget. In the case of the Department of Energy (DOE), the agency-projected budget is constant in FY2010 dollars, and the optimistic budget used by the committee is also on a doubling track, consistent with the current administration's stated policy for the DOE Office of Science. The committee's recommended program that follows has been constrained to fit under the optimistic budget envelopes. Reductions that would be needed under less favorable budgets are also described.

A successful federal research program must also be balanced. There is a trade-off between investing in the development and construction of ambitious new

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telescopes and supporting broad-ranging observational and theoretical research that optimizes the return from operating facilities. The goal of the committee, consistent with its charge, has been to maximize the science return for a given budget. The committee found that in some cases the balance of resources is not optimal, and this report contains a number of recommendations to augment or adjust the foundations of the program.

The committee's proposed program (Chapter 7) is recommended on the basis of four general criteria—maximizing scientific contribution, building on the current astronomy and astrophysics enterprise, balancing this decade's programs against investing in the next decade's, and optimizing the science return given the highly constrained budget. These criteria are discussed further below. The resulting program emphasizes certain capabilities for U.S. leadership, including all-sky synoptic imaging on the ground and in space, large-aperture telescopes, exploration of non-electromagnetic portals to the universe, technology and software, public-private and international partnerships, frequent opportunities for new medium-scale instrumentation on the ground and in space, and interdisciplinary work, especially work involving connections between astrophysics and physics.

Finally, a key concern of the committee's is the stewardship of the present survey's recommended program. Although a good-faith attempt has been made to provide answers to all the questions raised by the charge, it is in the very nature of research that unforeseen issues requiring community advice will arise. In addition, there will be a need to monitor progress. Accordingly, implementation of the survey will require stewardship over the coming decade in the form of strategic advice requested by but generated independent of the agencies supporting the field.

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PROPOSED PROGRAM OF ACTIVITIES

The committee's recommended program is presented in terms of specific space-based[2] and ground-based projects and opportunities. In space, large-scale activities are those having a total appraised cost exceeding $1 billion, while medium-scale activities have a total cost estimated to range from $300 million to $1 billion. On the ground, large-scale activities are those whose total cost is appraised to exceed $135 million, while medium-scale activities have a total cost in the range of $4 million to $135 million. All values are in FY2010 dollars.[3]

Space Projects—Large—in Rank Order

Wide-Field Infrared Survey Telescope (WFIRST)

A 1.5-meter wide-field-of-view near-infrared-imaging and low-resolution-spectroscopy telescope, WFIRST will settle fundamental questions about the nature of dark energy, the discovery of which was one of the greatest achievements of U.S. telescopes in recent years. It will employ three distinct techniques—measurements of weak gravitational lensing, supernova distances, and baryon acoustic oscillations—to determine the effect of dark energy on the evolution of the universe. An equally important outcome will be to open up a new frontier of exoplanet studies by monitoring a large sample of stars in the central bulge of the Milky Way for changes in brightness due to microlensing by intervening solar systems. This census, combined with that made by the Kepler mission, will determine how common Earth-like planets are over a wide range of orbital parameters. It will also, in guest investigator mode, survey our galaxy and other nearby galaxies to answer key questions about their formation and structure, and the data it obtains will provide fundamental constraints on how galaxies grow. The telescope exploits the important work done

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by the joint DOE/NASA design team on the Joint Dark Energy Mission—specifically the JDEM-Omega concept—and expands its scientific reach. WFIRST is based on mature technologies with technical risk that is medium low and has medium cost and schedule risk. The independent cost appraisal is $1.6 billion, not including the guest investigator program. As a telescope capable of imaging a large area of the sky, WFIRST will complement the targeted infrared observations of the James Webb Space Telescope. The small field of view of JWST would render it incapable of carrying out the prime WFIRST program of dark energy and exoplanet studies, even if it were used exclusively for this task. The recommended schedule has a launch data of 2020 with a 5-year baseline mission. An extended 10-year mission could improve the statistical results and further broaden the science program. The European Space Agency (ESA) is considering an M-class proposal, called Euclid, with related goals. Collaboration on a combined mission with the United States playing a leading role should be considered so long as the committee's recommended science program is preserved and overall cost savings result.

