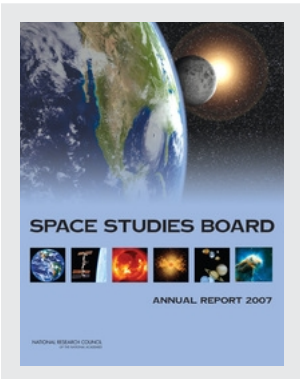

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DETAILS

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2007

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The Space Studies Board is a unit of the National Research Council, which serves as an independent advisor to the federal government on scientific and technical questions of national importance. The National Research Council, jointly administered by the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine, brings the resources of the entire scientific and technical community to bear through its volunteer advisory committees.

Support for the work of the Space Studies Board and its committees and task groups was provided by National Aeronautics and Space Administration contract NNH06CE15B, National Oceanic and Atmospheric Administration Contracts DG133R04CQ0009 and DG133R07SE1940, United States Geological Survey Grant 05HQGR0104, National Reconnaissance Office Contract NRO000-04-C-0174, and National Science Foundation Grant AST-0513177.

From the Chair

As 2007 comes to a close, the Space Studies Board is in the midst of celebrating its 50th year. As part of this celebration, the Board issued a CD containing all of its reports during these past 49 years. It is an amazing compilation, a testimony to the hard work and reasoned thought of the thousands of scientists and engineers who have made our reports possible. It is a testimony also to the breadth of our activities and to the influence that we have.

The activities in 2007 have been as important as in previous years, if not more so. These are turbulent times in the space program, and there is a continuing need for reasoned advice. Congress, NASA, and other federal agencies call upon us to consider the strategic issues that have been the hallmark of the Board's activities, as well as to provide advice on tactical issues of immediate importance. We endeavor to respond in a timely manner to all requests.

The year ahead and 2009 may be among the most turbulent for the space program. The presidential election and the ensuing transition to a new administration presages fundamental changes to the space program. It is very important that the Space Studies Board maintains its historic level of activity and continues to provide sound advice so that our nation can achieve a space program of unparalleled achievement.

L.A. Fisk
Chair
Space Studies Board

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Space Studies Board Chairs

Lloyd V. Berkner (deceased), Graduate Research Center, Dallas, Texas, 1958–1962

Harry H. Hess (deceased), Princeton University, 1962–1969

Charles H. Townes, University of California at Berkeley, 1970–1973

Richard M. Goody, Harvard University, 1974–1976

A.G.W. Cameron (deceased), Harvard College Observatory, 1977–1981

Thomas M. Donahue (deceased), University of Michigan, 1982–1988

Louis J. Lanzerotti, American Telephone & Telegraph Co., Bell Laboratories, 1989–1994

Claude R. Canizares, Massachusetts Institute of Technology, 1994–2000

John H. McElroy (deceased), University of Texas at Arlington, 2000–2003

Lennard A. Fisk, University of Michigan, 2003–

Space Studies Board Vice Chairs

George A. Paulikas, The Aerospace Corporation (retired), 2003–2006

A. Thomas Young, Lockheed Martin Corporation (retired), 2006–

1
Charter and Organization of the Board

THE ORIGINS OF THE SPACE SCIENCE BOARD

The National Academy of Sciences (NAS) was created in 1863 by an Act of Congress, signed by President Abraham Lincoln, to provide scientific and technical advice to the government of the United States. Over the years, the breadth of the institution has expanded, leading to the establishment of the National Academy of Engineering (NAE) in 1964 and the Institute of Medicine (IOM) in 1970. The National Research Council (NRC), the operational arm of the National Academies, was founded in 1916. The NAS, NAE, IOM, and NRC are collectively referred to as "The National Academies." More information is available at http://nationalacademies.org.

The original charter of the Space Science Board was established in June 1958, three months before the National Aeronautics and Space Administration (NASA) opened its doors. The Space Science Board and its successor, the Space Studies Board (SSB), have provided expert external and independent scientific and programmatic advice to NASA on a continuous basis from NASA's inception until the present. The Board has also provided such advice to other executive branch agencies, including the National Oceanic and Atmospheric Administration (NOAA), the National Science Foundation (NSF), the U.S. Geological Survey, and the Department of Defense, as well as to Congress.

The fundamental charter of the Board today remains that defined by National Academy of Sciences' president Detlev W. Bronk in a letter to Lloyd V. Berkner, first chair of the Board, on June 26, 1958, which established the SSB:

> We have talked of the main task of the Board in three parts—the immediate program, the long-range program, and the international aspects of both. In all three we shall look to the Board to be the focus of the interests and responsibilities of the Academy-Research Council in space science; to establish necessary relationships with civilian science and with governmental science activities, particularly the proposed new space agency, the National Science Foundation, and the Advanced Research Projects Agency; to represent the Academy-Research Council complex in our international relations in this field on behalf of American science and scientists; to seek ways to stimulate needed research; to promote necessary coordination of scientific effort; and to provide such advice and recommendations to appropriate individuals and agencies with regard to space science as may in the Board's judgment be desirable.
>
> As we have already agreed, the Board is intended to be an advisory, consultative, correlating, evaluating body and not an operating agency in the field of space science. It should avoid responsibility as a Board for the conduct of any programs of space research and for the formulation of budgets relative thereto. Advice to agencies properly responsible for these matters, on the other hand, would be within its purview to provide.

The Space Science Board changed its name to the Space Studies Board in 1989 to reflect its expanded scope, which now includes space applications and other topics. Today, the Space Studies Board exists to provide an independent, authoritative forum for information and advice on all aspects of space science and applications, and it serves as the focal point within the National Academies for activities on space research. It oversees advisory studies and program assessments, facilitates international research coordination, and promotes communications on space science and science policy among the research community, the federal government, and the interested public. The SSB also serves as the U.S. National Committee for the Committee on Space Research (COSPAR) of the International Council for Science (ICSU).

THE SPACE STUDIES BOARD TODAY

The Space Studies Board is a unit of the NRC's Division on Engineering and Physical Sciences (DEPS). DEPS is one of six major program units of the NRC through which the institution conducts its operations on behalf of NAS, NAE, and IOM. Within DEPS there are a total of 13 boards that cover a broad range of physical science and engineering disciplines and mission areas.

Members of the DEPS Committee on Engineering and Physical Sciences (DEPSCOM) provide advice on Board membership and on proposed new projects to be undertaken by ad hoc study committees formed under the SSB's auspices. Every 3 years, DEPSCOM reviews the overall operations of each of the DEPS Boards. The next review of the SSB will take place in 2010.

The "Space Studies Board" encompasses the Board itself, its standing committees (see Chapter 2), and ad hoc study committees (see Chapter 3), and its staff. The Board is composed of prominent scientists, engineers, industrialists, scholars, and policy experts in space research appointed for 2-year staggered terms. They represent seven space research disciplines: space-based astrophysics, heliophysics (also referred to as solar and space physics), Earth science, solar system exploration, microgravity life and physical sciences, space systems and technology, and science and technology policy. In 2007, there were 23 Board members. The chairs of the SSB's standing committees are members of the Board, and of its Executive Committee. The chair of the NRC's Aeronautics and Space Engineering Board (ASEB) and the U.S. representative to COSPAR are ex officio members. A standing liaison arrangement also has been established with the European Space Science Committee (ESSC), part of the European Science Foundation, and the NRC's Ocean Studies Board.

Organization

The organization of the SSB in 2007 is illustrated in Figure 1.1. Taken together, the Board and its standing and ad hoc study committees generally hold as many as 40 meetings during the year.

Major Functions of the Space Studies Board

The Board provides an independent, authoritative forum for information and advice on all aspects of space science and applications and serves as the focal point within the National Academies for activities on space research. The Board itself does not conduct studies, but it oversees advisory studies and program assessments conducted by ad hoc study committees (see Chapter 3) formed in response to a request from a sponsor. All projects proposed to be conducted by ad hoc study committees under the auspices of the SSB must be reviewed and approved by the chair and vice-chair of the Board (as well as other NRC officials).

Decadal surveys are a signature product of the Board, providing strategic direction to NASA, NOAA, and other agencies on the top priorities over the next 10 years in astronomy and astrophysics, solar system exploration, solar and space physics, and Earth science. (The astronomy and astrophysics decadal survey is a joint effort with the NRC's Board on Physics and Astronomy.)

The Board serves as a communications bridge on space research and science policy among the scientific research community, the federal government, and the interested public.

The Board ordinarily meets three times per year (March, June, and November) to review the activities of its committees and to be briefed on and discuss major space policy issues. The November Board meeting typically involves a workshop on a topic of current interest and results in a workshop report. In 2007, in collaboration with the Aeronautics and Space Engineering Board, that topic was U.S. Civil Space Policy (see Chapter 4). The goal of the workshop was not to develop definitive answers to such questions but to air a range of views and perspectives that will serve to inform public discussion of U.S. space policy. The workshop discussions will be summarized in an NRC report to be issued in early 2008.

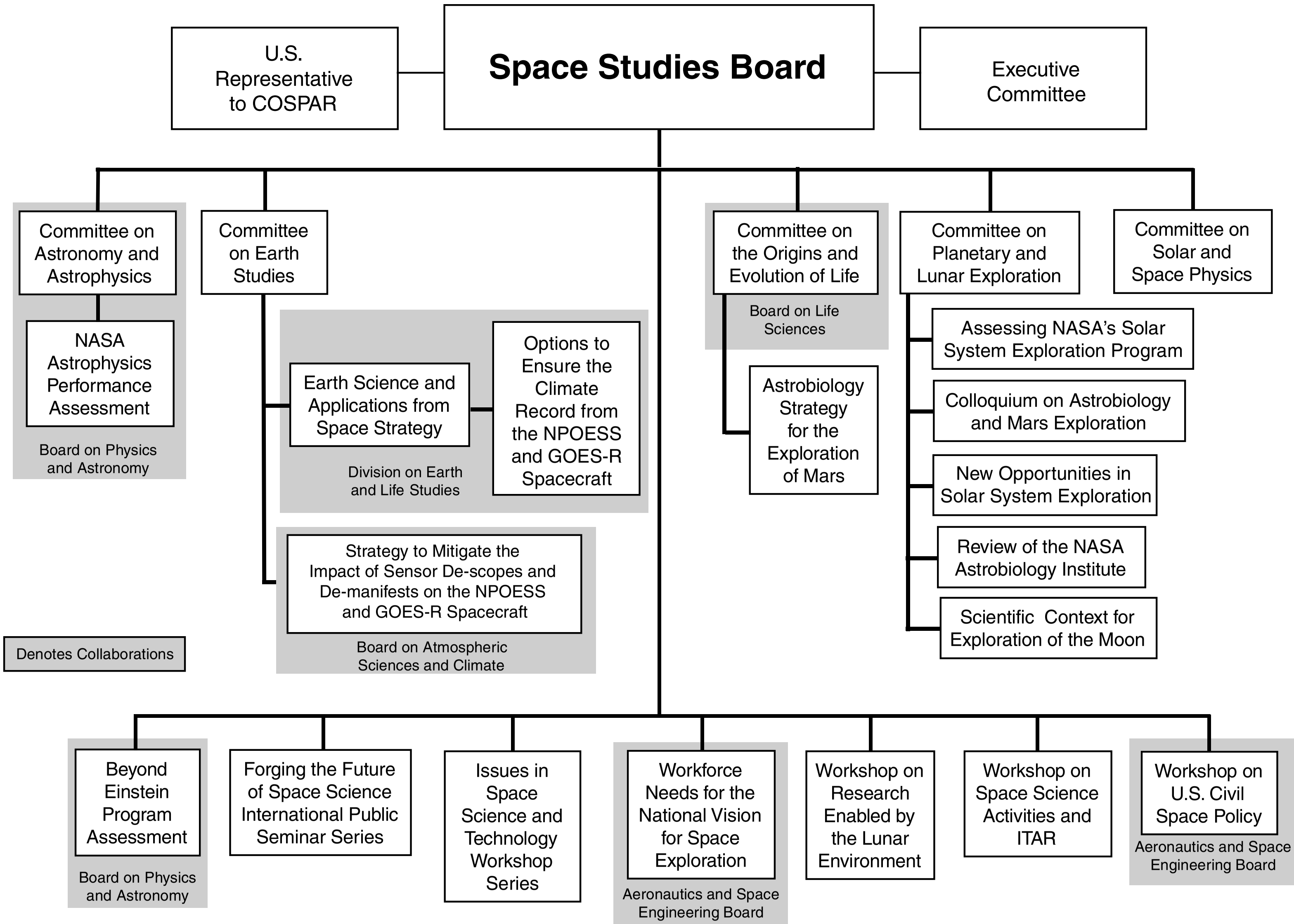

FIGURE 1.1 Organization of the Space Studies Board, its standing committees, ad hoc study committees, workshops, and special projects in 2007. Shaded boxes denote activities performed in cooperation with other National Research Council units.

International Representation and Cooperation

The Board serves as the U.S. National Committee for COSPAR, an international, multidisciplinary forum for exchanging space science research. Board members may individually participate in COSPAR scientific sessions to present their research or, occasionally, present the results of an SSB report to the international community, or conduct informal information exchange sessions with national entities within COSPAR scientific assemblies.

The Board also has a regular practice of exchanging observers with the European Space Science Committee, which is part of the European Science Foundation (see http://www.esf.org/).

Space Studies Board Committees

Executive Committee

The Executive Committee (XCOM), composed entirely of Board members, facilitates the conduct of the Board's business, permits the Board to move rapidly to lay the groundwork for new study activities, and provides strategic planning advice. XCOM meets annually for a session on the assessment of SSB operations and future planning. Its membership includes the chair and vice-chair of the Board, the chairs of the standing committees, and one Board member for each discipline that does not have a standing committee.

Standing Committees

Discipline-based standing committees are the means by which the Board conducts its oversight of specific space research disciplines. Each standing committee is composed of about a dozen specialists, appointed to represent the broad sweep of research areas within the discipline. Like the Board itself, each standing committee serves as a communications bridge with its associated research community and participates in identifying new projects and prospective members of ad hoc study committees. Standing committees do not, themselves, write reports, but oversee reports written by ad hoc study committees created under their auspices.

At the beginning of 2007, SSB had five standing committees:

- Committee on Astronomy and Astrophysics (CAA)
- Committee on Earth Studies (CES)
- Committee on the Origins and Evolution of Life (COEL)
- Committee on Planetary and Lunar Exploration (COMPLEX)
- Committee on Solar and Space Physics (CSSP)

Ad Hoc Study Committees

Ad hoc study committees are created by NRC action to conduct specific studies at the request of sponsors. These committees typically produce NRC reports that provide advice to the government and therefore are governed by Section 15 of the Federal Advisory Committee Act (FACA). Ad hoc study committees usually write their reports after holding two or three information-gathering meetings, although in some cases they may hold a workshop in addition to or instead of information gathering meetings.

In other cases, workshops are organized by ad hoc study committees that serve as organizers only, and the workshop report is written by a rapporteur and does not contain findings or recommendations. In those cases, the study committee is not governed by FACA Section 15 since no NRC advice results from the workshop.

The ad hoc study committees that were in place during 2007 are summarized in Chapter 3.

COLLABORATION WITH OTHER NATIONAL RESEARCH COUNCIL UNITS

Much of the work of the Board involves topics that fall entirely within its principal areas of responsibility and can be addressed readily by its members and committees. However, there are other situations in which the need for breadth of expertise, alternative points of view, or synergy with other NRC projects leads to collaboration with other units of the NRC.

The Space Studies Board has engaged in many such multi-unit collaborations. Among the NRC Boards with which the SSB works most often are the Aeronautics and Space Engineering Board, the Board on Physics and Astronomy, the Board on Atmospheric Sciences and Climate, the Board on Life Sciences, and the Ocean Studies Board. This approach to projects has the potential to bring more of the full capability of the National Academies to bear in preparing advice for the federal government and the public. Multi-unit collaborative projects also present new challenges—namely, to manage the projects in a way that achieves economies of scale and true synergy rather than just adding cost or complexity. Collaborative relationships between the SSB and other NRC units during 2007 are illustrated in Figure 1.1.

ASSURING THE QUALITY OF SSB REPORTS

A major contributor to the quality of the Space Studies Board reports (Table 1.1 lists the 2007 releases) is the requirement that NRC reports are peer-reviewed. Except for the *Space Studies Board Annual Report—2006,* all of the reports were subjected to extensive peer review, which is overseen by the NRC's Report Review Committee (RRC). Typically 4 to 7 reviewers (occasionally as many as 15 or more) are selected on the basis of recommendations by NAS and NAE section liaisons, SSB members, and staff. The reviewers are subject to approval by the NRC. The identities of external reviewers are not known to a report's authors until after the review has been completed and the report has been approved by the RRC. The report's authors, with the assistance of SSB staff, must provide some response to every specific comment from every external reviewer. To ensure that appropriate technical revisions are made to the report and that the revised report complies with NRC policy and standards, the response-to-review process is overseen and refereed by an independent arbiter that is knowledgeable about the report's issues. In some cases, there is a second independent arbiter that has a broader perspective on policy issues affecting the National Academies. All of the reviews emphasize the need for scientific and technical clarity and accuracy and for proper substantiation of the findings and recommendations presented in the report. Names of the external reviewers, including the monitor (and coordinator if one was appointed), are published in the final report, but their individual comments are not released.

Another important method to ensure high-quality work derives from the size, breadth, and depth of the cadre of experts who serve on SSB and its committees or participate in other ways in the activities of the Board. Some highlights of the demographics of the SSB in 2007 are presented in Tables 1.2 and 1.3. During 2007, a total of 335 individuals from 99 colleges and universities and 84 other public or private organizations served as formally-appointed members of the Board and its committees. Over 270 individuals participated in SSB activities either as presenters or as invited workshop participants. The report review process is as important as the writing of reports, and during 2007, 68 different external reviewers contributed to critiques of draft reports. Overall, more than 670 individuals from 101 academic institutions, 90 industry or nonprofit organizations, and 36 government agencies or offices participated in SSB activities. That number included 41 members of NAS, NAE, or IOM. Being able to draw on such a broad base of expertise is a unique strength of the NRC advisory process.

SSB AUDIENCE AND SPONSORS

The Space Studies Board's efforts have been relevant to a full range of government audiences in civilian space research—including NASA's Science Mission Directorate (SMD), NASA's Exploration Systems Directorate (ESMD), NASA's Program Analysis and Evaluation Office, the National Science Foundation (NSF), the National Oceanic and Atmospheric Administration (NOAA), the United States Geological Survey (USGS), and the Department of Energy (DOE). Reports on NASA-wide issues were addressed to multiple NASA offices or the whole agency; reports on science issues, to SMD; and reports on exploration systems issues, to ESMD. Within NASA, SMD has been the leading sponsor of SSB reports. Reports have also been sponsored by or of interest to agencies besides NASA—for example, NOAA, NSF, DOE, and the USGS.

TABLE 1.1 Space Studies Board Reports Released in 2007

Report Title	Sponsors	Oversight Committee or Board[a]	Principal Audiences[b] NASA/SMD	NASA/ESMD	NOAA	NSF	Other
Assessment of the NASA Astrobiology Institute	NASA	COMPLEX	X				
An Astrobiology Strategy for the Exploration of Mars	NASA	COEL	X				
Building a Better NASA Workforce: Meeting the Workforce Needs for the National Vision for Space Exploration	NASA	SSB	X				NASA/PA&E
Decadal Science Strategy Surveys: Report of a Workshop	NASA	SSB	X	X	X	X	USGS
Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond	NASA NOAA USGS	CES	X		X	X	USGS DOE Congress
Exploring Organic Environments in the Solar System	NASA	COMPLEX	X	X			
Grading NASA's Solar System Exploration Program: A Midterm Review	NASA	COMPLEX	X				Congress
The Limits of Organic Life in Planetary Systems	NASA	COMPLEX	X				
NASA's Beyond Einstein Program: An Architecture for Implementation	NASA DOE	SSB	X				DOE OSTP Congress
Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft: A Workshop Report	NASA NOAA	CES	X		X	X	USGS
A Performance Assessment of NASA's Astrophysics Program	NASA	CAA	X				Congress
Portals to the Universe: The NASA Astronomy Science Centers	NASA	SSB	X				
The Scientific Context for Exploration of the Moon	NASA	SSB	X				
Space Studies Board Annual Report—2006	NASA	SSB	X	X	X	X	DOE USGS

[a]Oversight committee or board within the National Research Council

CAA	Committee on Astronomy and Astrophysics
CES	Committee on Earth Studies
COEL	Committee on the Origins and Evolution of Life
COMPLEX	Committee on Planetary and Lunar Exploration
CSSP	Committee on Solar and Space Physics
SSB	Space Studies Board

[b]Principal audiences: Federal agencies that have funded or shown interest in SSB reports.

DOE	Department of Energy
NASA	National Aeronautics and Space Administration
NASA/ESMD	NASA Exploration Systems Mission Directorate
NASA/PA&E	NASA Program Analysis and Evaluation Office
NASA/SMD	NASA Science Mission Directorate
NOAA	National Oceanic and Atmospheric Administration
NSF	National Science Foundation
USGS	United States Geological Survey

TABLE 1.2 Experts Involved in the Space Studies Board and Its Subunits, January 1, 2007, to December 31, 2007

	Number of Board and Committee Members	Number of Institutions or Agencies Represented
Academia	214	98
Government and national facilities[a]	31	22
Private industry	39	30
Nonprofit and other[b]	51	32
Total[c]	335	182

[a]Includes NASA and other U.S. agencies and national facilities (e.g., Los Alamos National Laboratory (LANL), U.S. Geological Survey (USGS), U.S. Department of Health and Human Services (HHS), Smithsonian Institution, Naval Research Laboratory (NRL), NOAA).
[b]Other includes foreign institutions and entities not classified elsewhere.
[c]Includes 41 NAS, NAE, IOM members.

TABLE 1.3 Summary of Participation in Space Studies Board Activities, January 1, 2007, to December 31, 2007

	Academia	Government and National Facilities[a]	Private Industry	Nonprofit and Others	Total Individuals
Board/committee members	214	31	39	51	335
Guest experts	37	65	6	39	147
Reviewers	55	2	5	6	68
Workshop participants	41	43	23	20	127
Total	347	141	73	116	677

NOTE: Counts of individuals are subject to an uncertainty of ±3 due to possible miscategorization.

[a]Includes government agencies and national facilities (e.g., National Optical Astronomy Observatory (NOAO), LANL, National Radio Astronomy Observatory, Space Telescope Science Institute, Applied Physics Laboratory, Lawrence Berkeley National Laboratory, Naval Research Laboratory).

Total number of NAS, NAE, and/or IOM members	41
Total number of non-U.S. participants	19
Total number of countries represented, including United States	9
Total number of participants by gender	358(M); 93(F)
Total number of different institutions represented	
Academia	101
Government and national facilities	36
Industry	46
Nonprofit and other	44

U.S. government agencies represented: NASA, NOAA, National Science Foundation, NIST, USGS, Environmental Protection Agency (EPA), Office of Science and Technology Policy, Office of Management and Budget, Smithsonian Institution, U.S. Congress.

SSB OUTREACH AND DISSEMINATION

Enhancing outreach to a variety of interested communities and improving dissemination of Board reports remains a high priority for the SSB. In 2007, the SSB continued to distribute its quarterly newsletter by electronic means to subscribers.

The Board teamed with other NRC units (including the Division on Earth and Life Studies, the Board on Physics and Astronomy, the National Academies Press, the Office of News and Public Information, and the Proceedings of the National Academy of Sciences) to take exhibits to national meetings of the American Geophysical Union and the American Astronomical Society. Popular versions of three of the decadal surveys (*Astronomy and Astrophysics in the New Millennium*, *New Frontiers in the Solar System*, and *The Sun to the Earth—and Beyond*) continue to be widely distributed to the science community and the general public. A popular version of the Earth science decadal survey, *Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond,* is being prepared for publication in 2008. Over 2,000 reports were disseminated in addition to the copies distributed to study committee members, the Board, and sponsors.

Formal reports delivered to government sponsors constitute one of the primary products of the work of the SSB, but the dissemination process has a number of other important elements. The Board is always seeking ways to ensure that its work reaches the broadest possible appropriate audience and that it has the largest beneficial impact. Copies of reports are routinely provided to key executive branch officials, members and staffs of relevant congressional committees, and members of other interested NRC and federal advisory bodies. Members of the press are notified about the release of each new report, and the Board maintains a substantial mailing list for distribution of reports to members of the space research community. The SSB publishes summaries of all new reports in its quarterly newsletter. The Board also offers briefings by committee chairs and members or SSB staff to officials in Congress, the executive branch, and scientific societies. Reports are posted on the SSB Web home page at http://www7.nationalacademies.org/ssb and linked to the National Academies Press Web site for reports at http://www.nap.edu.

INTERNSHIP PROGRAM

The SSB has operated a very successful competitive summer internship program since 1992. The general goal of each internship is to provide a promising undergraduate student an opportunity to work in civil space research policy in the nation's capital, under the aegis of the National Academies. Interns work with the Board, its committees, and staff on one or more of the advisory projects currently underway. Other interns, paid or unpaid, also join the Board staff on an ad hoc basis.

As part of its celebration of the 50th anniversary of its founding, SSB expanded the scope of the Space Policy Intern program in the Fall of 2007 by initiating the Lloyd V. Berkner Space Policy Internships. Dr. Berkner, the Board's first chair, played an instrumental role in creating and promoting the International Geophysical Year (IGY), a global effort that made it possible for scientists from around the world to coordinate observations of various geophysical phenomena.

For intern opportunities at the SSB, and a list of past SSB interns, visit the SSB Web site at http://www7.nationalacademies.org/ssb/Berkner_Space_Policy_Internships.html.

2
Board and Standing Committees: Activities and Membership

During 2007, the Space Studies Board (SSB) had five standing committees representing various disciplines: the Committee on Astronomy and Astrophysics (jointly with the Board on Physics and Astronomy), the Committee on Earth Studies, the Committee on the Origins and Evolution of Life (jointly with the Board on Life Sciences), the Committee on Planetary and Lunar Exploration, and the Committee on Solar and Space Physics. The Board and its standing committees provide strategic direction and oversee activities of ad hoc study committees (see Chapter 3), interact with sponsors, and serve as a communications conduit between the government and the scientific community. They do not provide formal advice and recommendations, and therefore are not subject to the Federal Advisory Committee Act (FACA), Sec. 15.

SPACE STUDIES BOARD

HIGHLIGHTS OF SPACE STUDIES BOARD ACTIVITIES

First Quarter

The Space Studies Board held its 152nd meeting at the National Academies' Main Building in Washington, D.C., on March 5-7, 2007. The meeting time was devoted to reviewing the status of selected ongoing SSB studies, planning near-term consultations with government officials regarding potential future studies and planning the next SSB meeting. One major topic for discussion was the administration's FY2008 budget proposal. Guest speakers included Colleen Hartman, NASA Science Mission Directorate; Carl Walz, NASA Exploration Systems Mission Directorate; Mary Kicza, NOAA-NESDIS; Wayne van Citters and Robert Robinson, National Science Foundation; Robie Samanta Roy, Office of Science and Technology Policy; Paul Shawcross and Amy Kaminski, Office of Management and Budget; and three congressional staff—Jeff Bingham, Senate Commerce Committee; Dick Obermann, House Science and Technology Subcommittee on Space and Aeronautics; and Ed Feddeman, House Science and Technology Subcommittee on Space and Aeronautics.

Special guest, S. Alan Stern, Executive Director, Space Science and Engineering Division, Southwest Research Institute, joined us via teleconference and provided his views as the incoming NASA Associate Administrator for Science.

Finally, NRC staff officer Tim Meyer, Board on Physics and Astronomy, provided an illuminating, and very enjoyable presentation on his experience teaching science policy to high school students.

Second Quarter

The Space Studies Board held its 153rd meeting at the Jet Propulsion Laboratory (JPL) in Pasadena, CA, on June 27-28, 2007. The first day of the meeting was devoted to an overview of JPL and its projects, including a tour of the ATHLETE lunar robot laboratory and the Mars Science Laboratory projects among others. The second day focused on sounding rockets, launch vehicle options, small satellites, and independent cost estimating. The Board was also given updates from ad hoc committee chairs and staff on completed, ongoing, and new projects, and the SSB seminar series Forging the Future of Space Science. Guest speakers included JPL Director Charles Elachi and a number of JPL experts. The Board also heard from Phil Eberspeaker, Chief, NASA Sounding Rockets Program Office; Bill Wrobel, Assistant Associate Administrator for Launch Services at NASA Headquarters; Pete Worden, Director of NASA's Ames Research Center; Joe Hamaker, Senior Cost Analyst at SAIC; and David Bearden, Principal Director for NASA programs at the Aerospace Corporation.

Third Quarter

The Space Studies Board did not meet during this quarter; however, the SSB executive committee (XCOM) did meet on August 20-22, 2007, at the J. Erik Jonsson Woods Hole Center in Woods Hole, MA, for its annual strategic planning session. The XCOM received a visit from Dr. Alan Stern, Associate Administrator for Science at NASA. Dr. Stern shared his thoughts and received input from the XCOM members on several topics, including PI Mission modes, the Mars program, Earth science missions, a restructured Navigator Program, and the Lunar Science program and community.

In addition to the discussion with Dr. Stern, the XCOM spoke with congressional staff from the Senate Commerce, Science and Transportation Committee and the House Committee on Science and Technology on the outlook from Capitol Hill.

The committee continued general discussion on the roles and operations of the Board and its standing committees, ad hoc committees, the financial status of the Board, the NRC efforts to streamline internal processes, and planning for the November SSB meeting and Space Policy Workshop.

Fourth Quarter

The Space Studies Board held a half-day meeting at the Arnold and Mabel Beckman Center in Irvine, California, on November 29, 2007. The Board meeting was followed by a one and a half day Board-sponsored workshop on U.S. civil space policy, discussed in Chapter 4.

The Board chair and vice-chair reported on discussions held at the Board's Executive Committee meeting in August 2007. Board members were presented with the status of several SSB activities, including a study entitled "Critical Issues in U.S. Space Policy" which is being funded by the National Academies; the ongoing seminar series "Forging the Future of Space Science—The Next 50 Years" (discussed in Chapter 4); and the James A. Van Allen Lectureship, which will be presented on June 26, 2008. The annual balance and composition discussion was also held.

The Board ended the meeting with a brief discussion of the objectives for the Workshop on U.S. Civil Space Policy.

SPACE STUDIES BOARD MEMBERSHIP

July 1, 2006–June 30, 2007

Lennard A. Fisk (chair), University of Michigan
A. Thomas Young (vice chair), Lockheed Martin Corporation (retired)
Spiro K. Antiochos, Naval Research Laboratory
Daniel N. Baker, University of Colorado, Boulder
Steven J. Battel, Battel Engineering
Charles L. Bennett, Johns Hopkins University
Judith A. Curry, Georgia Institute of Technology
Jack D. Farmer, Arizona State University
Jack D. Fellows, University Corporation for Atmospheric Research
Jacqueline N. Hewitt, Massachusetts Institute of Technology
Tamara E. Jernigan, Lawrence Livermore National Laboratory
Klaus Keil, University of Hawaii, Manoa
Berrien Moore III, University of New Hampshire
Kenneth H. Nealson, University of Southern California
Norman P. Neureiter, American Association for the Advancement of Science
Suzanne Oparil, University of Alabama, Birmingham
James A. Pawelczyk, Pennsylvania State University
Ronald F. Probstein, Massachusetts Institute of Technology
Harvey D. Tananbaum, Smithsonian Astrophysical Observatory
Richard H. Truly, National Renewable Energy Laboratory
Joseph F. Veverka, Cornell University
Warren M. Washington, National Center for Atmospheric Research
Gary P. Zank, University of California, Riverside

July 1, 2007–June 30, 2008

Lennard A. Fisk, (chair) University of Michigan
A. Thomas Young, (vice chair) Lockheed Martin Corporation (retired)
Spiro K. Antiochos,* Naval Research Laboratory
Daniel N. Baker, University of Colorado at Boulder
Steven J. Battel, Battel Engineering
Charles L. Bennett, Johns Hopkins University
Elizabeth R. Cantwell, Los Alamos National Laboratory
Alan Dressler, Observatories of the Carnegie Institution
Jack D. Fellows, University Corporation for Atmospheric Research
Fiona A. Harrison, California Institute of Technology
Tamara E. Jernigan, Lawrence Livermore National Laboratory
Klaus Keil, University of Hawaii at Manoa
Molly K. Macauley, Resources for the Future, Inc.
Berrien Moore III, University of New Hampshire
Kenneth H. Nealson, University of Southern California
James A. Pawelczyk, Pennsylvania State University
Soroosh Sorooshian, University of California, Irvine
Richard H. Truly, National Renewable Energy Laboratory
Joan Vernikos, Thirdage LLC
Joseph F. Veverka, Cornell University
Warren M. Washington, National Center for Atmospheric Research
Charles E. Woodward, University of Minnesota, Minneapolis
Gary P. Zank, University of California, Riverside

*Term ended December 31, 2007.

Ex Officio and Liaison Members

Raymond S. Colladay, Lockheed Martin Astronautics (retired) (ex-officio, Chair, NRC Aeronautics and Space Engineering Board)
Jean-Pierre Swings, Institute d'Astrophysique (liaison, Chair of the European Space Science Committee)
Frank E. Muller-Karger, University of South Florida (ex-officio, member of the NRC Ocean Studies Board)
Edward C. Stone, California Institute of Technology (liaison, U.S. representative to COSPAR)

Membership of the 2006 SSB Executive Committee

July 1, 2006–June 30, 2007

Lennard A. Fisk (chair), University of Michigan
A. Thomas Young (vice chair), Lockheed Martin Corporation (retired)
Daniel N. Baker, University of Colorado, Boulder
Charles L. Bennett, Johns Hopkins University
Berrien Moore III, University of New Hampshire
Kenneth H. Nealson, University of Southern California
Suzanne Oparil, University of Alabama, Birmingham
Joseph F. Veverka, Cornell University

July 1, 2007–June 30, 2008

Lennard A. Fisk (chair), University of Michigan
A. Thomas Young (vice chair), Lockheed Martin Corporation (retired)
Daniel N. Baker, University of Colorado, Boulder
Charles L. Bennett, Johns Hopkins University
Molly K. Macauley, Resources for the Future, Inc.
Berrien Moore III, University of New Hampshire
Kenneth H. Nealson, University of Southern California
James A. Pawelczyk, Pennsylvania State University
Joseph F. Veverka, Cornell University

Staff

Marcia S. Smith, Director
Joseph K. Alexander, Senior Program Officer
Arthur A. Charo, Senior Program Officer
Sandra J. Graham, Senior Program Officer and Interim Associate Director (from October)
Robert L. Riemer, [†] Senior Program Officer
David H. Smith, Senior Program Officer
Brian D. Dewhurst,[†] Program Officer
Dwayne A. Day, Program Officer
Victoria Swisher, Research Associate
Barbara S. Akinwole, Information Management Associate
Tanja Pilzak, Administrative Coordinator
Christina O. Shipman, Financial Associate
Catherine A. Gruber, Assistant Editor
Carmela J. Chamberlain, Program Associate
Theresa M. Fisher, Program Associate
Claudette K. Baylor-Fleming, Administrative Assistant (through July)
Rodney N. Howard, Senior Program Assistant
Celeste A. Naylor, Senior Program Assistant

[†]Staff from other NRC Boards who are shared with the SSB.

Consultants

Diana Alexander (from March)
Johannes Loschnigg (from October)
Harvey Meyerson (February through July)
Ian Pryke (from July)

Space Policy Interns

Stephanie Bednarek, Summer
Abigail Fraeman, Summer
Amanda Purcell, Autumn

U.S. NATIONAL COMMITTEE FOR COSPAR

The Committee on Space Research (COSPAR) held several business meetings at its headquarters in Paris during the first quarter. These included the COSPAR Publication Committee (March 18-19), the Scientific Program Committee (March 19-20), the COSPAR Scientific Advisory Committee (March 21), and the COSPAR Bureau (March 22).

The Committee on Space Research (COSPAR) did not meet for the remainder of the 2007. The next COSPAR business meetings will take place in Paris, France on March 25-28, 2008, and the next COSPAR Scientific Assembly will take place in Montreal, Canada, on July 13-20, 2008.

Edward C. Stone, California Institute of Technology (U.S. Representative to COSPAR)
David H. Smith, Senior Program Officer, Space Studies Board (Executive Secretary for COSPAR)
Carmela J. Chamberlain, Program Associate, Space Studies Board

STANDING COMMITTEES

COMMITTEE ON ASTRONOMY AND ASTROPHYSICS

The Committee on Astronomy and Astrophysics (CAA), which operates under the joint auspices of the SSB and the Board on Physics and Astronomy (BPA), did not meet during the first quarter.

At CAA's meeting in Washington, D.C., at the Keck Center on May 22-23, 2007, the committee heard presentations from Jon Morse and Yvonne Pendleton of NASA's Science Mission Directorate on the status of NASA's Astrophysics Program and the associated Individual Investigator grants. The committee also heard from the Department of Energy's (DOE's) High Energy Physics program and the National Science Foundation's (NSF's) Astronomy program. Finally, the committee discussed the SOFIA mission with the program's project manager.

CAA, on a hiatus until the completion of the next astronomy and astrophysics decadal survey, did not meet for the remainder of 2007. BPA held a planning meeting for the decadal survey on April 28, 2007, and has submitted proposals for supporting the study to NASA, NSF, and DOE. The BPA and SSB have also held several joint town hall meetings at meetings of the AAS and APS.

A historical summary of reports from CAA and related committees is presented in Figure 2.1.

Membership

July 1, 2006–June 30, 2007

Charles L. Bennett (co-chair), Johns Hopkins University
C. Megan Urry (co-chair), Yale University
Donald Backer, University of California, Berkeley
Michell C. Begelman, University of Colorado, Boulder
Thomas J. Bogdan, University Corporation for Atmospheric Research
Adam S. Burrows, University of Arizona
Alexei Filippenko, University of California, Berkeley
Timothy M. Heckman, Johns Hopkins University
Lynne Hillenbrand, California Institute of Technology
Charles McGruder III, Western Kentucky University
Stephan S. Meyer, University of Chicago
Scott D. Tremaine, Princeton University
Jean L. Turner, University of California, Los Angeles

July 1, 2007–June 30, 2008

Charles L. Bennett (co-chair), Johns Hopkins University
C. Megan Urry (co-chair),† Yale University
Michell C. Begelman, University of Colorado, Boulder
Adam S. Burrows, University of Arizona
Lynne Hillenbrand, California Institute of Technology
Charles McGruder III, Western Kentucky University

†Term ended December 31, 2007

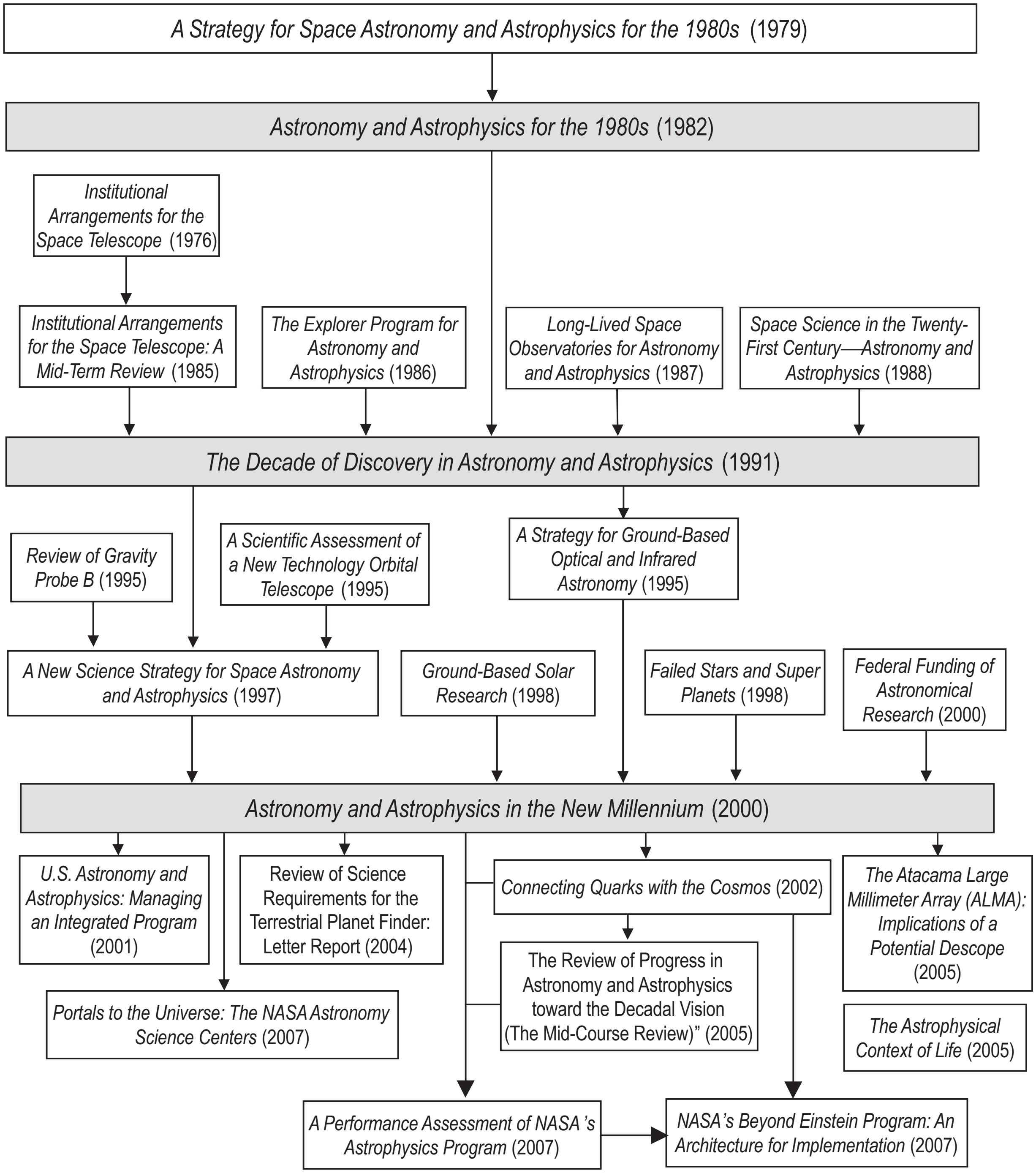

FIGURE 2.1 SSB-NRC advice on astronomy and astrophysics (1979-2007).

Staff

Brian D. Dewhurst, Program Officer, Board on Physics and Astronomy
Celeste A. Naylor, Senior Program Assistant, Space Studies Board

COMMITTEE ON EARTH STUDIES

The Committee on Earth Studies (CES) continued to stand down as work continued on the decadal survey "Earth Science and Applications from Space: A Community Assessment and Strategy for the Future" and the follow-on decadal survey activity—the Panel on Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft and the ad hoc Committee on A Strategy to Mitigate the Impact of Sensor Descopes and Demanifests on the NPOESS and GOES-R Spacecraft. Resumption of committee activities is anticipated in late 2007 or early 2008.

During the fourth quarter committee formation began for the appointment of a new chair, vice chair, and additional committee members. CES will meet late in the first quarter of 2008.

A historical summary of reports from CES and related committees is presented in Figure 2.2.

Staff

Arthur A. Charo, Senior Program Officer, Space Studies Board
Theresa M. Fisher, Program Associate, Space Studies Board

COMMITTEE ON THE ORIGINS AND EVOLUTION OF LIFE

The Committee on the Origins and Evolution of Life (COEL), which operates under the joint auspices of the SSB and the Board on Life Sciences, held its first meeting of 2007 at the National Academies' Keck Center in Washington, D.C., on February 19-20. The majority of the meeting was devoted to presentations and discussions relating to the status of, and future prospects for, astrobiology in light of NASA's budget for FY2007 and the President's proposed budget for FY2008. Following the committee's discussions, a short presentation was drafted for use at the March 5-7 SSB meeting.