WFIRST addresses fundamental and pressing scientific questions and will contribute to a broad range of astrophysics. It complements the committee's proposed ground-based program in two key science areas: dark energy science and the study of exoplanets. It is a part of coordinated and synergistic programs in fields in which the United States has pioneered the progress to date. It presents opportunities for interagency and perhaps international collaboration that would tap complementary experience and skills. It also presents relatively low technical and cost risk, making its completion feasible within the decade, even in a constrained budgetary environment. For all these reasons it is the committee's top-priority recommendation for a space mission.

Explorer Program Augmentation

The Explorer program supports small and medium-size missions, selected through competitive peer review, that are developed and launched on roughly 5-year timescales. The Explorer program enables rapid responses to new discoveries and provides platforms for targeted investigations essential to the breadth of NASA's astrophysics program. Explorers have delivered a scientific return on investment at the highest level over the past two decades. The three astrophysics Medium-scale Explorer (MIDEX) missions launched to date —the Wilkinson Microwave Anisotropy Probe (WMAP), Swift, and the Wide-Field Infrared Survey Explorer (WISE)—have provided high-impact science for a combined cost significantly less than that of a single flagship mission.[4] WMAP, launched just 5 years after the

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Cosmic Background Explorer (COBE) discovered that the cosmic microwave background (CMB) has measurable fluctuations, demonstrated that these tiny variations imprint precise information about the early universe. WMAP is credited with obtaining the best measurements of the age, geometry, and content of the universe. The Swift mission has transformed understanding of explosive gamma-ray burst events, and it holds the record for detecting the most distant object in the universe. The WISE mid-infrared survey, extending over the entire sky, is studying the coolest stars, the universe's most luminous galaxies, and some of the dimmest near-Earth asteroids and comets. Small Explorer (SMEX) missions, as well as Mission of Opportunity contributions to non-NASA missions, have made essential advances in understanding of phenomena ranging from the explosive release of energy in flares on the Sun (with the Reuven Ramaty High Energy Solar Spectroscopic Imager) to the assembly of galaxies (with the Galaxy Evolution Explorer). The promise of future Explorer missions is as great as ever, and this program will be essential to enabling new opportunities, and to maintaining breadth and vibrancy in NASA's astrophysics portfolio in a time of budgetary stress. This survey recommends that the annual budget of the astrophysics component of the Explorer program be increased from $40 million to $100 million by 2015.

The categorization of the recommended Explorer program augmentation as a large-category activity reflects the total cost of the augmentation for the decade 2012-2021, and its high ranking is motivated by the committee's view that expanding the Explorer program is a very effective way to maximize scientific progress for a given outlay.

Laser Interferometer Space Antenna (LISA)

LISA employs three separated spacecraft to detect long-wavelength ripples in the fabric of space-time, thereby opening a new window on the universe. LISA will detect the mergers of black holes with masses ranging from 10,000 to 10 million solar masses at cosmological distances, and will make a census of compact binary systems throughout the Milky Way. LISA promises new discoveries as well as progress on central questions such as understanding the growth of galaxies and black holes. LISA will also test general relativity with exquisite precision in regimes inaccessible on Earth. LISA complements the search for gravitational radiation being made at shorter wavelengths by the ground-based Advanced LIGO. LISA is a partnership with ESA, and so its schedule is dependent on ESA's selection of the next L-class mission opportunity—LISA is one of three contenders for this opportunity. LISA's key technologies will be demonstrated on the ESA-led LISA Pathfinder mission, due for launch in 2012. With the success of Pathfinder and a decision by ESA to move forward, LISA could launch by 2025. Independent review found LISA's technical risk, assuming Pathfinder success, to be medium, and

the NASA appraised cost, based on a 50 percent participation and including the costs of partnering at such a level, to be $1.4 billion. The cost and schedule risk classification is medium high. If Pathfinder is not a success or if a roughly equal partnership is not possible, the committee recommends that NASA request advice from a decadal survey implementation advisory committee (DSIAC) to review the situation mid-decade. LISA presents a compelling scientific opportunity, and there is readiness to address its remaining technical challenges.

Overall the recommendation and prioritization for LISA reflect its compelling science case and the relative level of technical readiness.

International X-ray Observatory (IXO)

IXO is a versatile, large-area, high-spectral-resolution X-ray telescope that will make great advances on broad fronts ranging from characterization of black holes to elucidation of cosmology and the life cycles of matter and energy in the cosmos. Central to many of the science questions identified by this survey, IXO will revolutionize high-energy astrophysics with more than an-order-of-magnitude improvement in capabilities. IXO is a partnership among NASA, ESA, and the Japan Aerospace Exploration Agency (JAXA), and, like LISA, it is a candidate for the next L-class ESA launch opportunity. On the basis of a 50 percent participation, it has an appraised cost to NASA, including the cost of partnering, of $3.1 billion, and the cost and schedule risk is medium high. The technical risk is also medium high. Cost threats and uncertainties due to the immaturity of some of the required technologies have added considerably to the cost appraisal. The budget profiles used by the committee to define an overall program are unlikely to permit a start before the end of the decade—allowing time for the necessary technology maturation and risk reduction. However, this situation does not diminish the committee's assessment of the importance of the discoveries that IXO would make. Because of IXO's high scientific importance, a technology development program is recommended for this decade with sufficient resources—estimated to be on the order of $200 million—to prepare IXO for favorable consideration in the next survey in 2020. The committee thinks that allowing IXO, or indeed any major mission, to exceed $2 billion in total cost to NASA would unacceptably imbalance NASA's astrophysics program, given the present budgetary constraints. If the technology development program is not successful in bringing cost estimates below this level, descope options must be considered. Should ESA select IXO as the first L-class mission, NASA should proceed immediately with a DSIAC review to determine an appropriate path forward to realize IXO as soon as possible with acceptable cost and schedule risk.

The ranking of IXO as the fourth-priority large space mission reflects the technical, cost, and programmatic uncertainties associated with the project at the current time. Many high-priority science questions require an X-ray

observatory on this scale that can continue the great advances made by Chandra and XMM-Newton. Furthermore, the science of IXO is quite complementary with that of LISA.

Space Projects—Medium—in Rank Order

New Worlds Technology Development Program

One of the fastest-growing and most exciting fields in astrophysics is the study of planets beyond our solar system. The ultimate goal is to image rocky planets that lie in the habitable zone—at a distance from their central star where water can exist in liquid form—and to characterize their atmospheres. To prepare for this endeavor, the committee recommends a program to lay the technical and scientific foundations for a future space imaging and spectroscopy mission. NASA and NSF should support an aggressive program of ground-based high-precision radial velocity surveys of nearby stars to identify potential candidates. In the first part of the decade NASA should support competed technology development to advance multiple possible technologies for a next-decade planet imager, and should accelerate measurements of exozodiacal light levels that will determine the size and complexity of such missions. The committee recommends an initial NASA funding level of $4 million per year so as to achieve a clear set of design requirements and technology gateways to be passed. If, by mid-decade, a DSIAC review determines that sufficient information has become or is becoming available on key issues such as planet frequency and exozodiacal dust distribution, a technology down-select should be made and the level of support increased to enable a mission capable of studying nearby Earth-like planets to be mature for consideration by the 2020 decadal survey, with a view to a start early in the 2020 decade. The committee estimates that an additional $100 million will be required for the mission-specific development.

Inflation Probe Technology Development Program

Detecting the distinctive imprint on the cosmic microwave background caused by gravitational waves produced during the first few moments of the universe would provide evidence for the theory of inflation and open a new window on exotic physics in the early universe. Progress in detecting this signal is rapid, with advances from ground-based telescopes, suborbital flights, and the recently launched Planck satellite. The committee recommends a technology program to advance detection techniques at an annual funding level of $1 million to $2 million. If the polarization pattern imprinted by gravitational waves from the epoch of inflation is detected during this decade, the committee recommends a technology

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selection and mission development to design a mission to study the signal. The resulting proposal would be considered by the 2020 decadal survey. The committee estimates a budget requirement of $60 million for the development, to be triggered in the event of a convincing detection.

Small Additions and Augmentations to Space Research Program (Unranked)

U.S. Contribution to the JAXA-ESA SPICA Mission

The Space Infrared telescope for Cosmology and Astrophysics (SPICA) is a Japanese-led 3.5-meter infrared telescope that will operate from 5 to 210 microns. SPICA will address many of this survey's science priorities, including understanding the birth of galaxies, stars, and planets as well as the motion of matter through our own interstellar medium. A competed U.S. science and instrument contribution at an estimated level of $150 million over the decade is recommended.