During the second quarter, COEL met at the National Academies' Keck Center in Washington, D.C., on May 14-15. The committee continued its discussion from the February meeting on the status of, and future prospects for, astrobiology in light of NASA's budget for FY2007 and the President's proposed budget for FY2008. The committee also discussed several potential study activities including an astrobiology strategy for the exploration of the outer solar system, an assessment of the status of current theories on the origin and early evolution of life, and a review of NASA's astrobiology roadmap.

COEL did not meet during the third quarter. The committee is in the process of appointing seven new members to replace those whose terms have ended

The committee held its final meeting of 2007 at the Arnold and Mabel Beckman Center in Irvine, California, on November 7-9. In addition to briefings on various aspects of NASA's astrobiology programs and science presentations, the committee devoted time to discussing future study activities. The future projects discussed included reviewing and updating the planetary protection requirements for Mars sample-return missions and the drafting of an astrobiology strategy for the exploration of the outer solar system.

A historical summary of reports from COEL and related committees is presented in Figure 2.3.

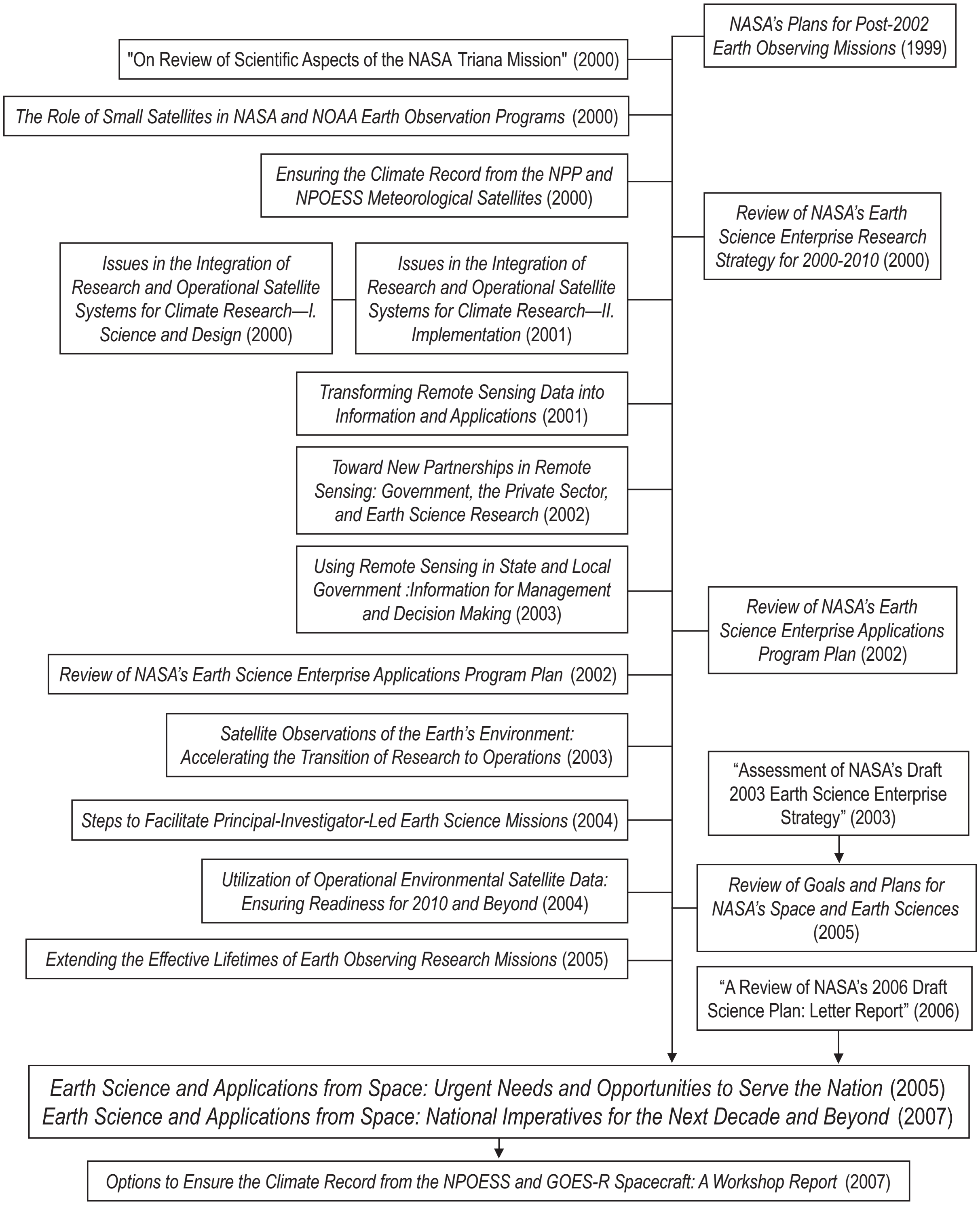

FIGURE 2.2 SSB-NRC advice on Earth science and applications in space (1979-2007).

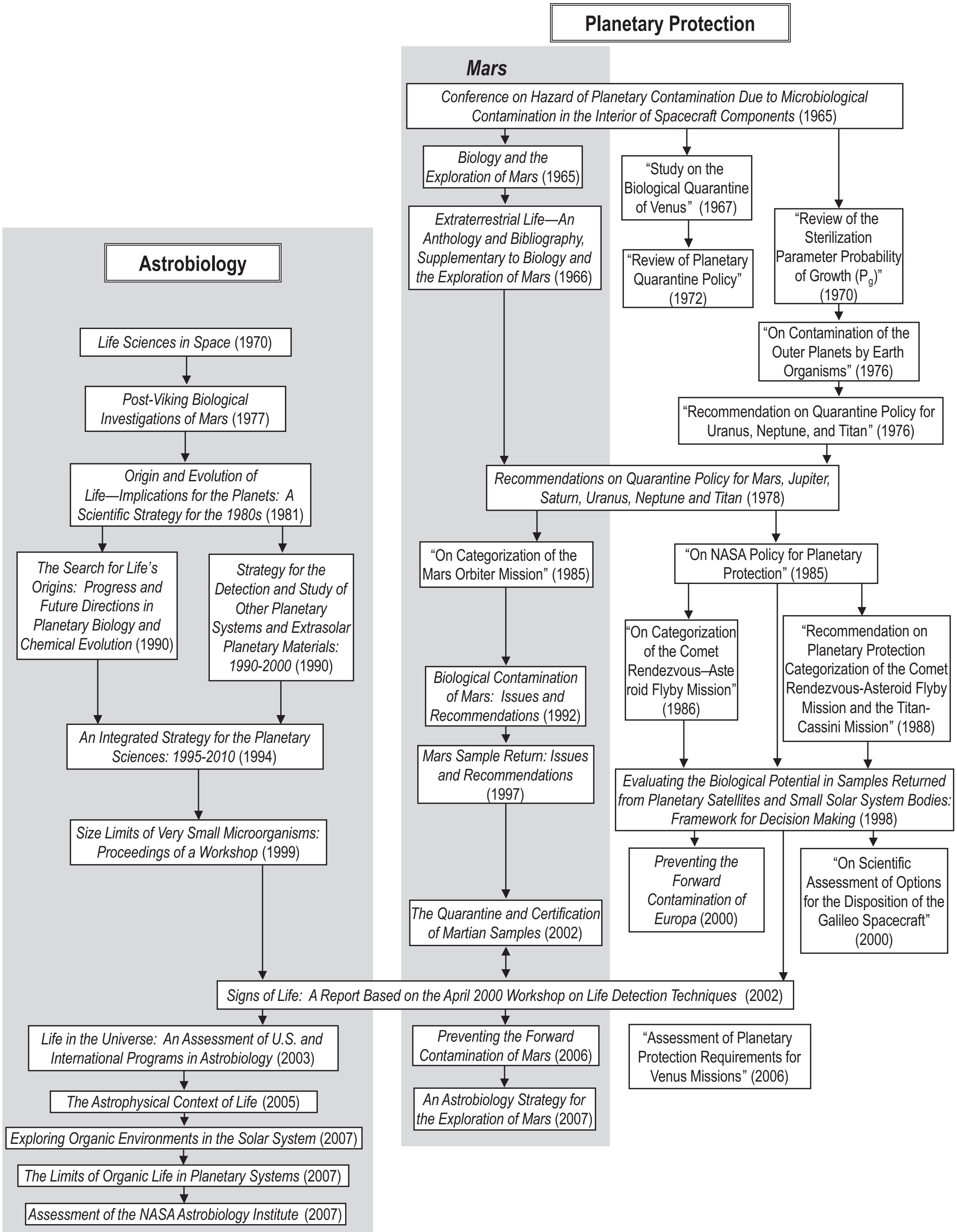

FIGURE 2.3 SSB-NRC advice on astrobiology and planetary protection (1965-2007).

Membership

July 1, 2006–June 30, 2007

Kenneth H. Nealson (co-chair), University of Southern California
Bruce M. Jakosky (co-chair), University of Colorado, Boulder
Jan P. Amend, Washington University
Michael H. Carr, U.S. Geological Survey (retired)
Harry Y. McSween, Jr., University of Tennessee, Knoxville
Barbara Sherwood Lollar, University of Toronto
Andrew Steele, Carnegie Institution of Washington
Meenakshi Wadhwa, Arizona State University

July 1, 2007–June 30, 2008

Kenneth H. Nealson (co-chair), University of Southern California
Bruce M. Jakosky (co-chair), University of Colorado, Boulder
Jan P. Amend, Washington University
Stanley M. Awramik, University of California, Santa Barbara
Michael H. Carr, U.S. Geological Survey (retired)
Paul G. Falkowski, Rutgers, The State University of New Jersey, New Brunswick
Antonio Lazcano, Universidad Nacional Autonoma de Mexico
Ralph D. Lorenz, Johns Hopkins University, Applied Physics Laboratory
Harry Y. McSween, Jr., University of Tennessee, Knoxville
John C. Priscu, Montana State University
Sara Seager, Massachusetts Institute of Technology
Barbara Sherwood Lollar, University of Toronto
Everett Shock, Arizona State University
Andrew Steele, Carnegie Institution of Washington
Meenakshi Wadhwa, Arizona State University

Staff

David H. Smith, Senior Program Officer, Space Studies Board
Robert L. Riemer, Senior Program Officer, Board on Physics and Astronomy
Rodney N. Howard, Senior Program Assistant, Space Studies Board

COMMITTEE ON PLANETARY AND LUNAR EXPLORATION

The Committee on Planetary and Lunar Exploration (COMPLEX) met at the National Academies' Keck Center in Washington, D.C., on April 11-13. In addition to status reports on NASA's solar system exploration and Mars exploration activities, the bulk of the meeting was devoted to presentations and discussions relating to two topics: (1) studies of primitive solar system bodies, including the results from the Stardust mission, the current status of near-Earth object search programs and future prospects for radar studies; and (2) New Frontiers missions, including a status report on the New Horizons mission to Pluto and the scientific results from the spacecraft's recent flyby of Jupiter, and the managerial and programmatic lessons learned from the New Horizons and Juno missions and the applicability of these lessons to the third planned New Frontiers mission. The committee's discussions were motivated by NASA's recent request that the SSB undertake a study to address several issues relating to the drafting of the Announcement of Opportunity for the third New Frontiers mission.

As a result of the committee's discussions, COMPLEX drafted a list of potential candidates who could serve on the New Frontiers study committee. COMPLEX also discussed the possibility of organizing a meeting of experts for NASA to assess the new results from recent missions to primitive solar system bodies and discuss prospects for future missions.

In addition, the committee heard a science presentation on the results from the high-resolution camera on the Mars Reconnaissance Orbiter and were briefed on the developing plans for the Colloquium on Astrobiology and Mars Exploration held on July 8 in Pasadena, California.

COMPLEX did not meet in the second quarter and during the third quarter met at the J. Erik Jonsson Woods Hole Center in Woods Hole, Massachusetts, on July 23-25, 2007.

At their December 6-8, 2007, meeting at the Arnold and Mabel Beckman Center in Irvine, California, the committee heard updates regarding NASA's Science Mission Directorate, including the Mars Exploration Program. Other presentations included updates on the status of the Arecibo Radar and Comet Surface Sample Return. The committee held a preliminary discussion on the planning needed for the next decadal study.

A historical summary of reports from COMPLEX and related committees is presented in Figure 2.4.

Membership

January–June 2007

Joseph F. Veverka (chair), Cornell University
W. Bruce Banerdt, Jet Propulsion Laboratory
Penelope J. Boston, New Mexico Institute of Mining and Technology
Donald E. Brownlee, University of Washington
Bonnie J. Buratti, Jet Propulsion Laboratory
Roger N. Clark, U.S. Geological Survey
Michael R. Combi, University of Michigan
John Grant, Smithsonian Institution, National Air and Space Museum
Timothy J. McCoy, Smithsonian Institution, National Museum of Natural History
Alfred S. McEwen, University of Arizona
Francis Nimmo, University of California, Santa Cruz
Louise M. Prockter, Johns Hopkins University, Applied Physics Laboratory
Darrell F. Strobel, Johns Hopkins University

July–December 2007

Joseph F. Veverka (chair), Cornell University
W. Bruce Banerdt, Jet Propulsion Laboratory
Penelope J. Boston, New Mexico Institute of Mining and Technology
Donald E. Brownlee, University of Washington
Bonnie J. Buratti, Jet Propulsion Laboratory
Roger N. Clark, U.S. Geological Survey
Michael R. Combi, University of Michigan
John Grant, Smithsonian Institution, National Air and Space Museum
Timothy J. McCoy, Smithsonian Institution, National Museum of Natural History
Alfred S. McEwen, University of Arizona
Francis Nimmo, University of California, Santa Cruz
Louise M. Prockter, Johns Hopkins University, Applied Physics Laboratory
Darrell F. Strobel, Johns Hopkins University
Dawn Y. Sumner,[†] University of California, Davis

[†]Term began November 2.

Staff

Sandra J. Graham, Senior Program Officer (from October)
David H. Smith, Senior Program Officer, Space Studies Board (through September)
Rodney N. Howard, Senior Program Assistant, Space Studies Board (through September)
Celeste A. Naylor, Senior Program Assistant (from October)

COMMITTEE ON SOLAR AND SPACE PHYSICS

The Committee on Solar and Space Physics (CSSP) did not meet during the first quarter; however, members were active in preparation for an April 2-3, 2007, meeting. During this quarter, the committee also prepared a briefing to the SSB on the implications of NASA's FY2008 budget for heliophysics.

Highlights of CSSP's April 2-3, 2007, meeting in Washington, D.C., included briefings from Richard Fisher, NASA HQ, and Barbara Giles, NASA GSFC, on the NASA Heliophysics Program and a detailed look at the state of the NASA sounding rocket program courtesy of Phil Eberspeaker, Sounding Rocket Project Office, NASA WFF, Rob Pfaff, NASA GSFC, and Mary Mellot, NASA HQ. An update on the solar-terrestrial research sponsored by the NSF was provided by Richard Behnke, acting head of the Atmospheric Sciences Division. The committee continues to discuss plans for a workshop on the economic impacts of severe space weather events and was very interested in a presentation by Bill Murtagh from NOAA's Space Environment Center that summarized the results of a space weather customer needs survey. The committee also received an update by Pat Mulligan of NOAA NESDIS on the status of the DSCOVR spacecraft, whose payload includes a solar wind monitor, and the ongoing attempts to secure the requisite commercial and agency support to adapt the spacecraft to an expendable launch vehicle and send it to an orbit at L-1.

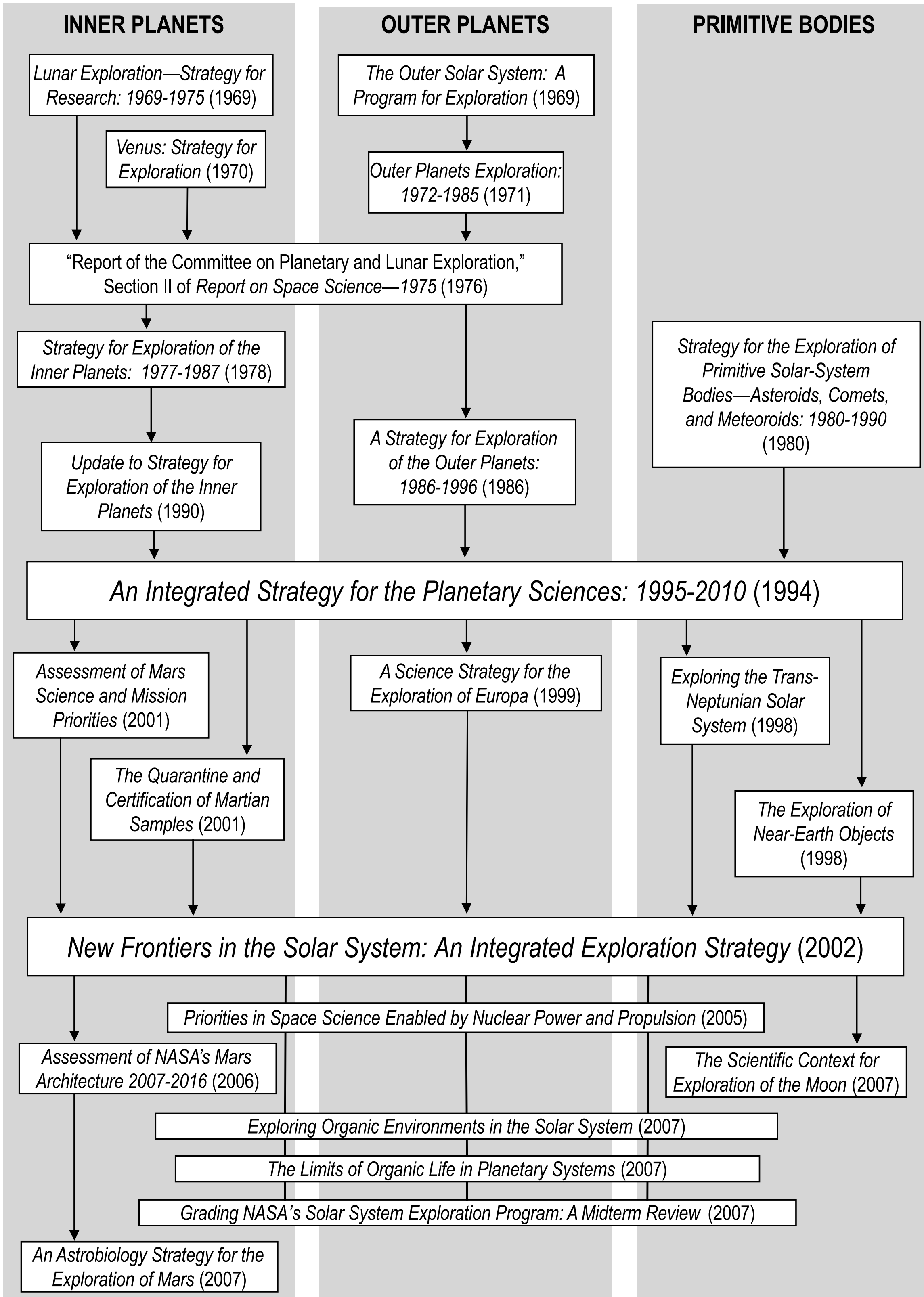

FIGURE 2.4 SSB-NRC advice on solar system exploration (1969-2007). Origins of life topics are covered in Figure 2.3.

During this quarter the committee also prepared a summary of a briefing given to the SSB in March on the implications of NASA's FY2008 budget for heliophysics. Committee chair Dan Baker and CSSP member Spiro Antiochos published a summary of this briefing in the SSB's first quarter newsletter and in the May 1, 2007, edition of the newsletter of the AGU Solar Physics and Aeronomy section. The themes in these summaries were also repeated in testimony by Dr. Baker at a May 2, 2007, hearing, "NASA's Space Science Programs: Fiscal Year 2008 Budget Request and Issues," before the House Committee on Science and Technology, Subcommittee on Space and Aeronautics (see full testimony in Chapter 6).

While the committee did not meet during the third or fourth quarters, staff have been working on reconstituting the committee. The committee's first meeting of 2008 may include discussions about ground-based neutron monitors, guest investigator programs, and new topics such as the general state of health of the NASA Explorer program (particularly in the FY 2008 budget), the NASA R&A budget and the suborbital program. In addition, the committee will discuss support for an ad hoc committee being formed for the congressionally-mandated NASA Heliophysics Performance Assessment.

A historical summary of reports from CSSP and related committees is presented in Figure 2.5.

Membership

July 1, 2006–June 30, 2007

Daniel N. Baker (chair), University of Colorado, Boulder
Joseph F. Fennell, The Aerospace Corporation
Jack R. Jokipii, University of Arizona
Krishan Khurana, University of California, Los Angeles
Paul M. Kintner, Cornell University
William S. Lewis, Southwest Research Institute
Dana W. Longcope, Montana State University
Kristina A. Lynch, Dartmouth College
Richard A. Mewaldt, California Institute of Technology
Howard J. Singer, National Oceanic and Atmospheric Administration
Leonard Strachan, Jr., Harvard-Smithsonian Center for Astrophysics
Niescja Turner, Florida Institute of Technology
Ronald E. Turner, ANSER Corporation
Thomas H. Zurbuchen, University of Michigan

July 1, 2007–June 30, 2008

Daniel N. Baker (chair), University of Colorado, Boulder
Joseph F. Fennell, The Aerospace Corporation
Krishan Khurana, University of California, Los Angeles
Kristina A. Lynch, Dartmouth College
Richard A. Mewaldt, California Institute of Technology
Ronald E. Turner, ANSER Corporation

Staff

Arthur A. Charo, Senior Program Officer, Space Studies Board
Johannes Loschnigg, Consultant, Space Studies Board (from October)
Theresa M. Fisher, Program Associate, Space Studies Board

SPACE RESEARCH DISCIPLINES WITHOUT STANDING COMMITTEE REPRESENTATION

Although there are no longer standing committees representing microgravity research or space biology and medicine, a life and microgravity decadal survey is being planned. A historical summary of NRC-SSB advice in space biology and medicine is presented in Figure 2.6, and a historical summary of NRC-SSB advice microgravity research is presented in Figure 2.7.

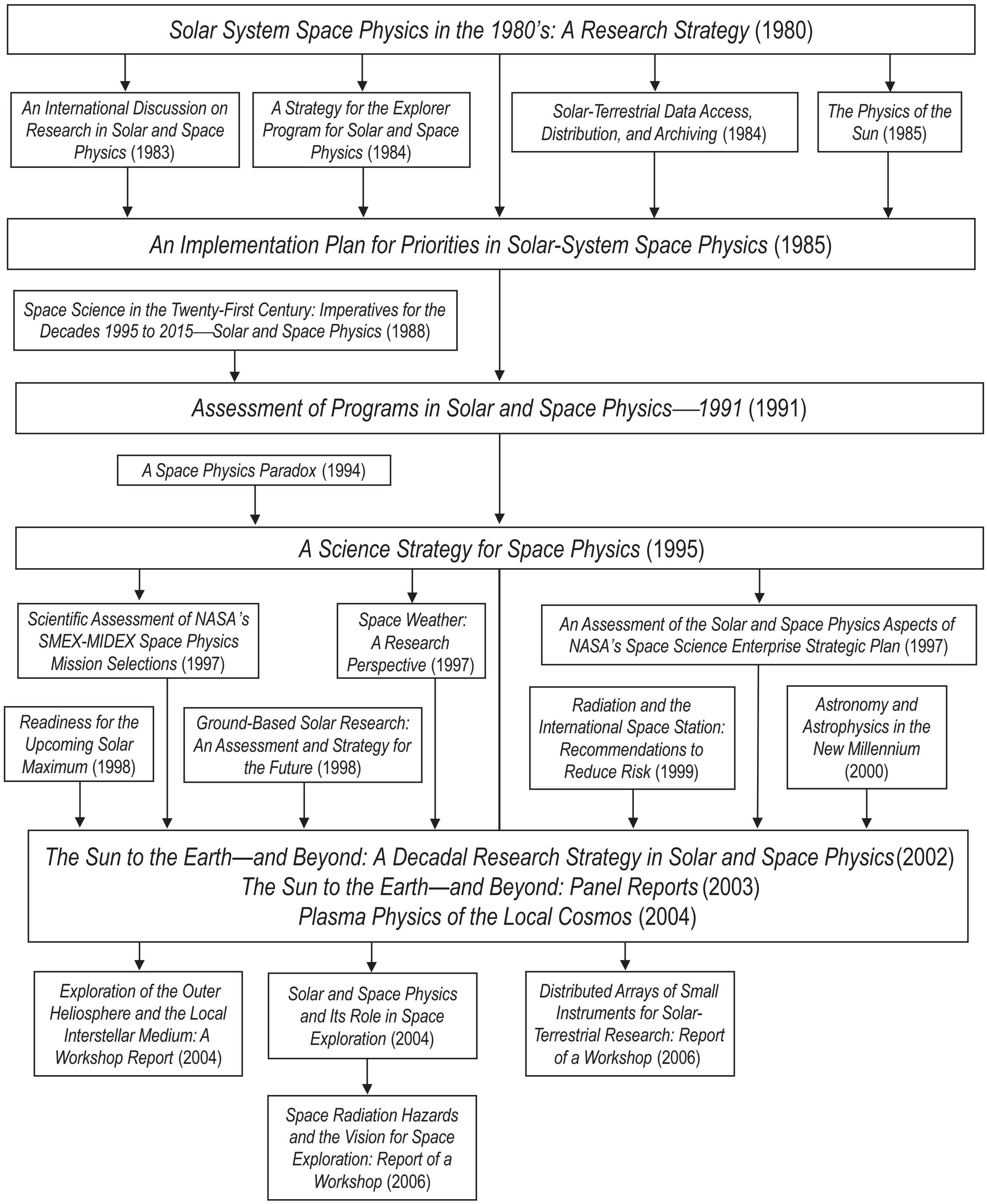

FIGURE 2.5 SSB-NRC advice on solar and space physics (1980-2007).

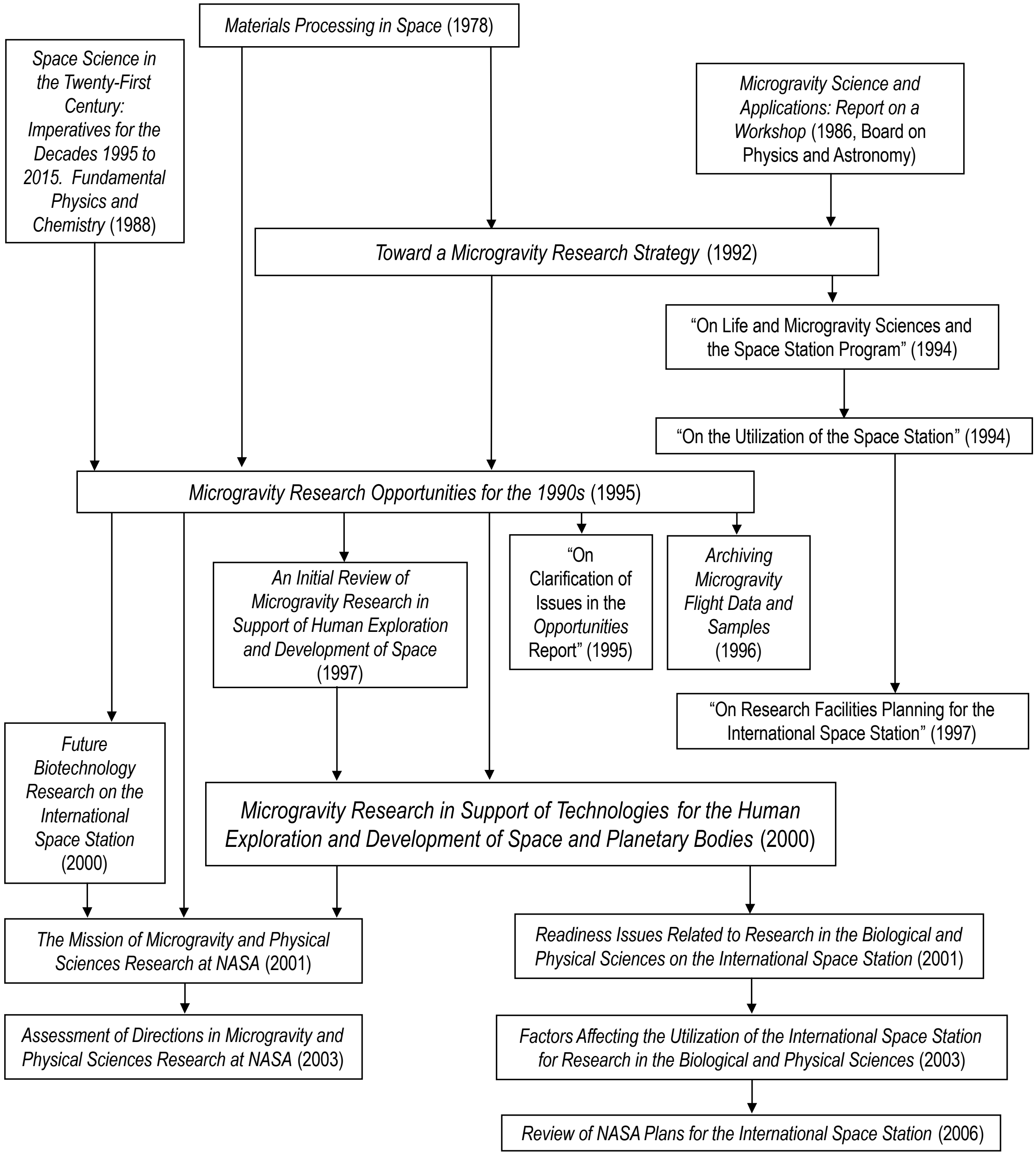

FIGURE 2.6 SSB-NRC advice on microgravity research (1978-2006).

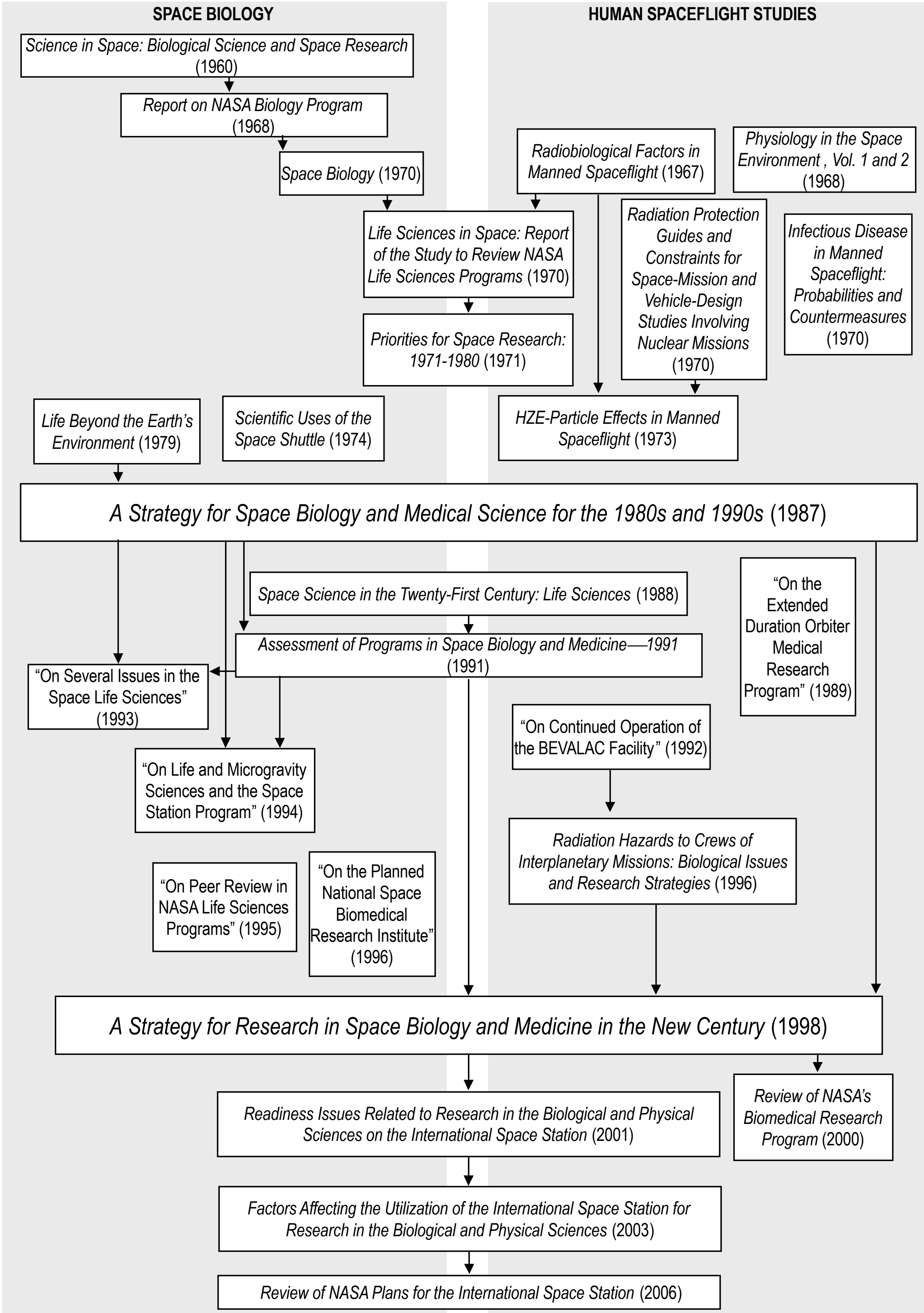

FIGURE 2.7 SSB-NRC advice on space biology and medicine (1960-2006).

3
Ad Hoc Study Committees: Activities and Membership

When a sponsor requests that the Space Studies Board conduct a study, an ad hoc committee is established for that purpose. The committee terminates when the study is completed. These study committees are subject to the Federal Advisory Committee Act (FACA), Section 15, because they provide advice and recommendations to the federal government. The SSB and/or one of its standing committees provide oversight for ad hoc study committee activities. Eleven ad hoc committees were organized, met, or released studies during 2007. (Activities and membership are summarized below.)

In addition, two ad hoc committees that produced reports in 2006 were formally disbanded in 2007: Committee for the Review of the Next Decade Mars Architecture and the Committee on the Review of the NASA Science Mission Directorate Science Plan. Their reports were summarized in the 2006 annual report.

An ad hoc Committee on Critical Issues in U.S. Civil Space Policy is being organized, in collaboration with the Aeronautics and Space Engineering Board, to conduct a study to advise the nation on key goals and critical issues in 21st-century U.S. civil space policy. The committee is expected to begin its work in early 2008 and to complete a report early in the second quarter of 2009.

ASSESSMENT OF SOLAR SYSTEM EXPLORATION

The ad hoc Committee to Assess Solar System Exploration was formed to evaluate NASA's plans and progress to date against the recommendations established in the 2003 solar system exploration decadal survey, *New Frontiers in Solar System Exploration*. The committee held its first two meetings at the National Academies' Keck Center on February 22-24, and March 26-28. The committee's third and final meeting was held on May 7-9, at the Arnold and Mabel Beckman Center in Irvine, CA. At these meetings the committee heard presentations from NASA, former members of the solar system exploration decadal survey and other representatives of the science community.

Several members of the committee made site visits to the Applied Physics Laboratory, Goddard Space Flight Center, Ames Research Center, and the Jet Propulsion Laboratory.

The committee wrote its report during June-August and delivered its final report, *Grading NASA's Solar System Exploration Program: A Midterm Report*, in unedited prepublication format, to NASA November 20, 2007, and to the public on November 28, 2007. The Executive Summary is reprinted in Chapter 5; the full report is available at <http://books.nap.edu/catalog.php?record_id=12070>. The final, edited text will be published in early 2008.

Membership

Wesley T. Huntress, Jr., Carnegie Institution of Washington (co-chair)
Norine E. Noonan, College of Charleston (co-chair)

Sushil K. Atreya, University of Michigan
Carrine Blank, University of Montana
William V. Boynton, University of Arizona
Bernard F. Burke, Massachusetts Institute of Technology
William D. Cochran, University of Texas at Austin
Larry W. Esposito, University of Colorado at Boulder
Mr. G. Scott Hubbard, Stanford University
William M. Jackson, University of California, Davis
Margaret G. Kivelson, NAS University of California, Los Angeles
Ralph McNutt, Johns Hopkins University, Applied Physics Laboratory
William B. Moore, University of California, Los Angeles
Janet L. Siefert, Rice University
Spencer R. Titley, University of Arizona

Dwayne A. Day, Senior Program Associate, Space Studies Board (study director)
Celeste A. Naylor, Senior Program Assistant, Space Studies Board

ASTROBIOLOGY STRATEGY FOR THE EXPLORATION OF MARS

The ad hoc Committee on the Astrobiology Strategy for the Exploration of Mars (Mars Astrobiology) completed all of its meetings in 2006. The committee's report, *An Astrobiology Strategy for the Exploration of Mars*, was released as a prepublication report on May 29. The final, edited report was delivered to NASA headquarters on July 5. The Executive Summary is reprinted in Chapter 5; the full report is available at <http://books.nap.edu/catalog.php?record_id=11937>.

The Associate Administrator for NASA's Science Mission Directorate was briefed on the report's conclusions and recommendations on June 25. As part of the dissemination activities for this report and other recent NRC reports, and, in part, to support NASA's activities related to the martian component of the Vision for Space Exploration, the Space Studies Board convened for NASA a one-day scientific forum, "The Colloquium on Astrobiology and Mars Exploration" on July 8 to discuss selected aspects of the future robotic and human exploration of Mars.

Dissemination activities for the *Astrobiology Strategy* continued at the 7th International Mars Conference on July 9-13 in Pasadena, California, and (under aegis of the Committee on the Origins and Evolution of Life) at the July 16-20 Bioastronomy 2007 Conference in San Juan, Puerto Rico.

Membership

Bruce M. Jakosky, University of Colorado, Boulder (chair)
Jan P. Amend, Washington University in St. Louis
William M. Berelson, University of Southern California
Ruth Blake, Yale University
Susan L. Brantley, Pennsylvania State University
Michael H. Carr, U.S. Geological Survey (retired)
James K. Fredrickson, Pacific Northwest National Laboratory
Anthony D. Keefe, Archemix Corporation
Martin Keller, Oak Ridge National Laboratory
Harry Y. McSween, Jr., University of Tennessee, Knoxville
Kenneth H. Nealson, University of Southern California
Barbara Sherwood-Lollar, University of Toronto
Andrew Steele, Carnegie Institution of Washington
Roger E. Summons, Massachusetts Institute of Technology
Meenakshi Wadhwa, Arizona State University

David H. Smith, Senior Program Officer, Space Studies Board (study director)
Robert L. Riemer, Senior Program Officer, Board on Physics and Astronomy
Rodney N. Howard, Senior Program Assistant, Space Studies Board

ASTRONOMY SCIENCE CENTERS: AN ASSESSMENT OF BEST PRACTICES AND GUIDING PRINCIPLES FOR THE FUTURE

The ad hoc Committee on Astronomy Science Centers was formed to review lessons learned from experience with NASA's ensemble of space astronomy science centers in order to recommend a set of guiding principles and best practices for consideration in making decisions about approaches to meeting the needs of the astronomy community with future science centers.

The committee released its report, *Portals to the Universe: The NASA Astronomy Science Centers,* in June 2007 and briefed NASA on the contents shortly thereafter. The report's Summary is reprinted in Chapter 5; the full report is available at <http://books.nap.edu/catalog.php?record_id=11909>.

Membership

Steven R. Bohlen, Joint Oceanographic Institutions (chair)
Roger G. Barry, University of Colorado, Boulder
Stephen S. Holt, Olin College
Richard A. McCray, University of Colorado, JILA
Alexander S. Szalay, Johns Hopkins University
Paula Szkody, University of Washington
Paul Vanden Bout, National Radio Astronomy Observatory

Brian D. Dewhurst, Senior Program Associate, Board on Physics and Astronomy (study director after January 2007)
Pamela L. Whitney, Senior Program Officer, Space Studies Board (study director through January 2007)
Carmela J. Chamberlain, Program Associate, Space Studies Board

BEYOND EINSTEIN PROGRAM ASSESSMENT

The ad hoc Beyond Einstein Program Assessment Committee was formed to assess the five proposed Beyond Einstein missions (Constellation X-ray Observatory, Laser Interferometer Space Antenna, Joint Dark Energy Mission, Inflation Probe, and Black Hole Finder probe) based on their potential scientific impact and preliminary technology, management plans and cost estimates, and to recommend one mission for first development and launch.

The committee met in Newport Beach, California, on January 30-February 1 to hear detailed technical briefings from each mission proposal team. The committee discussed strategies for developing report conclusions, set writing assignments, and began drafting the report. During this period the committee also held three Town Hall meetings in various locations across the country (Newport Beach, California, Boston, Massachusetts, and Baltimore, Maryland) to gather input on Beyond Einstein opportunities from the scientific community.

The committee held its final town hall meeting in Chicago, Illinois, on April 4-7, and then proceeded to discuss the interplay between ground and space observations in addressing the dark energy question. The committee met again at the National Academies' Keck Center in Washington, D.C. on June 6-8 to discuss its draft report.

The prepublication version of the committee's report, *NASA's Beyond Einstein Program: An Architecture for Implementation,* was delivered to NASA and DOE sponsors on August 31 and released to the public on September 5. The report's Executive Summary is reprinted in Chapter 5; the full report is available at <http://books.nap.edu/catalog.php?record_id=12006>.

The committee briefed the agency sponsors (NASA and DOE) on September 4, and also briefed OSTP, OMB, and interested congressional staff. The committee also held a well-attended public briefing on the results of the study at the National Academies' Keck Center on September 6.

An additional briefing for sponsors was held on November 6 which focused on the methodologies used for the report's cost and technical assessments. A final town hall meeting will be held on January 9, 2008, during the AAS 211th meeting in Austin, Texas, to discuss the report findings with the astronomy and astrophysics community.

Membership

Charles F. Kennel, University of California, San Diego (co-chair)
Joseph H. Rothenberg, Universal Space Network (co-chair)
Eric G. Adelberger, University of Washington
William B. Adkins, Adkins Strategies, LLC
Thomas Appelquist, Yale University
James S. Barrowman, Independent Consultant
David A. Bearden, The Aerospace Corporation
Mark Devlin, University of Pennsylvania
Joseph Fuller, Jr., Futron Corporation
Karl Gebhardt, University of Texas, Austin
William C. Gibson, Southwest Research Institute
Fiona A. Harrison, California Institute of Technology
Andrew J. Lankford, University of California, Irvine
Dennis McCarthy, Independent Consultant
Stephan S. Meyer, University of Chicago
Joel R. Primack, University of California, Santa Cruz
Lisa J. Randall, Harvard University
Craig L. Sarazin, University of Virginia
James S. Ulvestad, National Radio Astronomy Observatory
Clifford M. Will, Washington University
Michael S. Witherell, University of California, Santa Barbara
Edward L. Wright, University of California, Los Angeles

Brian D. Dewhurst, Senior Program Associate, Board on Physics and Astronomy (co-study director)
Sandra J. Graham, Senior Program Officer, Space Studies Board (co-study director after January 2007)
Pamela L. Whitney, Senior Program Officer, Space Studies Board (co-study director through January 2007)
Victoria Swisher, Research Associate, Space Studies Board
Carmela J. Chamberlain, Program Associate, Space Studies Board
Celeste A. Naylor, Senior Program Assistant, Space Studies Board

EARTH SCIENCE AND APPLICATIONS FROM SPACE: A COMMUNITY ASSESSMENT AND STRATEGY FOR THE FUTURE

The ad hoc Committee on Earth Science and Applications from Space: A Community Assessment and Strategy for the Future (ESAS) was formed to conduct a decadal survey to generate consensus recommendations from the Earth and environmental science and applications communities regarding a systems approach to space-based and ancillary observations that encompasses the research programs of NASA; the related operational programs of NOAA; and associated programs such as Landsat, a joint initiative of USGS and NASA. The survey committee oversaw and synthesized the work of seven thematically organized study panels.