Core Research Program

NASA's core research programs, from theoretical studies to innovative technology development, are fundamental to mission development and essential for scientific progress. They provide the long-term foundation for new ideas that stretch the imagination, and they lay the groundwork for far-future vision missions. They support the maturation of new technologies needed for nearer-term Explorer and flagship missions. They provide the means to understand and interpret scientific results. Maintaining these core activities has a high priority for the survey committee, and the budget allocations should not be allowed to decrease to address overruns in the costs of large and medium missions. In addition, the following unranked specific augmentations are recommended.

Astrophysics Theory Program. To enhance the scientific return from operating missions and inform the investment in new ones, an augmentation of $35 million to the current funding level for the decade is recommended.for the decade is recommended.

Definition of a Future Ultraviolet-Optical Space Capability. To prepare for a future major ultraviolet mission to succeed the Hubble Space Telescope, it will be necessary to carry out a mission-definition program. A budget of roughly $40 million over the decade for mission studies and initial technology development is recommended.

Intermediate Technology Development. A gap has emerged within NASA between long-term so-called “Blue Skies” investigations and shorter-term mission-specific technology development. Formally this gap is associated with technology readiness

levels 3 to 5. An augmentation beginning at $2 million per year and increasing to $15 million per year by the end of the decade would address this imbalance.

Laboratory Astrophysics. Herschel, JWST, SPICA, and IXO, with their fine spectral capabilities, will place new demands on basic nuclear, ionic, plasma, atomic, and molecular astrophysics. Care should be taken to ensure that these needs are met. An increase by $2 million per year in the funding of the present program is recommended.

Suborbital Program. The balloon and sounding rocket programs provide fast access to space for substantive scientific investigations and flight testing of new technology. The balloon program in particular is important for advancing detection of the cosmic microwave background and particle detection. These programs also provide a training ground for the principal investigators of tomorrow’s major missions. A growth in the budget by $15 million per year is recommended.

Theory and Computation Networks. To enable the large-scale theoretical investigations identified as science priorities by this survey, the committee proposes a new competed program to support coordinated theoretical and computational research—particularly that of fundamental relevance to upcoming space observatories. For NASA an annual budget of $5 million is recommended. For DOE an annual funding level of $1 million is recommended for activities related to space-based research.

Ground Projects—Large—in Rank Order

Large Synoptic Survey Telescope (LSST)

LSST is a multipurpose observatory that will explore the nature of dark energy and the behavior of dark matter, and will robustly explore aspects of the time-variable universe that will certainly lead to new discoveries. LSST addresses a large number of the science questions highlighted in this report. An 8.4-meter optical telescope to be sited in Chile, LSST will image the entire available sky every 3 nights. Over a 10-year lifetime, LSST will be a unique facility that, building on the success of the Sloan Digital Sky Survey, will produce a 100-billion-megabyte publicly accessible database. The project is relatively mature in its design. The appraised construction cost is $465 million, two-thirds of which the committee recommends be borne by NSF through its Major Research Equipment and Facilities Construction (MREFC) line and a quarter by DOE using Major Item of Equipment (MIE) funds, with the remaining fraction coming from international and private partners. The annual operations costs are estimated at $42 million, of which $28 million is recommended to be split between NSF and DOE at two-thirds and one-third, respectively. The committee recommends that LSST be submitted immediately for NSF's MREFC consideration with a view to achieving first light before the end of the decade. Independent review judged the cost and schedule risk, as well as the technical risk, to be medium low.

The top rank accorded to LSST is a result of (1) its compelling science case and capacity to address so many of the science goals of this survey and (2) its readiness for submission to the MREFC process. LSST was judged by its technical maturity, the survey's assessment of risk, and appraised construction and operations costs. Having made considerable progress in terms of its readiness since the 2001 survey, LSST was judged as the most "ready-to-go."

Mid-Scale Innovations Program

New discoveries and technical advances enable small- to medium-scale experiments and facilities that advance forefront science. A large number of compelling proposed research activities submitted to this survey were highly recommended by the Program Prioritization Panels, with costs ranging between the limits of the NSF Major Research Instrumentation and MREFC programs, $4 million to $135 million. The committee recommends a new competed program to significantly augment the current levels of NSF support for mid-scale programs. An annual funding level of $40 million per year is recommended—just over double the amount currently spent on projects in this size category through a less formal programmatic structure.

The principal rationale for the committee's ranking of the Mid-Scale Innovations Program is the many highly promising projects for achieving diverse and timely science.