A pre-publication version of *Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond* was delivered to the study sponsors (NASA, NOAA, and the USGS) on January 5. Public release of the report and a press conference occurred on January 15 in conjunction with a special session at the annual meeting of the American Meteorological Society held in San Antonio, Texas. The report received extensive coverage by the print media, including major national newspapers. The report was also the subject of editorials in the *New York Times*, *Washington Post*, *Miami Herald*, and other newspapers.

Drs. Anthes and Moore and the study director, Art Charo, briefed the report to NASA, NOAA, the Office of Science and Technology Policy (OSTP), the Office of Management and Budget (OMB), and staff of the House Science and Technology Committee and the Senate Committee on Commerce, Space, and Transportation. Together or individually the co-chairs also testified about the report's findings at hearings before the House Science and Technology Committee; the Senate Committee on Commerce, Space, and Transportation; and the Subcommittee on Space and Aeronautics of the Senate Committee on Commerce, Space, and Transportation (see Chapter 6).

During the second quarter of 2007, the decadal survey continued to be an item of keen interest in the Congress and at OMB and OSTP. Richard Anthes, co-chair of decadal survey steering committee, and Eric Barron, chair of the survey's Panel on Climate Variability and Change, testified at a June 28, 2007, House Science Committee hearing, "NASA's Earth Science and Applications Programs: Fiscal Year 2008 Budget Request and Issues" (reprinted in Chapter 6).

The final, edited version of the decadal survey report was delivered to the study sponsors (NASA, NOAA, and the USGS) on October 3, 2007. The Executive Summary is reprinted in Chapter 5; the full report is available at <http://www.nap.edu/catalog.php?record_id=11820>.

A popularization of the report is also being prepared; it will be published in 2008.

ESAS Executive Committee Membership

Richard A. Anthes, University Corporation for Atmospheric Research (co-chair)
Berrien Moore III, University of New Hampshire (co-chair)
James G. Anderson, Harvard University
Susan K. Avery, University of Colorado, Boulder
Eric J. Barron, University of Texas, Austin
Susan L. Cutter, University of South Carolina
Ruth DeFries, University of Maryland
William B. Gail, Microsoft Virtual Earth
Bradford H. Hager, Massachusetts Institute of Technology
Anthony Hollingsworth,* European Centre for Medium-Range Weather Forecasts (retired)
Anthony C. Janetos, Joint Global Change Research Institute, Pacific Northwest National Laboratory/University of Maryland
Kathryn A. Kelly, University of Washington
Neal F. Lane, Rice University
Dennis P. Lettenmaier, University of Washington
Bruce D. Marcus, TRW Inc. (retired)
Warren M. Washington, National Center for Atmospheric Research
Mark L. Wilson, University of Michigan
Mary Lou Zoback, Risk Management Solutions

Stacey W. Boland, Jet Propulsion Laboratory (consultant)

Arthur A. Charo, Senior Program Officer, Space Studies Board (study director)
Theresa M. Fisher, Program Associate, Space Studies Board

*The committee notes with deep regret Anthony Hollingsworth's death on July 29, 2007.

ESAS Panel on Earth Science Applications and Societal Benefits Membership

Anthony C. Janetos, Joint Global Change Research Institute, Pacific Northwest National Laboratory/University of Maryland (chair)
Roberta Balstad, Columbia University (vice chair)
Jay Apt, Carnegie Mellon University
Philip E. Ardanuy, Raytheon Information Solutions
Randall Friedl, Jet Propulsion Laboratory
Michael F. Goodchild, University of California, Santa Barbara
Molly K. Macauley, Resources for the Future, Inc.
Gordon McBean, University of Western Ontario
David L. Skole, Michigan State University
Leigh Welling, Crown of the Continent Learning Center

Thomas J. Wilbanks, Oak Ridge National Laboratory
Gary W. Yohe, Wesleyan University

Arthur A. Charo, Senior Program Officer, Space Studies Board (study director)
Theresa M. Fisher, Program Associate, Space Studies Board

ESAS Panel on Land-use Change, Ecosystem Dynamics, and Biodiversity Membership

Ruth S. Defries, University of Maryland (chair)
Otis B. Brown, Jr., University of Miami (vice chair)
Mark R. Abbott, Oregon State University
Christopher B. Field, Carnegie Institution of Washington
Inez Y. Fung, University of California, Berkeley
Marc Levy, Center for International Earth Sciences Information Network
James J. McCarthy, Harvard University
Jerry M. Melillo, Marine Biological Laboratory
David S. Schimel, University Corporation for Atmospheric Research

Arthur Charo, Senior Program Officer, Space Studies Board (study director)
Dan Walker, Senior Program Officer, Ocean Studies Board
Sandra J. Graham, Senior Program Officer, Space Studies Board
Carmela J. Chamberlain, Program Associate, Space Studies Board

ESAS Panel on Weather Science and Applications Membership

Susan K. Avery, University of Colorado, Boulder (chair)
Thomas H. Vonder Haar, Colorado State University (vice chair)
Edward V. Browell, NASA Langley Research Center
William B. Cade III, Air Force Weather Agency
Bradley R. Colman, National Weather Service
Eugenia Kalnay, University of Maryland, College Park
Christopher Ruf, University of Michigan
Carl F. Schueler, Raytheon Company
Jeremy Usher, Weathernews Americas, Inc.
Christopher S. Velden, University of Wisconsin-Madison
Robert A. Weller, Woods Hole Oceanographic Institution

Arthur A. Charo, Senior Program Officer, Space Studies Board (study director)
Curtis Marshall, Program Officer, Board on Atmospheric Sciences and Climate
Theresa M. Fisher, Senior Program Assistant, Space Studies Board

ESAS Panel on Climate Variability and Change Membership

Eric J. Barron, University of Texas, Austin (chair)
Joyce E. Penner, University of Michigan (vice chair)
Gregory Carbone, University of South Carolina
James A. Coakley, Jr., Oregon State University
Sarah T. Gille, Scripps Institution of Oceanography
Kenneth C. Jezek, Ohio State University
Judith L. Lean, Naval Research Laboratory
Gudrun Magnusdottir, University of California, Irvine
Paola Malanotte-Rizzoli, Massachusetts Institute of Technology
Michael Oppenheimer, Princeton University

Claire L. Parkinson, NASA Goddard Space Flight Center
Michael J. Prather, University of California, Irvine
Mark R. Schoeberl, NASA Goddard Space Flight Center
Byron D. Tapley, University of Texas, Austin

Arthur A. Charo, Senior Program Officer, Space Studies Board (study director)
Celeste A. Naylor, Senior Program Assistant, Space Studies Board

ESAS Panel on Water Resources and the Global Hydrologic Cycle Membership

Dennis P. Lettenmaier, University of Washington (chair)
Anne W. Nolin, Oregon State University (vice chair)
Wilfried H. Brutsaert, Cornell University
Anny Cazenave, Centre National d'Etudes Spatiales
Carol Anne Clayson, Florida State University
Jeff Dozier, University of California, Santa Barbara
Dara Entekhabi, Massachusetts Institute of Technology
Richard Forster, University of Utah
Charles D.D. Howard, Independent Consultant
Christian D. Kummerow, Colorado State University
Steven W. Running, University of Montana
Charles J. Vorosmarty, University of New Hampshire

Arthur A. Charo, Senior Program Officer, Space Studies Board (study director)
William Logan, Senior Program Officer, Water Science and Technology Board
Theresa M. Fisher, Program Associate, Space Studies Board

ESAS Panel on Human Health and Security Membership

Mark L. Wilson, University of Michigan (chair)
Rita R. Colwell, University of Maryland, College Park (vice chair)
Daniel G. Brown, University of Michigan
Walter F. Dabberdt, Vaisala, Inc.
William F. Davenhall, ESRI
John R. Delaney, University of Washington
Gregory Glass, Johns Hopkins Bloomberg School of Public Health
Daniel J. Jacob, Harvard University
James H. Maguire, University of Maryland School of Medicine
Paul M. Maughan, MyoSite Diagnostics, Inc.
Joan B. Rose, Michigan State University
Ronald B. Smith, Yale University
Patricia Ann Tester, National Oceanic and Atmospheric Administration

Arthur A. Charo, Senior Program Officer, Space Studies Board (study director)
Raymond Wassel, Senior Program Officer, Board on Environmental Studies and Toxicology
Theresa M. Fisher, Program Associate, Space Studies Board

ESAS Panel on Solid-Earth Hazards, Natural Resources, and Dynamics Membership

Bradford H. Hager, Massachusetts Institute of Technology (chair)
Susan L. Brantley, Pennsylvania State University (vice chair)
Jeremy Bloxham, Harvard University
Richard K. Eisner, State of California, Governor's Office of Emergency Services

Alexander F.H. Goetz, University of Colorado, Boulder
Christian J. Johannsen, Purdue University
James W. Kirchner, University of California, Berkeley
William I. Rose, Michigan Technological University
Haresh C. Shah, Stanford University
Dirk Smit, Shell Exploration and Production Technology Company
Howard A. Zebker, Stanford University
Maria T. Zuber, Massachusetts Institute of Technology

Arthur A. Charo, Senior Program Officer, Space Studies Board (study director)
Dan Walker, Senior Program Officer, Ocean Studies Board
Sandra J. Graham, Senior Program Officer, Space Studies Board
Carmela J. Chamberlain, Program Associate, Space Studies Board

MEETING THE WORKFORCE NEEDS FOR THE NATIONAL VISION FOR SPACE EXPLORATION

The Committee on Meeting the Workforce Needs for the National Vision for Space Exploration, under the auspices of SSB and the Aeronautics and Space Engineering Board, was organized to assess the current and future supply of personnel for a qualified U.S. aerospace workforce to meet the needs of NASA and the larger aerospace science and engineering community in the context of the nation's long-term space exploration vision.

The committee's report, *Building a Better NASA Workforce: Meeting the Workforce Needs of the National Vision for Space Exploration,* was delivered to NASA in April. The Executive Summary is reprinted in Chapter 5; the full report is available at <http://books.nap.edu/catalog.php?record_id=11916>.

Committee chairs Daniel Hastings and David Black testified before the House Science and Technology Committee on May 15 in Washington, D.C. (reprinted in Chapter 6). Following the testimony, the chairs briefed OMB and OSTP.

Membership

David C. Black, Universities Space Research Association (co-chair)
Daniel E. Hastings, Massachusetts Institute of Technology (co-chair)
Burt S. Barnow, Johns Hopkins University
John W. Douglass, Aerospace Industries Association of America, Inc.
Ray M. Haynes, Northrop Grumman Space Technology
Margaret G. Kivelson, University of California, Los Angeles
William Pomerantz, X PRIZE Foundation
Joseph H. Rothenberg, Universal Space Network
Kathryn C. Thornton, University of Virginia

Dwayne A. Day, Senior Program Associate, Space Studies Board (study director)
Victoria Swisher, Research Associate, Space Studies Board
Celeste A. Naylor, Senior Program Assistant, Space Studies Board

NASA ASTROPHYSICS PERFORMANCE ASSESSMENT

The ad hoc Committee on NASA Astrophysics Performance Assessment was tasked with assessing NASA's performance in achieving the goals laid out by the 2000 NRC astronomy and astrophysics decadal survey, *Astronomy and Astrophysics in the New Millennium*, as well as in the 2003 NRC report *Connecting Quarks with the Cosmos*.

A pre-publication version of its report, *A Performance Assessment of NASA's Astrophysics Program,* was released on January 23. The committee began briefing the sponsor and others in April. The final, edited version of its report was published in May. The Summary is reprinted in Chapter 5; the full report is available at < http://books.nap.edu/catalog.php?record_id=11828>.

Membership

Kenneth H. Keller, University of Minnesota at Minneapolis (chair)
Martha P. Haynes, Cornell University (vice chair)
Steven J. Battel, Battel Engineering
Charles L. Bennett, Johns Hopkins University
Catherine Cesarsky, European Southern Observatory
Megan Donahue, Michigan State University
Rolf-Peter Kudritzki, University of Hawaii at Manoa
Stephen S. Murray, Harvard-Smithsonian Center for Astrophysics
Robert Palmer, Independent Consultant
Joseph H. Taylor, Jr., Princeton University
Michael S. Turner, University of Chicago
Rainer Weiss, Massachusetts Institute of Technology
Charles E. Woodward, University of Minnesota, Minneapolis

Brian D. Dewhurst, Senior Program Associate, Board on Physics and Astronomy (study director)
David B. Lang, Research Associate, Board on Physics and Astronomy
Celeste A. Naylor, Senior Program Assistant, Space Studies Board

REVIEW NEW OPPORTUNITIES IN SOLAR SYSTEM EXPLORATION

The ad hoc Committee to Review New Opportunities in Solar System Exploration was formed to conduct an analysis of a number of issues that relate to NASA's next New Frontiers Announcement of Opportunity (AO) and provide criteria and guiding principles for determining the list of candidate missions. At the request of the sponsor, NASA's Science Mission Directorate (SMD), the study's statement of task was revised to reflect SMD's new interest in possibly including Mars in the New Frontiers program. In addition to its original requirements, the study will make recommendations about whether or not Mars mission proposals should be considered in the New Frontiers AO, or remain separate, as has been true historically.

In 2007, the committee met on August 6-8 in Washington, D.C., October 1-3 in Irvine, California, and November 14-16 at the Lunar and Planetary Institute in Houston, Texas. The committee's report is expected to be delivered to NASA by March 2008.

Membership

Reta F. Beebe, New Mexico State University (co-chair)
Warren W. Buck, University of Washington (co-chair)
Douglas P. Blanchard, NASA Johnson Space Center (retired)
Robert Braun, Georgia Institute of Technology
Bernard F. Burke, Massachusetts Institute of Technology
Alan Delamere, Ball Aerospace and Technologies Corporation (retired)
Rosaly M. Lopes-Gautier, Jet Propulsion Laboratory
Stephen Mackwell, Lunar and Planetary Institute
Timothy J. McCoy, Smithsonian Institution
Ralph McNutt, Johns Hopkins University, Applied Physics Laboratory
Sandra Pizzarello, Arizona State University
Gerald Schubert, University of California, Los Angeles
Donna L. Shirley, Science Fiction Museum
John Spencer, Southwest Research Institute
Elizabeth P. Turtle, Johns Hopkins University, Applied Physics Laboratory

Dwayne A. Day, Senior Program Associate, Space Studies Board (study director)
Celeste A. Naylor, Senior Program Assistant, Space Studies Board

REVIEW THE NASA ASTROBIOLOGY INSTITUTE

The ad hoc Committee to Review the NASA Astrobiology Institute was formed to conduct a review to evaluate the progress made by the NASA Astrobiology Institute (NAI) in developing the field of astrobiology, both from the perspective of NAI members and that of the larger community of NASA-supported scientists, both within the NASA Astrobiology Program and outside of it.

The committee held a preliminary conference call on July 13 and held the following meetings in 2007: July 25-27, Sunnyvale, California; August 16-18, Washington, D.C.; August 31- September 2, Costa Mesa, California.

An unedited prepublication version of the committee's report, *Assessment of the NASA Astrobiology Institute*, was sent to NASA on November 28 and released to the public on December 10. The final, edited text will be published in the first quarter of 2008. The Executive Summary is reprinted in Chapter 5; the full report is available at <http:\\books.nap.edu/catalog.php?record_id= 12071>.

Membership

John M. Klineberg, Loral Space and Communications, Ltd. (chair)
Luann Becker, University of California, Santa Barbara
Yvonne C. Brill, Independent Consultant
Jack D. Farmer, Arizona State University
Monika E. Kress, San Jose State University
David W. Latham, Harvard-Smithsonian Center for Astrophysics
Antonio Lazcano, Universidad Nacional Autonoma de Mexico
Cindy L. Van Dover, Duke University

David H. Smith, Senior Program Officer, Space Studies Board (study director)
Rodney N. Howard, Senior Program Assistant, Space Studies Board

SCIENTIFIC CONTEXT FOR THE EXPLORATION OF THE MOON

The ad hoc Committee on the Scientific Context for the Exploration of the Moon completed its work in 2007. Earlier in the year, members of the committee conducted outreach activities to engage the lunar science community, including a presentation by Carlé Pieters at the NASA Advisory Council Workshop on Science Associated with the Lunar Exploration Architecture in Tempe, Arizona, on February 27-March 2, 2007 (about a third of the committee was present for the workshop).

The prepublication version of the committee's report, *The Scientific Context for Exploration of the Moon,* was released in early June, and the final, edited version was published in July 2007. The Executive Summary is reprinted in Chapter 5; the full report is available at <http:\\books.nap.edu/catalog.php?record_id=11954>.

Membership

George A. Paulikas, The Aerospace Corporation (retired) (chair)
Carlé M. Pieters, Brown University (vice chair)
William B. Banerdt, Jet Propulsion Laboratory
James L. Burch, Southwest Research Institute
Andrew Chaikin, Science Journalist, Arlington, Vermont
Barbara A. Cohen, University of New Mexico
Michael Duke,* Colorado School of Mines
Harald Hiesinger, Westfälische Wilhelms-Universität, Münster, Germany
Noel W. Hinners, University of Colorado
Ayanna M. Howard, Georgia Institute of Technology
David J. Lawrence, Los Alamos National Laboratory
Daniel F. Lester, McDonald Observatory
Paul G. Lucey, University of Hawaii

Stefanie Tompkins, Science Applications International Corporation
Francisco P.J. Valero, Scripps Institution of Oceanography
John W. Valley, University of Wisconsin-Madison
Charles D.Walker, Independent Consultant, Annandale, Virginia
Neville J. Woolf, University of Arizona

Robert L. Riemer, Senior Program Officer, Board on Physics and Astronomy (study director)
David H. Smith, Senior Program Officer, Space Studies Board
Rodney N. Howard, Senior Project Assistant, Space Studies Board

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*During committee deliberations, Dr. Duke recused himself from discussion of the finding and recommendation related to the South Pole-Aitken Basin.

STRATEGY TO MITIGATE THE IMPACT OF SENSOR DESCOPES AND DEMANIFESTS ON THE NPOESS AND GOES-R SPACECRAFT

The ad hoc Committee on A Strategy to Mitigate the Impact of Sensor Descopes and De-manifests on the NPOESS and GOES-R Spacecraft was formed shortly before the SSB held a workshop on Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft (summary of the workshop proceedings is available at <http://books.nap.edu/catalog.php?record_id=12033>). NASA and NOAA requested that the NRC form this ad hoc committee to carry out a fast turn-around follow-on study that would (1) prioritize capabilities, especially those related to climate research that were lost or placed at risk following recent changes to NPOESS and the GOES-R series of polar and geostationary environmental monitoring satellites and (2) present strategies to recover these capabilities.

Antonio Busalacchi, chair of both the ad hoc committee and the workshop planning committee, testified on July 11 at a U.S. Senate Committee on Commerce, Science, and Transportation hearing, “U.S. Weather and Environmental Satellites: Ready for the 21st Century?” (his testimony is reprinted in Chapter 6).

A short report from the committee is expected in February 2008.

Membership

Antonio J. Busalacchi, Jr., University of Maryland, College Park (chair)
Philip E. Ardanuy, Raytheon Information Solutions
Judith A. Curry, Georgia Institute of Technology
Craig J. Donlon, Meteorological Office Hadley Centre for Climate Prediction and Research
Judith L. Lean, Naval Research Laboratory
Berrien Moore III, University of New Hampshire
R. Steven Nerem, University of Colorado at Boulder
Anne W. Nolin, Oregon State University
Jay S. Pearlman, The Boeing Company
Joyce E. Penner, University of Michigan
James F.W. Purdom, Colorado State University
Carl F. Schueler, Raytheon Company (retired)
Graeme L. Stephens, Colorado State University
Christopher S. Velden, University of Wisconsin-Madison
Robert A. Weller, Woods Hole Oceanographic Institution
Frank J. Wentz, Remote Sensing Systems

Arthur A. Charo, Senior Program Officer, Space Studies Board (study director)
Theresa M. Fisher, Senior Program Assistant, Space Studies Board

4
Workshops, Symposia, Meetings of Experts, and Other Special Projects

In 2007, 6 workshops, 2 colloquia (one as part of the public seminar series), and 3 public seminar series lectures and panel discussions were convened, and 2 workshop reports were published. (Projects are summarized below.) The planning committees for these projects do not provide advice and, therefore, are not governed by FACA Section 15.

Also in 2007, planning began for an ad hoc committee to organize the Workshop on Societal and Economic Impacts of Severe Space Weather Events.

COLLOQUIUM ON ASTROBIOLOGY AND MARS EXPLORATION

The Space Studies Board convened for NASA a one-day scientific forum, "The Colloquium on Astrobiology and Mars Exploration" on July 8, 2007, to discuss selected aspects of the future robotic and human exploration of Mars, to support NASA's activities related to the martian component of the Vision for Space Exploration, and to disseminate several recent NRC reports, including *An Astrobiology Strategy for the Exploration of Mars, Exploring Organic Environments in the Solar System,* and *The Limits of Organic Life in Planetary Systems*.

David H. Smith, Senior Program Officer, Space Studies Board
Rodney N. Howard, Senior Program Assistant, Space Studies Board

FORGING THE FUTURE OF SPACE SCIENCE: INTERNATIONAL PUBLIC SEMINAR SERIES

The Forging the Future of Space Science international public seminar series commemorates the 50th anniversary of the International Geophysical Year (IGY) and the Space Studies Board, engaging the public and the scientific community in discussions about the advances that have been achieved over the past 50 years in space science, and the discoveries that await us in the next 50 years. In this context, "space science" incorporates space-based astrophysics, heliophysics, Earth science, solar system exploration, and microgravity life and physical sciences.

The seminar series began in September 2007 and includes several regional gatherings featuring afternoon panel discussions and evening lectures, and two all-day colloquia featuring panel discussions and lectures. The events are sponsored by the National Academies, NASA, Boeing, Lockheed Martin, Orbital, Northrop Grumman, Ball Aerospace & Technologies Corporation, ATK, and the Aerospace Corporation. A number of co-sponsors have assisted in publicizing the events, including the American Astronautical Society, the American Astronomical Society, the American Institute of Aeronatics and Astronautics, COSPAR, the International Space University, the National Space Society, and the Planetary Society.

The first three regional events, held in Baltimore, Maryland, Durham, New Hampshire, and Huntsville, Alabama, featured lectures by John Mather of the NASA Goddard Space Flight Center and recipient of the 2006 Nobel Prize in Physics; Ralph J. Cicerone, President of the National Academy of Sciences; and Wesley T. Huntress, Director Emeritus of the Geophysical Laboratory at the Carnegie Institution.

The first all-day colloquium, held on December 1, 2007, included panel discussions on the search for life, dark energy, and robots in space. It also featured lectures by Roger Launius on *Transcendence and Meaning in the First 50 Years of Space Science*, Soroosh Sorooshian on *Global Climate Change: The Latest News from Space*, and Roger Bonnet on *The Evolution of International Cooperation in Space Science.* On the evening preceding the all-day colloquium, an international roundtable on the role of international cooperation in space science, starting with the IGY, was held. The roundtable included Jacques Blamont, Roger Bonnet, Len Fisk, Takashi Kubota, Antonio Lazcano, and Alexander Pavlov.

More information on past and future events, including presentations, webcasts, and podcasts can be found at <http://www7.nationalacademies.org/ssb/IGY_SSB_2007_webcasts_and_presentations.html>.

ISSUES IN SCIENCE AND TECHNOLOGY WORKSHOP SERIES

An ad hoc committee under the auspices of the Space Studies Board will organize a series of public workshops summarizing space and Earth science and technology issues that will be convened to complement Space Studies Board meetings.

The first in the series, "Summary of Space and Earth Science Issues from the Workshop on U.S. Civil Space Policy," took place on November 29-30, 2007, at a joint event with the Aeronautics and Space Engineering Board. The workshop was designed as an opportunity to assess where the Vision for Space Exploration and U.S. civil space policy in general, stands today.

Planning Committee Membership

A. Thomas Young, Lockheed Martin Corporation (retired) (chair)
Daniel N. Baker, University of Colorado
Charles L. Bennett, Johns Hopkins University
Molly Macauley, Resources for the Future, Inc.
Berrien Moore III, University of New Hampshire
Kenneth H. Nealson, University of Southern California
James Pawelczyk, Pennsylvania State University
Joseph F. Veverka, Cornell University
Charles E. Woodward, University of Minnesota

Marcia S. Smith, Director, Space Studies Board
Sandra J. Graham, Senior Program Officer, Space Studies Board
Carmela J. Chamberlain, Program Associate, Space Studies Board

WORKSHOP ON DECADAL SCIENCE STRATEGY SURVEYS

The ad hoc Planning Committee for the Decadal Science Strategy Surveys Workshop organized a workshop that was held on November 14-16, 2006, in Irvine, California, to promote discussion of the use of NRC decadal surveys for developing and implementing scientific priorities, to review lessons learned from the most recent surveys, and to seek to identify potential approaches for future surveys that can enhance their realism, utility, and endurance.

The summary report, *Decadal Science Strategy Surveys: Report of a Workshop*, prepared by SSB member Jack Fellows and SSB staff, was released in April 2007 and published in final form in June. The Summary is reprinted in Chapter 5; the full report is available at <http://books.nap.edu/catalog.php?record_id=11894>.

Planning Committee Membership

Lennard A. Fisk, University of Michigan (chair)
Charles L. Bennett, Johns Hopkins University
Berrien Moore III, University of New Hampshire
Suzanne Oparil, University of Alabama, Birmingham
Joseph F. Veverka, Cornell University
Warren M. Washington, National Center for Atmospheric Research
A. Thomas Young, Lockheed Martin Corporation (retired)

Jack D. Fellows, University Corporation for Atmospheric Research (rapporteur)

Joseph K. Alexander, Senior Program Officer, Space Studies Board
Claudette K. Baylor-Fleming, Administrative Assistant, Space Studies Board

WORKSHOP ON OPTIONS TO ENSURE THE CLIMATE RECORD FROM THE NPOESS AND GOES-R SPACECRAFT

In late 2006, in response to a request by NASA and NOAA for a follow-on activity to the Earth science and applications from space decadal survey, the Panel on Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft was formed. The panel was charged with organizing a workshop and writing a summary report of the workshop. At its April 23-24, 2007, meeting, the panel received extensive briefings related to its charge.

The "Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft" workshop was held on June 19-21, 2007, at the National Academies' Keck Center in Washington, D.C., attracting some 100 scientists and engineers from academia, government, and industry. The workshop gave participants a chance to review and comment on the NASA/NOAA assessments of the climate impacts associated with the instrument cancellations and de-scopes to NPOESS, which occurred following the June 2006 Nunn-McCurdy review, as well as an opportunity to comment on a variety of suggested mitigation scenarios. Presentations from the April 23-24, 2007, meeting and the June 19-21, 2007, workshop were made available at <http://www7.nationalacademies.org/ssb/ SSB_NPOESS2007_Presentations.html>.

The workshop summary report, *Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft: A Workshop Report,* released in unedited prepublication format on October 2, 2007, does not include any panel findings or recommendations. The workshop sponsors requested the NRC perform a study that will provide findings and recommendations (see "Strategy to Mitigate the Impact of Sensor Descopes and Demanifests on the NPOESS and GOES-R Spacecraft" in Chapter 3).

Planning Committee Membership

Antonio J. Busalacchi, Jr., University of Maryland, College Park (chair)
Philip E. Ardanuy, Raytheon Information Solutions
Judith A. Curry, Georgia Institute of Technology
Randall Friedl,* Jet Propulsion Laboratory
Judith L. Lean, Naval Research Laboratory
Berrien Moore III, University of New Hampshire
Jay S. Pearlman, The Boeing Company
James F.W. Purdom, Colorado State University
Christopher S. Velden, University of Wisconsin-Madison
Thomas H. Vonder Haar, Colorado State University
Frank J. Wentz, Remote Sensing Systems

Arthur A. Charo, Senior Program Officer, Space Studies Board
Theresa M. Fisher, Senior Program Assistant, Space Studies Board

*Resigned from committee May 2007.

WORKSHOP ON RESEARCH ENABLED BY THE LUNAR ENVIRONMENT

The ad hoc Planning Committee for the Workshop on Research Enabled by the Lunar Environment was formed to organize a workshop to generate a community exchange of ideas and a discussion of recent research relevant to human space exploration topics. The committee has held two teleconferences to discuss topics for the workshop. The workshop took place on June 14-15, 2007, at the National Academy of Sciences building in Washington, D.C.

The workshop focused on capabilities that NASA will need to develop in order to enable future human exploration of space, with the intent of obtaining a wide range of inputs on the key scientific and technological questions that can be addressed through research during lunar missions. Sixty-five people participated, including 32 invited speakers. Topics included food supply challenges, challenges in power generation and storage, challenges in in-situ resource utilization (ISRU), challenges in human physiology and performance, life support challenges, and challenges associated with lunar surface operations. The organizing committee briefed NASA on the issues raised at the workshop. A workshop website was created as a resource to the community and included presentations from the workshop and relevant background documents.

Planning Committee Membership

Mary Jane Osborn, University of Connecticut Health Center (chair)
Edward D. McCullough, Boeing Phantom Works
Susan C. Doll, Earth Institute at Columbia University
Simon Ostrach, Case Western Reserve University
Jack Knight, Independent Consultant

Sandra Graham, Senior Program Officer, Space Studies Board
Maureen Mellody, Senior Program Officer, Aeronautics and Space Engineering Board
Victoria Swisher, Research Associate, Space Studies Board
Carmela Chamberlain, Program Associate, Space Studies Board

WORKSHOP ON SOCIETAL AND ECONOMIC IMPACTS OF SEVERE SPACE WEATHER EVENTS

The Societal and Economic Impacts of Severe Space Weather Events Workshop will examine the nation's current and future ability to manage the effects of space weather events on a wide range of critical infrastructures, and their resulting societal and economic impacts. An organizational meeting will be held on February 19-21, 2008, in Washington, D.C.

Sandra Graham, Senior Program Officer, Space Studies Board
Theresa M. Fisher, Program Associate, Space Studies Board

WORKSHOP ON SPACE SCIENCE ACTIVITIES AND INTERNATIONAL TRAFFIC IN ARMS REGULATIONS (ITAR)

An ad hoc planning committee organized a Workshop on Space Science Activities and International Traffic in Arms Regulations (ITAR) to initiate a dialogue among State Department regulators and policymakers; university researchers, ITAR officials, and faculty; NASA officials; and other interested parties to explore concerns about ITAR's effect on space science activities.

The workshop was held on September 12-13, 2007, at the National Academy of Sciences building in Washington, D.C. Approximately 70 individuals, including researchers and administrators from universities and national laboratories and representatives of key government agencies, congressional committees, and the relevant policy community, met to explore concerns about ITAR's effect on space science activities. The dialogue helped to delineate where ITAR requirements, or uncertainties about the interpretation of those requirements, are creating problems for space science activities and to identify opportunities for near-term actions to help mediate those problems.

A summary report of the workshop discussions is being prepared for release in the first quarter of 2008.

Planning Committee Membership

Norman P. Neureiter, American Association for the Advancement of Science (chair)
Spence M. (Sam) Armstrong, National Aeronautics and Space Administration (retired)
Daniel N. Baker, University of Colorado at Boulder
Reta F. Beebe, New Mexico State University
Claude R. Canizares, Massachusetts Institute of Technology
John R. Casani, Jet Propulsion Laboratory
Jacqueline N. Hewitt, Massachusetts Institute of Technology

Margaret Finarelli, George Mason University (rapporteur)

Joseph K. Alexander, Senior Program Officer, Space Studies Board
Carmela J. Chamberlain, Program Associate, Space Studies Board
Victoria Swisher, Research Assistant, Space Studies Board
Sandra Wilson, Program Assistant, Space Studies Board

WORKSHOP ON U.S. CIVIL SPACE POLICY

The ad hoc Space Policy Workshop Planning Committee was established under the auspices of the Space Studies Board, in collaboration with the Aeronautics and Space Engineering Board, to organize a public workshop for the purpose of encouraging broad national discussion about future directions of the U.S. civil space program.

The workshop was held on November 29-30, 2007, concurrent with the SSB meeting at the Arnold and Mabel Beckman Center in Irvine, California. Approximately 60 individuals participated—including members of the SSB and ASEB and other experts from academia, industry, not-for-profit organizations, relevant federal agencies, and Congress. The workshop drew upon invited talks, panel discussions, and general discussions to consider aspects of the question "What are the principal purposes, goals, and priorities of U.S. civil space?" The goal of the workshop was not to develop definitive answers to such questions but to air a range of views and perspectives that will serve to inform public discussion of U.S. space policy. The workshop discussions will be summarized in an NRC report to be issued in early 2008.

Planning Committee Membership

Lennard A. Fisk, University of Michigan (chair)
Charles L. Bennett, Johns Hopkins University
Raymond S. Colladay, Lockheed Martin Astronautics (retired)
Berrien Moore III, University of New Hampshire
George A. Paulikas, The Aerospace Corporation (retired)
Warren M. Washington, National Center for Atmospheric Research
A. Thomas Young, Lockheed Martin Corporation (retired)

Molly Macauley, Resources for the Future, Inc. (rapporteur)

Joseph K. Alexander, Senior Program Officer, Space Studies Board
Kerrie Smith, Program Officer, Aeronautics and Space Engineering Board
Victoria Swisher, Research Assistant, Space Studies Board
Carmela J. Chamberlain, Program Associate, Space Studies Board
Sandra Wilson, Program Assistant, Space Studies Board

5
Summaries of Major Reports

This chapter reprints the summaries of reports that were released in 2007 (note that the official publication date may be 2008).

5.1 A Performance Assessment of NASA's Astrophysics Program

A Report of the Ad Hoc NASA Astrophysics Performance Assessment Committee

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

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NOTE: "Summary" reprinted from *A Performance Assessment of NASA's Astrophysics Program,* The National Academies Press, Washington, D.C., 2007, pp. 1-3.

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.

5.2 An Astrobiology Strategy for the Exploration of Mars

A Report of the Ad Hoc Committee on an Astrobiology Strategy for the Exploration of Mars

Executive Summary

From Mars Pathfinder to Mars Express, Mars Reconnaissance Orbiter, and the Mars Exploration Rovers *Spirit* and *Opportunity*, the recent spate of robotic missions to the Red Planet has led to a wealth of new information about the planet's environment, including strong evidence of a watery past and the possible discovery of atmospheric methane. In addition, new developments in our understanding of life in extreme conditions on Earth suggest the possibility of microbial viability in the harsh martian environment. Together, these results have greatly increased interest in the search for life on Mars, both within the scientific community and beyond.

Such scientific interest achieved a new focus on January 14, 2004, when President George W. Bush announced the new Vision for Space Exploration, directing NASA to focus its efforts on robotic and human exploration of space, particularly of the Moon and Mars. Included in the Vision is an explicit directive to "[c]onduct robotic exploration of Mars to search for evidence of life . . . ."

Given the enhanced scientific and political interest in the search for life on Mars, it is surprising that NASA's most recent end-to-end strategy for the detection of martian life, contained in the report *An Exobiological Strategy for Mars Exploration*, was published as long ago as 1995.

Against this backdrop, NASA's Science Mission Directorate requested the Space Studies Board's assistance in developing an up-to-date integrated astrobiology strategy for Mars exploration that brings together all the threads of this diverse topic into a single source for science mission planning. In particular, NASA asked that the strategy developed by the Committee on an Astrobiology Strategy for the Exploration of Mars address the following topics:

- The characteristics of potential targets for Mars exploration particularly suited for elucidating the prebiotic and possibly biotic history of Mars, and methods for identifying these targets;
- A catalog of biosignatures that reflect fundamental and universal characteristics of life (i.e., not limited to an Earth-centric perspective);
- Research activities that would improve exploration methodology and instrumentation capabilities to enhance the chances of astrobiological discovery; and
- Approaches to the exploration of Mars that would maximize the astrobiological science return.

THE SEARCH FOR LIFE ON MARS

Mars is the most logical place to look for life elsewhere in the solar system because it is the most Earth-like of all the other planetary bodies in terms of its geological environment and the availability of liquid water at or near the surface throughout time. Moreover, Mars is the most accessible planetary body other than the lifeless Moon. The finding of evidence for past or present life beyond Earth would have profound philosophical and scientific ramifications, and a finding either that life was present or that it was not would have dramatic implications for the prospects for life elsewhere in the universe.

The search for life on Mars requires a detailed understanding of the nature of life on Earth and how it functions in different environments. The search also requires a very broad understanding of Mars as an integrated planetary system. Such an integrated understanding requires investigation of the following:

- The geological and geophysical evolution of Mars;
- The history of Mars's volatiles and climate;
- The nature of the surface and the subsurface environments;
- The temporal and geographical distribution of liquid water;

NOTE: "Executive Summary" reprinted from *An Astrobiology Strategy for the Exploration of Mars,* The National Academies Press, Washington, D.C., 2007, pp. 1-10.

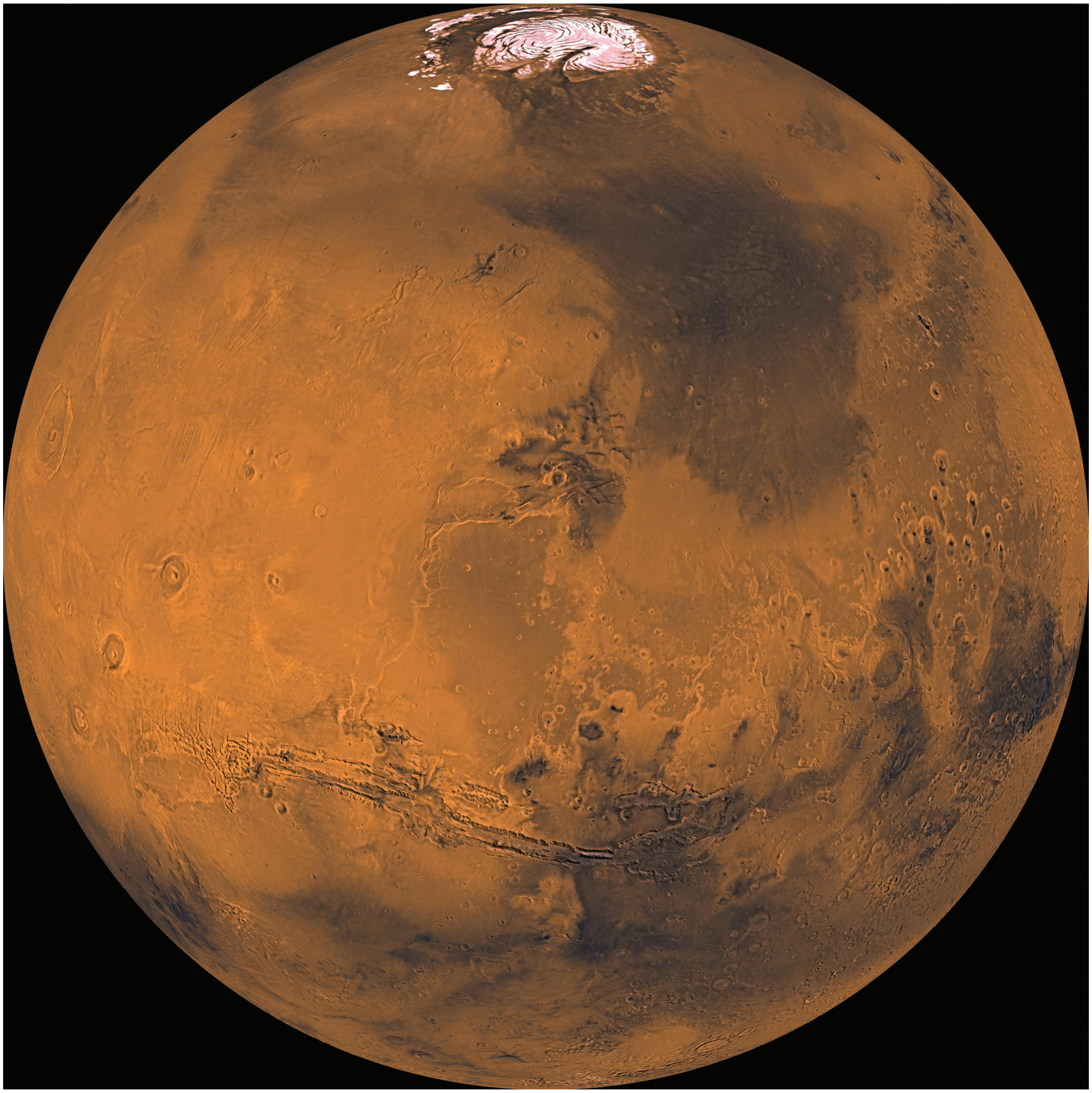

FIGURE ES.1 This global image of Mars was made by piecing together about 1,000 images from the Viking orbiters. This view encompasses the north polar cap (top), the Valles Marineris (lower center), and the Tharsis volcanic region (left). This image and the lead figure in each chapter have been selected to illustrate the geological diversity of the martian environment. SOURCE: Courtesy of NASA/JPL and the U.S. Geological Survey.

- The availability of other resources (e.g., energy) that are necessary to support life; and
- An understanding of the processes that controlled each of the factors listed above.

Although it is not the only possible emphasis for the Mars program, astrobiology provides a scientifically engaging and broad approach that brings together multiple disciplines to address an important set of scientific questions that are also of tremendous interest to the public.

***Finding.* The search for evidence of past or present life, as well as determination of the planetary context that creates habitable environments, is a compelling primary focus for NASA's Mars Exploration Program.**

The astrobiology science goals for the exploration of Mars extend beyond the search for present and past life to encompass an understanding of the geological and environmental context that determines planetary habitability; habitability is defined as a general term referring to the potential of an environment (past or present) to support microbial life of any kind. Such an undertaking entails understanding the geological and geochemical evolution of the planet, its internal structure, and the nature of its interaction with the space environment. Such a broad approach will likely be required to enable astrobiologists to determine which characteristics of martian materials result from nonbiological processes and which result from biological processes and so could be used as biosignatures.

Any comprehensive program focusing on the astrobiological exploration of Mars must be undertaken with the full understanding that the outcome is uncertain. It is entirely possible that surface water did not survive on Mars for a period of time sufficient for an origin of life, or that Mars never had life. Astrobiologists seek to explore Mars to better understand the nature of the planet, to assess its biological potential and habitability, and to determine how far chemical evolution proceeded and whether life was present. A finding of "no life" would be just as important scientifically as a finding of life, in terms of constraining our views of how life originates and spreads and of how widespread life might be in the universe.

NASA's 1995 report *An Exobiological Strategy for Mars Exploration* took the approach of starting from the global perspective and focusing increasingly on the local perspective. This approach involved a series of steps:

1. Global reconnaissance that focused on the history of water and the identification of sites for detailed in situ analysis;
2. In situ analysis at sites that hold promise for understanding the history of water;
3. Deployment of experiments that address astrobiology science questions, including the nature of martian organic molecules and the presence of features indicative of present-day or prior life;
4. Return of martian samples to Earth for detailed study; and
5. Human missions that would provide the detailed geological context for astrobiology measurements and the detection of modern-day "oases" for life.

This reasoned and measured approach provides the best opportunity for determining the geological and geochemical context in which the most useful and appropriate astrobiological measurements can be determined, implemented, and then properly interpreted and understood. It combines a broad, interdisciplinary approach to understanding Mars as a whole with the detailed, focused investigations that allow researchers to understand the astrobiology of Mars.

***Finding.* The search for life and understanding the broad planetary context for martian habitability will require a broad, multidisciplinary approach to Mars exploration.**

***Finding.* At the same time, the astrobiological science goals can best be addressed by an implementation that allows researchers to address increasingly focused questions that relate to astrobiology goals in particular.**

An appropriate strategy for studies of Mars's potential for life is to focus on the elements most relevant to life, especially carbon. This will require a determination of whether organic molecules are present on Mars and where, and of the chemical characteristics that will distinguish between meteoritic (nonbiological), prebiotic, and biological organic molecules. In addition, there is still much to be learned about the history and availability of water, and thus NASA should not abandon its current strategy of "following the water."

***Finding.* The very successful intellectual approach of "follow the water" should be expanded to include "follow the carbon," along with other key biologically relevant elements.**

Any search for possible organic molecules within martian soil or rock samples must be undertaken in such a way as to avoid contamination by similar materials inadvertently transported from Earth. Similarly, life-detection experiments must avoid false-positive results caused by microbes inadvertently transported from Earth. Obtaining the desired science results demands that the contamination issues be addressed by appropriate planetary protection approaches. But it would be a mistake to not develop, because of concerns over planetary protection, appropriate procedures (e.g., new techniques for spacecraft cleaning and bioload reduction) that would allow access to the most promising sites for scientific discovery.

Finding. **The desire to visit and sample the highest-priority astrobiological sites requires that future surface missions to Mars take the necessary and appropriate planetary protection measures.**

Useful science analysis of martian surface samples can be carried out either in situ on the martian surface or in terrestrial laboratories with samples returned to Earth. Although in situ missions have many advantages, sample return offers the opportunity to carry out many more analyses on a sample than can be done in situ, to follow up exciting measurements with additional measurements that had not previously been anticipated, and to make measurements or observations using instruments that are not amenable to being accommodated on a lander or rover mission or that were not available at the time of mission development.

Finding. **The greatest advance in understanding Mars, from both an astrobiology and a more general scientific perspective, will come about from laboratory studies conducted on samples of Mars returned to Earth.**

Current astrobiology science goals for the exploration of Mars can be addressed via a series of robotic spacecraft missions in the near- to mid-term future. It is critical that any astrobiological evidence that might be present on Mars not be compromised by robotic or human activities before definitive measurements or sample return occur.

Finding. **The scientific study of Mars, including return to Earth of astrobiologically valuable samples that can be used to address the questions being asked today, can be done with robotic missions.**

CHARACTERISTICS OF POTENTIAL TARGET SITES

What are the characteristics of potential targets for Mars exploration that are particularly suited for elucidating the prebiotic and possibly biotic (and postbiotic) history of Mars, and what methods can be used to identify these targets? Current understanding of the interplay between organisms and their geological and planetary environment is strongly influenced by terrestrial experience. As such, the most relevant martian environments to investigate and the types of individual materials at those sites that should be studied are those with past or present associations with liquid water and having the potential to retain organic carbon.

Recommendation. **Sites that NASA should target with highest priority to advance astrobiology science objectives are those places where liquid water might exist today or might have existed in the past and where organic carbon might be present or might have been preserved.**

Sites pertinent to present-day or geologically recent water include the following:

- The surface, interior, and margins of the polar caps;
- Cold, warm, or hot springs or underground hydrothermal systems; and
- Source or outflow regions associated with near-surface aquifers that might be responsible for the "gullies" that have been observed.

Sites pertinent to geologically ancient water include the following:

- Source or outflow regions for the catastrophic flood channels;
- Ancient highlands that formed at a time when surface water might have been widespread (e.g., in the Noachian); and
- Deposits of minerals that are associated with surface or subsurface water or with ancient hydrothermal systems or cold, warm, or hot springs.

Although new measurements are likely to include major discoveries relevant to the identification of specific sites of relevance to astrobiology, a foundation of data is already available to identify exciting and appropriate sites for either in situ analysis or sample return.

Finding. **Identification of appropriate landing sites for detailed analysis (whether in situ or by sample return) can be done with the data sets now available or imminently available from currently active missions.**

Many of the types of sites listed above as being important for in situ investigation pertinent to ancient or recent life are not confined to the low latitudes and/or low altitudes accessible with current entry, descent, and landing technologies. Rather, many important sites are at high elevations or at polar latitudes. These include most of the ancient Noachian terrain that would tell researchers about the potential earliest life and polar regions where melting of ice (e.g., at relatively recent epochs of high obliquity) could provide liquid water to sustain life. Past and present Mars landers and rovers have not been able to access such sites because of technological limitations associated with their power supplies (e.g., sites in the polar regions) and entry, descent, and landing systems (e.g., sites at high altitudes). Technical advances are required if the most astrobiologically promising sites are to be accessible to future missions. Additional development of entry, descent, and landing technologies is, for example, especially important to enable landing within a readily traversable distance of a given point on Mars (e.g., to access small, high-value sites—such as hydrothermal vents—or to retrieve cached samples) or if high-mass payloads are to land at technically challenging sites, such as those at high elevation.

Recommendation. **Future surface missions must have the capability to visit most of the martian surface, including Noachian terrains and polar and high-latitude areas, and to access the subsurface.**

Exposure to strongly oxidizing environments or to high fluxes of radiation is not conducive to the preservation of biologically diagnostic carbon compounds. Earth-based experience suggests that the rock types best able to preserve biosignatures include fine-grained sedimentary rocks, evaporites, and hydrothermal deposits.

Recommendation. **Selection of samples for analysis (either in situ or of samples returned from Mars to Earth) should emphasize those having the best chance of retaining biosignatures.**

BIOSIGNATURES

What biosignatures reflect fundamental and universal characteristics of life? Unfortunately, there is no single comprehensive or unique biosignature whose presence would indicate life and whose absence would uniquely indicate the absence of life. Experience from studies of the martian meteorite ALH 84001 and analysis of evidence relating to claims of the earliest life on Earth have demonstrated that the potential interplay between putative organisms and their geological environment is so complex that researchers may never be able to identify a unique biosignature that would work in all environments and at all times. Rather, the sum total of all measurements on a sample, in the context of understanding of the origin and evolution of the martian environment, will be required.

Complicating the committee's task was the specific injunction in the charge that the discussion of biosignatures not be limited by an Earth-centric perspective. As a result, the committee made some specific assumptions about the likely characteristics of martian life forms. These assumptions are as follows:

- They are based on carbon, hydrogen, oxygen, nitrogen, phosphorus, sulfur, and the bio-essential metals.
- They require water.
- They exist as self-contained cell-like entities.
- They have sizes, shapes, and gross metabolic characteristics of organisms found on Earth.
- They employ complex organic molecules in biochemical roles.

The last assumption is particularly important because it implies that martian organisms will produce and use a wide range of small molecules and organic polymers that can serve as chemical biosignatures in their intact or fragmentary states. Experience with studies of terrestrial materials suggests that of all the various life-detection techniques available, analysis of carbon chemistry is the first among equals. In other words, organic analysis is likely to provide a more robust way to detect life than imaging technologies, mineral assemblages, isotopic measurements, or any one other single technique. This is the case because, on Earth, the patterns of biogenic carbon compounds reflect organized polymerization of smaller subunits, or precursors, and comprise mixtures with a limited range of atomic spatial arrangements very different from those made by abiological processes. However, organic analysis alone is insufficient to detect life. An ensemble of all of the relevant methodologies, combined with analysis of geological and environmental plausibility, will likely provide the best evidence for the presence or absence of life in a sample.

Recommendation. **The lack of a comprehensive understanding of all of the potential biosignatures for Mars exploration means that NASA should employ a combination of techniques that utilize both Earth-centric and non-Earth-centric approaches that focus on the basic concepts in carbon chemistry, imaging, mineral assemblages, and isotopic measurements.**

Specific aspects of carbon chemistry that should be investigated include the following:

- The presence of polymers based on repeating universal subunits;
- Patterns in the carbon isotopic compositions of organic molecules that reflect organized polymerization of smaller subunits or precursors;
- Patterns in the carbon numbers of organic compounds; and
- The presence of carbon compounds that have only a subset of the possible connectivities or atomic spatial arrangements (i.e., just a few structural isomers or stereoisomers and/or strong chiral preferences).

EXPLORATION METHODOLOGIES AND INSTRUMENTATION

What research activities would improve exploration methodology and instrumentation capabilities to enhance the chances of astrobiological discovery? Currently operating and planned Mars missions all are, or will be, returning scientific data that directly address astrobiology goals in substantive ways. Thus, if astrobiologists are to advance their science goals for the exploration of Mars, they must work with NASA to ensure that the upcoming missions proceed as scheduled, and then take advantage of the scientific data these spacecraft will collect. Ensuring the success of future missions will require attention to the following activities:

- Technology development,
- Research and analysis activities, and
- Supporting activities such as studies of martian meteorites and Mars-analog environments on Earth.

Future Mars missions will require significant technical advances if they are to be carried out successfully. Technology development must occur both in mission-related areas (e.g., entry, descent, and landing systems, including a precision landing capability; sample-return technology; in situ sample processing and handling; and planetary protection) and in astrobiological science instrumentation development (so that the necessary next generation of instruments is ready to go).

Recommendation. **The Mars Exploration Program must make stronger investments in technology development than it does currently.**

Research and analysis (R&A) programs are the mechanism by which scientific results are extracted from the data returned by current missions and by which concepts for future missions are developed. R&A programs are the primary vehicle by which the Mars Exploration Program can maintain its vitality in response to new discoveries, and such programs represent a vital investment in nurturing the next generation of space scientists, engineers, and program managers.

Recommendation. **Continued strong support of NASA's basic research and snalysis programs is an essential investment in the long-term health of the Mars Exploration Program.**

Analysis of martian meteorites has been central to the development of the current understanding of Mars, its potential for life, and the development of current ideas about detection of present or fossil life. It is especially important to search for martian meteorites that formed during early periods of Mars's history, as well as meteorites of sedimentary origin. This is an important area of collaboration between NASA and the National Science Foundation.

Recommendation. **Collection and analysis of martian meteorites must continue, even though biases in the compositions and ages of these meteorites, their uncertain provenance, and the effects of impact-ejection and transfer to Earth mean that they cannot take the place of samples returned from Mars.**

As with the analysis of martian meteorites, studies of Mars-analog sites on Earth are essential to mission development and execution and to the training of the scientists, engineers, and managers engaged.

Recommendation. **Terrestrial analog studies should include testing instrumentation, developing techniques for measuring biosignatures under Mars-like conditions, and conducting technological proof-of-concept studies.**

MAXIMIZATION OF SCIENCE

What approaches to the exploration of Mars will maximize the astrobiological science return? The astrobiology science goals for Mars are extremely broad, and it can be argued that virtually any mission can return data of relevance to issues relating to the habitability of Mars. However, consideration of the data from past and current Mars missions and expectations for those currently in development for launch during the 2007 and 2009 opportunities suggest that the greatest increase in understanding of Mars will come from the collection and return to Earth of a well-chosen suite of martian surface materials. Given the Mars Exploration Rover experience and current understanding of the nature of materials on the martian surface, a "grab sample" obtained from a stationary lander is not likely to be sufficient to provide the necessary data.

Finding. **Sample return should be seen as a program that NASA and the Mars science community have already embarked upon rather than as a single, highly complex, costly, and risky mission that is to occur at some future time.**

Recommendation. **The highest-priority science objective for Mars exploration must be the analysis of a diverse suite of appropriate samples returned from carefully selected regions on Mars.**

Programmatically, sample return should be phased over three or more launch opportunities. That is, samples can be collected and cached on Mars by one or more missions. A selected cache can be retrieved by a subsequent mission and launched into orbit about Mars for collection and return to Earth at a later date. Sample caching could be carried out by each surface mission, utilizing a minimalist approach so as not to make sample caching a cost- or technology-driver. Such a strategy, accompanied by a reduction in the size of the landing error ellipse, should allow collection of diverse samples and mitigate the costs of sample-return missions.

Irrespective of the compelling scientific arguments for the return of martian samples to Earth, the implementation of a sample-return mission will be a technically challenging, high-risk, high-cost endeavor. Because it will be comparable in expense to the highest-priority activities proposed by other scientific communities, the decision to implement a Mars sample-return mission will hinge on factors beyond the scope of this study. As such, it behooves the astrobiology community to plan for the possibility that a Mars sample-return mission is not an integral component of current mission plans.

Recommendation. **If it is not feasible to proceed directly toward sample return, then a more gradual approach should be implemented that involves sample caching on all surface missions that follow the Mars Science Laboratory, in a way that would prepare for a relatively early return of samples to Earth.**

If a commitment is not made for sample return, then high-priority, astrobiologically relevant science still can be done on Mars with missions such as the Astrobiology Field Laboratory or the Mid Rovers, provided that they are instrumented appropriately. However, it must be recognized that the ability of these missions to make fundamental discoveries is much more limited than would be the case with a sample-return mission.

International collaboration has the potential to make expensive undertakings such as a Mars sample-return mission affordable. But the benefit has to be balanced against the political difficulties of working with multiple countries and multiple space agencies.

Recommendation. **International collaboration in Mars missions should be pursued in order to make expensive missions affordable, especially in the areas of sample caching and sample return.**

5.3 Assessment of the NASA Astrobiology Institute

A Report of the Ad Hoc Committee on the Review of the NASA Astrobiology Institute

Executive Summary

Astrobiology is a scientific discipline devoted to the study of life in the universe—its origin, evolution, distribution, and future. It brings together the physical and biological sciences to address some of the most fundamental questions of the natural world: How do living systems emerge? How do habitable worlds form and how do they evolve? Does life exist on worlds other than Earth? As an endeavor of tremendous breadth and depth, astrobiology requires interdisciplinary investigation in order to be fully appreciated and examined.

As part of a concerted effort to undertake such a challenge, the NASA Astrobiology Institute (NAI) was established in 1998 as an innovative way to develop the field of astrobiology and provide a scientific framework for flight missions. Now that the NAI has been in existence for almost a decade, the time is ripe to assess its achievements.

At the request of NASA's Associate Administrator for the Science Mission Directorate (SMD), the Committee on the Review of the NASA Astrobiology Institute undertook the assignment to determine the progress made by the NAI in developing the field of astrobiology (Appendix A). It must be emphasized that the purpose of this study was not to undertake a review of the scientific accomplishments of NASA's Astrobiology program, in general, or of the NAI, in particular. Rather, the objective of the study is to evaluate the success of the NAI in achieving its stated goals of:

1. Conducting, supporting, and catalyzing collaborative interdisciplinary research;
2. Training the next generation of astrobiology researchers;
3. Providing scientific and technical leadership on astrobiology investigations for current and future space missions;
4. Exploring new approaches, using modern information technology, to conduct interdisciplinary and collaborative research among widely distributed investigators; and
5. Supporting outreach by providing scientific content for use in K-12 education programs, teaching undergraduate classes, and communicating directly with the public.

The committee's assessment of the NAI's progress in these five areas is presented in Chapters 2 to 6, respectively. In evaluating the success of the NAI in achieving these five goals, the committee was requested to address the following considerations:

a. Has the NAI developed, as envisioned, as an evolving experiment in cutting-edge, distributed, collaborative science and education in astrobiology?

b. Does the NAI provide a unique and useful complement to other Astrobiology program support mechanisms (e.g., individual grants to principal investigators), and if improvements need to be made in this area, what are they?

c. Are the research, training, and public educational activities of the NAI appropriately balanced in terms of investments and outcomes, services to NAI members and external partners, and activities that engage and support the wider astrobiology community and the needs of young professionals?

d. What other activities or roles not currently undertaken by the NAI might be appropriate in the future?

The committee's responses to these four criteria can be found in subsections in Chapters 2 to 6. Specific recommendations and suggestions as to how the recommendations might be implemented can be found in the final subsection of the same chapters.

NOTE: "Executive Summary" reprinted from *Assessment of the NASA Astrobiology Institute,* The National Academies Press, Washington, D.C., 2008, pp. 1-4; approved for release in 2007.

Information on the origins of NASA's Astrobiology program and the NAI; a summary of comments on the role, status, and scientific importance of astrobiology from previous NRC reports; and some information on the budgetary history and the impact of recent cuts to the Astrobiology budget can be found in Chapter 1.

FINDINGS AND RECOMMENDATIONS

Overall, the committee is unanimous in finding that the NAI has fulfilled its original mandate. The NAI has played a key role in supporting the development of astrobiology, has positively affected NASA's current and future missions, and should continue to be supported. Specific findings and recommendations are organized according to the five goals and four criteria listed above

NAI Goal 1—Interdisciplinary Research

Although the committee was not charged to undertake a review of the NAI's scientific contributions, it is difficult to evaluate the NAI's success in conducting, supporting, and catalyzing collaborative interdisciplinary research without some brief mention of the NAI's scientific achievements. Consideration of the NAI's major scientific contributions reveals that some are highly interdisciplinary but that some are not. In the committee's view, interdisciplinarity must be viewed as the orientation and emergent quality of an overall enterprise and not as a requirement or expectation levied on every piece of work produced by that enterprise. Thus, with respect to the goal of conducting, supporting, and catalyzing collaborative interdisciplinary research, the committee finds that the NAI has:

- Successfully promoted interdisciplinary science;
- Stimulated many scientific achievements;
- Successfully integrated life sciences into NASA programs;
- Often effectively leveraged ongoing and new research;
- Contributed to the establishment of new astrobiology programs worldwide; and
- Supported programs that are widely distributed throughout the United States.

The committee makes the following recommendations:

- The NAI should improve the accountability of its nodes to the goal of promoting astrobiology as a field of interdisciplinary and collaborative study by instituting better measures of performance and progress;
- The NAI should improve the tracking and critical assessment of its publications; and
- The NAI should encourage and cultivate relationships with non-NAI astrobiology teams and organizations (e.g., the International Society for the Study of the Origin of Life and the Bioastronomy Commission of the International Astronomical Union).

Suggestions as to how these recommendations might be implemented can be found in Chapter 2.

NAI Goal 2—Training the Next Generation of Astrobiologists

NAI's commendable effort to train the next generation of astrobiologists faces many challenges. The continuation of funding beyond the 5-year lifetime of NAI teams is not guaranteed. Young researchers seeking to establish themselves outside the protective environment of NAI teams face particular challenges when trying to accomplish interdisciplinary research within the highly discipline-oriented organization of research universities. The pool of resources for training new researchers is limited. Nevertheless, with respect to the goal of training the next generation of astrobiology researchers, the committee finds that the NAI has:

- Trained graduates who are now employed in academic and other positions;
- Promoted the establishment of programs and new faculty positions in astrobiology at several universities;
- Encouraged continued involvement of scientists in NASA programs; and

- Not been sufficiently proactive in countering the negative effects on training and education programs caused by recent cuts to NASA's Astrobiology budget.

The committee recommends that the NAI should work toward more consistent educational and training opportunities and should ensure stable support of graduate students and postdoctoral researchers in astrobiology. Suggestions as to how this recommendation might be implemented can be found in Chapter 3.

NAI Goal 3—Leadership for Current and Future Space Missions

Although the NAI has not played a significant role in the selection or execution of NASA missions, the field of astrobiology provides the intellectual and scientific foundation for much if not all of NASA's current robotic solar system exploration missions and many of its astrophysical activities relating to the search for and characterization of extrasolar planets. The NAI's influence has been indirect and has come through the actions of individual scientists affiliated with NAI teams. This is probably the most appropriate vehicle for the NAI's involvement in NASA's flight program. Thus, with respect to the goal of providing scientific and technical leadership on astrobiology investigations for current and future space missions, the committee finds that the NAI has:

- Encouraged astrobiologists to provide needed recommendations and expertise to NASA for mission planning and has promoted their participation in the science teams for current and future missions;
- Organized activities, such as focus groups, that have strongly influenced NASA missions; and
- Identified astrobiology questions that underpin most of NASA's current flight programs.

The committee believes that the NAI must remain clearly focused on supporting NASA's spaceflight missions, and so its highest-priority recommendation is as follows: Because its most critical function is to ensure that its portfolio of research activities remains clearly focused on contributing to NASA's current and future spaceflight activities, the NAI should be more proactive in identifying future astrobiology missions and should actively encourage a partnership between astrobiologists and their engineering counterparts to help define future NASA missions.

The committee also recommends that in selecting new nodes, the NAI should give more weight to the potential contribution of the proposed research to future NASA missions.

Suggestions as to how these recommendations might be implemented can be found in Chapter 4.

NAI Goal 4—Use of Information Technology

The NAI experience with information technology has been mixed. Those aspects of the application of information technology within the control of NAI Central—e.g., its extensive and informative Web page with its archive of astrobiology seminars and research results—are second to none in NASA. But those aspects of the utilization of information technology outside the direct control of NAI Central—e.g., the use of collaborative work tools by the researchers affiliated with NAI teams—has been less successful. The lack of success most likely results from social rather than technical factors. Thus, with respect to the goal of exploring new approaches using modern information technology to conduct interdisciplinary and collaborative research among widely distributed investigators, the committee finds that:

- The substantial efforts by NAI Central to improve communications among NAI members have achieved some significant successes; and
- Additional efforts by NAI Central are needed to ensure that new communications tools enhance the effectiveness of interdisciplinary and collaborative research and training.

The committee recommends that the NAI should vigorously pursue the goal of using modern information technologies to increase the effectiveness of the NAI nodes. Suggestions as to how this recommendation might be implemented can be found in Chapter 5.

NAI Goal 5—Education and Outreach

The public's interest in the subject matter of astrobiology has enabled the effective leveraging of funds, partnerships, and expertise far greater in scope than those made available by the NAI itself. Thus, with respect to the goal of supporting outreach by providing scientific content for K-12 education programs, teaching undergraduate classes, and communicating directly with the public, the committee finds that the NAI has:

- Successfully promoted astrobiology as a field with broad-based public appeal;
- Developed effective programs for outreach to the general public; and
- Enabled minority educational activities.

The committee makes the following recommendations:

- The NAI should be more strategic in exploiting synergies in K-12 education, minority education, and teacher training among nodes; and
- The NAI should form a focus group and undertake other necessary actions to address the specifics of teaching astrobiology at the undergraduate level.

Suggestions as to how these recommendations might be implemented can be found in Chapter 6.

5.4 Building a Better NASA Workforce: Meeting the Workforce Needs for the National Vision for Space Exploration

A Report of the Ad Hoc Committee on Meeting the Workforce Needs for the National Vision for Space Exploration

Executive Summary

The Vision for Space Exploration (VSE) announced by President George W. Bush in 2004 sets NASA and the nation on a bold path to return to the Moon and one day put a human on Mars.[1] The long-term endeavor represented by the VSE is, however, subject to the constraints imposed by annual funding. Given that the VSE may take tens of years to implement, a significant issue is whether NASA and the United States will have the workforce needed to achieve that vision. The issues range from short-term concerns about the current workforce's skills for overseeing the development of new spacecraft and launch vehicles for the VSE to long-term issues regarding the training, recruiting, and retaining of scientists and engineers in-house as well as in industry and academia.

Asked to explore science and technology (S&T) workforce needs to achieve the nation's long-term space exploration vision (see Appendix B for the full statement of task), the Committee on Meeting the Workforce Needs for the National Vision for Space Exploration concluded that in the short term, NASA does not possess the requisite in-house personnel with the experience in human spaceflight systems development needed to implement the VSE. But the committee acknowledges that NASA is cognizant of this fact and has taken steps to correct it, primarily by seeking to recruit highly skilled personnel from outside NASA, including persons from industry and retirees.

For the long term, NASA has to ask if it is attracting and developing the talent it will need to execute a mission to return to the Moon, and the agency must identify what it needs to do to attract and develop a world-class workforce to explore other worlds. A major challenge for NASA is reorienting its human spaceflight workforce from the operation of current vehicles to the development of new vehicles at least throughout the next decade, as well as starting operations with new rockets and new spacecraft.

NASA's April 2006 *Workforce Strategy* discussed agency workforce competency trends, identifying an increased need for personnel in five skill areas through 2009.[2] These include 150-200 full-time employees in program/project management, 100-150 in systems engineering and integration engineering, and 200-240 in mission operations—numbers driven primarily by the establishment of the Constellation program.[3] At the time NASA established those requirements, the agency had a total of approximately 1650 persons in these categories involved in other projects. The committee concluded that although many of the employees required in the mission operations category could be transitioned from the Space Shuttle program, many of the 250-350 full-time employees needed in the program/project management and systems engineering and integration engineering competencies are unlikely to currently reside in the agency and will have to be acquired from industry. NASA last had substantial in-house involvement in human spaceflight systems engineering in the 1970s, during the design phase of the Space Shuttle program, and the people skilled in human spaceflight are now likely to exist only in industry.

Although human and robotic systems are distinct, they do share many management and engineering process as well as system technical characteristics. Given that the bulk of the development activities over the past 10 years have been in robotic spacecraft, NASA needs to leverage the robotic spacecraft workforce skill development opportunities to meet some of the human spaceflight program development skill needs. *The committee believes that systems engineering methodology and technical skills acquired from complex robotic spacecraft development can serve as an important base for the transition to systems engineering of human spacecraft.*

NOTE: "Executive Summary" reprinted from *Building a Better NASA Workforce: Meeting the Workforce Needs for the National Vision for Space Exploration,* The National Academies Press, Washington, D.C., 2007, pp. 1-8.

[1]The Vision for Space Exploration initiative was announced by President George W. Bush on January 14, 2004, and is outlined in *The Vision for Space Exploration*, NP-2004-01-334-HQ, NASA, Washington, D.C., 2004.

[2]NASA, *National Aeronautics and Space Administration Workforce Strategy*, April 2006, pp. 15-16, available at http://nasapeople.nasa.gov/HCM/WorkforceStrategy.pdf.

[3]NASA's Constellation program includes development of the Orion spacecraft, the Ares I and Ares V launch vehicles, and the lunar lander required to return Americans to the Moon. NASA has begun initial development of the Orion and the Ares I, with the goal of conducting the first piloted Orion launch by the middle of the next decade.

A problem faced by the committee was a lack of data, as well as differing interpretations of future requirements for certain skills and an absence of information correlating levels of expertise required with the numbers of employees anticipated to be needed. Based on available demographic data, however, the committee concluded that, in the broadest sense, there is no looming national shortage of skilled scientists and engineers to implement the VSE over the long term. For example, the committee saw no evidence of a downturn in the national supply of new talent in aerospace engineering and space sciences. The supply of and the demand for new aerospace workers appear to be relatively well matched at present, as evidenced in part by the fact that salaries in aerospace engineering are not increasing sharply relative to those in other fields.[4] Although the committee acknowledges the difficulties in predicting future demand for particular labor skills, it notes that past predictions of substantially increased demand for aerospace workers have not been borne out in reality. However, the committee also notes that both NASA and the aerospace industry employ engineers from many different fields besides aerospace engineering, such as electrical engineering and mechanical engineering, making a comparison of supply and demand for NASA more complicated than it would at first appear.

Much of the workforce on which NASA has historically relied, and will continue to rely, exists outside the agency: Most of the engineers who work on NASA projects are in industry, and most of the scientists are at universities. Retaining the proper numbers of each component of the broader aerospace workforce poses fundamentally different challenges. For example, engineers in industry generally have to rely less on NASA for the long-term financial stability of their projects than do scientists in universities, who often cannot move to military or commercial projects if NASA's support for their work ends. Science talent might be lost and not readily regained even a few years later if NASA withdraws support for scientific research even for a limited period and scientists leave the field or are unwilling to return to NASA-sponsored work for which funding has proven unstable in the past. Thus, the policies that NASA pursues to sustain the workforce outside the agency will, and must, be different for scientists and engineers.

Furthermore, because NASA represents only a fraction of the overall aerospace workforce, the agency's current workforce and its potential workforce are affected by numerous other government agencies, industry, and academia. In some cases, NASA must compete directly with these organizations for appropriately skilled workers. The committee therefore concluded that some method of coordinating overall strategies concerning aerospace workforce issues between related institutions, such as NASA and the Department of Defense, would be valuable.

The agency also has to address the retention and development of NASA's in-house workforce. NASA's policies for ensuring sufficient in-house talent will possibly diverge from those it pursues for maintaining and accessing the external workforce.

In considering these issues, the committee sought in particular to identify requirements common to both the scientific and the engineering talent needed within as well as from outside the agency. Ultimately, the committee concluded that the salient requirement was the need for junior-level members of the workforce—including current and potential employees—to gain hands-on experience that would satisfy one of the perennial issues facing the agency: the need for highly skilled program/project managers and systems engineers.

In addition, the committee noted that over approximately the past decade and a half, the average age of NASA's workers has marched steadily upward, and the agency now has a relatively low number of younger workers to assume future leadership roles in NASA as older workers retire. If it does nothing to achieve a better age distribution across its overall internal workforce, NASA will suffer a gap not only in technical leadership, but also in overall technical experience, especially if the development dates for key VSE components slip and highly skilled workers with experience in the Space Shuttle program retire. The committee concluded that if NASA is to avoid a long-term shortage of the required in-house technical expertise in human spaceflight systems and other areas, it will have to adopt a strategy to address potential long-term shortfalls.

Fortunately, NASA does have programs and methods currently available for meeting its workforce needs. These include, but are not limited to, legislative authority to enhance recruitment of workers with the required skills, internal training programs such as the intramural Academy of Program/Project and Engineering Leadership (APPEL; focused primarily on engineers), and extramural programs such as the Graduate Student Researchers Program (GSRP; focused currently on scientists). Although the committee highlights these specific programs, there

[4] In contrast, the committee notes that engineering salaries in some areas, such as the petrochemical industry, are increasing rapidly because the supply is not meeting the increased demand.

are also others by which the agency trains current and potential members of its workforce. However, the committee found that many of these programs have atrophied over time and require revitalization and restructuring. In the case of GSRP, for example, NASA sponsors fellowships with award amounts that are significantly smaller than those provided by other government agencies, placing NASA at a competitive disadvantage.[5] The committee also noted with alarm that shortly before it completed its work, NASA headquarters restricted its GSRP for budgetary reasons to accept only returning applicants, not new applicants. *The amount of money required to fix these problems is not large. The committee believes that, in some cases, adding to the selection criteria for small science projects a consideration of their contributions to the training of students and junior-level professionals would improve NASA's ability to recruit, train, and retain a skilled workforce.*

The GSRP has allowed NASA to develop close ties with universities in the sciences. However, similar opportunities in engineering are far more limited. In addition, NASA has had a strong relationship with university faculty and their students as members of space science teams in technology development, mission planning, small-mission development, and mission operations. But there have been fewer close interactions in engineering and human spaceflight. In the committee's view, NASA could benefit significantly by expanding to the engineering disciplines its approaches to establishing relationships in the sciences at the university level.

NASA already spends a significant amount of money—over $162 million in fiscal year 2006—on education. But much of this funding is congressionally earmarked for kindergarten through grade 12 and public education programs (such as science centers and museums) that do not directly assist the agency in developing the specific workforce that NASA requires.

The committee believes that training students to design and build satellites and satellite instruments, gain hands-on experience with the unique demands of satellite and spacecraft systems environments and operations, and acquire early knowledge of systems engineering techniques is an extremely important investment for NASA to make. NASA needs to play a role in training the potential workforce in the skills that are unique to the work the agency conducts (see Figures ES.1 and ES.2). If NASA does not nurture and train its own potential workforce, there is no guarantee that any other government agency or private entity will do so, nor that the agency will receive the high-quality personnel that it requires to achieve the ambitious goals of returning humans to the Moon and eventually sending them to Mars.

The committee emphasizes further that when evaluating its future workforce requirements, NASA has to consider not only programs for students, but also training opportunities for its current employees. NASA's training programs at the agency's various field centers, which are focused on NASA's civil service talent, require support to prevent the agency's internal skill base from withering. Furthermore, NASA faces the risk that, if it fails to nurture its own internal workforce, skilled personnel will be attracted to other government agencies and industry.

Finally, the committee notes that not only is the contemporary workforce more fluid than it once was, but there are also new opportunities and organizations that might play a valuable role in attracting, training, and developing the workforce. These include the emerging entrepreneurial space sector, often called the "alt-space" or "new space" sector. Although to date NASA has looked at these organizations in terms of acquisition of capabilities and services (such as the Commercial Orbital Transportation Services contracts), they also offer an opportunity for tapping and developing new workforce resources.

The committee reached its conclusions after benefiting from input from public meetings at which it heard from representatives from NASA, the Department of Defense, the National Science Foundation, the Bureau of Labor Statistics, the aerospace industry, and university science and engineering schools and from analysis of documents from NASA and other organizations. The committee concluded that NASA has done much commendable work on understanding the problems it faces in the workforce arena but still has much left to do, both in understanding its requirements and in ensuring its support for programs that can fulfill them. After reviewing NASA's own workforce analyses and plans and reviewing available data, the committee developed the following specific findings and recommendations.

Finding 1: NASA has undertaken a commendable top-down (i.e., headquarters-directed) analysis of current agency needs and the skill levels of its current workforce that the committee believes is an excellent first step. But

[5]For example, a NASA GSRP slot is valued at $30,000 and awarded for 1 year; the reward is renewable for up to 3 years. For comparison, graduate fellowship funding can reach $35,000 at NIH, $37,000 at EPA, $40,500 at NSF, $42,200 to $52,200 at DOE, and $55,000 at DOD. (Figures are from 2005. More recent data were not available as of this writing.)

FIGURE ES.1 NASA technicians and university students require hands-on experience both to develop the skills and knowledge required for work on sophisticated space programs and to ensure that the agency has the systems engineers and program managers that it requires. (Top) A technician prepares a sounding rocket payload. (Bottom) A student works on an instrument in preparation for the Cosmic Ray Energetics and Mass (CREAM) 3 payload (http://cosmicray.umd.edu/cream) planned for launch in late 2007. SOURCE: Courtesy of NASA and the University of Maryland.

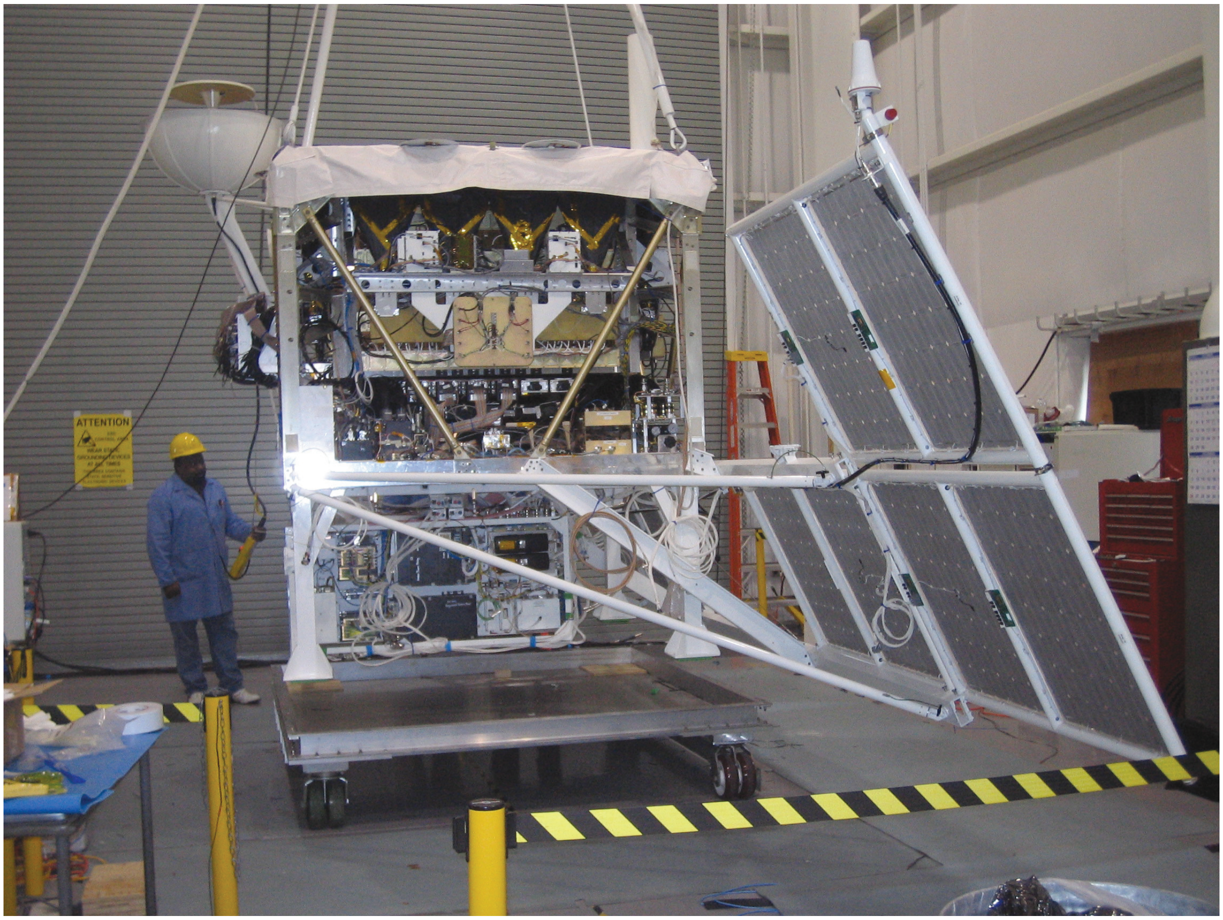

FIGURE ES.2 Some current balloon-borne payloads are in effect "mini-satellites" equipped with sophisticated subsystems and instruments. Here a NASA-sponsored Cosmic Ray Energetics and Mass (CREAM) payload is integrated at NASA Wallops Flight Facility prior to shipment to Antarctica for launch. SOURCE: See http://cosmicray.umd.edu/cream. Illustration courtesy of NASA and the University of Maryland.

although NASA has considered workforce needs for the agency as a whole, it has not yet projected its requirements for future hiring in terms of (1) the numbers and specific skill sets of workers expected to be needed by each NASA center over time and (2) the timeframes for hiring based on anticipated retirements of the present workforce. The committee believes that understanding future hiring requirements will depend on an accurate, detailed assessment of the skills, VSE-related development capabilities, and expected attrition of the workforce for each center.

Recommendation 1: Collect detailed data on NASA workforce requirements.

The committee recommends that NASA collect detailed data on and develop accurate assessments of the capabilities possessed by the current workforce and required for the future S&T workforce.

- **Because each NASA center has unique mission requirements and the mobility of personnel between centers is limited, NASA should complete a center-developed, bottom-up assessment of the current skills, experience levels, and projected attrition of the workforce for each individual NASA center.**
- **NASA should use the data obtained from such assessments to develop a model for projecting future NASA priorities for VSE skill development and hiring by competencies, experience levels, and centers, as well as a model for the best mix of skill development conducted within NASA versus within industry.**

- **NASA should translate identified workforce needs from competencies and experience levels into specific positions to be implemented at individual centers at specific points in time.**
- **NASA should assess whether the skill levels of in-house scientists at each field center are appropriate to fulfilling that center's scientific leadership and service responsibilities and should ensure that appropriate efforts are made to maintain the scientific competency and currency of each center's scientific workforce.**
- **NASA should ensure that hiring constraints—such as pay levels, personnel ceilings, and ability to recruit suitable candidates—guide make-or-buy decisions about how staffing needs will be met.**
- **NASA should ensure that appropriate workforce strategies—including providing training for staff (e.g., through the NASA Academy of Program/Project and Engineering Leadership program), contracting out work to industry and academia, facilitating exchange programs, and hiring temporary contract and term employees—are applied at each center.**

The committee believes that it is premature to recommend a particular mix of strategies for obtaining the desired worker skill mix until NASA fully defines its staffing needs.

Finding 2: In the short term, NASA has too few program/project managers and systems engineers with the requisite experience in human spaceflight systems development to successfully oversee VSE projects. Given the lack of detailed data on NASA's near-term workforce skills and needs as well as uncertainties over NASA's budget, the committee did not attempt to assess the likely success of NASA's planned steps to address near-term workforce problems.

Recommendation 2: Hire and retain younger workers within NASA.

The committee recommends that NASA implement a long-term strategy for hiring a steady supply of younger workers and subsequently retaining those workers as they rise to senior management positions so that a balanced distribution of age and skill is maintained throughout the agency's entire workforce.

- **NASA should take full advantage of the NASA Flexibility Act of 2004, which was passed to facilitate the agency's recruitment of employees from industry. NASA has already utilized the act to a considerable extent, and the committee encourages the agency to continue to do so, as well as to inform Congress of any additional hiring flexibility that is required.**
- **NASA, working with Congress and the executive branch, should develop solutions to legal problems that limit the flow of senior and highly skilled employees from industry to NASA even when such employees are willing to accept lower salaries. Issues regarding shareholding, pensions, and perceived or actual conflicts of interest severely hamper personnel exchanges between industry and NASA. These problems stem from policy issues that cannot be resolved by NASA alone but instead require action by Congress and the executive branch working in concert with NASA.**

Finding 3: NASA's workforce requirements and challenges cannot be considered in isolation from those of other government and industry organizations. NASA is part of an aerospace workforce ecosystem in which the health and needs of one organization or sector can affect another. Thus, NASA's workforce issues require the intervention and assistance of higher-level government organizations such as the Office of Science and Technology Policy in the Executive Office of the President.

Recommendation 3: Ensure a coordinated national strategy for aerospace workforce development among relevant institutions.

The committee recommends that representatives from relevant government agencies, the aerospace industry, including the emerging private sector, and the academic community work together to develop a coordinated national strategy to ensure an effective aerospace workforce ecosystem.

Finding 4: There is a longstanding, widely recognized requirement for more highly skilled program/project managers and systems engineers who have acquired substantial experience in space systems development. Although the need exists across all of NASA and the aerospace industry, it seems particularly acute for human spaceflight systems because of the long periods between initiation of new programs (i.e., the Space Shuttle program in the 1970s and the Constellation program 30 years later). NASA training programs are addressing some of the agency's requirements in this experience base, but the current requirement for a strong base of highly skilled program/project management and systems engineering personnel, and limited opportunities for junior specialists to gain hands-on space project experience, remain impediments to NASA's ability to successfully carry out VSE programs and projects.

Recommendation 4: Provide hands-on training opportunities for NASA workers.

The committee recommends that NASA place a high priority on recruiting, training, and retaining skilled program/project managers and systems engineers and that it provide the hands-on training and development opportunities for younger and junior personnel required to establish and maintain the necessary capabilities in these disciplines. Specific and immediate actions to be taken by NASA and other parts of the federal government include the following:

- **In establishing its strategy for meeting VSE systems engineering needs, NASA should determine the right balance between in-house and out-of-house work and contractor roles and responsibilities, including the use of support service contractors.**
- **NASA should continue and also expand its current employee training programs such as those being conducted by the Academy of Program/Project and Engineering Leadership (APPEL). To facilitate the development of key systems engineering and project management skills, NASA should increase the number of opportunities for entry-level employees to be involved in hands-on flight and end-to-end development programs. A variety of programs—including those involving balloons, sounding rockets, aircraft-based research, small satellites, and so on—can be used to give these employees critical experience relatively early in their careers and allow them to contribute as systems engineers and program managers more quickly.**

Finding 5: NASA relies on a highly trained technical workforce to achieve its goals and has long accepted a responsibility for supporting the training of those who are potential employees. In recent years, however, training for students has been less well supported by NASA. A robust and stable commitment to creating opportunities at the university level for experience in hands-on flight mission development, graduate research fellowships for science and engineering students, and research is essential for recruiting and developing the long-term supply of competent workers necessary to implement NASA's future programs.