Giant Segmented Mirror Telescope (GSMT)

Transformative advances in optical and infrared (OIR) astronomy are now possible by building adaptive optics telescopes with roughly 10 times the collecting area and up to 80 times the near-infrared sensitivity of current facilities. These observatories will have enormous impact across a large swath of science and will greatly enhance the research that is possible with several other telescopes, especially JWST, the Atacama Large Millimeter/submillimeter Array (ALMA), and LSST. A federal investment to provide access for the entire U.S. astronomy and astrophysics community to an optical-infrared 30-meter-class adaptive optics telescope is strongly recommended. Two U.S.-led projects, the Giant Magellan Telescope (GMT) and the Thirty Meter Telescope (TMT), are being developed by international collaborations led by U.S. private consortia. The committee recommends that a choice between

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the two projects be made as soon as possible for a federal partnership at a level of about a 25 percent investment in one of them. A schedule and budget plan should then be developed. The survey appraises a total GSMT construction cost in the range of $1.1 billion (GMT appraisal) to $1.4 billion (TMT appraisal) and assumes that the federal share of the capital cost will be borne by MREFC, while recognizing that the total share may be secured through whatever combination of capital cost, operating funds, and instrumentation support is most favorable. The operations federal cost share is expected to carried by NSF-Astronomy. Both telescope projects estimated their annual operations costs (including facility and instrument upgrades) at around $50 million ($36 million, GMT; $55 million, TMT). Although the committee did not analyze these estimates in detail, they are far below the usual rule of thumb for large projects (10 percent of construction costs per year).

The committee believes that a GSMT will, as large telescopes have in the past, transform U.S. astronomy because of the telescope's broad and powerful scientific reach, and that federal investment in a GSMT is vital to U.S. competitiveness in ground-based optical astronomy over the next two decades. These are the main reasons for the committee's strong recommendation of GSMT.

The third-place ranking also results from the requirement in the committee's charge that the survey's prioritization be informed not only by scientific potential but also by the technical readiness of the components and the system, the sources of risk, and the appraisal of costs. LSST and several of the concatenation of candidates for the Mid-Scale Innovations Program were deemed to be ahead of GSMT in these areas. The committee also took into account programmatic concerns such as the time it will take to implement the committee's recommendation for a choice to be made on which one of the two U.S.-led GSMT concepts NSF will partner, and the time it would take for any MREFC decision to be made and federal funds awarded. The committee's setting of the relative positions of its top three ranked activities resulted from its consideration of all these various factors.

Atmospheric Čerenkov Telescope Array (ACTA)

The past decade has seen the coming of age of very high energy tera-electron-volt (TeV) gamma-ray astronomy. Plans are underway to capitalize on recent scientific advances by building a large facility that uses light created as gamma rays interact with the atmosphere and that will achieve an order-of-magnitude greater sensitivity compared to current telescopes. This new gamma-ray observatory will detect a wide variety of high-energy astrophysical sources and seek indirect evidence for dark matter annihilation. Two facilities, the European Čerenkov Telescope Array (CTA) and the U.S. Advanced Gamma-ray Imaging System (AGIS), have been proposed. The survey appraised the full AGIS project cost to be in the $400 million range. The technical risk was judged to be medium low. The committee recommends

that the U.S. AGIS team collaborate as a partner with the European CTA team and that a U.S. budget for construction and operations of approximately $100 million over the decade be shared between DOE, NSF-Physics, and NSF-Astronomy.

The recommendation for ongoing U.S. involvement in TeV astronomy is based largely on the demonstrated recent accomplishments of this field and the prospect of building fairly quickly a much more capable facility to address a broad range of astronomy and physics questions over the next decade.

Ground Project—Medium

CCAT

CCAT (formerly the Cornell-Caltech Atacama Telescope) is a powerful wide-field-of-view 25-meter telescope to be constructed at a high site in Chile just above the ALMA site. CCAT will perform sensitive millimeter and submillimeter imaging surveys of large fields, enabling studies of galaxies, stars, planets, and interstellar gas, as well as objects in the outer solar system. CCAT will complement ALMA by finding many of the sources that ALMA will follow up. The committee appraises the total development and construction cost at $140 million. The estimated start of operations is 2020, and the survey judges the cost and schedule risk, and technical risk, as medium. The committee recommends NSF support for the construction costs, on the order of $37 million, and a $7.5 million share of the operations costs, provided that the U.S. community has appropriate access to both the results of the surveys and competed observing time.