- Faculty research not only is fundamental to student training but also leads to the development of new technology and tools for future applications in space. Programs supporting critical scientific and technological expertise are highly desirable.
- Hands-on experience for students is provided by suborbital programs, Explorer and other small spacecraft missions, and design competitions, all of which rely on continuing NASA support.
- The Graduate Student Researchers Program supports the education and training of prospective NASA employees and deserves augmented support.
- Undergraduate and graduate co-op student programs are particularly effective in giving students early hands-on experience and in exposing students and NASA to each other to help enable sound career choices and hiring decisions.

Recommendation 5: Support university programs and provide hands-on opportunities at the college level.

The committee recommends that NASA make workforce-related programs such as the Graduate Student Researchers Program and co-op programs a high priority within its education budget. NASA should also invest in the future workforce by partnering with universities to provide hands-on experiences for students and opportunities for fundamental scientific and engineering research specific to NASA's needs. These experiences should include significant numbers of opportunities to participate in all aspects of suborbital and Explorer-class flight programs and in research fellowships and co-op student assignments.

Finding 6: Although NASA's primary role is not education or outreach, improved support of the higher-education community and of young professionals is critical to maintaining a sufficiently talented workforce. Involvement in providing development and educational opportunities, especially hands-on flight and vehicle development opportunities, will pay future dividends not only by encouraging larger numbers of talented students to enter the field, but also by improving the abilities of incoming employees. Indeed, a failure to invest in today's students and young professionals will ultimately lead to a crisis when that generation is expected to assume the mantle of leadership within the U.S. aerospace community.

Recommendation 6: Support involvement in suborbital programs and nontraditional approaches to developing skills.

The committee recommends that NASA increase its investment in proven programs such as sounding rocket launches, aircraft-based research, and high-altitude balloon campaigns, which provide ample opportunities for hands-on flight development experience at a relatively low cost of failure.

Rather than viewing sounding rockets, aircraft-based research, and balloon programs simply as low-cost, competed, scientific missions, NASA should also recognize as an equal factor in the criteria for their selection their ability to provide valuable hands-on experience for its younger workers and should investigate the possibility of funding such programs through its education budget.

In addition, NASA should take advantage of nontraditional institutions and approaches both to inspire and to train potential future employees. Investment in programs such as Centennial Challenge prizes and other innovative methods has the potential to pay benefits many times greater than their cost, by simultaneously increasing NASA's public visibility, training a new generation of workers, and pushing the technology envelope.

Strategic planning for workforce issues is difficult because budget and program decisions often have major impacts on the workforce that make strategic planning irrelevant. The committee heard from industry representatives who stated that NASA's ability to attract junior-level personnel and retain senior personnel will be heavily influenced by perceptions about how compelling and stably funded the Vision for Space Exploration is. The committee thus believes that NASA must adopt policies that, while relatively inexpensive, can have a longer-term impact on its ability to obtain the highest-quality personnel.

5.5 Decadal Science Strategy Surveys: Report of a Workshop

Jack D. Fellows, Rapporteur, Joseph K. Alexander, Editor

Summary

The Workshop on Decadal Science Strategy Surveys was held on November 14-16, 2006, to promote discussions of the use of National Research Council (NRC) decadal surveys for developing and implementing scientific priorities, to review lessons learned from the most recent surveys, and to identify potential approaches for future surveys that can enhance their realism, utility, and endurance. The workshop involved approximately 60 participants from academia, industry, government, and the NRC. This report summarizes the workshop presentations, panel discussions, and general discussions on the use of decadal surveys for developing and implementing scientific priorities in astronomy and astrophysics, planetary science, solar and space physics, and Earth science.

WORKSHOP BACKGROUND

NRC decadal surveys provide broad assessments of the status of research fields, and they develop recommendations for scientific and programmatic priorities for future investments in the fields. Workshop participants from both inside and outside the government shared the view that the decadal surveys are important, especially because they have positive impacts on federal agency planning and decision making and on science community unity. Efforts by survey committees to draw wide community participation, engage in consensus building, and set explicit science-based priorities were repeatedly cited as features of the surveys that make them the gold standard for advice on research program planning.

While the surveys have been widely successful, government agencies and the scientific communities that have tried to follow survey advice have had to deal with several notable problems, including the following:

- *Cost and technical risk.* Survey estimates of program cost and technology readiness have sometimes proved to be overly optimistic or have not included the full life-cycle costs of initiatives.
- *Resiliency and execution.* Surveys have not always provided guidance on how to respond to budgetary, programmatic, or policy changes that significantly impact survey recommendations. Nor have they addressed the impacts on a balanced portfolio of large, medium, and small projects when a large development project encounters cost and/or technical trouble.
- *Planning, management, and collaboration.* The charges to survey committees have often been very broad and open ended, and the surveys themselves have not always been timed to match agency or political planning cycles. Survey committees have also encountered problems when research programs are not well coordinated between agencies or nations. Some ask whether survey users should have recommendations on broad science objectives or on specific missions or facilities. Recent surveys have not always explicitly reconsidered the recommendations of previous surveys.

LEVERAGING PAST SUCCESSES AND IMPROVING FUTURE SURVEYS

The workshop brought together subject experts, previous survey committee members, and a broad range of survey users over a 3-day period to discuss the pros and cons of various approaches of recent surveys to issues such as those noted above so that future surveys can handle these issues as effectively as possible. This rich discussion touched on each of the three problem areas noted above, with the views expressed by many participants highlighted below.

NOTE: “Summary” reprinted from *Decadal Science Strategy Surveys: Report of a Workshop,* The National Academies Press, Washington, D.C., 2007, pp. 1-5.

Cost and Technical Risk

Many participants noted that program cost estimates, which were based largely on information from the National Aeronautics and Space Administration, have become problematic and that too often costs have turned out to be as much as four times the original cost estimates. Participants urged that agencies continue to develop and improve cost and schedule parametric models for space missions. These models should reflect that software has become a dominant factor that impacts cost and schedule for many missions in ways just as important as traditional factors such as spacecraft mass or power. Some experts also noted that instrument development is a leading contributor to cost risk, so that programs for reducing risk related to instruments are also especially important.

In looking to future decadal surveys, participants appeared to agree that survey committees need to do four things: (1) include cost assessment and technology experts on survey committees, (2) obtain independent cost estimates and include cradle-to-grave life-cycle costs, (3) include cost uncertainty indexes to help define the risk of cost growth, and (4) use common costing approaches so that costs for different missions or facilities can be compared. The discussions repeatedly made the point that early cost estimates are better for comparing mission costs than they are for providing absolute cost projections, because most mission candidates are far short of being ready for a preliminary design review. Consequently, participants argued for the use of cost bins, or ranges, rather than specific costs. They also suggested that the technology being considered for a mission or facility needs to be well understood or characterized before a cost for it can be estimated.

Resiliency and Execution

Workshop participants acknowledged that unanticipated, but seemingly inevitable, changes in the budgetary, programmatic, and/or political environment present a challenge to the ability of government agencies and the research community to implement the priorities set in a survey report. They identified a number of steps that could enhance the resiliency of a survey's recommendations and increase the likelihood that a recommended program could be executed as proposed. For example, participants argued that survey committees should establish metrics for creating and maintaining a balanced set of projects. Many speakers felt that survey committees need to recognize that proposing a range of missions or facilities (small, medium, and large, plus core research and technology activities) will be intrinsically more resilient than proposing mostly large, complex initiatives. They supported the idea that maximizing the number of projects to be selected competitively will enhance program resiliency.

In discussions about how to cope with a large increase in the cost of projects and the attendant impacts of such growth, some participants felt that survey committees need to be very conservative about recommending large missions that are not yet well defined or understood. As one speaker put it, "If a mission isn't understood well enough to derive a good cost estimate, then it doesn't deserve a priority." Several experts argued that even after a presumably well-founded cost estimate is in hand, a reserve (~20 percent of the expected mission cost) must be held separately from the project manager's mission or facility development budget contingency funds.

Finally, several participants noted that survey committees would be wise to start with a more realistic sense of agency budgetary and policy environments and to build stronger partnerships with agencies so that surveys can be more resilient.

Planning, Management, and Collaboration

Discussions about the timing of decadal surveys touched on both the time required to complete a survey and the time span over which a survey should look. There appeared to be broad agreement that 2 years is roughly appropriate for completing a comprehensive survey and that 10 years is about the right planning horizon. Agency representatives mentioned that they get plenty of conflicting advice, so that having a stable, long-term survey is very important. While there were also arguments that surveys should not be arbitrarily revised, it might make sense to build triggers into surveys, where cost growth or policy changes would require the survey to be revisited by a qualified group. A number of speakers suggested that scenario analysis be a part of any survey whose users want it to remain robust over a decade. Or, even better, that decision rules be included in the survey for dealing with unforeseen changes. Discussions of timing also drew suggestions that surveys need to be synchronized with other key planning processes (e.g., agency planning milestones and political cycles).

Several important factors for planning and organizing decadal surveys were mentioned repeatedly. First, former survey committee chairs noted that the survey charge must be clear and focused to avoid open-ended tasks and should be vetted fully with the research community and relevant government agencies. There was widespread agreement that surveys should have substantial community ownership and input. Some participants argued that all of the stakeholders (including the science community, federal agencies, and Congress) need to be part of the survey process (including definition, information gathering, and dissemination of results). A point that became clear in discussions of a survey's assessment of cost, technology risk, and program execution was that survey committees need to include not only scientific disciplinary expertise, but also expertise in other areas such as hardware development, program management, systems engineering, cost estimating, and policy. There was also general agreement that survey planning should include how to disseminate the survey report to users and how to make it comprehensible and appealing to the public.

Workshop participants expressed support for the idea that surveys should remain focused on science first so that there would be a clear and compelling presentation of the important science to be done and that the subsequent presentation of programmatic priorities and recommendations should always be traceable back to the science. Speakers also agreed that it is important to highlight applications that can be drawn from basic science missions and that can benefit society in an immediate and tangible way. Finally, many participants noted that priorities recommended in previous surveys should be readdressed in the context of the new survey and its priorities, and recommendations in the previous surveys should not be assumed to be guaranteed or irreversible.

The workshop also stimulated discussion about several aspects of internal and external coordination. First, some participants acknowledged that while interagency and international cooperative programs have a chance of promoting cooperation across organizational boundaries, they tend to be a great challenge and rarely result in substantial cost savings. Therefore, agencies need to give extra attention to integrating such cooperative efforts as effectively as possible. Second, participants noted that as long as human exploration of space is a major national space goal, future surveys should not altogether ignore such exploration. However, science surveys should stick with the principle of "science first" while integrating research that can be enabled by human spaceflight into overall science priorities to be recommended in a survey report.

5.6 Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond

A Report of the Ad Hoc Committee on Earth Science and Applications from Space:
A Community Assessment and Strategy for the Future

Executive Summary

A VISION FOR THE FUTURE

Understanding the complex, changing planet on which we live, how it supports life, and how human activities affect its ability to do so in the future is one of the greatest intellectual challenges facing humanity. It is also one of the most important challenges for society as it seeks to achieve prosperity, health, and sustainability.

These declarations, first made in the interim report of the Committee on Earth Science and Applications from Space: A Community Assessment and Strategy for the Future,[1] are the foundation of the committee's vision for a decadal program of Earth science research and applications in support of society—a vision that includes advances in fundamental understanding of the Earth system and increased application of this understanding to serve the nation and the people of the world. The declarations call for a renewal of the national commitment to a program of Earth observations in which attention to securing practical benefits for humankind plays an equal role with the quest to acquire new knowledge about the Earth system.

The committee strongly reaffirms these declarations in the present report, which completes the National Research Council's (NRC's) response to a request from the National Aeronautics and Space Administration (NASA) Office of Earth Science, the National Oceanic and Atmospheric Administration (NOAA) National Environmental Satellite Data and Information Service, and the U.S. Geological Survey (USGS) Geography Division to generate consensus recommendations from the Earth and environmental science and applications communities regarding (1) high-priority flight missions and activities to support national needs for research and monitoring of the dynamic Earth system during the next decade, and (2) important directions that should influence planning for the decade beyond.[2] The national strategy outlined here has as its overarching objective a program of scientific discovery and development of applications that will enhance economic competitiveness, protect life and property, and assist in the stewardship of the planet for this and future generations.

Earth observations from satellites and in situ collection sites are critical for an ever-increasing number of applications related to the health and well-being of society. The committee found that fundamental improvements are needed in existing observation and information systems because they only loosely connect three key elements: (1) the raw observations that produce information; (2) the analyses, forecasts, and models that provide timely and coherent syntheses of otherwise disparate information; and (3) the decision processes that use those analyses and forecasts to produce actions with direct societal benefits.

Taking responsibility for developing and connecting these three elements in support of society's needs represents a new social contract for the scientific community. The scientific community must focus on meeting the demands of society explicitly, in addition to satisfying its curiosity about how the Earth system works. In addition, the federal institutions responsible for the Earth sciences' contributions to protection of life and property, strategic economic development, and stewardship of the planet will also need to change. In particular, the clarity with which Congress links financial resources with societal objectives, and provides oversight to ensure that these objectives are met, must keep pace with emerging national needs. Individual agencies must develop an integrated framework

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NOTE: "Executive Summary" reprinted from *Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond,* The National Academies Press, Washington, D.C., 2007, pp. 1-16.

[1]National Research Council (NRC), *Earth Science and Applications from Space: Urgent Needs and Opportunities to Serve the Nation*, The National Academies Press, Washington, D.C., 2005, p. 1; referred to hereafter as the "interim report."

[2]The other elements of the committee's charge are shown in Appendix A. As explained in the Preface, the committee focused its attention on items 2, 3, and 4 of the charge.

that transcends their particular interests, with clear responsibilities and budget authority for achieving the most urgent societal objectives. Therefore, the committee offers the following overarching recommendation:

Recommendation: **The U.S. government, working in concert with the private sector, academe, the public, and its international partners, should renew its investment in Earth-observing systems and restore its leadership in Earth science and applications.**

The objectives of these partnerships would be to facilitate improvements that are needed in the structure, connectivity, and effectiveness of Earth-observing capabilities, research, and associated information and application systems—not only to answer profound scientific questions, but also to effectively apply new knowledge in pursuit of societal benefits.

The world faces significant environmental challenges: shortages of clean and accessible freshwater, degradation of terrestrial and aquatic ecosystems, increases in soil erosion, changes in the chemistry of the atmosphere, declines in fisheries, and the likelihood of substantial changes in climate. These changes are not isolated; they interact with each other and with natural variability in complex ways that cascade through the environment across local, regional, and global scales. Addressing these societal challenges requires that we confront key scientific questions related to ice sheets and sea-level change, large-scale and persistent shifts in precipitation and water availability, transcontinental air pollution, shifts in ecosystem structure and function in response to climate change, impacts of climate change on human health, and the occurrence of extreme events, such as severe storms, heat waves, earthquakes, and volcanic eruptions. The key questions include:

- Will there be catastrophic collapse of the major ice sheets, including those of Greenland and West Antarctic and, if so, how rapidly will this occur? What will be the time patterns of sea-level rise as a result?
- Will droughts become more widespread in the western United States, Australia, and sub-Saharan Africa? How will this affect the patterns of wildfires? How will reduced amounts of snowfall change the needs for water storage?
- How will continuing economic development affect the production of air pollutants, and how will these pollutants be transported across oceans and continents? How are these pollutants transformed during the transport process?
- How will coastal and ocean ecosystems respond to changes in physical forcing, particularly those subject to intense human harvesting? How will the boreal forest shift as temperature and precipitation change at high latitudes? What will be the impacts on animal migration patterns and on the prevalence of invasive species?
- Will previously rare diseases become common? How will mosquito-borne viruses spread with changes in rainfall and drought? Can we better predict the outbreak of avian flu? What are the health impacts of an expanded ozone hole that could result from a cooling of the stratosphere, which would be associated with climate change?
- Will tropical cyclones and heat waves become more frequent and more intense? Are major fault systems nearing the release of stress via strong earthquakes?

The required observing system is one that builds on the current fleet of space-based instruments and brings to a new level of integration our understanding of the Earth system.

SETTING THE FOUNDATION: OBSERVATIONS IN THE CURRENT DECADE

As documented in this report, the extraordinary U.S. foundation of global observations is at great risk. Between 2006 and the end of the decade, the number of operating missions will decrease dramatically, and the number of operating sensors and instruments on NASA spacecraft, most of which are well past their nominal lifetimes, will decrease by some 40 percent (see Figures ES.1 and ES.2). Furthermore, the replacement sensors to be flown on the National Polar-orbiting Operational Environmental Satellite System (NPOESS)[3] are generally less capable than their Earth Observing System (EOS) counterparts.[4] Among the many measurements expected to cease over the next few years, the committee has identified several that are providing critical information now and

[3]See a description at http://www.ipo.noaa.gov/.

[4]NASA's Earth Observing System (EOS) includes a series of satellites, a science component, and a data system supporting a coordinated series of polar-orbiting and low-inclination satellites for long-term global observations of the land surface, biosphere, solid Earth, atmosphere, and oceans. See http://eospso.gsfc.nasa.gov/eos_homepage/description.php.

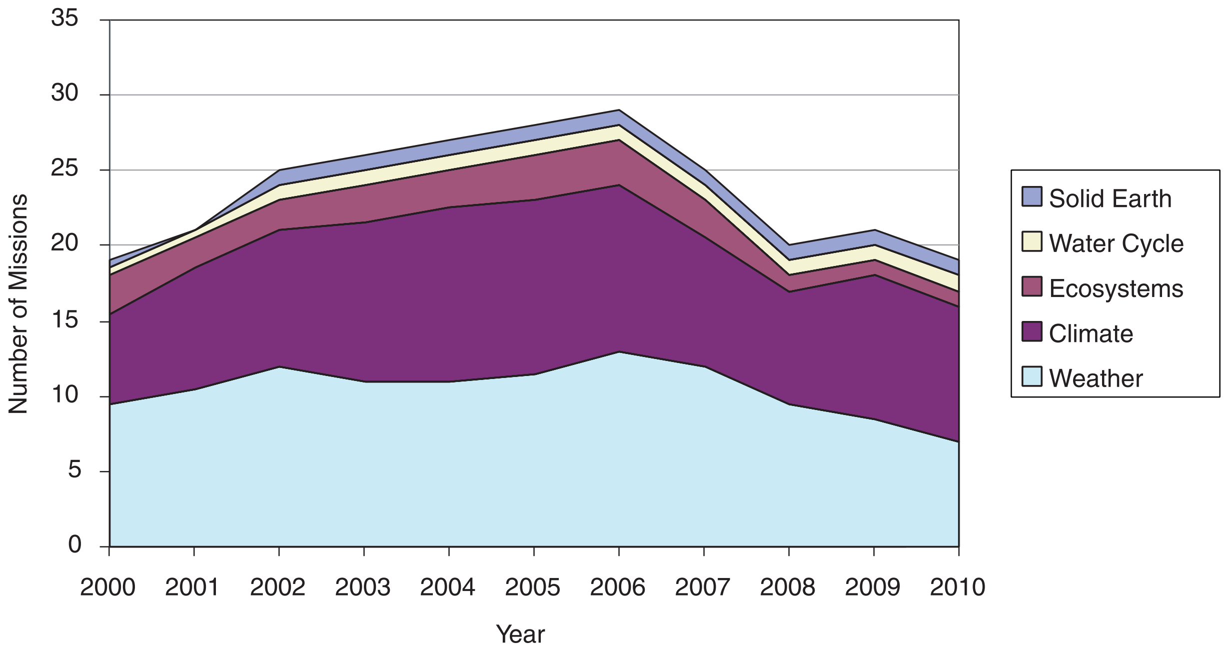

FIGURE ES.1 Number of U.S. space-based Earth observation missions in the current decade. An emphasis on climate and weather is evident, as is a decline in the number of missions near the end of the decade. For the period from 2007 to 2010, missions were generally assumed to operate for 4 years past their nominal lifetimes. Most of the missions were deemed to contribute at least slightly to human health issues, and so health is not presented as a separate category. SOURCE: Information from NASA and NOAA Web sites for mission durations.

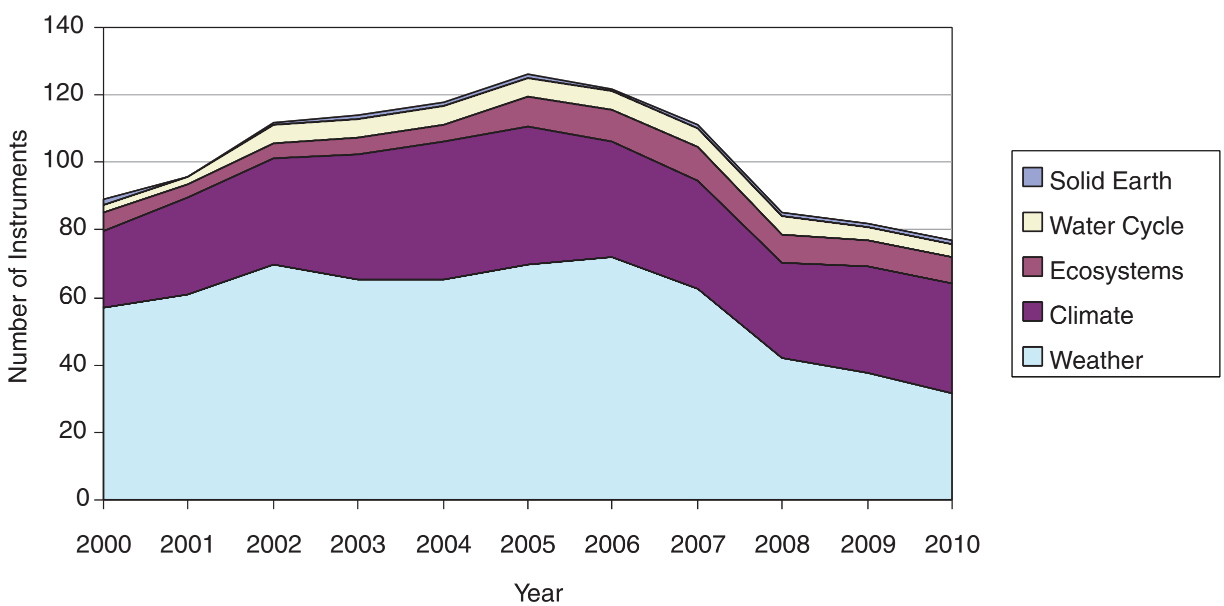

FIGURE ES.2 Number of U.S. space-based Earth observation instruments in the current decade. An emphasis on climate and weather is evident, as is a decline in the number of instruments near the end of the decade. For the period from 2007 to 2010, missions were generally assumed to operate for 4 years past their nominal lifetimes. Most of the missions were deemed to contribute at least slightly to human health issues, and so health is not presented as a separate category. SOURCE: Information from NASA and NOAA Web sites for mission durations.

that need to be sustained into the next decade—both to continue important time series and to provide the foundation necessary for the recommended future observations. These include measurements of total solar irradiance and Earth radiation and vector sea-surface winds; limb sounding of ozone profiles; and temperature and water vapor soundings from geostationary and polar orbits.[5]

As highlighted in the committee's interim report, there is substantial concern that substitution of passive microwave sensor data for active scatterometry data will worsen El Niño and hurricane forecasts as well as weather forecasts in coastal areas.[6] Given the status of existing surface wind measurements and the substantial uncertainty introduced by the cancellation of the CMIS instrument on NPOESS, the committee believes it imperative that a measurement capability be available to prevent a data gap when the NASA QuikSCAT mission, already well past its nominal mission lifetime, terminates.

Questions about the future of wind measurement capabilities are part of a larger set of issues related to the development of a mitigation strategy to recover capabilities lost in the recently announced descoping and cancellations of instruments and spacecraft planned for the NPOESS constellation. A request for the committee to perform a fast-track analysis of these issues was approved by the NRC shortly before this report was released. Nevertheless, based on its analysis to date, the committee makes the following recommendations:

Recommendation:[7] **NOAA should restore several key climate, environmental, and weather observation capabilities to its planned NPOESS and GOES-R[8] missions; namely:**

- **Measurements of ocean vector winds and all-weather sea-surface temperatures descoped from the NPOESS C1 launch should be restored to provide continuity until the CMIS replacement is operational on NPOESS C2 and higher-quality active scatterometer measurements (from XOVWM, described in Table ES.1) can be undertaken later in the next decade.**
- **The limb sounding capability of the Ozone Monitoring and Profiling Suite (OMPS) on NPOESS should be restored.[9]**

The committee also recommends that NOAA:

- **Ensure the continuity of measurements of Earth's radiation budget (ERB) and total solar irradiance (TSI) through the period when the NPOESS spacecraft will be in orbit by:**
  - **—Incorporating on the NPOESS Preparatory Project (NPP)[10] spacecraft the existing "spare" CERES instrument, and, if possible, a TSI sensor, and**
  - **—Incorporating these or similar instruments on the NPOESS spacecraft that will follow NPP, or ensuring that measurements of TSI and ERB are obtained by other means.**

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[5]As discussed in the Preface and in more detail in Chapter 2, the continuity of a number of other critical measurements, such as sea-surface temperature, is dependent on the acquisition of a suitable instrument on NPOESS to replace the now-canceled CMIS sensor.

[6]Also, see pp. 4-5 of the Oceans Community Letter to the Decadal Survey, available at http://cioss.coas.oregonstate.edu/CIOSS/Documents/Oceans_Community_Letter.pdf, and the report of the NOAA Operational Ocean Surface Vector Winds Requirements Workshop, June 5-7, 2006, National Hurricane Center, Miami, Fla., P. Chang and Z. Jelenak, eds.

[7]Inaccurate wording of this four-part recommendation in the initially released prepublication copy of this report was subsequently corrected by the committee to reflect its intent to recommend a capability for ensuring continuity of the ongoing record of measurements of total solar irradiance and of Earth's radiation budget. As explained in the description of the CLARREO mission in Chapter 4, the committee recommends that the CERES Earth radiation budget instrument and a total solar irradiance sensor be flown on the NPOESS Preparatory Project (NPP) satellite and that these instruments or their equivalent be carried on the NPOESS spacecraft or another suitable platform.

[8]GOES-R is the designation for the next generation of geostationary operational environmental satellites (GOES). See https://osd.goes.noaa.gov/ and http://goespoes.gsfc.nasa.gov/goes/spacecraft/r_spacecraft.html. The first launch of the GOES-R series satellite was recently delayed from the 2012 time frame to December 2014.

[9]Without this capability, no national or international ozone-profiling capability will exist after the EOS Aura mission ends in 2010. This capability is key to monitoring ozone-layer recovery in the next two decades and is part of NOAA's mandate through the Clean Air Act.

[10]The NASA-managed NPP, a joint mission involving NASA and the NPOESS Integrated Program Office (IPO), has a twofold purpose: (1) to provide continuity for a selected set of calibrated observations with the existing Earth Observing System measurements for Earth science research and (2) to provide risk reduction for four of the key sensors that will fly on NPOESS, as well as the command and data-handling system. The earliest launch set for NPP is now September 2009, a delay of nearly 3 years from the plans that existed prior to the 2006 Nunn-McCurdy recertification. See http://jointmission.gsfc.nasa.gov/ and http://www.nasa.gov/pdf/150011main_NASA_Testimony_for_NPOESS-FINAL.pdf.

- **Develop a strategy to restore the previously planned capability to make high-temporal- and high-vertical-resolution measurements of temperature and water vapor from geosynchronous orbit.**

The high-temporal- and high-vertical-resolution measurements of temperature and water vapor from geosynchronous orbit were originally to be delivered by the Hyperspectral Environmental Sensor (HES) on the GOES-R spacecraft. Recognizing the technological challenges and accompanying potential for growth in acquisition costs for HES, the committee recommends consideration of the following approaches:

- **Working with NASA, complete the GIFTS instrument, deliver it to orbit via a cost-effective launch and spacecraft opportunity, and evaluate its potential to be a prototype for the HES instrument, and/or**
- **Extend the HES study contracts focusing on cost-effective approaches to achieving essential sounding capabilities to be flown in the GOES-R time frame.**

The committee believes that such approaches will both strengthen the technological foundation of geostationary Earth orbit (GEO)-based soundings and provide the requisite experience for efficient operational implementation of GEO-based soundings.

The recommendations above focus on issues whose resolution requires action by NOAA. The committee also notes two issues of near-term concern mostly for NASA:

1. Understanding the changing global precipitation patterns that result from changing climate, and
2. Understanding the changing patterns of land use due to the needs of a growing population, the expansion and contraction of economies, and the intensification of agriculture.

Both of these concerns have been highlighted in the scientific and policy literature;[11] they were also highlighted in the committee's interim report. The committee believes that it is vital to maintain global precipitation measurements as offered by the Global Precipitation Measurement (GPM) mission, and to continue to document biosphere changes indicated by measurements made with instruments on the Landsat series of spacecraft.

Recommendation: **NASA should ensure continuity of measurements of precipitation and land cover by:**

- **Launching the GPM mission in or before 2012, and**
- **Securing before 2012 a replacement for collection of Landsat 7 data.**

The committee also recommends that NASA continue to seek cost-effective, innovative means for obtaining information on land cover change.

Sustained measurements of these key climate and weather variables are part of the committee's strategy to achieve its vision for an Earth observation and information system in the next decade. The recommended new system of observations that will help deliver that vision is described below.

NEW OBSERVATIONS FOR THE NEXT DECADE

The primary work in developing a decadal strategy for Earth observation took place within the survey's seven thematically organized panels (see Preface). Six of the panels were organized to address multidiscipline issues in climate change, water resources, ecosystem health, human health, solid-Earth natural hazards, and weather. This categorization is similar to the organizing structure used in the Global Earth Observation System of Systems (GEOSS) process. Each panel first set priorities among an array of candidate space-based measurement approaches and mission concepts by applying the criteria shown in Box ES.1. The assessment and subsequent prioritization were based on an overall analysis by panel members of how well each mission satisfied the criteria and high-

[11] For example, see the IPCC Third Assessment Report, *Climate Change 2001,* available at http://www.ipcc.ch/pub/reports.htm or at http://www.grida.no/climate/ipcc_tar/, and the 2005 Millennium Ecosystem Assessment Synthesis reports, which are available at http://www.maweb.org/en/Products.aspx#.

BOX ES.1 CRITERIA USED BY THE PANELS TO CREATE RELATIVE RANKINGS OF MISSIONS

- Contribution to the most important scientific questions facing Earth sciences today (scientific merit, discovery, exploration)
- Contribution to applications and policy making (societal benefits)
- Contribution to long-term observational record of Earth
- Ability to complement other observational systems, including planned national and international systems
- Affordability (cost considerations, either total costs for mission or costs per year)
- Degree of readiness (technical, resources, people)
- Risk mitigation and strategic redundancy (backup of other critical systems)
- Significant contribution to more than one thematic application or scientific discipline

Note that these guidelines are not in priority order, and they may not reflect all of the criteria considered by the panels.

level community objectives. Recommendations in previous community-based reports, such as those of the World Meteorological Organization, were also considered.

The complete set of high-priority missions and observations identified by the panels numbered approximately 35, a substantial reduction from the more than 100 missions suggested in the responses to the committee's request for information (see Appendixes D and E) and numerous other mission ideas suggested by panel members (see Table 2.3). The panel reports in Part III of this report document the panels' analyses. As described in Chapter 2, the committee derived a total of 17 missions for implementation by NASA and NOAA.

In developing the recommended set of missions, the committee recognized that a successful Earth observation program is more than the sum of its parts. The committee's prioritization methodology was designed to achieve a robust, integrated program—one that does not crumble if one or several missions in the prioritized list are removed or delayed or if the mission list must evolve to accommodate changing needs. The methodology was also intended to enable augmentation or enhancement of the program should additional resources become available beyond those anticipated by the committee. Robustness is thus measured by the strength of the overall program, not by the particular missions on the list. It is the range of observations that must be protected rather than the individual missions themselves.

The committee's recommended Earth observation strategy consists of:

- 14 missions for implementation by NASA,
- 2 missions for implementation by NOAA, and
- 1 mission (CLARREO) that has separate components for implementation by NASA and NOAA.

These 17 missions are summarized in Tables ES.1 (NOAA portion) and ES.2 (NASA portion). The recommended observing strategy is consistent with the recommendations from the U.S. Global Change Research Program (USGCRP), the U.S. Climate Change Science Program (CCSP), and the U.S. component of GEOSS. Most importantly, the observing strategy enables significant progress across the range of important societal issues. The number of recommended missions and associated observations is only a fraction of the number of currently operating Earth missions and observations (see Figures ES.1 and ES.2). *The committee believes strongly that the missions listed in Tables ES.1 and ES.2 form a minimal, yet robust, observational component of an Earth information system that is capable of addressing a broad range of societal needs.*

Recommendation: **In addition to implementing the re-baselined NPOESS and GOES program and completing research missions currently in development, NASA and NOAA should undertake the set of**

TABLE ES.1 Launch, Orbit, and Instrument Specifications for Missions Recommended to NOAA

Decadal Survey Mission	Mission Description	Orbit[a]	Instruments	Rough Cost Estimate (FY 06 $million)
2010-2013				
CLARREO (instrument reflight components)	Solar and Earth radiation characteristics for understanding climate forcing	LEO, SSO	Broadband radiometer	65
GPSRO	High-accuracy, all-weather temperature, water vapor, and electron density profiles for weather, climate, and space weather	LEO	GPS receiver	150
2013-2016				
XOVWM	Sea-surface wind vectors for weather and ocean ecosystems	LEO, SSO	Backscatter radar	350

NOTE: Missions are listed by cost. Mission costs, estimated by the committee, are categorized as medium-cost ($300 million to $600 million) and small-cost (<$300 million). The missions are described in detail in Part II, and Part III provides the foundation for selection.

[a]LEO, low Earth orbit; SSO, Sun-synchronous orbit.

17 missions[12] recommended in Tables ES.1 and ES.2 comprising low-cost (<$300 million), medium-cost ($300 million to $600 million), and large-cost ($600 million to $900 million) missions and phased appropriately over the next decade.[13] Larger, facility-class (>$1 billion) missions are not recommended. As part of this strategy:

- **NOAA should transition to operations three research observations. These are vector sea-surface winds; GPS radio occultation temperature, water vapor, and electron density soundings; and total solar irradiance (restored to NPOESS). Approaches to these transitions are provided through the recommended XOVWM, GPSRO, and CLARREO missions listed in Table ES.1.**
- **NASA should implement a set of 15 missions phased over the next decade. All of the appropriate low Earth orbit (LEO) missions should include a Global Positioning System (GPS) receiver to augment operational measurements of temperature and water vapor. The missions and their specifications are listed in Table ES.2.**

In developing its plan, the committee exploited both science and measurement synergies among the various priority missions of the individual panels to create a capable and affordable observing system. For example, the committee recognized that ice sheet change, solid-Earth hazards, and ecosystem health objectives are together well addressed by a combination of radar and lidar instrumentation. As a result, a pair of missions flying in the same time frame was devised to address the three societal issues.

The phasing of missions over the next decade was driven primarily by consideration of the maturity of key prediction and forecasting tools and the timing of particular observations needed for maintaining or improving those tools. For established applications with a clear operational use, such as numerical weather prediction (NWP), the need for routine vector sea-surface wind observations and atmospheric temperature and water vapor soundings by relatively mature instrument techniques set the early phasing, and these capabilities are recommended to

[12]One mission, CLARREO, has two components—a NASA component and a separate NOAA component.

[13]Tables ES.1 and ES.2 include cost estimates for the 17 missions. These estimates include costs for development, launch, and 3 years of operation for NASA research missions and 5 years of operation for NOAA operational missions. Estimates also include funding of a science team to work on algorithms and data preparation, but not funding for research and analysis to extract science from the data. All estimates are in fiscal year 2006 dollars.

TABLE ES.2 Launch, Orbit, and Instrument Specifications for Missions Recommended to NASA

Decadal Survey Mission	Mission Description	Orbit[a]	Instruments	Rough Cost Estimate (FY 06 $million)
2010-2013				
CLARREO (NASA portion)	Solar and Earth radiation; spectrally resolved forcing and response of the climate system	LEO, Precessing	Absolute, spectrally resolved interferometer	200
SMAP	Soil moisture and freeze-thaw for weather and water cycle processes	LEO, SSO	L-band radar L-band radiometer	300
ICESat-II	Ice sheet height changes for climate change diagnosis	LEO, Non-SSO	Laser altimeter	300
DESDynI	Surface and ice sheet deformation for understanding natural hazards and climate; vegetation structure for ecosystem health	LEO, SSO	L-band InSAR Laser altimeter	700
2013-2016				
HyspIRI	Land surface composition for agriculture and mineral characterization; vegetation types for ecosystem health	LEO, SSO	Hyperspectral spectrometer	300
ASCENDS	Day/night, all-latitude, all-season CO_2 column integrals for climate emissions	LEO, SSO	Multifrequency laser	400
SWOT	Ocean, lake, and river water levels for ocean and inland water dynamics	LEO, SSO	Ka- or Ku-band radar Ku-band altimeter Microwave radiometer	450
GEO-CAPE	Atmospheric gas columns for air quality forecasts; ocean color for coastal ecosystem health and climate emissions	GEO	High-spatial-resolution hyperspectral spectrometer Low-spatial-resolution imaging spectrometer IR correlation radiometer	550
ACE	Aerosol and cloud profiles for climate and water cycle; ocean color for open ocean biogeochemistry	LEO, SSO	Backscatter lidar Multiangle polarimeter Doppler radar	800
2016-2020				
LIST	Land surface topography for landslide hazards and water runoff	LEO, SSO	Laser altimeter	300
PATH	High-frequency, all-weather temperature and humidity soundings for weather forecasting and sea-surface temperature[b]	GEO	Microwave array spectrometer	450
GRACE-II	High-temporal-resolution gravity fields for tracking large-scale water movement	LEO, SSO	Microwave or laser ranging system	450
SCLP	Snow accumulation for freshwater availability	LEO, SSO	Ku- and X-band radars K- and Ka-band radiometers	500
GACM	Ozone and related gases for intercontinental air quality and stratospheric ozone layer prediction	LEO, SSO	UV spectrometer IR spectrometer Microwave limb sounder	600
3D-Winds (Demo)	Tropospheric winds for weather forecasting and pollution transport	LEO, SSO	Doppler lidar	650

NOTE: Missions are listed by cost. Mission costs, as estimated by the committee, are categorized as large-cost ($600 million to $900 million), medium-cost ($300 million to $600 million), and small-cost (<$300 million). Detailed descriptions of the missions are given in Part II, and Part III provides the foundation for their selection.

[a]LEO, low Earth orbit; SSO, Sun-synchronous orbit; GEO, geostationary Earth orbit.
[b]Cloud-independent, high-temporal-resolution, lower-accuracy sea-surface temperature measurement to complement, not replace, global operational high-accuracy sea-surface temperature measurement.

NOAA for implementation. For less mature applications, such as earthquake forecasting and mitigation models, the committee recommends obtaining new surface-deformation observations early in the decade to accelerate tool improvements. Observations of this type, which are more research-oriented, are recommended to NASA for implementation.

In setting the mission timing, the committee also considered mission costs relative to what it considered reasonable future budgets, technology readiness, and the potential of international missions to provide alternative sources of select observations. Rough cost estimates and technology readiness information for proposed missions were provided to the committee by NASA or culled from available information on current missions. The committee decided not to include possible cost sharing by international partners because such relationships are sometimes difficult to quantify. Cost sharing could reduce significantly the U.S. costs of the missions.

Given the relatively large uncertainties attached to cost and technology-readiness estimates, the committee chose to sequence missions among three broad periods in the next decade, namely, 2010-2013, 2013-2016, and 2016-2020. Missions seen to require significant technology development—such as high-power, multifrequency lasers for three-dimensional winds and aerosol and ozone profiling, and thin-array microwave antennas and receivers for temperature and water vapor soundings—were targeted for either the middle or late periods of the next decade; the exact placement depended on the perceived scientific and forecasting impact of the proposed observations (see Chapter 2).

Large uncertainties are also associated with attempts to factor international partner missions into the timing of U.S. missions during the next decade. For example, at the beginning of the next decade, there are international plans for GCOM-C (2011) and EarthCARE (2012), missions that are aimed at observing aerosol and clouds. As a result, the committee targeted for a later time a U.S. mission to explore cloud and aerosol interactions. The European Space Agency's Earth Explorer program has recently selected six mission concepts for Phase A studies, from which it will select one or two for launch in about 2013. All of the Phase A study concepts carry potential value for the broader Earth science community and provide overlap with missions recommended by this committee. Accordingly, the committee recognizes the importance of maintaining flexibility in the NASA observing program to leverage possible international activities, either by appropriate sequencing of complementary NASA and international partner missions or by exploring possible combinations of appropriate U.S. and internationally developed instruments on various launch opportunities.

The set of recommended missions listed in Tables ES.1 and ES. 2 reflects an integrated, cohesive, and carefully sequenced mission plan that addresses the range of urgent societal benefit areas. Although the launch order of the missions represents, in a practical sense, a priority order, it is important to recognize that the many factors involved in developing the mission plan preclude such a simple prioritization (see discussion in Chapter 3 and decision strategies summarized in Box ES.2).

The missions recommended for NASA do not fit neatly within the existing structure of the systematic mission line (i.e., strategic and/or continuous measurements typically assigned to a NASA center for implementation) and the Earth System Science Pathfinder (ESSP) mission line (i.e., exploratory measurements that are competed community-wide). The committee considers all of the recommended missions to be strategic in nature, but recognizes that some of the less complex and less technically challenging missions could be competed rather than assigned. The committee notes that historically the broader Earth science research community's involvement in space-borne missions has been almost exclusively in concert with various implementing NASA centers. Accordingly, the committee advises NASA to seek to implement the recommended set of missions as part of one strategic program, or mission line, using both competitive and noncompetitive methods to create a timely and effective program.

The observing system envisioned here will help to establish a firm and sustainable foundation for Earth science and associated societal benefits in the year 2020 and beyond. It can be achieved through effective management of technology advances and international partnerships, and through broad use of space-based science data by the research and decision-making communities. In looking beyond the next decade, the committee recognizes the need to learn from implementation of the 17 recommended missions *and* to efficiently move select research observations to operational status. These steps will create new space-based observing opportunities, foster new science leaders, and facilitate the implementation of revolutionary ideas. With those objectives in mind, the committee makes the following recommendation:

Recommendation: **U.S. civil space agencies should aggressively pursue technology development that supports the missions recommended in Tables ES.1 and ES.2; plan for transitions to continue demonstrably**

BOX ES.2 PROGRAMMATIC DECISION STRATEGIES AND RULES

Leverage International Efforts

- Restructure or defer missions if international partners select missions that meet most of the measurement objectives of the recommended missions; then (1) through dialogue establish data-access agreements, and (2) establish science teams to use the data in support of the science and societal objectives.
- Where appropriate, offer cost-effective additions to international missions that help extend the values of those missions. These actions should yield significant information in the identified areas at substantially less cost to the partners.

Manage Technology Risk

- Sequence missions according to technological readiness and budget risk factors. The budget risk consideration may favor initiating lower-cost missions first. However, technology investments should be made across all recommended missions.
- Reduce cost risk on recommended missions by investing early in the technological challenges of the missions. If there are insufficient funds to execute the missions in the recommended time frames, it is still important to make advances on the key technological hurdles.
- Establish technology readiness through documented technology demonstrations before a mission's development phase, and certainly before mission confirmation.

Respond to Budget Pressures and Shortfalls

- Delay downstream missions in the event of small (~10 percent) cost growth in mission development. Protect the overarching observational program by canceling missions that substantially overrun.
- Implement a system-wide independent review process that permits decisions regarding technical capabilities, cost, and schedule to be made in the context of the overarching science objectives. Programmatic decisions on potential delays or reductions in the capabilities of a particular mission could then be evaluated in light of the overall mission set and integrated requirements.
- Maintain a broad research program under significantly reduced agency funds by accepting greater mission risk rather than descoping missions and science requirements. Aggressively seek international and commercial partners to share mission costs. If necessary, eliminate specific missions related to a theme rather than whole themes.
- *In the event of large budget shortfalls,* re-evaluate the entire set of missions in light of an assessment of the current state of international global Earth observations, plans, needs, and opportunities. Seek advice from the broad community of Earth scientists and users and modify the long-term strategy (rather than dealing with one mission at a time). Maintain narrow, focused operational and sustained research programs rather than attempting to expand capabilities by accepting greater risk. Limit thematic scope and confine instrument capabilities to those well demonstrated by previous research instruments.

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useful research observations on a sustained, or operational, basis; and foster innovative space-based concepts. In particular:

- **NASA should increase investment in both mission-focused and cross-cutting technology development to decrease technical risk in the recommended missions and promote cost reduction across multiple missions. Early technology-focused investments through extended mission Phase A studies are essential.**
- **To restore more frequent launch opportunities and to facilitate the demonstration of innovative ideas and higher-risk technologies, NASA should create a new Venture class of low-cost research and application missions (~$100 million to $200 million). These missions should focus on fostering revolutionary innovation and on training future leaders of space-based Earth science and applications.**

- **NOAA should increase investment in identifying and facilitating the transition of demonstrably useful research observations to operational use.**

The Venture class of missions, in particular, would replace and be very different from the current ESSP mission line, which is increasingly a competitive means for implementing NASA's strategic missions. Priority would be given to cost-effective, innovative missions rather than those with excessive scientific and technological requirements. The Venture class could include stand-alone missions that use simple, small instruments, spacecraft, and launch vehicles; more complex instruments of opportunity flown on partner spacecraft and launch vehicles; or complex sets of instruments flown on suitable suborbital platforms to address focused sets of scientific questions. These missions could focus on establishing new research avenues or on demonstrating key application-oriented measurements. Key to the success of such a program will be maintaining a steady stream of opportunities for community participation in the development of innovative ideas, which requires that strict schedule and cost guidelines be enforced for the program participants.

TURNING SATELLITE OBSERVATIONS INTO KNOWLEDGE AND INFORMATION

Translating raw observations of Earth into useful information requires sophisticated scientific and applications techniques. The recommended mission plan is but one part of this larger program, all elements of which must be executed if the overall Earth research and applications enterprise is to succeed. The objective is to establish a program that is effective in its use of resources, is resilient in the face of the evolving constraints within which any program must operate, and is able to embrace new opportunities as they arise. Among the key additional elements of the overall program that must be supported to achieve the decadal vision are (1) sustained observations from space for research and monitoring, (2) surface-based and airborne observations that are necessary for a complete observing system, (3) models and data assimilation systems that allow effective use of the observations to make useful analyses and forecasts, and (4) planning and other activities that strengthen and sustain the Earth observation and information system.

Obtaining observations that serve the full array of science and societal challenges requires a hierarchy of measurement types, ranging from first-ever exploratory measurements to long-term, continuous measurements. Long-term observations can be focused on scientific challenges (sustained observations) or on specific societal applications (operational measurements). There is connectivity between sustained research observations and operational systems. Operational systems perform forecasting or monitoring functions, but the observations and products that result, such as weather forecasts, are also useful for many research purposes. Similarly, sustained observations, although focused on research questions, clearly include an aspect of monitoring and may be used operationally. While exploratory, sustained, and operational measurements often share the need for new technology, careful calibration, and long-term stability, there are also important differences among them; exploratory, sustained, and operational Earth observations are distinct yet overlapping categories.

An efficient and effective Earth observation system requires a continuing interagency evaluation of the capabilities and potential applications of numerous current and planned missions for transition of fundamental science missions into operational observation programs. *The committee is particularly concerned about the lack of clear agency responsibility for sustained research programs and the transitioning of proof-of-concept measurements into sustained measurement systems.* To address societal and research needs, both the quality and the continuity of the measurement record must be ensured through the transition of short-term, exploratory capabilities into sustained observing systems. Transition failures have been exhaustively described in previous reports,[14] whose recommendations the present committee endorses.

The elimination from NPOESS of requirements for climate research-related measurements is only the most recent example of the nation's failure to sustain critical measurements. The committee notes that despite NASA's involvement in climate research and its extensive development of measurement technology to make climate-quality measurements, the agency has no requirement for extended measurement missions, except for ozone measurements, which are explicitly mandated by Congress. **The committee endorses the recommendation of a 2006**

[14]NRC, *From Research to Operations in Weather Satellites and Numerical Weather Prediction: Crossing the Valley of Death,* National Academy Press, Washington, D.C., 2000, and NRC, *Satellite Observations of the Earth's Environment: Accelerating the Transition of Research to Operations,* The National Academies Press, Washington, D.C., 2003.

National Research Council report that stated, "NASA/SMD [Science Mission Directorate] should develop a science strategy for obtaining long-term, continuous, stable observations of the Earth system that are distinct from observations to meet requirements by NOAA in support of numerical weather prediction."[15]

The committee is concerned that the nation's civil space institutions (including NASA, NOAA, and USGS) are not adequately prepared to meet society's rapidly evolving Earth information needs. These institutions have responsibilities that are in many cases mismatched with their authorities and resources: institutional mandates are inconsistent with agency charters, budgets are not well matched to emerging needs, and shared responsibilities are supported inconsistently by mechanisms for cooperation. These are issues whose solutions will require action at high levels of the federal government. Thus, the committee makes the following recommendation:

Recommendation: **The Office of Science and Technology Policy, in collaboration with the relevant agencies and in consultation with the scientific community, should develop and implement a plan for achieving and sustaining global Earth observations. This plan should recognize the complexity of differing agency roles, responsibilities, and capabilities as well as the lessons from implementation of the Landsat, EOS, and NPOESS programs.**

The space-based observations recommended by the committee will provide a global view of many Earth system processes. However, satellite observations have limited spatial and temporal resolution and hence do not alone provide a picture of the Earth system that is sufficient for understanding all of the key physical, chemical, and biological processes. In addition, satellites do not directly observe many of the changes in human societies that are affected by, or will affect, the environment. To build the requisite knowledge for addressing urgent societal issues, data are also needed from suborbital and land-based platforms, as well as from socio-demographic studies. The committee finds that greater attention is needed to the entire chain of observations from research to applications and benefits. Regarding complementary observations, the committee makes the following recommendations:

Recommendation: **Earth system observations should be accompanied by a complementary system of observations of human activities and their effects on Earth.**

Recommendation: **Socioeconomic factors should be considered in the planning and implementation of Earth observation missions and in developing an Earth knowledge and information system.**

Recommendation: **Critical surface-based (land and ocean) and upper-air atmospheric sounding networks should be sustained and enhanced as necessary to satisfy climate and other Earth science needs in addition to weather forecasting and prediction.**

Recommendation: **To facilitate the synthesis of scientific data and discovery into coherent and timely information for end users, NASA should support Earth science research via suborbital platforms: airborne programs, which have suffered substantial diminution, should be restored, and unmanned aerial vehicle technology should be increasingly factored into the nation's strategic plan for Earth science.**

Myriad steps are necessary for providing quantitative information, analyses, and predictions for important geophysical and socioeconomic variables over the range of needed time scales. The value of the recommended missions can be realized only through a high-priority and complementary focus on modeling, data assimilation, data archiving and distribution, and research and analysis.[16] To this end, the committee makes the following recommendations:

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[15]NRC, "A Review of NASA's 2006 Draft Science Plan: Letter Report," The National Academies Press, Washington, D.C., 2006, p. iv.

[16]NASA's research and analysis (R&A) program has customarily supplied funds for enhancing fundamental understanding in a discipline and stimulating the questions from which new scientific investigations flow. R&A studies also enable conversion of raw instrument data into fields of geophysical variables and are an essential component in support of the research required to convert data analyses to trends, processes, and improvements in simulation models. They are likewise necessary for improving calibrations and evaluating the limits of both remote and in situ data. Without adequate R&A, the large and complex task of acquiring, processing, and archiving geophysical data would go for naught. Finally, the next generation of Earth scientists—the graduate students in universities—are often educated by performing research that has originated in R&A efforts. See NRC, *Earth Observations from Space: History, Promise, and Reality (Executive Summary)*, National Academy Press, Washington, D.C., 1995.

Recommendations:

- **Teams of experts should be formed to consider assimilation of data from multiple sensors and all sources, including commercial providers and international partners.**
- **NOAA, working with the Climate Change Science Program and the international Group on Earth Observations, should create a climate data and information system to meet the challenge of ensuring the production, distribution, and stewardship of high-accuracy climate records from NPOESS and other relevant observational platforms.**
- **As new Earth observation missions are developed, early attention should be given to developing the requisite data processing and distribution system, and data archive. Distribution of data should be free or at low cost to users, and provided in an easily accessible manner.**
- **NASA should increase support for its research and analysis (R&A) program to a level commensurate with its ongoing and planned missions. Further, in light of the need for a healthy R&A program that is not mission-specific, as well as the need for mission-specific R&A, NASA's space-based missions should have adequate R&A lines within each mission budget as well as mission-specific operations and data analysis. These R&A lines should be protected within the missions and not used simply as mission reserves to cover cost growth on the hardware side.**
- **NASA, NOAA, and USGS should increase their support for Earth system modeling, including provision of high-performance computing facilities and support for scientists working in the areas of modeling and data assimilation.**

SUSTAINING AN EARTH KNOWLEDGE AND INFORMATION SYSTEM

A successful Earth information system should be planned and implemented around long-term strategies that encompass the life cycle from research to operations to applications. The strategy must include nurturing an effective workforce, informing the public, sharing in the development of a robust professional community, ensuring effective and long-term access to data, and much more. An active planning process must be pursued that focuses on effectively implementing the recommendations for the next decade as well as sustaining and building the knowledge and information system beyond the next decade.

Recommendation: **A formal interagency planning and review process should be put into place that focuses on effectively implementing the recommendations made in the present decadal survey report and sustaining and building an Earth knowledge and information system for the next decade and beyond.**

The training of future scientists who are needed to interpret observations and who will turn measurements into knowledge and information is exceedingly important. To ensure that effective and productive use of data is maximized, resources must be dedicated to an education and training program that spans a broad range of communities. A robust program that provides training in the use of these observations will result in highly varied societal benefits, including improved weather forecasts, more effective emergency management, better land-use planning, and so on.

Recommendation: **NASA, NOAA, and USGS should pursue innovative approaches to educate and train scientists and users of Earth observations and applications. A particularly important role is to assist educators in inspiring and training students in the use of Earth observations and the information derived from them.**

5.7 Exploring Organic Environments in the Solar System

A Report of the Ad Hoc Task Group on Organic Environments in the Solar System

Executive Summary

The sources, distributions, and transformations of organic compounds throughout the solar system are being studied actively. The results can provide information about the evolution of the solar system and about possibilities for life elsewhere in the universe. All life on Earth is based on the complex interplay of diverse carbon compounds. In short, the chemistry of carbon is the chemistry of life. But carbon is extremely versatile. Its compounds can be synthesized in many different ways and from a wide variety of starting materials, the vast majority of which have nothing to do with biology. The chemical reactions involving carbon can be driven by many different sources of energy and can occur in diverse environments, many of which are inimical to life as we understand it. Many carbon compounds are extremely hardy, and their preservation in the geological record can tell researchers much about processes and environmental conditions in the distant past. Similarly, other carbon compounds are extremely fragile. The presence of organic compounds in various astronomical environments can tell researchers much about the conditions that prevail today.

The discovery of a single drop of oily residue on Mars, for example, would be enormously informative even if the residue were irrefutably abiotic in origin. Some might argue that such a discovery would be like finding an encyclopedia from a Mars library, which would tell linguists so much about the inhabitants even if they could not translate it. To recover the information carried by extraterrestrial carbon compounds, researchers must improve their ability to recognize the signals that point to specific syntheses and conditions.

PURPOSE AND APPROACH OF THIS REPORT

The purpose of this report is to tell the story of carbon: to follow carbon through a variety of terrestrial and extraterrestrial environments, to track its changes as it is subjected to a variety of physical and chemical processes, and to attempt to convey what the study of carbon and its compounds tells us about the origin and evolution of the solar system. In particular, the Task Group on Organic Environments in the Solar System surveys what is known about the sources of reduced carbon compounds throughout the solar system and examines how planetary exploration can improve our understanding. It is not the purpose of this report to recommend expensive new research activities and propose costly new initiatives. Rather, the task group's goal is to place a variety of disparate activities in a unified context. As part of this process, the task group considers a number of closely related questions, including the following:

1. What are the sources of reactants and energy that lead to abiotic synthesis of organic compounds and to their alteration in diverse solar system environments?
2. What are the distribution and history of reduced carbon compounds in the solar system, and which features of that distribution and history, or of the compounds themselves, can be used to discriminate among synthesis and alteration processes?
3. What are the criteria that distinguish abiotic from biotic organic compounds?
4. What aspects of the study of organic compounds in the solar system can be accomplished from ground-based studies (theoretical, laboratory, and astronomical), Earth orbit, and planetary missions (orbiters, landers, and sample return), and which new capabilities might have the greatest impact on each?

The task group found it most convenient and logical to address the third question first. The reason for this approach is simple. The principal features that distinguish biotic and abiotic carbon compounds are closely related to the physical and chemical characteristics of organic compounds. Thus, these distinguishing criteria are elaborated in the context of a general introduction to organic chemistry in Chapter 1. With regard to the indicators that

NOTE: "Executive Summary" reprinted from *Exploring Organic Environments in the Solar System,* The National Academies Press, Washington, D.C., 2007, pp. 1-8.

might differentiate between a biotic and an abiotic origin for particular organic compounds, the task group found that the most compelling indicators of an abiotic origin include the following:

- The presence of a smooth distribution of organic compounds in a sample, e.g., a balance of even versus odd numbers of carbon atoms in alkanes;
- The presence of all possible structures, patterns, isomers, and stereoisomers in a subset of compounds such as amino acids;
- A balance of observed entantiomers; and
- The lack of depletions or enrichments of certain isotopes with respect to the isotopic ratio normally expected.

Likewise, the converse of the above items is an indicator of possible biotic synthesis. Thus, for example, an imbalance of even versus odd numbers of carbon atoms in, for example, alkanes or the presence of only a small subset of all possible structures, patterns, isomers, and stereoisomers is an indicator of possible biotic origin. However, some abiotic processes can mimic biotic ones and vice versa, and inferences will necessarily be based on several indicators and will of course be probabilistic.

The answers to the first two questions—sources of reactants and energy that lead to abiotic synthesis and the distribution of organic compounds in the solar system—depend strongly on what part of the solar system is being considered. This report therefore deals separately with the various solar system environments—which range from the surfaces of cold, dark asteroids in remote, eccentric orbits to the hot, turbulent atmospheres of the giant gas planets. It considers what is known about the origins and histories of the organic materials in each setting. This discussion is contained in Chapters 2 through 6 of this report.

The fourth question, research opportunities, is addressed in each of those chapters as well. In addition, Chapter 7 outlines two general strategies recommended by the task group as integral to a planned approach to searching for and understanding organic material in the solar system.

RECOMMENDED RESEARCH

In selecting the best research opportunities for enhancing understanding of organic material in the solar system, the task group considered the following factors:

1. The likelihood that significant organic material would be found;
2. The feasibility of the investigation; and
3. The likely impact or significance of the results.

The recommendations and a brief rationale are given below. A detailed discussion is presented in Chapters 2 through 6 of this report.

Overall Approach to Research

Two recommendations are better characterized as general strategies rather than specific opportunities:

Recommendation: Strategy 1—Every opportunity should be seized to increase the breadth and detail in inventories of organic material in the solar system. As results accumulate, each succeeding investigation should be structured to provide information that will allow improved comparisons between environments. Analyses should determine abundance ratios for the following:

- Compound classes (e.g., aliphatic, aromatic, acetylenic);
- Individual compounds (e.g., methane/ethane);
- Elements in organic material (e.g., C/H/N/O/S); and
- The isotopes of elements such as C, H, N, and O.

Investigators should strive to interpret these results in terms of precursor-product relationships.

These objectives are broadly applicable and represent systematic steps toward addressing questions of biogenicity, lines of inheritance of organic material, and mechanisms of synthesis. With limited funds, returns from investigations like those proposed below (in "Selected Opportunities for Research") will move NASA more smoothly toward ultimate success. For example, the task group proposes that newer, more sensitive, and specific analytical methods be used for the analysis and reanalysis of carbonaceous chondrites. As these studies proceed and the results from flight experiments are obtained, it will become apparent which of these new techniques should be adapted to flight experiments. Moreover, the ground-based investigations of chondrites will pave the way for better analyses of returned samples, whenever they become available.

Recommendation: Strategy 2—Organic-carbon-related flight objectives should be coordinated across missions and structured to provide a stepwise accumulation of basic results. Some of the objectives that should be included in such missions are as follows:

- Quantitation of the amount of organic carbon present to ±30 percent precision and accuracy over a range of 0.1 parts per million to 1 percent;
- Repetitive analyses of diverse samples at each landing site;
- Comparability so that relatable data are obtained from a wide range of sites; and
- Elemental and isotopic analyses so that the composition (H/C, N/C, O/C, and S/C) is obtained together with the isotope ratios of all the carbon-bearing phases.

These recommended approaches to research will allow scientists to build an overview of the distribution of organic carbon in the solar system; provide information about heterogeneity at each location studied; and support preliminary estimates of relationships, if any, between organic materials at diverse sites.

Selected Opportunities for Research

The selected research opportunities were divided by the task group into three general categories based on the cost of the research and the time frame in which it could be undertaken. The recommended research is given by category below.

Near-Term Opportunities

The first category of research—near-term opportunities—includes ground-based studies that can be carried out in the very near term and for a minimal cost relative to the other recommended research activities.

Chondritic and Mars Meteorites. Carbonaceous meteorites are an important source of abiotic, extraterrestrial carbon that is delivered to Earth at no cost. Together with the unequilibrated ordinary chondrites, a few martian meteorites, and fragments of crust from the earliest Earth, they represent immediately available samples of great relevance to studies of organic material in the solar system. New analyses of carbonaceous chondrites would benefit from modern analytical methods (e.g., compound-specific isotopic analysis) that allow the separation of signals from terrestrial contamination and indigenous extraterrestrial organic matter, thus overcoming a problem that severely hindered analyses throughout the 1960s and 1970s. A more sensitive and detailed analysis of carbonaceous chondrites is a cost-effective step that would be of great value in enhancing understanding of the formation of these organic materials and, therefore, yielding new information about organic-chemical processes in the early solar system. The results would provide reference points for comparison with the organics in samples returned by missions to other bodies in the solar system. Analyses should examine the following:

- The location and relative abundances of the organic molecules within the mineral matrices and on mineral surfaces;
- The structural composition of all organic phases including, to the greatest extent possible, any macromolecular material;
- The isotopic compositions of all molecules and other definable subfractions; and
- The nature of contaminants and the mechanisms by which samples can become contaminated, both before and after collection.

Recommendation: Plans should be developed for the establishment of an informal, community-based forum—modeled on the highly successful Mars Exploration Program Analysis Group (MEPAG)—charged to coordinate plans and develop priorities for the intensive investigation of the composition of organic materials in carbonaceous chondrites, SNC meteorites, and ordinary chondrites containing volatiles (including rare gases) that suggest relationships to the carbonaceous chondrites. The existing Curation and Analysis Planning Team for Extraterrestrial Materials (CAPTEM) may provide the seed from which such a community-based forum can be nurtured.

To provide comparability and to bring the best techniques to bear on each object, samples should be shared extensively between laboratories.

Martian Regolith Simulation. It has been proposed that any organic matter in the martian regolith will have been modified via reaction with strong oxidants present in the soil. Carefully designed laboratory experiments will allow an assessment of this problem and will point to the most effective strategies for direct analysis of organic materials by future Mars landers such as the Mars Science Laboratory. Regolith simulations may help address issues related to, for example, optimal minimum drilling depths for future Mars lander missions.

Recommendation: Laboratory models of Mars soil chemistry should be used to study plausible mechanisms for the oxidative alteration of organic materials in the martian regolith and to evaluate their integrated effects. Materials studied should include likely exogenous products (organic compounds like those found in meteorites) as well as conceivable martian prebiotic and biotic products.

Increasing the Supply of Meteorites Available for Study. The ready availability of and access to meteorites for laboratory studies, particularly the rare carbonaceous chondrites, is a key facet of the exploration of organic environments in the solar system. The preferred means for acquiring samples—collecting them in the field—has led to major searches in those places where meteorites are most likely to be spotted, the hot and cold deserts of the world. Both locations have their advantages and disadvantages; a detailed cost-benefit analysis of all of the relevant factors is beyond the scope of this report. There is, however, another approach to increasing the supply of meteorites: the selective purchase or exchange of important samples. Indeed, the task group suggests that the greatest near-term scientific impact from a given expenditure of funds will result not from the enhancement of meteorite collecting programs but rather from the acquisition by purchase or exchange of a significant piece of the Tagish Lake meteorite.

Recommendation: The scientific significance of the Tagish Lake meteorite is such that NASA, the National Science Foundation, the Smithsonian Institution, and other relevant organizations and agencies in the United States and their counterparts in Canada should examine the means by which a significant portion of this fall can be acquired, by purchase, exchange, or some other mechanism, so that samples can be made more widely available for study by the scientific community.

Laboratory Studies to Support Observations of Primitive Bodies. Laboratory studies are a prerequisite for all observational studies and are an essential precursor to the design of inherently expensive spacecraft instrumentation. At present, the relevant optical constants have been measured for only a few of the organic and inorganic compounds that are likely to be present in primitive bodies of interest. Without a suite of materials with known constants to incorporate in the spectral models, the identification of many of the observed spectral features remains challenging. With modest support for laboratory work of this kind, great progress could be made in understanding the organic component in these bodies.

Recommendation: The physical, chemical, and spectroscopic properties of ices of potential hydrocarbon species should be studied to facilitate the detection of organic materials.

Support for Telescope Studies of Organic Materials in the Solar System. Access to a small number of unique, publicly available, ground-based infrared astronomical facilities has enhanced and will continue to advance understanding of the organic constituents of various solar system bodies through direct observations and through observations conducted in support of spacecraft missions. Activities that would significantly enhance ground-based observations of organic materials in the solar system include increasing NASA's share of the observing time on the Keck telescope and replacing the NASA Infrared Telescope Facility with a larger instrument capable of making these observations.

Recommendation: The task group reiterates the call made in the 2003 report of the National Research Council's Solar System Exploration Decadal Survey Committee, *New Frontiers in the Solar System,* that NASA's support for planetary observations with ground-based astronomical instruments, such as the Infrared Telescope Facility and the Keck telescopes, be continued and upgraded as appropriate, for as long as they provide significant scientific return and/or mission-critical support services.[1]

Interplanetary Dust and Molecules. Present particle-collection programs utilize aircraft and flights with other primary missions, and schedules are controlled by factors other than the timing of meteor showers. Quantitative yields and the ranges of materials sampled could be greatly improved if flights were timed to utilize these opportunities.

Recommendation: A program specifically designed to collect dust in the stratosphere during meteor showers should be implemented.

Relatively Near-Term Missions Consistent with Previous Decadal Strategy Study

Recommendations in the category of relatively near-term missions are for research that can be implemented or carried out in 5 to 10 years and that is also supported by the findings and recommendations of the 2003 solar system exploration decadal survey report, *New Frontiers in the Solar System*.[2]

Mars. On Earth, the most suitable lithologies for the preservation and accumulation of organic matter are sedimentary rocks that are typically fine-grained and are characterized by well-defined, aqueously derived mineral assemblages. Thus, it may be possible to obtain additional information about the associated organic matter present in these mineral assemblages in a single measurement of the organic and inorganic material present.

Recommendation: Currently planned missions to Mars should seek to identify silicified martian terrains associated with ancient low-temperature hot springs in concert with a high probability of ground ice deposits to locate organic materials formed on Mars. Similarly, the identification of shallow marine and/or lacustrine sediments would provide another terrain well worth exploring in future missions as sites for martian endogenous organosynthesis.

As instrument development continues for future robotic missions to Mars, it is important that such missions be designed so that they are capable of assessing as fully as possible the inventory of organic matter there. Clearly such development should be strongly guided by the information provided by the Mars Exploration Rovers, Spirit and Opportunity.

Although future robotic missions will be equipped with instrumentation to analyze samples, these analyses will never be able to achieve the capabilities of Earth-based laboratories. The discovery by the Mars Exploration Rovers of unambiguous sedimentary outcrops greatly increases the impetus for a martian sample-return mission. Similarly, the discovery of the halogens bromine and chlorine in abundance at the location of the Spirit rover landing site strongly suggests the former presence of surface water. Samples from either location might very well contain organic matter derived from extinct (or perhaps even extant) life. The successes of Spirit and Opportunity further validate the need to implement the 2003 solar system exploration decadal survey's recommendation for a flagship mission to Mars—that is, to begin the developments necessary so that martian samples can be brought back to Earth for study in terrestrial laboratories as early as possible in the next decade.[3]

Far-Term Research Opportunities

The far-term research recommended by the task group would probably be carried out 10 years or more in the future but might require some near-term planning. This recommended research is ranked in terms of its potential for expanding knowledge of carbon compounds in the solar system and for its close relationship to research and missions currently in progress or recently completed.

Titan. Titan is believed to be a major reservoir of organic materials in the solar system, and the dynamic processes of Titan's atmospheric chemistry provide an ongoing example of the abiotic formation of complex organics from methane. This satellite merits close scrutiny by continued ground-based observation and computer and laboratory modeling of its atmospheric chemistry.

Recommendation: Planning should start now for a follow-up of the Cassini mission to Titan that would include a lander sent to sample its surface, since the complexity of the organics there is expected to be much greater than that of the organics in its atmosphere. The lander should have the capability of sampling organic materials that are solids at 96 K as well as those that are liquids. The Titan Explorer mission considered by the solar system exploration decadal survey is a good starting point for this planning.

Primitive Bodies. The successful landing of the NEAR spacecraft on the asteroid Eros has demonstrated the feasibility of sending a probe to an asteroid. The solar system exploration decadal survey report recommended in situ and sample-return missions to asteroids and comets to provide direct information on the structures of the organic compounds present in comets and asteroids and to provide information about whether or not the asteroids are the sources of meteorites and dust reaching Earth.

Recommendation: In situ analyses as well as sample-return missions should be performed for both asteroids and comets. The task group points to the solar system exploration decadal survey report's recommended New Frontiers-class Comet Surface Sample Return mission[4] as an example of an activity that would greatly enhance understanding of the organic constituents of the solar system's primitive bodies.

Current and upcoming missions are targeted to active comets; Pluto/Charon and perhaps one or two Kuiper Belt objects; and asteroids of spectral classes S, G, and V. Most of these missions have not been optimized for the study of organic materials even though the population of primitive small bodies may preserve organic materials from a wide range of nebular heliocentric distances. A rich research opportunity exists to explore these different chemical and thermal regimes, thus enabling an understanding of the distribution and history of organic materials in the solar system.

Recommendation: Every opportunity should be taken to direct space missions to small bodies to do infrared spectral studies of these targets, especially a D- or P-type asteroid, to determine if these dark bodies contain an appreciable amount of carbon compounds and, if so, whether they are the sources of the carbonaceous meteorites and dust reaching Earth.

In this regard, a possible opportunity is conducting such studies as an adjunct to the Trojan Asteroid/Centaur Reconnaissance flyby mission described in the solar system exploration decadal survey.[5] Although this mission was not ranked in the survey's final list of priorities, the possibility of using a single spacecraft to make a sequential flyby of three different classes of primitive bodies—i.e., a D- or P-type main-belt asteroid, a jovian Trojan asteroid, and a Centaur—has sufficient merit to warrant additional study for possible implementation as a New Frontiers mission at some time in the future.

Europa, Callisto, and Ganymede. In the early 2000s, NASA's solar system exploration plans included a Europa Orbiter mission that would undertake flyby observations of Callisto and Ganymede prior to entering orbit about Europa. Although excessive cost growth led to the cancellation of this mission, scientific interest in the study of Jupiter's large, icy satellites continues to be strong. The Europa Geophysical Explorer, a somewhat more elaborate version of the Europa Orbiter, was the highest-priority large mission recommended by the 2003 solar system exploration decadal survey.[6] NASA responded to the survey's recommendation by initiating the development of the Jupiter Icy Moons Orbiter (JIMO) mission, the first of a line of advanced-technology spacecraft with significantly expanded science capabilities compared to previous concepts for missions to Europa. JIMO would have conducted global mapping of all three icy satellites, at resolutions of 10 m or better, and might have included a small Europa lander. Organic materials can be studied by making provisions for high-signal-to-noise-ratio spectroscopy at resolutions adequate to discriminate potential carbon-bearing species in both high- and low-albedo regions. JIMO was indefinitely deferred in 2005, and NASA and the planetary science community are currently assessing plans for a more conventional and very much less expensive alternative.[7]

Recommendation: The task group reiterates the solar system exploration decadal survey's findings and conclusions with respect to the exploration of Europa and recommends that NASA and the space science community develop a strategy for the development of a capable Europa orbiter mission and that such a mission be launched as soon as it is financially and programmatically feasible. Any future Europa lander mission should be equipped with a mass spectrometer capable of identifying simple organic materials in a background of water and hydrated silicates.

NOTES

1. National Research Council (NRC), *New Frontiers in the Solar System: An Integrated Exploration Strategy*, The National Academies Press, Washington, D.C., 2003, pp. 206-207.
2. NRC, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, 2003.
3. NRC, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, 2003, pp. 198-200.
4. NRC, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, 2003, p. 195.
5. NRC, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, 2003, p. 25.
6. NRC, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, 2003, p. 4.
7. NRC, *Priorities in Space Science Enabled by Nuclear Power and Propulsion*, The National Academies Press, Washington, D.C., 2006, pp. 17-20.

5.8 Grading NASA's Solar System Exploration Program: A Midterm Review

A Report of the Ad Hoc Committee on Assessing the Solar System Exploration Program

Executive Summary

The Committee on Assessing the Solar System Exploration Program has reviewed NASA's progress to date on implementing the recommendations made in the National Research Council's (NRC's) 2003-2013 solar system exploration decadal survey, *New Frontiers in the Solar System*,[1] and in its Mars architecture report, *Assessment of NASA's Mars Architecture 2007-2016*.[2] For each individual recommendation in these two reports the committee assessed NASA's progress and assigned an academic-style grade, explained the rationale for the grade and trend, and offered recommendations for improvement. The committee generally sought to develop recommendations in cases where it determined that the grade, the trend, or both were worrisome and that achievement of the decadal survey recommendation would require some kind of corrective action on NASA's part. This usually meant that the committee sought to offer a recommendation when the grade was a "C" or lower. However, the committee did offer recommendations in connection with some higher grades where it believed minor corrective action was possible and desirable. More importantly, the committee did not offer recommendations for some lower grades, particularly in the enabling technologies area (Chapter 6), because the committee determined that only the restoration of funding and the development of a strategic technology development program would solve these problems.

The general meanings of the assigned grades are as follows:

A	Achieved or exceeded the goal established in the decadal survey.
B	Partially achieved the decadal goal, or made significant progress.
C	Some progress toward meeting the decadal goal, or achieved a supporting objective.
D	Little progress toward meeting the decadal goal.
F	Regressed or made no progress toward meeting the decadal goal.
Withdrawn	Goal or objective dropped.
Incomplete	Unable to make an assessment due to lack of data, inconclusive decision process, or other factors.

In addition to a grade for progress to date, for each recommendation in the two NRC reports cited above, the committee assessed the direction of progress:

Trend: ↑	Improving.
Trend: ↓	Getting worse.
Trend: →	No change.

The committee's results are organized into five categories: (1) science goals and objectives; (2) flight missions; (3) Mars; (4) research and analysis (R&A), planetary astronomy, and mission data analysis programs; and (5) enabling technologies.

In making its assessments, the committee considered the recommendations of the two reports as essentially sacrosanct and made virtually no allowances for circumstances that might have led to a less than satisfactory grade, although the committee does acknowledge that the decadal survey in particular was limited in its ability to adequately predict the cost and complexity of some missions (such as Venus in situ measurements and Comet sample return). The committee did consider, in remarking on the rationale for the grade and in recommending remedial measures, what these circumstances might have been. Because this is a "mid-term" assessment, NASA still has the

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NOTE: "Executive Summary" reprinted from the prepublication version of *Grading NASA's Solar System Exploration Program: A Midterm Review*, The National Academies Press, Washington, D.C., 2008, pp. 1-10; approved for release in 2007.

[1]National Research Council, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, The National Academies Press, Washington, D.C., 2003.

[2]National Research Council, *Assessment of NASA's Mars Architecture 2007-2016,* The National Academies Press, Washington, D.C., 2006.

FIGURE ES.1 The Mars Science Laboratory entering Mars' atmosphere in 2010. SOURCE: Jet Propulsion Laboratory.

ability to significantly improve these grades before the next decadal survey is produced. However, the committee also notes that the situation could get considerably worse, and the current overall trend is alarming.

Although this report mentions a number of proposed but not yet funded missions—some of which are currently under evaluation by NASA—the committee is not endorsing specific mission proposals, especially when those proposals are being made in the competitive evaluation process that NASA uses for its Discovery, Mars Scout, and New Frontiers programs.

In the formal letter from NASA to the Space Studies Board requesting the study, NASA asked for recommendations not only for NASA itself, but also for the next decadal survey starting in 2008 (and expected to last 2 years). The committee noted that during its study, NASA undertook four studies of possible flagship missions to Europa, Titan, the Jupiter system (focusing on Ganymede), and Saturn's moon Enceladus. These kinds of studies are vital to the decadal survey process and help establish a baseline of mission options for future solar system exploration plans. The committee encourages NASA to conduct such future studies. The committee also encourages NASA to allow more time and to provide more resources for support of the mission concept and cost-estimation process for the next decadal survey. These surveys achieve their maximum utility when informed by credible cost-estimates for all potential missions, not just the flagship category.

OVERALL SUMMARY FOR SOLAR SYSTEM EXPLORATION

Grade: B
Trend: ↓

Halfway into the 2003-2013 decade covered by the decadal survey *New Frontiers in the Solar System,* NASA has made significant progress toward implementing the recommendations of the decadal survey and the Mars architecture report. The current planetary exploration program is highly productive, carrying out exciting missions and making fundamental discoveries.

However, the committee awarded a downward trend arrow because the committee concluded that this progress is unlikely to continue at the present rate, and that *on its current course NASA will not be able to fulfill the recom-*

mendations of the decadal survey. The reasons for this are reduced investment in research, data analysis, technology development, and smaller mission programs, coupled with increasing mission costs, overruns on approved flight projects, and spiraling launch vehicle costs. The committee weighted these areas more than others and notes that these are all areas that are required for further progress to continue. The trends in these individual areas mean that future progress toward fulfilling the recommendations of the decadal survey is unlikely. NASA has also made insufficient investment in vital infrastructure such as the Deep Space Network. The committee also notes that NASA has failed to start the Europa mission that was the highest-priority mission recommended by the decadal survey. In addition, NASA has neglected work on the Mars Sample Return mission, particularly technology development. Although the agency indicates that this situation may change, the committee notes that only significant progress can erase skepticism about the prospects in this area.

Yesterday's investments have created a momentum that will carry the program for a few more years before the consequences of today's reductions become apparent. *The future of the nation's solar system exploration program as laid out in the decadal survey for 2003-2013 is in jeopardy unless NASA makes an effort to improve the situation.*

SUMMARY OF KEY ELEMENTS

A summary of the committee's assessment of the Planetary Science Division program's key programmatic elements from the decadal survey and Mars architecture follows.

Science Questions **Grade: B** **Trend: →**

In many respects, NASA has done a good job of meeting the science goals outlined in the decadal survey. The agency should be commended for this progress. However, there is one disturbing note in this area. Only in the Mars program has there been progress toward the recommendation addressing whether life exists (or did exist) beyond Earth. The Mars program has an integrated strategy for addressing this science goal. However, the funding reductions for astrobiology research and technology development have had serious and very adverse impacts on addressing this goal throughout the solar system exploration program. The science goals in the decadal survey, and NASA's progress in addressing them to date, are summarized in Table ES.1.

TABLE ES.1 Science Questions and Progress

Crosscutting Themes and Key Questions[a]	Grade	Trend
The First Billion Years of Solar System History		
1. What processes marked the initial stages of planet and satellite formation?	B	↑
2. How long did it take the gas giant Jupiter to form, and how was the formation of the ice giants (Uranus and Neptune) different from that of Jupiter and its gas giant sibling, Saturn?	C	↑
3. How did the impactor flux decay during the solar system's youth, and in what way(s) did this decline influence the timing of life's emergence on Earth?	B	→
Volatiles and Organics: The Stuff of Life		
4. What is the history of volatile compounds, especially water, across the solar system?	A	↑
5. What is the nature of organic material in the solar system and how has this matter evolved?	B	↑
6. What global mechanisms affect the evolution of volatiles on planetary bodies?	B	→
The Origin and Evolution of Habitable Worlds		
7. What planetary processes are responsible for generating and sustaining habitable worlds, and where are the habitable zones in the solar system?	A	↓
8. Does (or did) life exist beyond Earth?	C	↓
9. Why have the terrestrial planets differed so dramatically in their evolutions?	A	↑
10. What hazards do solar system objects present to Earth's biosphere?	B	↑
Processes: How Planetary Systems Work		
11. How do the processes that shape the contemporary character of planetary bodies operate and interact?	B	↓
12. What does the solar system tell us about the development and evolution of extrasolar planetary systems, and vice versa?	B	→

[a]Reprinted from National Research Council, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, The National Academies Press, Washington, D.C., 2003, p. 30.

Flight Missions **Grade: B** **Trend: ↓**

There are some troubling indicators in the flight missions area. The launch rate for missions in all size categories is lower than envisioned in the decadal survey. This is especially true in the Discovery program. This low-cost, community-driven flight program is key to maintaining the pipeline of data returned from the solar system and is essential to the training of new mission scientists and students, ongoing efforts vital for a thriving solar system exploration community. If NASA approves the start of a Europa flagship mission, and approves two new Discovery missions and a New Frontiers mission, the committee believes that this trend will reverse. However, the current lack of approval of a Europa flagship mission, plus the lack of new Discovery mission opportunities, has led the committee to assess the trend for flight missions as downward at this time. The committee is also concerned about various pressures on future missions, including increasing launch costs and the lack of technologies required to accomplish the other missions recommended in the decadal survey.

The committee recognizes that the primary reason—although by no means the only reason—that the launch rate in all mission categories is lower is the shortage of funding. Increasing the launch rate would require more money, which NASA is unlikely to receive. The agency will be forced to make hard choices in this area.[3]

The flight missions, including Mars missions, recommended in the decadal survey and NASA's progress in implementing them to date are summarized in Table ES.2.

Mars **Grade: A** **Trend: →**

NASA's Mars Exploration Program (MEP), which was redesigned in 2000, has been highly successful to date and appears on track through the end of the current decade. Both the Mars Science Laboratory (in 2009) and a 2011 Scout mission that will be selected soon meet the recommendations of the decadal survey. A key element of the success of this program is that it is not a series of isolated missions, but rather a highly integrated set of strategically designed missions, each building on the discoveries and technology of the previous missions and fitting into long-term goals to understand the planet, whether or not it ever had or does now have life, and how Mars fits into the origin and evolution of terrestrial planets. The strategic scientific thread thus far has been to "follow the water" on Mars: its history, amount, form, and location. A new thread is emerging to "follow the carbon," "follow the organics," or "find the life," which can only be accomplished if astrobiology instruments and capabilities become available. The committee assesses the Mars Exploration Program for the period 2000-2010 as meriting an "A."

However, the recommendations of the decadal survey and other NRC Mars reviews focus on the period out to 2017 and occasionally beyond. For those years, the program has suffered from a lack of technological progress toward a sample return mission, lack of a commitment to a landed network mission, and indecision on the 2016 and 2018 launch opportunities.

As this document was being prepared, NASA presented to the committee new tentative Mars plans that the agency designated the "Ideal Mars Next Decade Campaign." This new mission queue, described in detail in Chapter 4, appears to address most of the issues raised in the NRC reports cited, except for the landed network. The "Ideal Campaign" commits the Mars Exploration Program to a sample return mission "anchored in 2020" (with actual samples being returned some years later) and using the Astrobiology Field Laboratory as the prime sample collection mission. The committee is cautiously optimistic about this approach, while emphasizing that it should be subjected to rigorous community review once it has been further investigated. The committee believes that major changes in the Mars Exploration Program should not be made if they contradict what is recommended in the decadal survey; otherwise such changes would effectively render the entire decadal survey process irrelevant. Extensive community involvement is a major factor in the success of the Mars Exploration Program.

The committee was also disappointed to learn that NASA was simultaneously suggesting that Mars missions after the 2011 Scout mission might be abandoned with only the promise of a Mars Sample Return in 2020, but no clear investment or programmatic path to make it happen. At least some Mars missions during this period would have to be selected via the New Frontiers competition, thereby jeopardizing the strategic planning approach that has served the Mars Exploration Program and science community so well.

[3]The committee notes that the flight rate was also affected by policy and management choices. For instance, the expenditure of significant amounts of money on the Jupiter Icy Moons Orbiter mission, which was canceled because of its high price tag, prevented the effective start of a flagship mission that would have met the recommendation of the decadal survey.

TABLE ES.2 Flight Missions and Implementation

Flight Missions	Recommendation[a]	Status	Grade	Trend
"Large" flagship Missions				
"Large" flagship missions overall	One per decade	None yet to date.	D	→
Europa Explorer	Start Europa mission	Under extensive study, no new start to date		
"Medium" New Frontiers Missions				
"Medium" New Frontiers missions overall	3-4 per decade	One launched, one in development, new AO imminent	B	→
Kuiper Belt/Pluto Explorer	Top priority	New Horizons mission launched	A	→
Jupiter Polar Orbiter with Probes	Third priority	JUNO orbiter selected w/o probes	A	→
South Pole Aitken Basin Sample Return	Second priority	Option for next AO	—	—
Comet Surface Sample Return	Fifth priority	Option for next AO	—	—
Venus In Situ Explorer	Fourth priority	Option for next AO	—	—
"Small" Discovery Missions				
"Small" Discovery missions overall Both full missions and missions of opportunity determined by competition	One launch every 18 months	No full mission selected in 5 years; two missions of opportunity selected	D	→
Mars Exploration Program				
Mars Exploration Program overall			A	→
Mars Science Laboratory 2009	Conduct Mars Science Laboratory	In development for 2009	B	→
Mars Science and Telecom Orbiter 2013	Conduct Mars Science Orbiter	Planned for 2013, science still under definition	A	↑
Mars Astrobiology Field Laboratory	Option for 2016	Instrument development required	A	→
Mars Mid-rovers	Option for 2016	Option for 2016	A	→
Mars Long-lived Lander Network	Option for 2016	Option for 2016	A	→
Mars Scouts	One launch every 26 months	Phoenix launch 2007, selection for 2011 imminent	A	↑
Mars Sample Return	Start technology development for Mars Sample Return	Progress spotty on enabling technology and no recent systematic mission planning, but recent signs that this will change	C	↓

NOTE: AO, Announcement of Opportunity.
[a]Recommendations summarized from National Research Council, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, The National Academies Press, Washington, D.C., 2003, and National Research Council, *Assessment of NASA's Mars Architecture 2007-2016,* The National Academies Press, Washington, D.C., 2006.