CCAT is called out to progress promptly to the next step in development because of its strong science case, its importance to ALMA, and its readiness.

Small Additions and Augmentations to Ground Research Program (Unranked)

Advanced Technologies and Instrumentation (ATI)

ATI supports instrumentation and technology development, including computing at astronomical facilities in support of the research program. The current level of funding is roughly $10 million per year, which the committee proposes to increase to $15 million per year to accommodate key opportunities, including, especially, adaptive optics development and radio instrumentation.

Astronomy and Astrophysics Research Grants Program (AAG)

Individual investigator grants provide critical support for astronomers to conduct the research for which the observatories and instruments are built. The current

funding level has fluctuated, especially due to the welcome injection of ARRA[5] funding, but the rough baseline is $46 million. An increase of $8 million to bring the baseline to $54 million is recommended. This increase should include the support of new opportunities in Laboratory Astrophysics.

Gemini Augmentation

The imminent withdrawal of the United Kingdom (UK) from the Gemini partnership will require that additional support come from the remaining partners. Set against this need is a desire to operate the telescopes more efficiently and a belief that cost savings are achievable. An augmentation of $2 million in the annual budget is recommended subject to the results of negotiations between the Gemini Board and NSF.

Telescope System Instrument Program (TSIP)

TSIP supports telescope instrumentation on privately operated telescopes in exchange for observing time. It is a vital component of the OIR system that was instituted following a recommendation of the 2001 decadal survey, AANM. It is currently supporting research at a rate of $2 million to $3 million per year, and an increment to $5 million per year is proposed.

Theory and Computation Networks

This is a new competed program coordinated between NSF and DOE to support coordinated theoretical and computational attacks on selected key projects that are judged ripe for such attention. An NSF annual funding level of $2.5 million is recommended. For DOE an annual funding level of $1 million is recommended. A similar program is proposed for NASA and DOE above in the space-based program recommendations.

OTHER CONCLUSIONS AND RECOMMENDATIONS

The field of astronomy is far more than telescopes and discoveries. It involves people—students for whom it provides a gateway to all science and technology, members of the public who share a fascination with learning about the universe, and astronomers themselves. Within the United States, it involves three science agencies, DOE, NASA, and NSF, and many individuals and private foundations that have generously supported the field in the past and promise to do so in the future.

Beyond the U.S. astronomy community is a vast network of researchers, facilities, and plans that interface in complex ways, sometime competitively, but increasingly collaboratively. Each of these expressions of the field of astronomy raises policy issues that are also encompassed by the charge to the committee and are mentioned below and discussed in detail in Chapters 3 through 6. The major conclusions and recommendations offered in those chapters are discussed below.

Partnership in Astronomy and Astrophysics Research

The opportunities described in the reports from the survey's Program Prioritization Panels on optical and infrared and on radio, millimeter, and submillimeter astronomy from the ground; on electromagnetic observations from space; and on particle astrophysics and gravitation are compelling. Having reviewed so many opportunities for building research facilities and instruments that would be dependent on multiple approaches to collaborative science, the committee was easily convinced of the value of a continued emphasis on forging new and strong partnerships.

International Collaboration

Dramatic discoveries about the universe have stimulated a substantial growth of interest in astronomy, in other countries and in allied disciplines like physics. Although the federal investment in astronomy has increased, that of the rest of the world has grown much faster. Astronomical research is becoming a more international enterprise. Almost all new major facilities involve scientists and engineers from all around the world and are built and operated with funds from diverse sources. These changes necessitate new approaches to providing access and sharing data that are both more flexible and more equitable.

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International Strategic Planning

Another consequence of the globalization of astronomy is that it no longer suffices to make national strategic plans. Indeed, much of the challenge of the present survey derives from this realization. It is neither realistic nor advisable to imagine creating a single international strategic plan that separates the science from the funding authority. However, a regular comparison of national and, in the case of Europe, continental plans can provide a forum for reviewing developments in science and technology and can create a fertile environment where successful collaborations can grow. One large international project for which such a forum would be beneficial is the Square Kilometer Array (SKA). Despite the unqualified enthusiasm for the science that this facility could deliver and the recognition that it represents the long-term future of radio astronomy, the committee encountered a major discrepancy between the schedule advertised by the international SKA community and the timescale on which NSF could realistically make a significant contribution to SKA's construction and operations costs.