Research and Analysis, Planetary Astronomy, Flight Mission Data Analysis Grade: C Trend: ↓

The committee is deeply concerned with both the grade and the trend in the area of research and analysis, planetary astronomy, and flight mission data. The grade is driven by falling investment in fundamental science and two failing grades in planetary astronomy. Research and analysis funding is essentially the "seed corn" that helps to define future missions and carry them out, and serious cutbacks in this area have harmed NASA's ability to conduct future solar system exploration. Most importantly, the committee has serious concerns about the current and projected funding levels for the research and analysis program in the Planetary Science Division, with particular concern for astrobiology, resulting in the assessment of a downward trend. The problems in planetary astronomy reflect NASA's lack of participation in the Large Synoptic Survey Telescope (LSST) and its inattention to the decadal survey recommendation regarding the ability of the James Webb Space Telescope to track moving objects in the solar system.

Enabling Technologies **Grade: D** **Trend: ↓**

NASA's investment portfolio in technology development at the beginning of the decadal period appeared adequate and well structured to meet the needs projected by the decadal survey. However, severe reductions in funding since that time now pose a serious risk to enabling future flight missions. Of special concern is the lack of investment in aerocapture, optical communications, spacecraft autonomy, advanced avionics, instrument miniaturization, in situ sample gathering, handling, and analysis, autonomous mobility, and ascent vehicles. In reviewing missions under consideration, or even in the active planning stages for the next 5 years and beyond, it is clear that without a considerable, sustained investment in technology development, much of the technical risk of those missions cannot be reduced to levels that would instill confidence about mission success. In addition, NASA is encouraged to proceed to implementation with its plan for upgrading and revamping the Deep Space Network and to work aggressively to deal with the impending crisis in launch vehicles brought on by the planned retirement of the Delta II rocket, the spiraling costs of launch services, and uncertainty about the future availability of appropriately sized launch vehicles for smaller missions.

NASA has made progress in science and flight missions, but there is a clear threat to meeting the goals of the *New Frontiers in the Solar System* report and the Mars architecture report over the next 5 years. To repeat the committee's primary finding: **on its current course NASA will not be able to fulfill the recommendations of the *New Frontiers in the Solar System* decadal survey.**

The committee offers the following recommendations toward redressing these problems.

RECOMMENDATIONS

The committee considers its findings to be represented in the grades, trends, and explanations for each subject area. The committee developed recommendations for those subjects that it felt were in particular need of corrective action (or, in some instances, where corrective action could be applied relatively easily).

Chapter 2, Science

Recommendation: The next decadal survey should address the objectives and merits of a Neptune/Triton mission.

Recommendation: NASA should return Astrobiology Science and Technology Instrument Development funding and Astrobiology Science and Technology for Exploring Planets funding back to at least their individual Planetary Instrument Definition and Development levels. However, this should not be accomplished to the detriment of the astrobiology research and analysis program, which has already suffered large cutbacks.

Chapter 3, Flight Missions

Recommendation: To ensure that flagship mission costs do not negatively impact missions in other cost classes, NASA should apply sufficient resources to obtain good cost estimates in the earliest phases and rigorously review mission costs before selection.

Recommendation: NASA should continue studying possible flagship missions to both the inner and the outer planets as input to the next decadal study.

Recommendation: NASA should select a Europa mission concept and secure a new start for the project before 2011.

Recommendation: NASA should increase the rate of selection and launch of New Frontiers missions.

Recommendation: The New Frontiers missions should follow a two-stage development process, starting with (1) an opportunity to submit a proposal for funding for 1 or 2 years to develop mission concepts. This earlier stage would provide for some endorsement of the best ideas so that they can attract industry and NASA center support.

Such support, in turn, would (2) allow more concepts to reach a level of maturity required for considering full-scale proposal development.

Recommendation: NASA should select two of the three Discovery missions currently in phase-A studies (if two are sufficiently meritorious to be selectable) and should seek to achieve an 18-month period between selections for the rest of the decade. These steps can help to restore vitality to this important program.

Recommendation: NASA should return to conducting Senior Reviews once every 2 years to improve efficiency.

Chapter 4, Mars

Recommendation: NASA should begin actively planning for Mars Sample Return, including precursor missions that identify and cache well-characterized samples of both geological and biological interest.

Recommendation: NASA should begin consulting various groups such as MEPAG and the astrobiology/exobiology research community to assess the current state-of-the-art in laboratory analysis instruments, identify where further development would be beneficial for Mars sample analysis and biosignature detection, and verify that the needed instruments, laboratory facilities, and new researcher training will be made available as part of the sample-handling facility as soon as samples are returned.

Recommendation: NASA should begin robust technology investment aimed at reducing the risk associated with the four major engineering challenges of a successful Mars Sample Return, that is, the definition, design, and development of:

1. A Mars sample receiving facility that can serve to certify the samples as safe for distribution;
2. A sample return vehicle that can provide a high probability of successful sample return to Earth consistent with the NASA Planetary Protection Officer's and Committee on Space Research (COSPAR) guidelines;
3. Autonomous on-orbit rendezvous and docking capability at Mars for sample transfer and return; and
4. A Mars ascent vehicle that is capable of being transported to Mars, landing, and returning cached samples to Mars orbit.

Recommendation: NASA should take all the scientific, programmatic, and technical information available and make a decision on a mission queue that includes the 2016 and 2018 Mars launch opportunities.

Recommendation: NASA should seek community review to carefully scrutinize the new Mars architecture and its budget implications to ensure that the value of the sample returned is worth the cost to the Mars Exploration Program.

Chapter 5, Research and Analysis

Recommendation: NASA should restore an adequate funding level for astrobiology research, based on consultation with the scientific community, that will lead to the achievement of the goals of the *New Frontiers in the Solar System* decadal survey. NASA should provide a stable and sustainable funding environment that is adequate to ensure the vitality and continued scientific productivity of all its research and analysis programs.

Recommendation: NASA should continue to work to more completely integrate astrobiology into all solar system science disciplines.

Recommendation: NASA should improve the visibility of its Fellowships for Early Career Researchers program and advertise it as a postdoctoral program. NASA should also expand the participating research program areas to include origins of solar systems, as well as all appropriate space mission data analysis programs.

Recommendation: NASA should establish formal contacts with the Large Synoptic Survey Telescope project.

Recommendation: NASA should incorporate into the James Webb Space Telescope as quickly as possible the capability to track moving solar system objects.

Recommendation: NASA Announcements of Opportunity should require each space mission proposal to explicitly estimate and budget for archiving activities.

Recommendation: NASA should consider encouraging principal investigators to offer archival data sets in their initial proposal, so that the review panels can assess their desirability.

Chapter 6, Enabling Technologies

Recommendation: NASA should develop a strategic plan for technology development and infusion independent of flight programs. In addition, NASA should restore funding to its New Millennium program.

Recommendation: NASA should conduct a study of the trade-offs of the cost versus risk of developing a Ka-band array system to handle the required transmissions (uplink and downlink) and determine whether optical communications are required for data delivery during the 2013-2023 time frame. Prior to the next decadal survey, NASA should present the results of such a study to the science community.

Recommendation: NASA should make an assessment of which technologies will be required for Mars Sample Return and conduct an independent assessment of the analogous technology needs for the Moon, Venus, asteroids, and other targets.

Recommendation: NASA should fund the Small Aperture Receive Array for the Deep Space Network and plan to replace the 70-meter antennas with arrays of small-diameter antennas by 2015.

5.9 The Limits of Organic Life in Planetary Systems

A Report of the Ad Hoc Committee on the Limits of Organic Life in Planetary Systems of the Committee on the Origins and Evolution of Life

Executive Summary

Reflecting the near inevitability of human missions to Mars and other locales in the solar system where life might exist, and given the interest of the public in the question, Are we alone?, the National Aeronautics and Space Administration (NASA) commissioned the National Research Council, which formed the Committee on the Limits of Organic Life in Planetary Systems, to address the following questions:

- What can be authoritatively said today about limits of life in the cosmos?
- What Earth-based research must be done to explore those limits so that NASA missions would be able to recognize, conserve, and study alien life that is encountered?

Theory, data, and experiments suggest that life requires (in decreasing order of certainty):

- A thermodynamic disequilibrium;
- An environment capable of maintaining covalent bonds, especially between carbon, hydrogen, and other atoms;
- A liquid environment; and
- A molecular system that can support Darwinian evolution.

Earth abundantly displays life that uses solar, geothermal, and chemical energy to maintain thermodynamic disequilibria, covalent bonds between carbon, water as the liquid, and DNA as a molecular system to support Darwinian evolution. Life with those characteristics can be found wherever water and energy are available.

The natural tendency toward terracentricity[1] requires that we make a conscious effort to broaden our ideas of where life is possible and what forms it might take. The long history of terran chemistry tempts us to become fixated on carbon because terran life is based on carbon. But basic principles of chemistry warn us against terracentricity. It is easy to conceive of chemical reactions that might support life involving noncarbon compounds, occurring in solvents other than water, or involving oxidation-reduction reactions without dioxygen.

The committee found no compelling reason to limit the environment for life to water as a solvent, even if life is constrained to use carbon as the scaffolding element for most of its biomolecules. In water, a wide array of molecular structures are conceivable that could (in principle) support life but be so different from those for life on Earth that they would be overlooked by unsophisticated life-detection tools. Evidence suggests that Darwinian processes require water, or a solvent like water, if they are supported by organic biopolymers (such as DNA). Although macromolecules that use silicon are known, few thoughts suggest how they might have emerged spontaneously to support a biosphere.

Many of the definitions of *life* include the phrase *undergoes Darwinian evolution*. The implication is that phenotypic changes and adaptation are necessary to exploit unstable environmental conditions, to function optimally in the environment, and to provide a mechanism to increase biological complexity. The canonical characteristics of life are inherent capacities to adapt to changing environmental conditions and to increase in complexity by multiple mechanisms, particularly by interactions with other living organisms.

One of the apparent generalizations that can be drawn from knowledge of Earth life is that lateral gene transfer is an ancient and efficient mechanism for rapidly creating diversity and complexity. The unity of biochemistry among all Earth's organisms emphasizes the ability of organisms to interact with other organisms to

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NOTE: "Executive Summary" reprinted from *The Limits of Organic Life in Planetary Systems,* The National Academies Press, Washington, D.C., 2007, pp. 1-4.

[1] The committee uses the term *terran* to denote a particular set of biological and chemical characteristics that are displayed by all life on Earth. Thus "Earth life" has the same meaning as "terran life" when the committee is discussing life on Earth, but if life were discovered on Mars or any other nonterrestrial body, it might be found to be terran or nonterran, depending on its characteristics.

form coevolving communities, to acquire and transmit new genes, to use old genes in new ways, to exploit new habitats, and, most important, to evolve mechanisms to help to control their own evolution. Those characteristics are likely to be present in extraterrestrial life even if it has had a separate origin and a very different unified biochemistry from that of Earth life.

Because we have only one example of biomolecular structures that solve problems posed by life and because the human mind finds it difficult to create ideas truly different from what it already knows, it is difficult for us to imagine how life might look in environments very different from what we find on Earth. Recognizing the challenges in mitigating that difficulty, the committee chose instead to embrace it. In constructing its outlook, it exploited a strategy that began by characterizing the terran life that humankind has known well, first because of its macroscopic visibility and then through microscopic observation that began in earnest 4 centuries ago.

As the next step in its strategic process, the committee assembled a set of observations about life that is considered exotic when compared with human-like life. The committee asked, Can we identify environments on Earth where Darwinian processes exploiting human-like biochemistry cannot exploit available thermodynamic disequilibria? The answer to that question is an only slightly qualified no. It appears that wherever the thermodynamic minimum for life is met on Earth and water is present, life is found. Furthermore, the life that is found appears to be descendent from an ancestral life form that also served as the ancestor of humankind (we might not have recognized it if its ancestry were otherwise), and it exploits fundamentally human-like biochemistry. The committee reviewed evidence about abiotic processes that manipulate organic material in a planetary environment. It asked whether the molecules that we see in contemporary terran life might be understood as the inevitable consequences of abiotic reactivity.

The committee then surveyed the inventory of environments in the solar system and asked which ones might be suitable for life of the terran type. The survey made clear that most locales in the solar system are at thermodynamic disequilibrium, an absolute requirement for chemical life, and that many locales at thermodynamic disequilibrium also have solvents in liquid form and environments where the covalent bonds between carbon and other lighter elements are stable. Those are weaker requirements for life, but the three together appear, perhaps simplistically, to be sufficient for life. The committee asked whether it could conceive of a biochemistry adapted to those exotic environments, much as human-like biochemistry is adapted to terran environments. Because few detailed hypotheses are available, the committee reviewed what is known, or might be speculated, and considered research directions that might expand or constrain understanding about the possibility of life in such exotic environments. Finally, the committee considered more exotic solutions to problems that must be solved to create the emergent properties that we agree characterize life.

The committee found that using thermal and chemical energy to maintain thermodynamic disequilibria, covalent bonds between carbon atoms, water as the liquid, and DNA as a molecular system to support Darwinian evolution is not the *only* way to create phenomena that would be recognized as life. Indeed, the emerging field of synthetic biology has already provided laboratory examples of alternative chemical structures that support genetics, catalysis, and Darwinian evolution. Organic chemistry offers many examples of useful chemical reactivity in nonwater liquids. Macromolecular structures reminiscent of those found in terran biology can be formed with silicon and other elements.

Accordingly, the committee identified high-priority Earth-based laboratory and field studies aimed at doing the following:

- Explore the limits of life on Earth, with an emphasis on detection of life in extreme environments that might have chemical structures and metabolisms different from those of terran life that has already been characterized.
- Pursue the origin of life, especially on the basis of information from NASA missions, the inventory of organic materials in the cosmos, and interactions between organic materials and minerals set in a planetary context.
- Contribute basic research to understand interactions of organic and inorganic species in exotic solvents, including water under extreme conditions (as found on Venus, Mars, Europa, Enceladus, and elsewhere), water-ammonia eutectics at low temperatures (as might be possible on Titan), and liquid cryosolvents (as found on Triton and elsewhere).
- Contribute to laboratory synthetic-biology research into molecular systems that are capable of Darwinian evolution but are different from standard DNA and RNA, especially those designed to improve understanding of the chemical possibilities of supporting Darwinian evolution.

The committee offers the following recommendations:

Recommendation 1. The National Aeronautics and Space Administration and the National Science Foundation should support these kinds of laboratory research:

- Origin-of-life studies, including prebiotic-chemistry and directed-evolution studies that address physiologies different from those of known organisms;
- Further studies of chirality, particularly studies focused on the hypothesis that specific environmental conditions can favor chiral selection, or on an alternative model that life with L-amino acids and D-sugars is better "fit," from an evolutionary perspective, to evolve into complex organisms; and
- Work to understand the environmental characteristics that can affect the ability of organisms to fractionate key elements, including not only carbon but also sulfur, nitrogen, iron, molybdenum, nickel, and tungsten.

Recommendation 2. The National Aeronautics and Space Administration and the National Science Foundation should support these kinds of field research:

- A search for remnants of an RNA world in extant extremophiles that are deeply rooted in the phylogenetic tree of life;
- A search for organisms with novel metabolic and bioenergetic pathways, particularly pathways involved in carbon dioxide and carbon monoxide reduction and methane oxidation coupled with electron acceptors other than oxygen;
- A search for organisms that derive some of their catalytic activity from minerals rather than protein enzymes;
- A search for organisms from environments that are limited in key nutrients, including phosphorus and iron, and determination of whether they can substitute other elements, such as arsenic, for phosphorus;
- A search for life that can extract essential nutrients—such as phosphorus, iron, and other metals—from rocks, such as pyrites and apatite;
- A search for anomalous gene sequences in conserved genes, particularly DNA- and RNA-modifying genes;
- Study of the resistance of microorganisms that form biofilms on minerals to the harsh conditions of interplanetary transport; and
- A search for life that stores its heredity in chemicals other than nucleic acids.

Recommendation 3. The National Aeronautics and Space Administration should support these kinds of space research:

- Programs that combine the exploration of potential metabolic cycles with the synthetic biology of unnatural nucleic acid analogues and their building blocks and that use the results to guide the design of instruments;
- Astrobiology measurements that can potentially distinguish between life on Mars (and possibly other bodies) that arrived via material ejected from Earth (or vice versa) and life that emerged on another body independently of life on Earth;
- Inclusion in missions planned for Mars of instruments that detect lighter atoms, simple organic functional groups, and organic carbon to help distinguish between "replicator-first" and "metabolism-first" theories of the origin of life; similar considerations should guide inclusion of small-organic-molecule detectors that could function on the surfaces of Europa, Enceladus, and Titan; and
- Consideration, in view of the discovery of evidence of liquid water-ammonia eutectics on Titan and active water geysers on Saturn's moon Enceladus, of whether the planned missions to the solar system should be reordered to permit returning to Titan or Enceladus earlier than is now scheduled.

5.10 NASA's Beyond Einstein Program: An Architecture for Implementation

A Report of the Ad Hoc Committee on NASA's Einstein Program: An Architecture for Implementation

Executive Summary

BACKGROUND

"Beyond Einstein science" is a term that applies to a set of new scientific challenges at the intersection of physics and astrophysics. Observations of the cosmos now have the potential to extend our basic physical laws beyond where 20th-century research left them. Such observations can provide stringent new tests of Einstein's general theory of relativity, indicate how to extend the Standard Model of elementary-particle physics, and—if direct measurements of gravitational waves were to be made—give astrophysics an entirely new way of observing the universe. New physical understanding may be required to explain cosmological observations, and the challenge of investigating the laws of physics using astronomical techniques promises to bring higher precision, clarity, and completeness to many astrophysical investigations relating to galaxies, black holes, and the large-scale structure of the universe, among other areas.

In 2003, NASA, working with the astronomy and astrophysics communities, prepared a research roadmap entitled *Beyond Einstein: From the Big Bang to Black Holes*.[1] This roadmap proposed that NASA undertake space missions in five areas in order to study dark energy, black holes, gravitational radiation, and the inflation of the early universe, and to test Einstein's theory of gravitation. Two of the five planned mission areas were Einstein Great Observatories: Constellation-X (Con-X) and the Laser Interferometer Space Antenna (LISA). The other three were planned as smaller Einstein Probes: the Black Hole Finder Probe (BHFP), the Inflation Probe (IP), and the Joint Dark Energy Mission (JDEM). Candidate missions for all of these mission areas are currently in various stages of definition and development.

Prompted by congressional language inserted in the formulation of the fiscal year (FY) 2007 budget, NASA and the Department of Energy (DOE) asked the National Research Council (NRC) to prepare a report reviewing NASA's Beyond Einstein Program. The NRC appointed the Committee on NASA's Beyond Einstein Program to carry out this study. The report was to assess the five proposed Beyond Einstein mission areas and recommend one mission area for first development and launch utilizing a Beyond Einstein Program funding wedge[2] that will start in FY 2009. To accomplish this task, the committee assessed all five mission areas, using criteria that address both potential scientific impact and technical readiness. In addition, the report was to assess each mission in sufficient detail to provide input for decisions by NASA and for the NRC's next astronomy and astrophysics decadal survey regarding both the ordering of the remaining missions and the investment strategy for future technology development within the Beyond Einstein Program. In responding to this latter charge, the committee has attempted to indicate what next steps each of the missions would need to take in order to prepare for future assessments.

MISSION ASSESSMENTS

The criteria adopted by the committee in assessing the missions fell into two general categories. First, the committee looked at the potential scientific impact within the context of other existing and planned space-based and ground-based missions. Here the committee considered how directly each assessed mission would address the research goals of the Beyond Einstein research program, likely contributions to the broader field of astrophysics,

NOTE: "Executive Summary" reprinted from *NASA's Beyond Einstein Program: An Architecture for Implementation,* The National Academies Press, Washington, D.C., 2007, pp. 1-8.

[1]National Aeronautics and Space Administration, *Beyond Einstein: From the Big Bang to Black Holes*, Washington, D.C., January 2003. This document was part of NASA's 2003 roadmapping effort required under the Government Performance Results Act of 1993 (Public Law No. 103-62).

[2]NASA's FY 2007 budget request projected NASA's level of support for Beyond Einstein missions covering the years FY 2007 through FY 2011. This projection begins to increase significantly in FY 2009 and continues to increase through FY 2010 and FY 2011. The projected increase is identified in the report as the "Beyond Einstein funding wedge."

the potential for revolutionary scientific discovery, the scientific risks and readiness of the mission, and its competition from other ground- and space-based instruments.

Second, the committee considered the realism of preliminary technology and management plans and of cost estimates. Criteria used by the committee included plans for the maturity of critical mission technology, technical performance margins, schedule margins, risk-mitigation plans, and the proposal's estimated costs versus independent probable cost estimates prepared by the committee.

The committee made its recommendations on the basis of the above criteria, but during its deliberations it identified several policy-related issues relevant to the Beyond Einstein Program. These issues included implications for U.S. science and technology leadership, program funding constraints, relationships in interagency and international partnerships, investments in underlying research and technology and supporting infrastructure, and the impact of International Traffic in Arms Regulations. The committee reviewed these issues in order to understand the broader context of the study.

Using the criteria described above, the committee performed extensive assessments for each mission. It is impossible to adequately summarize here all of the points that factored into the final mission selection. Rather, each of the missions reviewed by the committee is briefly described below, along with a summary of a few of the major points from the committee's assessment.

Science Impact and Technology Readiness

Black Hole Finder Probe (BHFP)

The two Black Hole Finder Probe mission concepts presented to the committee are called EXIST (Energetic X-ray Imaging Survey Telescope) and CASTER (Coded Aperture Survey Telescope for Energetic Radiation). Both of these concepts use wide-field coded-aperture hard x-ray telescopes, divided into arrays of subtelescopes at two different energy bands. With their arrays of subtelescopes, either would survey the entire sky between a few kiloelectronvolts and 600 keV during the course of their 95 minute orbits, providing information about source variability on time scales ranging from milliseconds to many days.

Science Importance and Readiness BHFP is designed to find black holes on all scales, from one to billions of solar masses. It will observe high-energy x-ray emission from accreting black holes and explosive transients and will address the question of how black holes form and grow.

BHFP will be unique among current or planned missions in high-energy x-ray sensitivity combined with large field of view and frequent coverage of the sky. The resulting hard x-ray sky maps, temporal variability data, and the large number of short-lived transient detections will have a direct impact on a number of important astrophysical questions. BHFP will provide a unique window into the properties and evolution of astronomical objects whose physics is dominated by strong gravity.

The committee found the science risk for BHFP mission candidates to be rather high. Although a census of massive black holes in galaxies can be achieved, only very-high-luminosity and very-high-mass black holes will be seen at high redshifts. In addition, the very uncertain conversion from x-ray luminosity to black hole growth rate implies that BHFP will not provide a unique value (to better than a factor of 10) of the black hole growth rate (e.g., in solar masses per year) in any individual galaxy or even in the entire universe. Finally, the difficulty in identifying host galaxies also yields significant risk in the interpretation of BHFP results. Both multiwavelength observational data and theoretical advances (e.g., in black hole accretion modeling) will be necessary in order for BHFP to realize its full scientific potential.

Technology Readiness The two BHFP mission candidates differ primarily in their selection of detector material. CASTER faces more technology maturity challenges, as the detector technology in general is at lower Technology Readiness Levels (TRLs) than that of EXIST, as discussed in Chapter 3 of this report. The estimated costs for both mission concepts are higher than projected in the original Beyond Einstein roadmap: there the Einstein Probes had been envisioned as medium-scale missions that could be executed much more rapidly and cheaply than the flagship LISA and Con-X missions. However, the BHFP probe concepts now have costs that the mission teams estimate are in the vicinity of a billion dollars. This report's independent assessment (Chapter 3) also finds probable costs inconsistent with the original Einstein Probe cost range. The committee suggests that judicious trade-offs among sensitivity, detector area, and observing time may enable a smaller telescope to carry out the most important BHFP science at lower cost.

Constellation-X (Con-X)

The Constellation-X mission has been designed to be a general-purpose astrophysical observatory. Its primary new capability is very-high-spectral-resolution, high-throughput x-ray spectroscopy, representing an increase in these capabilities of roughly two orders of magnitude over missions currently flying.

Science Importance and Readiness Con-X will make the broadest and most diverse contributions to astronomy of any of the candidate Beyond Einstein missions. The committee understands that Con-X has the potential to make strong contributions to Beyond Einstein science through the study of the evolution of supermassive black holes and the mapping of the dynamics of clusters of galaxies. However, other Beyond Einstein missions will address both the measurement of dark energy parameters and tests of strong-field general relativity in a more focused and definitive manner and, as a result, the committee did not choose Con-X as one of the highest priorities for Beyond Einstein funding. The committee concluded that the merits of Con-X can be fully assessed only when it is judged as a major astrophysics mission in a context broader than that of the Beyond Einstein Program. Given that Con-X was ranked second only to the James Webb Space Telescope in the NRC's 2001 astronomy and astrophysics decadal survey, *Astronomy and Astrophysics in the New Millennium*,[3] NASA's characterization of it as a Beyond Einstein mission understates its significance to general astronomy.

Technology Readiness Con-X is one of the best studied and tested of the missions presented to the committee. Aside from the well-known risks of satellite implementation, a number of technical risks have been called out by the Con-X mission team and are discussed in Chapter 3. Chief among these is the achievement of the needed mirror angular resolution and the development of the position-sensitive microcalorimeters. The Con-X team has reasonable plans to mature both of these technologies, and, given adequate resources and time, there is little reason to expect that these technologies will limit the main science goals of the observatory.

Con-X development activities need to continue aggressively in areas such as achieving the mirror angular resolution, cooling technology, and x-ray microcalorimeter arrays to improve the Con-X mission's readiness for consideration in the next astronomy and astrophysics decadal survey. The committee, however, does not believe that the current Beyond Einstein funding wedge should fund these activities. Beyond Einstein is not the sole justification for Con-X, as its primary science capabilities support a much broader research program.

Inflation Probe (IP)

The Inflation Probe mission effort seeks to study for the first time the conditions that existed during the crucial phase of exponential expansion in the early history of the universe. Four IP mission concepts have been proposed to date. Three propose to study the signal impressed on the polarization of the cosmic microwave background (CMB) radiation by gravity waves induced during the inflationary period. The fourth proposes to measure the structure in the universe on various length scales, arising from the primordial density fluctuations induced by inflation.

Science Importance and Readiness Understanding inflation is an important goal of the Beyond Einstein Program. The exponential expansion during the era of inflation may have similarities with the much more slowly accelerating expansion occurring today that is attributed to the presence of dark energy. A deeper understanding of both inflation and dark energy is needed in order to explore that similarity. Studying inflation may also lead to understanding the source of the largest structures in the universe, which appear to be linked to quantum fluctuations and phenomena at the smallest scales. The theoretical framework for understanding the results of both the CMB and high-redshift galaxy observations is already in place.

[3]National Research Council, *Astronomy and Astrophysics in the New Millennium*, National Academy Press, Washington, D.C., 2001.

Technology Readiness One of the four mission concepts, the Cosmic Inflation Probe (CIP), has a mission design that is a modification of existing missions. Although the state of CIP technology is more advanced than that of the polarization missions, it would benefit from advances in grating technologies. NASA's Astrophysics Research Grants Program is already in place to fund these types of investigations. However, it should be noted that the scope of this program may need to be changed to accommodate aggressive IP development.

The three CMB polarization Inflation Probes collectively are in a stage of development earlier than that of the CIP. The three CMB proposals outline detector and instrument concepts that are extrapolations from existing experiments. The CMB polarization experiments—Experimental Probe of Inflationary Cosmology (EPIC-F), Einstein Polarization Interferometer for Cosmology (EPIC-I), and CMB Polarimeter (CMBPol)—all require extremely sensitive millimeter-wave continuum detectors and extremely effective rejection of the common-mode noise from the anisotropy signal. All three of these missions have proposed to use state-of-the-art detectors to reach the required high sensitivity. If the European Space Agency's (ESA's) Planck mission is successful, it will go a long way but not all the way toward proving the readiness of the detector technology. Along with the continued investment in grating technology required to continue to mature CIP, significant continued support of detector and ultracool cryocoolers (sub-100 mK) is needed to push the three polarization missions along. Given the missions' state of development, it is not necessary to provide direct technology development support to each of the mission teams. Although the state of CIP technology is more advanced than the technology of the polarization missions, CIP would benefit from intensive theoretical investigations as well as further refinement of grating technologies.

Joint Dark Energy Mission (JDEM)

The Joint Dark Energy Mission is a partner mission between NASA and DOE that would use an optical-to-near-infrared wide-field survey telescope to investigate the distribution of dark energy. Three concepts for a JDEM have thus far been proposed: the Supernova Acceleration Probe (SNAP), the Dark Energy Space Telescope (DESTINY), and the Advanced Dark Energy Physics Telescope (ADEPT).

Science Importance and Readiness Understanding the nature of dark energy is one of the most important scientific endeavors of this era. A central goal of JDEM is a precision measurement of the expansion history of the universe to determine whether the contribution of dark energy to the expansion rate varies with time. A discovery that the expansion history is not consistent with Einstein's cosmological constant would have a fundamental impact on physics and astronomy.

JDEM will significantly advance both dark energy and general astrophysical research. The wide-field optical and near-infrared surveys required for dark energy studies will create large, rich data sets useful for many other astrophysics studies, enlarging an already significant discovery potential. A full-sky, near-infrared spectroscopic survey, such as proposed by ADEPT, has never been performed, and no comparable mission is planned. This survey would open the emission-line universe, providing new probes of star formation during the epoch when galaxies grew, along with data for many other astrophysics studies. A low-background, wide-field imaging survey, such as proposed by DESTINY and SNAP, would provide a much larger diffraction-limited near-infrared survey than would be available otherwise. Such a survey would revolutionize the understanding of how and when galaxies acquire their mass, as well as providing copious data for many other astrophysics studies.

The principal JDEM science risk, common to many dark energy studies, arises from the need to control systematic uncertainties sufficiently to achieve significantly improved precision. Space measurements have the potential to control observational uncertainties better than ground techniques do, but the space techniques have not yet been demonstrated to the required levels. External systematic uncertainties of an astrophysical nature could conceivably prove irreducible during the mission lifetime. JDEM will try to mitigate both types of risk by employing multiple complementary observational techniques and by collecting rich data sets. Cross-checking with large, statistically significant data sets should help to sort out systematic trends in the data.

Technology Readiness As described in Chapter 3, two of the three candidate missions for JDEM—DESTINY and SNAP—are relatively mature, since most of their critical technologies are at TRLs of 5-6 or higher. (The SNAP charge-coupled devices are an exception at TRL 4-5, but there is a good plan to bring them to flight readiness.) The ADEPT team did not provide the committee with adequate data to evaluate readiness, but in general the team's critical technology has flight heritage and no major challenges.

Laser Interferometer Space Antenna (LISA)

The proposed Laser Interferometer Space Antenna is a gravitational-wave antenna. At the low frequencies where a rich variety of strong signals are expected to exist, gravity waves can be detected only from space. LISA will consist of an array of three spacecraft orbiting the Sun, each separated from its neighbor by about 5 million kilometers. Laser beams will be used to measure the minute changes in distance between the spacecraft induced by passing gravitational waves.

Science Importance and Readiness LISA promises to open a completely new window into the heart of the most energetic processes in the universe, with consequences fundamental to both physics and astronomy. During its proposed 5-year mission, LISA is expected to detect gravitational waves from the merger of massive black holes in the centers of galaxies or stellar clusters at cosmological distances and from stellar-mass compact objects as they orbit and fall into massive black holes. Studying these waves will allow researchers to trace the history of the growth of massive black holes and the formation of galactic structure, to test general relativity in the strong-field dynamical regime, and to determine if the black holes in nature are truly described by the geometry predicted in Einstein's theory. LISA will measure the signals from close binaries of white dwarfs, neutron stars, or stellar-mass black holes in the Milky Way and nearby galaxies. This will permit a census of compact binary objects throughout the Galaxy. There may also be waves from exotic or unexpected sources, such as cosmological backgrounds, cosmic string kinks, or boson stars. LISA will also be able to measure the speed of gravitational waves to very high precision, may study whether there are more than the two polarizations predicted by general relativity, and will be able to measure absolute distances to faraway objects.

Technology Readiness LISA has had considerable technology development since entering Phase A development in 2004 and has had a baseline mission architecture in place for some time. Nevertheless, a number of critical technologies and performance requirements must be developed and verified before LISA is technically ready to move into the implementation phase. Some critical technologies will be tested on the ESA-NASA LISA Pathfinder scheduled for launch in October 2009. Success of the Pathfinder is a prerequisite for LISA to proceed with implementation.

Not all of the critical LISA technologies and performance will be tested on the Pathfinder. Therefore, given the scientific importance of LISA, the committee strongly believes that a high priority for NASA's Beyond Einstein Program is to accelerate the maturation of those remaining LISA technologies not tested on Pathfinder. Candidates for this funding include the following: micronewton thruster technology development and lifetime tests; the point-ahead actuator; the phase measurement system; and laser frequency noise suppression. As discussed in this report, these were assessed to be at TRLs of 4 or less.

Cost Realism

The committee was asked to evaluate the cost realism of the candidate Beyond Einstein mission set. The committee worked with an experienced outside contractor to develop independent cost estimates and a probable cost range for each mission. The probable cost ranges were also compared with those of previous missions of similar scope and complexity. In all cases, the committee's assessment indicates higher costs and longer schedules than those estimated by the mission teams. This is typical of the differences between the estimates developed by mission teams and by independent cost estimators at this stage of a program. Given the long history of missions comparable to the Beyond Einstein mission candidates, the committee does believe that the most realistic cost range for each of these missions is significantly more than the current team estimates.

The committee also compared its most probable funding profiles with NASA's projected Beyond Einstein funding wedge. This analysis showed that the funding wedge alone is inadequate to develop any candidate Beyond Einstein mission on its nominal schedule. However, the committee used these data to indicate how the JDEM and LISA development and funding profiles could be adjusted to fit within NASA's funding wedge, given that DOE expects to cofund JDEM up to approximately $400 million[4] and ESA plans approximately $500 million for LISA.[5]

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[4]Kathy Turner, Program Manager, Office of High Energy Physics, Department of Energy, "Note to BEPAC Regarding DOE's JDEM Plans," e-mail communication, March 30, 2007.

[5]European Space Agency LISA budget data provided to the committee by David Southwood, ESA Director of Science, in discussions on ESA's Astrophysics and Fundamental Physics Program, April 5, 2007.

MAJOR FINDINGS AND RECOMMENDATIONS

In light of the considerations summarized above and described in considerably more detail in the body of this report, the committee developed the following major findings and principal recommendations. The findings are not listed in order of priority but rather in a sequence that conveys the committee's reasoning.

Finding 1. The Beyond Einstein scientific issues are so compelling that research in this area will be pursued for many years to come. All five mission areas in NASA's Beyond Einstein Program address key questions that take physics and astronomy beyond where the century of Einstein left them.

Finding 2. The Constellation-X mission will make the broadest and most diverse contributions to astronomy of any of the candidate Beyond Einstein (BE) missions. While Con-X can make strong contributions to Beyond Einstein science, other BE missions address the measurement of dark energy parameters and tests of strong-field general relativity in a more focused and definitive manner.

Finding 3. Two mission areas stand out for the directness with which they address Beyond Einstein goals and their potential for broader scientific impact: LISA and JDEM.

Finding 4. LISA is an extraordinarily original and technically bold mission concept that will open up an entirely new way of observing the universe, with immense potential to enlarge the understanding of physics and astronomy in unforeseen ways. LISA, in the committee's view, should be the flagship mission of a long-term program addressing Beyond Einstein goals.

Finding 5. The ESA-NASA LISA Pathfinder mission that is scheduled for launch in late 2009 will assess the operation of several critical LISA technologies in space. The committee believes that it is more responsible technically and financially to propose a LISA new start after the Pathfinder results are taken into account. In addition, Pathfinder will not test all technologies critical to LISA. Thus, it would be prudent for NASA to invest further in LISA technology development and risk reduction, to help ensure that NASA is in a position to proceed with ESA to a formal new start as soon as possible after the LISA Pathfinder results are understood.

Finding 6. A JDEM mission will set the standard in the precision of its determination of the distribution of dark energy in the distant universe. By clarifying the properties of 70 percent of the mass-energy in the universe, JDEM's potential for the fundamental advancement of both astronomy and physics is substantial. A JDEM mission will also bring important benefits to general astronomy. In particular, JDEM will provide highly detailed information for understanding how galaxies form and acquire their mass.

Finding 7. The JDEM candidates identified thus far are based on instrument and spacecraft technologies that either have been flown in space or have been extensively developed in other programs. A JDEM mission selected in 2009 could proceed smoothly to a timely and successful launch.

Finding 8. The present NASA Beyond Einstein funding wedge alone is inadequate to develop any candidate Beyond Einstein mission on its nominal schedule. However, both JDEM and LISA could be carried out with the currently forecasted NASA contribution if DOE's contribution that benefits JDEM is taken into account and if LISA's development schedule is extended and funding from ESA is assumed.

Recommendation 1. NASA and DOE should proceed immediately with a competition to select a Joint Dark Energy Mission for a 2009 new start. The broad mission goals in the request for proposals should be (1) to determine the properties of dark energy with high precision and (2) to enable a broad range of astronomical investigations. The committee encourages the agencies to seek as wide a variety of mission concepts and partnerships as possible.

Recommendation 2. NASA should invest additional Beyond Einstein funds in LISA technology development and risk reduction to help ensure that the agency is in a position to proceed in partnership with ESA to a new start after the LISA Pathfinder results are understood.

Recommendation 3. NASA should move forward with appropriate measures to increase the readiness of the three remaining mission areas—Black Hole Finder Probe, Constellation-X, and Inflation Probe—for consideration by NASA and the next NRC decadal survey of astronomy and astrophysics.

The committee strongly believes that future technology investment is required and warranted in all of the Beyond Einstein mission areas. The candidates for JDEM, the committee's first-priority mission area, need continued funding until NASA and DOE conduct a competition and selection for a JDEM. Furthermore, the committee believes that the competition to select a JDEM should be open to other mission concepts, launch opportunities, measurement techniques, and international partnerships. The committee's next-highest priority for funding from the current 2009 Beyond Einstein NASA funding wedge is to accelerate the maturation of those mission-critical LISA technologies that are currently at low TRLs. This funding will be needed until and if NASA initiates a post-Pathfinder mission start for LISA.

The current Beyond Einstein budget profile will not support technology development beyond JDEM and LISA. The committee did not develop a priority order for the remaining mission areas and believes that all their component missions require additional technology maturity before they can be fully evaluated. Their technology development should continue to be supported in NASA's broader astrophysics program, at least at a level that allows a sound appraisal by the next astronomy and astrophysics decadal survey.

5.11 Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft: A Workshop Report

A Report of the Ad Hoc Panel on Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft

Summary

The nation's next-generation National Polar-orbiting Operational Environmental Satellite System (NPOESS) was created by Presidential Decision Directive/National Science and Technology Council (NSTC)-2 of May 5, 1994, whereby the military and civil meteorological programs were merged into a single program. Within NPOESS, NOAA is responsible for satellite operations, the Department of Defense (DOD) is responsible for major acquisitions, and NASA is responsible for the development and infusion of new technologies. In 2000, the NPOESS program anticipated purchasing six satellites for $6.5 billion, with a first launch in 2008. Costs have since escalated dramatically, and the expected date of first launch has slipped to 2013. By November 2005, it became apparent that NPOESS would overrun its cost estimates by at least 25 percent, triggering the so-called Nunn-McCurdy review by the Department of Defense.

As a result of the June 2006 Nunn-McCurdy certification of NPOESS, the planned acquisition of six spacecraft was reduced to four, the launch of the first spacecraft was delayed until 2013, and several sensors were canceled or descoped in capability as the program was re-focused on "core" requirements related to the acquisition of data to support numerical weather prediction. "Secondary" sensors that would provide crucial continuity to some long-term climate records, as well as other sensors that would have provided new measurement capabilities, are not funded in the new NPOESS program.[1] Costs for NOAA's next generation of geostationary weather satellites, GOES-R, have also risen dramatically, and late last year NOAA canceled plans to incorporate a key instrument on the spacecraft—HES (Hyperspectral Environmental Suite).

As described in the Preface to this report, the National Research Council held a workshop, "Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft," in Washington, D.C., on June 19-21, 2007, to review options to recover measurement capabilities, especially those related to climate research, that were lost as a result of the Nunn-McCurdy actions and the cancellation of the HES sensor on GOES-R. Some 100 scientists and engineers from academia, government, and industry attended the workshop, which gave participants a chance to review and comment on a mitigation plan developed by NASA-NOAA as well as to explore options that were not included in the NASA-NOAA study. This report is meant to summarize these discussions; by design, it does not present the organizing panel's findings or recommendations. A follow-on study that will develop consensus findings and recommendations is underway; a report from this study is scheduled for release on January 31, 2008.

Subjects that were raised repeatedly by workshop participants, and that will be explored in more detail in the follow-on NRC study, include:

- **Preservation of long-term climate records.** Many participants noted that the demanifesting of climate sensors from NPOESS has placed many long-term climate records at risk, including multi-decadal records of total solar irradiance, Earth radiation budget, sea surface temperature, and sea ice extent. Some of these most fundamental data records require observational overlap to retain their value and require immediate attention to ensure their continuation. To ensure continuity of critical long-term climate measurements, many participants

NOTE: "Summary" reprinted from prepublication version of *Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft: A Workshop Report,* The National Academies Press, Washington, D.C., approved for release in 2007.

[1] In congressional testimony, the NOAA administrator stated, "Although the primary mission for NPOESS is to provide data for weather forecasting, many of the core sensors mentioned above and some of the secondary sensors would provide some additional climate and space weather observations. Unfortunately, difficult choices and trade-offs had to be made and the cost to procure these sensors is not included in the certified program; however, the program will plan for and fund the integration of these sensors on the spacecraft. Some of these sensors provide continuity to certain long-term climate records while other sensors would provide new data. . . . We specifically decided that the NPOESS spacecraft will be built with the capability to house all of the sensors and the program budget will include the dollars to integrate them on the spacecraft. This decision was made because the EXCOM agreed any additional funding gained through contract renegotiation or in unutilized management reserve would be used to procure these secondary sensors." Written testimony of Vice Admiral Conrad C. Lautenbacher, Jr. (U.S. Navy, ret.), Under Secretary of Commerce for Oceans and Atmosphere and NOAA Administrator, "Oversight Hearing on the Future of NPOESS: Results of the Nunn-McCurdy Review of NOAA's Weather Satellite Program," before the Committee on Science, U.S. House of Representatives, June 8, 2006.

also stressed the need to pursue international partnerships and, when feasible, the leveraging of foreign Earth observation missions.