Society, Astronomy, and Astronomers

Serving the Nation

The committee's recommended ambitious program of research in astronomy and astrophysics is driven in part by the benefits to society. Although the impetus for public support for the astronomy and astrophysics research enterprise will always be primarily the quest for an ever-deepening knowledge of our universe, as discussed elsewhere in this report that public support also produces significant additional benefits for the nation and its people.

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The urgency for federal investment in science, technology, engineering, and mathematics (STEM) education and research was highlighted in the influential 2007 National Academies report *Rising Above the Gathering Storm: Energizing and Employing America for a Brighter Economic Future*.[6]

As further service to the nation, important roles in government can be played by suitably skilled scientists. Not only are they able to inform the decision-making process, but they also can develop a rare appreciation of the challenges of the political process, which they are well-placed to communicate to other scientists.

Career Planning

A consequence of the current excitement in the field of astronomy is that it attracts many highly capable students who contribute substantially to the research enterprise. Not all of these will take up long-term positions in astronomy, and so it is fortunate that training in astronomical research appears to be well matched in practice to much broader career opportunities. However, the situation also appears to be changing rapidly, and there is a need for students and postdoctoral scholars to be responsibly informed about their employment options on the basis of reliable

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and current information. There is a particular need to educate and expose young researchers to issues of public policy.

Underrepresented Groups

By all measures minority Americans are seriously underrepresented among professional astronomers, and women have not yet achieved parity. For many reasons, improving the involvement of minority Americans and women is a matter of the highest priority. As discussed in Chapter 4, the committee came to the following two conclusions:

Sustaining Core Capabilities

Theory

The role of theorists has changed greatly in recent times, and they have become more engaged in the interpretation of current data as well as the planning of future facilities and missions. In addition, computational approaches have expanded greatly the range of problems that can be solved with confidence. The committee concluded that a new approach to supporting theory is needed, a conclusion that is reflected in its proposed program.

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Data Handling

A related issue is the increasing importance of data handling in astronomical projects and the need to see data analysis as an integral part of any new project. In the committee's view the best proposals for new major ground-based facilities and instruments include such planning.

Data Curation

Many astronomical data sets have long-term value and benefits. The committee concluded that there is a need for attention to data curation.

Laboratory Astrophysics

Another important component of the astrophysical infrastructure is the ability to carry out crucial measurements in the laboratory that are relevant to interpreting observations from astronomical environments. The suite of recently launched and proposed facilities will make the acquisition of laboratory data even more crucial than it has been in the past.

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The committee believes that NASA, NSF, and DOE will need to include funding for laboratory astrophysics in support of new missions and facilities and supports this conclusion in its proposed program. Other funding models should be considered if it is deemed necessary and cost-effective.

Preparing for Tomorrow

Senior Reviews

Ground-based astronomical observatories are often long-lived, and their integrated operating costs frequently exceed their construction cost by a large factor. It is therefore good stewardship to manage the NSF portfolio wisely and to balance continued support of older facilities with the development and operation of newer ones. To address this challenge, NSF-Astronomy completed its first senior review exercise in 2006. The need for these reviews is ongoing.

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Ground-Based Optical Astronomy

OIR astronomy in the United States historically has benefited from significant private investment, with considerable progress made over the past decade in public-private collaboration and partnerships. The OIR future is certain to include ever more complex facilities.

Gemini is an international partnership that constructed and now operates two 8-meter optical-infrared telescopes, one in the Northern Hemisphere, the other in the Southern Hemisphere. The United Kingdom has recently announced an intention to leave the partnership in 2012, resulting in a need to replace the UK support. This change presents an opportunity to revisit the management of Gemini as it transitions to stable observatory operation.

Ground-Based Radio Astronomy

With the commissioning of ALMA and the expectation for SKA in the future, radio astronomy stands poised to continue to offer considerable promise in the exploration of our universe.

Ground-Based Solar Astronomy

U.S. solar astronomy is undergoing major changes with the commitment to construct the Advanced Technology Solar Telescope and the associated plan to close several existing facilities as well as to reorganize the National Solar Observatory. There have been great advances in space-based solar astronomy, most recently with the successful launch and deployment of the Solar Dynamics Observatory. In addition, there is a growing interest in the solar-terrestrial connection associated with climate research. These changes imply that it is time to reevaluate the management of the U.S. program.