- **The potential benefits of relatively minor and low-cost changes to the NPOESS program.** In several cases, a participant suggested small non-hardware changes to NPOESS that could address areas of climate interest. These included improved pre-launch characterization and documentation of all NPOESS instruments, adding minor software improvements to VIIRS[2] to make the data more climate-relevant, and downlinking full-resolution spectral data from CrIS[3] to enable creation of additional climate products.
- **The potential role of spacecraft formation flying in mitigation strategies.** Formation flight can allow for the synergistic combination of measurements from multiple satellites, sometimes launched years apart. In order to allow for subsequent formation flight with NPOESS platforms, some participants suggested consideration of the requisite orbit maintenance and operations requirements as part of the mitigation strategy for restoring deleted NPOESS and GOES-R climate observing capabilities.
- **Mitigation options beyond changes to NPOESS.** While particular long-term records can be secured via the remanifesting of certain sensors onto NPOESS, many participants noted that several could not be addressed even with the original suite of NPOESS instruments. Long-term records of sea level and ocean vector winds, for example, require different orbits and/or instruments to address critical climate needs. As a result, some participants heavily favored dedicated altimetry and scatterometry missions to fill this need. Further, some participants noted the critical importance of hyperspectral sounder measurements to climate science, suggesting restoration of CrIS/ATMS to the early-morning NPOESS orbit as well as the earliest-possible flight of a geostationary hyperspectral sounder to further improve temporal resolution.
- **The challenge of creating climate data records.** Although NPP- and NPOESS-derived environmental data records (EDRs) may have considerable scientific value, climate data records (CDRs)[4] are far more than a time series of EDRs. Many participants at the workshop emphasized the fundamental differences between products that are generated to meet short-term needs (EDRs) and those for which consistency of processing and reprocessing over years to decades is an essential requirement. Creation and maintenance of climate data records require different algorithms, data handling systems, calibration/validation, archival standards, access protocols, and pre-launch characterization compared to operational data products.
- **The specifications of the MIS instrument.** The specifications of the MIS instrument on NPOESS, which is to replace the now canceled CMIS instrument, were not known at the time of the workshop. Absent this information, participants were unable to fully analyze mitigation options. In addition, several participants warned about the consequences of not having an all-weather sea surface temperature retrieval capability, emphasizing the importance of retaining a low-frequency 6.9 GHz channel as the instrument is reconsidered.
- **Sustaining climate observations.** In the view of many participants, the loss of climate observations from NPOESS is of international concern and also imperils U.S. climate science leadership. Further, many participants noted that while discussions at the workshop were focused on solving near-term climate measurement continuity issues, there would remain a longer-term problem of sustaining support for climate science. Issues noted included finding an appropriate balance between new and sustained climate observations and managing technology infusion into long-term observational programs (including the challenges of doing so with a multiple-spacecraft—block-buy—procurement). Workshop discussions also included what many participants cited as a key challenge: accommodating research needs within an operational program. Some participants argued that the relative priority of climate measurement needs would have to be heightened across the implementing agencies if climate and operational weather functions remain combined. Their concern was that in exploiting the commonalities of weather and climate observations, the unique needs of climate scientists would be overlooked. The perceived lack of attention to climate science needs within the IPO, particularly calibration and validation requirements, led many participants to favor free-flyer options over integration with the NPOESS platforms.

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[2]The Visible/Infrared Imager/Radiometer Suite, VIIRS, collects visible/infrared imagery and radiometric data. A key sensor on the NPOESS spacecraft, VIIRS contributes to 23 environmental data records (EDRs) and is the primary instrument for 18 EDRs. See description at http://www.ipo.noaa.gov/Technology/viirs_summary.html.

[3]In conjunction with the Advanced Technology Microwave Sounder (ATMS), the Cross-track Infrared Sounder collects atmospheric data to permit the calculation of temperature and moisture profiles at high temporal (~daily) resolution. See discussion at http://www.ipo.noaa.gov/Technology/cris_summary.html.

[4]See NRC, *Ensuring the Climate Record from the NPP and NPOESS Meteorological Satellites,* National Academy Press, Washington, D.C., 2000, and NRC, *Climate Data Records from Environmental Satellites: Interim Report,* The National Academies Press, Washington, D.C., 2004.

5.12 Portals to the Universe: The NASA Astronomy Science Centers

A Report of the Ad Hoc Committee on NASA Astronomy Science Centers

Summary

The astronomy science centers established by the National Aeronautics and Space Administration (NASA) to serve as the interfaces between astronomy missions and the community of scientists who utilize the data have been enormously successful in enabling space-based astronomy missions to achieve their scientific potential. These centers have transformed the conduct of much of astronomical research, established a new paradigm for the use of large astronomical facilities, and advanced the science far beyond what would have been possible without them.

PURPOSE OF THE STUDY

NASA astronomy science centers take a number of forms and have been compared in terms of many factors, including cost, personnel, services offered, and the size of the community served (see Chapter 3 and Appendix A, Table A.1). The centers enable continuing scientific and educational use of the data during the operational life of a space-based astronomy mission and for years afterward. When NASA considers establishing new observing facilities, its decision on whether to use existing science centers, create new ones, or pursue other vehicles for data archiving, education and outreach, and community support for that mission is often critical. To that end, NASA asked the National Research Council (NRC) to examine current astronomy science centers with respect to their roles and services, to identify lessons learned and best practices, and to consider whether there are optimum sizes or approaches for such centers (see Appendix B for the study charge).

NASA ASTRONOMY SCIENCE CENTERS AND THEIR FUNCTIONS

NASA empowers a range of center types and sizes, from relatively modest facilities to large, full-service science centers, with budgets ranging from approximately $6 million to $80 million (Appendix A). As requested in the study charge, the committee examined a cross section of center types, including a small mission center, the Rossi X-ray Timing Explorer (RXTE) guest observer facility (GOF); a guest observer facility, the X-ray Multi-mirror Mission–Newton (XMM–Newton); three larger flagship mission science centers, the Space Telescope Science Institute (STScI), the Chandra X-ray Center (CXC), and the Spitzer Science Center (SSC); and a center focusing on interferometric data, the Michelson Science Center (MSC). The committee also considered two archival centers: the High Energy Astrophysics Science Archive Research Center (HEASARC) at NASA's Goddard Space Flight Center (GSFC) and the Infrared Processing and Analysis Center (IPAC) at the California Institute of Technology (Caltech). It discerned a consistent set of functions and services that allow the research community to utilize the data in creative ways that advance research and our understanding of the cosmos and to preserve the data and metadata for future use, including the following:

- Support of flight operations,
- Instrument support and calibration,
- Data analysis and Level 1 processing,
- Archiving and distribution of data to the research community,
- Software development and documentation for science analysis,
- Help desk and other user support services,
- User workshops and symposia,
- Proposal submission processing and peer review evaluation,
- Grant management and administration,
- Scientific research,

NOTE: "Summary" reprinted from *Portals to the Universe: The NASA Astronomy Science Centers*, The National Academies Press, Washington, D.C., 2007, pp. 1-5.

- Advocacy and strategic planning, and
- Education and public outreach.

The committee's assessment identified the factors that impeded or aided a center's ability to provide the full range of these functions effectively.

FINDINGS AND RECOMMENDATIONS

NASA Astronomy Science Centers for Managing Current and Planned Missions

The committee concluded that the core services of astronomy centers—mission support, scientific research, and data archiving—could be viewed as reaching their fullest performance at the following astronomy centers: (1) STScI, (2) CXC, (3) HEASARC and its associated RXTE and XMM–Newton guest observer facilities, and (4) IPAC and its associated Spitzer and Michelson science centers. The committee concluded that because a number of space-based astronomy missions had been delayed, the existing astronomy centers have sufficient scientific and programmatic expertise to manage all of NASA's astronomy center responsibilities now, for the foreseeable future, and after the active phases of current and planned missions have been completed.

Finding: The Chandra X-ray Center, the Space Telescope Science Institute, the High Energy Astrophysics Science Archive Research Center, and the Infrared Processing and Analysis Center have sufficient scientific and programmatic expertise to manage NASA's current science center responsibilities after the active phases of all current and planned space-based astronomy missions have been completed.

Recommendation 1. NASA should establish a large new center only when the following criteria are met: (1) the existing centers lack the capacity to support a major new scientific initiative and (2) there is an imminent need to develop a new infrastructure to support a broad base of users.

The committee viewed the presence of research scientists and visiting scientists at the NASA astronomy science centers as enhancing the role of those centers and their ability to provide exciting and intellectually rich environments for the research scientists they employ. No additional full-time researchers are required for a center to serve the community effectively, and the committee believes that all scientists at a center should be involved, at some level, in facilitating the mission with which the center is involved.

Finding: The ability of the Chandra X-ray Center, the Space Telescope Science Institute, the High Energy Astrophysics Science Archive Research Center, and the Infrared Processing and Analysis Center to provide the appropriate level of support to the scientific community depends critically on the extent to which they can attract, retain, and effectively deploy individuals with the mix of research and engineering skills necessary to maintain continuity of service.

Guest Observer Facilities and Explorer-Class Mission Centers

It was clear to the committee that all of the NASA astronomy science centers examined for the study can provide valuable services to the community, but that the smaller GOF and Explorer mission centers lack the resources and staff support to provide the full range of science center services effectively on their own. GOFs such as those for RXTE and XMM–Newton can manage a modest level of service in many areas only because they are able to draw on portions of the time of talented people who were engaged in other activities at their institutions. Associating GOFs or Explorer centers with the larger archival centers or flagship mission centers, which have staff and infrastructure in place, enables them to leverage necessary skills and services and serve their scientific constituents.

Finding: Embedding GOFs in existing science centers, such as the HEASARC, provides for efficient user support, especially when the scope of a space mission does not require establishing a separate center.

The archival centers provide an important service insofar as they are able to accommodate mission centers at varying stages of operation and to move staff among projects as missions start up or wind down. The sharing of staff scientists among center missions and the transitioning of staff as missions start and end provide both stability and flexibility. The archival centers also provide proposal and analysis software, search tools, and other resources that users can apply to the multiple databases they hold. Further benefits accrue in the knowledge base that staff acquire from one mission to the next, which allows for transferring best practices and lessons learned among missions.

BEST PRACTICES FOR ASTRONOMY SCIENCE CENTERS

The committee identified a set of best practices for the flagship and archival NASA astronomy science centers that, if adopted, can guide their continued effectiveness (Box S.1). Should the opportunity arise and the conditions be met for establishing a new center, the best practices can serve as input to selecting operational functions for it.

Recommendation 2. NASA should adopt a set of best practices as guiding principles to ensure the effectiveness of existing flagship and archival NASA astronomy science centers and to select the operational functions of any future centers.

BOX S.1 Best Practices for NASA Astronomy Science Centers

Mission Operations

NASA astronomy science centers can best operate the spacecraft and process the resulting data if they

- Have close interaction among scientists, engineers, and programmers. Such interaction is especially important for off-site principal investigator (PI) teams.
- Have research scientists who participate actively in mission operations and in policy decisions.
- Have mission staff knowledgeable about the instrumentation and the satellite in order to provide detailed advice and technical support to the user.
- Provide adequate instrument calibration.
- Provide functional software by the time data first arrive.

Science Operations

NASA astronomy science centers can best support their scientific user communities if they

- Support robust, accessible, well-documented software.
- Use common rather than instrument-specific software across missions when possible.
- Maintain adequate online supporting materials and a help desk with adequate staffing and rapid turnaround.
- Provide user-friendly protocols and software for proposal entry and require minimal technical details for the initial proposal.
- Enable coordinated observations and proposal submission among multiple space- and/or ground-based observatories.
- Co-locate staff to support multiple missions with related scientific objectives.
- Retain key science center staff by providing them with evolving opportunities in either multiple missions or within the host/managing institution.

COOPERATION AMONG SCIENCE CENTERS AND AGENCIES

Cooperation among NASA astronomy science centers and related agencies can lead to a greater impact on research results, data access, and educational activities. For example, researchers may be able to cross discipline and wavelength boundaries in analyzing astronomical data. Providing tools and formats that are common to all wavelength bands and supporting common protocols and formats for proposal entry can facilitate multiwavelength research. The committee concluded that astronomy science centers need to develop a coherent strategy for K-12 education if their educational activities are to have a greater impact.

Recommendation 3. NASA should ensure that NASA astronomy science centers cooperate among themselves and with other agencies to develop strategies and plans for

- **Developing common protocols and formats for proposal entry;**
- **Developing a universal infrastructure for data formats and metadata, archiving, and retrieval and analysis tools; and**
- **Providing curriculum materials and professional development programs for K-12 teachers.**

- Give scientists at science centers guaranteed research time but not guaranteed observation time.
- Have a visiting scientist program.

Data and Archiving

Science centers can best process, store, and disseminate their data if they

- Provide rapid (<24 hr) response to requests for data that have been calibrated and archived.
- Support common analysis software and protocols that can be used by all the science centers.
- Maintain mission expertise at the archive centers for the long-term support of active users.
- Ensure that standards for access to all astronomical data archives are coordinated by an entity such as the National Virtual Observatory and that the infrastructure, including formats and analysis tools, is accessible and sustainable.

Education and Public Outreach

Science centers can best communicate their results to the public if they

- Involve staff scientists and investigators in education and public outreach (EPO) activities.
- Coordinate EPO efforts of smaller missions with EPO systems of the large NASA astronomy science centers.
- Develop classroom resources that
  — Are designed iteratively through field testing and evaluation in actual classrooms.
  — Include hands-on activities when possible.
  — Support standards-based curricula.
  — Are packaged with protocols for measuring learning effectiveness.
  — Are accessible and cross-linked so that teachers can easily find them.
  — Include teacher support (e.g., Web-based teacher guides, training for master teachers).

5.13 The Scientific Context for Exploration of the Moon: Final Report

A Report of the Ad Hoc Committee on the Scientific Context for Exploration of the Moon

Executive Summary

We know more about many aspects of the Moon than about any world beyond our own, and yet we have barely begun to solve its countless mysteries. The Moon is, above all, a witness to 4.5 billion years (Ga) of solar system history, and it has recorded that history more completely and more clearly than has any other planetary body. Nowhere else can we see back with such clarity to the time when Earth and the other terrestrial planets were formed and life emerged on Earth.

Planetary scientists have long understood the Moon's unique place in the evolution of rocky worlds. Many of the processes that have modified the terrestrial planets have been absent on the Moon. The lunar interior retains a record of the initial stages of planetary evolution. Its crust has never been altered by plate tectonics, which continually recycle Earth's crust; or by planetwide volcanism, which resurfaced Venus only half a billion years ago; or by the action of wind and water, which have transformed the surfaces of both Earth and Mars. The Moon today presents a record of geologic processes of early planetary evolution in the purest form.

Lunar science provides a window into the early history of the Earth-Moon system, can shed light on the evolution of other terrestrial planets such as Mars and Venus, and can reveal the record of impacts within the inner solar system. By dint of its proximity to Earth, the Moon is accessible to a degree that other planetary bodies are not.

For these reasons, the Moon is priceless to planetary scientists. It remains a cornerstone for deciphering the histories of those more complex worlds. But because of the limitations of current data, researchers cannot be sure that they have read these histories correctly. Now, thanks to the legacy of the Apollo program and subsequent missions, such as Clementine and Lunar Prospector, and looking forward to the newly established Vision for Space Exploration (VSE),[1] scientists are able to pose sophisticated questions that are more relevant and focused than those that could be asked over three decades ago. Only by returning to the Moon to carry out new scientific explorations can we hope to narrow the gaps in understanding and learn the secrets that the Moon alone has kept for eons.

The Moon is not only of intrinsic interest as a cornerstone of the Earth-Moon system science, but it also provides a unique location for research in several other fields of science. The Moon's surface is in direct contact with the interplanetary medium, and the interaction of the Moon with the solar wind plasma flowing from the Sun forms a unique plasma physics laboratory. Astronomical and astrophysical observations as well as observations of Earth, its atmosphere, ionosphere, and magnetosphere may be made from the stable platform of the Moon. The absence of a significant ionosphere on the Moon should enable low-frequency radio astronomy to be carried out, particularly from the farside of the Moon where radio interference from terrestrial sources should be absent.

NASA asked the National Research Council (NRC) to provide guidance on the scientific challenges and opportunities enabled by a sustained program of robotic and human exploration of the Moon during the period 2008-2023 and beyond as the VSE evolves. This report was prepared by the Committee on the Scientific Context for Exploration of the Moon (brief biographies of the committee are presented in Appendix F).

The framework of the VSE was changing while this report was being prepared. However, the committee believes that its scientific rationale for lunar science and its goals and recommendations are independent of any particular programmatic implementation.

It is the unanimous consensus of the committee that the Moon offers profound scientific value. The infrastructure provided by sustained human presence can enable remarkable science opportunities if those opportunities are evaluated and designed into the effort from the outset. While the expense of human exploration cannot likely be justified on the basis of science alone, the committee emphasizes that careful attention to the science opportunity is very much in the interest of a stable and sustainable lunar program. In the opinion of the committee, a vigorous near-term robotic exploration program providing global access is central to the next phase of scientific explora-

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NOTE: "Executive Summary" reprinted from *The Scientific Context for Exploration of the Moon: Final Report,* The National Academies Press, Washington, D.C., 2007, pp. 1-6.

[1]National Aeronautics and Space Administration (NASA), *The Vision for Space Exploration*, NP-2004-01-334-HQ, NASA, Washington, D.C., 2004.

tion of the Moon and is necessary both to prepare for the efficient utilization of human presence and to maintain scientific momentum as this major national program moves forward.

PRIORITIES, FINDINGS, AND RECOMMENDATIONS

According to the committee's statement of task (see Appendix A):

> The current study is intended to meet the near-term needs for science guidance for the lunar component of the VSE. . . . [T]he *primary goals* of the study are to:
>
> 1. Identify a common set of prioritized basic science goals that could be addressed in the near-term via the LPRP[2] program of orbital and landed robotic lunar missions (2008-2018) and in the early phase of human lunar exploration (nominally beginning in 2018); and
>
> 2. To the extent possible, suggest whether individual goals are most amenable to orbital measurements, in situ analysis or instrumentation, field observation or terrestrial analysis via documented sample return.

Also outlined in the statement of task are the overall science scope for this study and several secondary tasks.

Overarching Themes

The committee identified four overarching themes of lunar science: early Earth-Moon system, terrestrial planet differentiation and evolution, solar system impact record, and lunar environment. The committee then constructed eight science concepts that address broad areas of scientific research. Each is multicomponent and is linked to different aspects of the overarching themes of lunar science.

The committee approached the challenge of prioritization by developing a hierarchy of priority categories. It used the prioritization criteria adopted by the decadal survey *New Frontiers in the Solar System: An Integrated Exploration Strategy*[3] as a guideline: the criteria are scientific merit, opportunity, and technological readiness.

The committee thus structured the prioritization of goals called for in the statement of task along three lines: (1) prioritization of science concepts, (2) prioritization of science goals, and (3) specific integrated high-priority recommendations. Although the rationales for these three are linked throughout the discussion of this report, the implementation requirements are different. As requested in the statement of task, the priorities and recommendations presented in this report relate to the near-term implementation of the VSE, which includes the robotic precursors and initial human excursions on the Moon. Planning for and implementing longer-term scientific activities on the Moon are beyond the scope of this study.

Prioritized Science Concepts

The committee evaluated only the scientific merit of each science concept in order to rank the concepts. It should be noted that *all* concepts discussed are viewed to be scientifically important. The science concepts are prioritized below and discussed in more detail in Chapter 3.

1. The bombardment history of the inner solar system is uniquely revealed on the Moon.
2. The structure and composition of the lunar interior provide fundamental information on the evolution of a differentiated planetary body.
3. Key planetary processes are manifested in the diversity of lunar crustal rocks.
4. The lunar poles are special environments that may bear witness to the volatile flux over the latter part of solar system history.
5. Lunar volcanism provides a window into the thermal and compositional evolution of the Moon.
6. The Moon is an accessible laboratory for studying the impact process on planetary scales.
7. The Moon is a natural laboratory for regolith processes and weathering on anhydrous airless bodies.

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[2]The Lunar Precursor and Robotic Program (LPRP) was how robotic missions were identified in the NASA letter that requested this study. The LPRP terminology is no longer in use.

[3]National Research Council, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, The National Academies Press, Washington, D.C., 2003.

8. Processes involved with the atmosphere and dust environment of the Moon are accessible for scientific study while the environment remains in a pristine state.

Prioritization of Science Goals

Within the 8 science concepts above, the committee identified 35 specific science goals that can be addressed, at least in part, during the early phases of the VSE. For these science goals, the committee evaluated science merit as well as the degree to which they can be achieved within current or near-term technical readiness and practical accessibility. Within their respective science concepts, the science goals are listed in the order of their overall priority ranking (a through e) in Table 3.1 in Chapter 3.

All 35 specific science goals were also evaluated and ranked as a group, separately from the science concepts with which they are associated. The highest-ranking lunar science goals are listed in Table 5.1 in Chapter 5 in priority order. For this group of goals the committee identifies possible means of implementation to achieve each goal.

FINDINGS AND RECOMMENDATIONS

Principal Finding: Lunar activities apply to broad scientific and exploration concerns.

Lunar science as described in this report has much broader implications than simply studying the Moon. For example, a better determination of the lunar impact flux during early solar system history would have profound implications for comprehending the evolution of the solar system, early Earth, and the origin and early evolution of life. A better understanding of the lunar interior would bear on models of planetary formation in general and on the origin of the Earth-Moon system in particular. And exploring the possibly ice-rich lunar poles could reveal important information about the history and distribution of solar system volatiles. Furthermore, although some of the committee's objectives are focused on lunar-specific questions, one of the basic principles of comparative planetology is that each world studied enables researchers to better understand other worlds, including our own. Improving our understanding of such processes as cratering and volcanism on the Moon will provide valuable points of comparison for these processes on the other terrestrial planets.

Finding 1: Enabling activities are critical in the near term.

A deluge of spectacular new data about the Moon will come from four sophisticated orbital missions to be launched between 2007 and 2008: SELENE (Japan), Chang'e (China), Chandrayaan-1 (India), and the Lunar Reconnaissance Orbiter (United States). Scientific results from these missions, integrated with new analyses of existing data and samples, will provide the enabling framework for implementing the VSE's lunar activities. However, NASA and the scientific community are currently underequipped to harvest these data and produce meaningful information. For example, the lunar science community assembled at the height of the Apollo program of the late 1960s and early 1970s has since been depleted in terms of its numbers and expertise base.

> **Recommendation 1a:** NASA should make a strategic commitment to stimulate lunar research and engage the broad scientific community[4] by establishing two enabling programs, one for fundamental lunar research and one for lunar data analysis. Information from these two recommended efforts—a Lunar Fundamental Research Program and a Lunar Data Analysis Program—would speed and revolutionize understanding of the Moon as the Vision for Space Exploration proceeds.

> **Recommendation 1b:** The suite of experiments being carried by orbital missions in development will provide essential data for science and for human exploration. NASA should be prepared to recover data lost due to failure of missions or instruments by reflying those missions or instruments where those data are deemed essential for scientific progress.

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[4]See also National Research Council, *Building a Better NASA Workforce: Meeting the Workforce Needs for the National Vision for Space Exploration*, The National Academies Press, Washington, D.C., 2007.

Finding 2: Strong ties with international programs are essential.

The current level of planned and proposed activity indicates that almost every space-faring nation is interested in establishing a foothold on the Moon. Although these international thrusts are tightly coupled to technology development and exploration interests, science will be a primary immediate beneficiary. NASA has the opportunity to provide leadership in this activity, an endeavor that will remain highly international in scope.

> **Recommendation 2:** NASA should explicitly plan and carry out activities with the international community for scientific exploration of the Moon in a coordinated and cooperative manner. The committee endorses the concept of international activities as exemplified by the recent "Lunar Beijing Declaration" of the 8th ILEWG (International Lunar Exploration Working Group) International Conference on Exploration and Utilization of the Moon (see Appendix D).

Finding 3: Exploration of the South Pole-Aitken Basin remains a priority.

The answer to several high-priority science questions identified can be found within the South Pole-Aitken Basin, the oldest and deepest observed impact structure on the Moon and the largest in the solar system. Within it lie samples of the lower crust and possibly the lunar mantle, along with answers to questions on crater and basin formation, lateral and vertical compositional diversity, lunar chronology, and the timing of major impacts in the early solar system.

Missions to South Pole-Aitken Basin, beginning with robotic sample returns and continuing with robotic and human exploration, have the potential to be a cornerstone for lunar and solar system research. (A South Pole-Aitken Basin sample-return mission was listed as a high priority in the 2003 NRC decadal survey report *New Frontiers in the Solar System: An Integrated Exploration Strategy*.[5])

> **Recommendation 3:** NASA should develop plans and options to accomplish the scientific goals set out in the high-priority recommendation in the National Research Council's *New Frontiers in the Solar System: An Integrated Exploration Strategy* (2003) through single or multiple missions that increase understanding of the South Pole-Aitken Basin and by extension all of the terrestrial planets in our solar system (including the timing and character of the late heavy bombardment).

Finding 4: Diversity of lunar samples is required for major advances.

Laboratory analyses of returned samples provide a unique perspective based on scale, precision, and flexibility of analysis and have permanence and ready accessibility. The lunar samples returned during the Apollo and Luna missions dramatically changed understanding of the character and evolution of the solar system. Scientists now understand, however, that these samples are not representative of the larger Moon and do not provide sufficient detail and breadth to address the fundamental science concepts outlined in Table 3.1 in this report.

> **Recommendation 4:** Landing sites should be selected that can fill in the gaps in diversity of lunar samples. Mission plans for each human landing should include the collection and return of at least 100 kg of rocks from diverse locations within the landing region. For all missions, robotic and human, to improve the probability of finding new, ejecta-derived diversity among smaller rock fragments, every landed mission that will return to Earth should retrieve at least 1 kg of rock fragments 2 to 6 mm in diameter separated from bulk soil. Each mission should also return 100 to 200 grams of unfractionated regolith.

Finding 5: The Moon may provide a unique location for observation and study of Earth, near-Earth space, and the universe.

The Moon is a platform that can potentially be used to make observations of Earth (Earth science) and to collect data for heliophysics, astrophysics, and astrobiology. Locations on the Moon provide both advantages and disadvantages. There are substantial uncertainties in the benefits and the costs of using the Moon as an observation platform as compared with alternate locations in space. The present committee did not have the required span and depth of expertise to perform a thorough evaluation of the many issues that need examination. A thorough study is required.

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[5]National Research Council, *New Frontiers in the Solar System: An Integrated Exploration Strategy*, The National Academies Press, Washington, D.C., 2003.

Recommendation 5: The committee recommends that NASA consult scientific experts to evaluate the suitability of the Moon as an observational site for studies of Earth, heliophysics, astronomy, astrophysics, and astrobiology. Such a study should refer to prior NRC decadal surveys and their established priorities.

RELATED ISSUES

The committee identified several related issues pertaining to optimal implementation of science in the VSE. This effort was driven by the stark realization that more than 30 years have passed since Apollo and that the nature of the VSE itself warrants a major reconsideration of the basic approach to conducting lunar science. In those more than 30 years, robotic capability has increased dramatically, analytical instrumentation has advanced remarkably, and the very understanding of how to explore has evolved as scientists have learned about planetary formation and evolution. The VSE offers new opportunity: there is no longer the limitation of short-duration lunar stays of 2 or 3 days and "emplacement science"; scientists on the Moon can operate as scientists, doing analytical work and deciphering sample/source relationships; site revisit with follow-up science is possible (e.g., an outpost); robotic-capable equipment can be used between missions; geophysical equipment can be used in survey modes; time-consuming deep drilling is possible; high-grade lunar samples can be selected for return to Earth. Nurturing a new approach to lunar exploration must be fostered if the potential of the VSE is to be reaped.

Finding 1R: The successful integration of science into programs of human exploration has historically been a challenge. It remains so for the VSE. Prior Space Studies Board reports by the Committee on Human Exploration (CHEX) examined how the different management approaches led to different degrees of success. CHEX developed principles for optimizing the integration of science into human exploration and recommended implementation of these principles in future programs.[6] This committee adopts in Recommendation 1R the CHEX findings in a form appropriate for the early phase of VSE.

Recommendation 1R: NASA should increase the potential to successfully accomplish science in the VSE by (1) developing an integrated human/robotic science strategy,[7] (2) clearly stating where science fits in the Exploration Systems Mission Directorate's (ESMD's) goals and priorities, and (3) establishing a science office embedded in the ESMD to plan and implement science in the VSE. Following the Apollo model, such an office should report jointly to the Science Mission Directorate and the ESMD, with the science office controlling the proven end-to-end science process.

Finding 2R: Great strides and major advances in robotics, space and information technology, and exploration techniques have been made since Apollo. These changes are accompanied by a greatly evolved understanding of and approach to planetary science and improvements in use of remote sensing and field and laboratory sample analyses. Critical to achieving high science return in Apollo was the selection of the lunar landing sites and the involvement of the science community in that process. Similarly, the scientific community's involvement in detailed mission planning and implementation resulted in efficient and productive surface traverses and instrument deployments.

Recommendation 2R: The development of a comprehensive process for lunar landing site selection that addresses the science goals of Table 5.1 in this report should be started by a science definition team. The choice of specific sites should be permitted to evolve as the understanding of lunar science progresses through the refinement of science goals and the analysis of existing and newly acquired data. Final selection should be done with the full input of the science community in order to optimize the science return while meeting engineering and safety constraints. Similarly, science mission planning should proceed with the broad involvement of the science and engineering communities. The science should be designed and implemented as an integrated human/robotic program employing the best each has to offer. Extensive crew training and mission simulation should be initiated early to help devise optimum exploration strategies.

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[6]See p. 128 of the third report in a series by the Committee on Human Exploration: National Research Council, *Science Management in the Human Exploration of Space*, National Academy Press, Washington, D.C., 1997.

[7]This CHEX Recommendation 1 refers to the development of science goals, strategy, priorities, and process methodology; CHEX Recommendation 3 (and this committee's Recommendation 1R) refers strictly to the implementation of science in a program of human exploration.

Finding 3R: The opportunity provided by the VSE to accomplish science, lunar and otherwise, is highly dependent for success on modernizing the technology and instrumentation available. The virtual lack of a lunar science program and no human exploration over the past 30 years have resulted in a severe lack of qualified instrumentation suitable for the lunar environment. Without such instrumentation, the full and promising potential of the VSE will not be realized.

> **Recommendation 3R:** NASA, with the intimate involvement of the science community, should immediately initiate a program to develop and upgrade technology and instrumentation that will enable the full potential of the VSE. Such a program must identify the full set of requirements as related to achieving priority science objectives and prioritize these requirements in the context of programmatic constraints. In addition, NASA should capitalize on its technology development investments by providing a clear path into flight development.

Finding 4R: The NASA curatorial facilities and staff have provided an exemplary capability since the Apollo program to take advantage of the scientific information inherent in extraterrestrial samples. The VSE has the potential to add significant demands on the curatorial facilities. The existing facilities and techniques are not sufficient to accommodate that demand and the new requirements that will ensue. Similarly, there is a need for new approaches to the acquisition of samples on lunar missions.

> **Recommendation 4R:** NASA should conduct a thorough review of all aspects of sample curation, taking into account the differences between a lunar outpost-based program and the sortie approach taken by the Apollo missions. This review should start with a consideration of documentation, collection, and preservation procedures on the Moon and continue to a consideration of the facilities requirements for maintaining and analyzing the samples on Earth. NASA should enlist a broad group of scientists familiar with curatorial capabilities and the needs of lunar science, such as the Curation and Analysis Planning Team for Extraterrestrial Materials (CAPTEM), to assist it with the review.

6
Congressional Testimony

Members of SSB committees may be invited to testify before committees of the U.S. House of Representatives or the U.S. Senate about the findings and recommendations of their reports. During 2007, six hearings were held where members of the SSB family testified to Congress. Their prepared statements are reprinted here (without references, notes, appendices, tables or figures).

At the February 13 hearing before the House Committee on Science and Technology, the co-chairs of the NRC Committee on Earth Science and Applications from Space, Richard A. Anthes and Berrien Moore III, testified on their thoughts on the national imperatives for Earth and climate sciences. The Honorable Jim Geringer, Director of the Policy Environmental Systems Research Institute also testified. The prepared statements of Richard Anthes and Berrien Moore are reprinted here and all of the statements are available at <http://www.science.house.gov/publications/hearings_markups_details.aspx?NewsID=1268>.

At the March 7 hearing before the Senate Committee on Commerce, Science and Transportation Subcommittee on Space, Aeronautics, and Related Sciences, Berrien Moore III, co-chair of the NRC Committee on Earth Science and Applications from Space and Otis Brown, member of the committee testified on their perspectives of the national imperatives for Earth science research. Michael Freilich, Director, Earth Science Division, NASA, and Nancy Colleton, President, Institute for Global Environment Strategies and Executive Director, Alliance for Earth Observations also testified. The prepared statements of Berrien Moore and Otis Brown are reprinted here, and all of the statements are available at <http://commerce.senate.gov/public/index.cfm?FuseAction=Hearings.Hearing&Hearing_ID=1825.

At the March 9 hearing before the House Committee on Appropriations Subcommittee on Commerce, Justice, Science and Related Agencies, SSB chair Lennard Fisk testified on his thoughts on the future of space and Earth science and human space exploration, and the balance of funding at NASA between space and Earth science and human space exploration.

At the May 2 hearing before the House Committee on Science and Technology Subcommittee on Space and Aeronautics, SSB chair Lennard Fisk, SSB member and chair of the Committee on Solar and Space Physics Daniel Baker, and former SSB member Joseph Burns testified on their perspectives on the state of space science activities at NASA. NASA's associate administrator for science, Alan Stern and Garth Illingworth, chair of the Astronomy and Astrophysics Advisory Committee also testified. The prepared statements of Lennard Fisk, Daniel Baker, and Joseph Burns are reprinted here and all of the statements are available at <http://www.science.house.gov/publications/hearings_markups_details.aspx?NewsID=1795>.

At the June 28 hearing before the House Committee on Science and Technology Subcommittee on Space and Aeronautics, Richard A. Anthes, co-chair of the NRC Committee on Earth Science and Applications from Space and Eric Barron, chair of the Climate Variability and Change Panel of the NRC Committee on Earth Science and Applications from Space testified on the decadal survey released by the committee. Michael Freilich, Director, Earth

Science Division, NASA; and Timothy Foresman, president, International Center for Remote Sensing Education also testified. The prepared statements of Richard A. Anthes and Eric Barron are reprinted here and all of the statements are available at <http://www.science.house.gov/publications/hearings_markups_details.aspx?NewsID=1899>.

At the July 11 hearing before the Senate Committee on Commerce, Science, and Transportation, Antonio J. Busalacchi, Jr., chair of the SSB Panel on Options to Ensure the Climate Record from NPOESS and GOES-R Spacecraft and the ad hoc Committee on a Strategy to Mitigate the Impact of Sensor Descopes and Demanifests on the NPOESS and GOES-R Spacecraft, testified on the budgetary, management, and schedule risks of weather and environmental satellite systems, as well as the potential lost capabilities in climate monitoring, modeling, and forecasting that are possible under the current program. His prepared statement is also available at <http://commerce.senate.gov/public/index.cfm?FuseAction=Hearings.Hearing&Hearing_ID=1881>.

6.1 National Imperatives for Earth and Climate Sciences

STATEMENTS BEFORE THE HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

February 13, 2007
Statement of Richard A. Anthes, Ph.D.
President of the University Corporation for Atmospheric Research (UCAR) and
Co-Chair, Committee on Earth Science and Applications from Space
National Research Council, The National Academies

Mr. Chairman, Ranking Minority Member, and members of the committee: thank you for inviting me here to testify today. My name is Richard Anthes, and I am the President of the University Corporation for Atmospheric Research, a consortium of 70 research universities that manages the National Center for Atmospheric Research, on behalf of the National Science Foundation, and additional scientific education, training and support programs. I am also the current President of the American Meteorological Society. I appear today in my capacity as co-chair of the National Research Council (NRC)'s Committee on Earth Science and Applications from Space: A Community Assessment and Strategy for the Future.

The National Research Council is the unit of the National Academies that is responsible for organizing independent advisory studies for the federal government on science and technology. In response to requests from NASA, NOAA, and the USGS, the NRC has recently completed a "decadal survey" of Earth science and applications from space. ("Decadal surveys" are the 10-year prioritized roadmaps that the NRC has done for 40 years for the astronomers; this is the first time it is being done for Earth science and applications from space.) Among the key tasks in the charge to the decadal survey committee were to:

- Develop a consensus of the top-level scientific questions that should provide the focus for Earth and environmental observations in the period 2005-2020; and
- Develop a prioritized list of recommended space programs, missions, and supporting activities to address these questions.

The NRC survey committee has prepared an extensive report in response to this charge, which I am pleased to be able to summarize here today. Over 100 leaders in the Earth science community participated on the survey steering committee or its seven study panels. It is noteworthy that this was the first Earth science decadal survey, and the committee and panel members did an excellent job in fulfilling the charge and establishing a consensus—a task many previously considered impossible. A copy of the full report has also been provided for your use.

The committee's vision is encapsulated in the following declaration, first stated in the committee's interim report, published in 2005:

> Understanding the complex, changing planet on which we live, how it supports life, and how human activities affect its ability to do so in the future is one of the greatest intellectual challenges facing humanity. It is also one of the most important challenges for society as it seeks to achieve prosperity, health, and sustainability.

As detailed in the committee's final report, and as we were profoundly reminded by the latest report from the International Panel on Climate Change (IPCC), the world faces significant and profound environmental challenges: shortages of clean and accessible freshwater, degradation of terrestrial and aquatic ecosystems, increases in soil erosion, changes in the chemistry of the atmosphere, declines in fisheries, and above all the rapid pace of substantial changes in climate. These changes are not isolated; they interact with each other and with natural variability in complex ways that cascade through the environment across local, regional, and global scales. Addressing these societal challenges requires that we confront key scientific questions related to ice sheets and sea level change, large-scale and persistent shifts in precipitation and water availability, transcontinental air pollution, shifts in ecosystem structure and function in response to climate change, impacts of climate change on human health, and occurrence of extreme events, such as hurricanes, floods and droughts, heat waves, earthquakes, and volcanic eruptions.

Yet at a time when the need has never been greater, we are faced with an Earth observation program that will dramatically diminish in capability over the next 5-10 years.

Last April, my co-chair, Dr. Berrien Moore, came before Congress to testify in response to release of the committee's 2005 interim report. His testimony highlighted the key roles played by NASA and NOAA over the past 30 years in advancing our understanding of the Earth system and in providing a variety of societal benefits through their international leadership in Earth observing systems from space. He noted that while NOAA had plans to modernize and refresh its weather satellites, NASA had no plans to replace its Earth Observing System platforms after their nominal six year lifetimes end. He also noted that NASA had cancelled, scaled back, or delayed at least six planned missions, including a Landsat continuity mission. This led to the main finding in the interim report, which stated "this system of environmental satellites is at risk of collapse."

Since the publication of the interim report, the Hydros and Deep Space Climate Observatory missions were cancelled; the flagship Global Precipitation Mission was delayed for another two and a half years; significant cuts were made to NASA's Research and Analysis program: the NPOESS Preparatory Project mission was delayed for a year and a half; a key atmospheric profiling sensor planned for the next generation of NOAA geostationary satellites was canceled; and the NPOESS program breached the Nunn-McCurdy budget cap. As you have all heard, the certified NPOESS program delays the first launch by 3 years, eliminates 2 of the planned 6 spacecraft, and de-manifests or de-scopes a number of instruments, with particular consequences for measurement of the forcing and feedbacks that need to be measured to understand the magnitude, pace, and consequences of global and regional climate change. It is against this backdrop that I discuss the present report.

As you will see in the report, between 2006 and the end of the decade, the number of operating missions will decrease dramatically and the number of operating sensors and instruments on NASA spacecraft, most of which are well past their nominal lifetimes, will decrease by some 35 percent, with a 50 percent reduction by 2015 (see Figure 1 below). Substantial loss of capability is likely over the next several years due to a combination of decreased budgets and aging satellites already well past their design lifetimes. **This will result in an overall degradation of the system of Earth observing satellites, with the following potential consequences**:

- After decades of steady improvement, weather forecasts, including those of severe weather such as hurricanes, may start becoming less accurate, putting more people at risk and diminishing the proven economic value of accurate forecasts.
- The ozone hole in the stratosphere has apparently reached its maximum intensity. Models predict it will start to slowly recover. Without observations we may not be able to verify its recovery or explain why it is occurring.
- Earth is warming because of a small imbalance between incoming solar radiation and outgoing radiation from Earth. Measuring this small imbalance is critical to determining how fast Earth is warming and when the warming will stop. Without the measurements we are recommending will not be able to quantify how this net energy imbalance is changing.
- Climate models have improved steadily over the years, but are far from perfect. We need observations of the Earth system, the atmosphere, oceans, land and ice to verify and improve the climate models. These models have real impact on the U.S. economy, in predicting El Nino and other seasonal fluctuations in climate, which are used in energy, water and agriculture management.
- Sea level is rising and ice around the world is melting, yet there is uncertainty in how fast these are occurring and whether or not they are accelerating or decelerating. Without the observations we are recommending, we will be unable to know for sure how these rates are changing and what the implications will be for coastal communities.
- There is controversy about whether the frequency and intensity of hurricanes are increasing as the climate warms; observations of the atmosphere and oceans are required to resolve this important issue.
- The risk of missing early detection of Earthquakes, tsunamis, and volcanic eruptions will increase.
- Air quality forecasts, which require the global perspectives of satellites to identify pollution transport across borders, will become less accurate, with negative implications for both human health and urban pollution management efforts.
- Earth science is based fundamentally on observations. While it is impossible to predict what scientific advances will not occur without the observations, or what surprises (like the ozone hole) we will miss, we can be sure the rate of scientific progress will be greatly slowed without a robust set of Earth observations.

In its report, the committee sets forth a series of near-term and longer-term recommendations in order to address these troubling trends. It is important to note that this report does not "shoot for the moon," and indeed the committee exercised considerable constraint in its recommendations, which were carefully considered within the context of challenging budget situations. Yet, while societal applications have grown ever-more dependent upon our Earth observing fleet, the NASA Earth science budget has declined some 30% in constant-year dollars since 2000 (see Figure 2 below). This disparity between growing societal needs and diminished resources must be corrected. This leads to the report's overarching recommendation:

> The U.S. government, working in concert with the private sector, academe, the public, and its international partners, should renew its investment in Earth observing systems and restore its leadership in Earth science and applications."

The report outlines near-term actions meant to stem the tide of capability deterioration and continue critical data records, as well as forward-looking recommendations to establish a balanced Earth observation program designed to directly address the most urgent societal challenges facing our nation and the world (see Figure 3 below for an example of how nine of our recommended missions support in a synergistic way one of the societal benefit areas—extreme event warnings). It is important to recognize that these two sets of recommendations are not an "either/or" set of priorities. *Both* near-term actions and longer-term commitments are required to stem the tide of capability deterioration, continue critical climate data records, *and* establish a balanced Earth observation program designed to directly address the most urgent societal challenges facing our nation and the world. It is important to "right the ship" for Earth science, and we simply *cannot* let the current challenges we face with NPOESS and other troubled programs stop progress on all other fronts. Implementation of the "stop-gap" recommendations concerning NPOESS, NPP, and GOES-R are important—and the recommendations for establishing a healthy program going forward are equally as important. Satisfying near-term recommendations without placing due emphasis on the forward-looking program is to ignore the largest fraction of work that has gone into this report. Moreover, such a strategy would result in a further loss of U.S. scientific and technical capacity, which could decrease the competitiveness of the United States internationally for years to come.

Key elements of the recommended program include:

1. Restoration of certain measurement capabilities to the NPP, NPOESS, and GOES-R spacecraft in order to ensure continuity of critical data sets.
2. Completion of the existing planned program that was used as a baseline assumption for this survey. This includes (but is not limited to) launch of GPM in or before 2012, securing a replacement to Landsat 7 data before 2012.
3. A prioritized set of 17 missions to be carried out by NOAA and NASA over the next decade (see Tables 1 and 2 below). This set of missions provides a sound foundation for Earth science and its associated societal benefits well beyond 2020. *The committee believes strongly that these missions form a minimal, yet robust, observational component of an Earth information system that is capable of addressing a broad range of societal needs.*
4. A technology development program at NASA with funding comparable to and in addition to its basic technology program to make sure the necessary technologies are ready when needed to support mission starts over the coming decade.
5. A new "Venture" class of low-cost research and application missions that can establish entirely new research avenues or demonstrate key application-oriented measurements, helping with the development of innovative ideas and technologies. Priority would be given to cost-effective, innovative missions rather than ones with excessive scientific and technological requirements.
6. A robust NASA Research and Analysis program, which is necessary to maximize scientific return on NASA investments in Earth science. Because the R&A programs are carried out largely through the Nation's research universities, such programs are also of great importance in supporting and training next generation Earth science researchers.
7. Suborbital and land-based measurements and socio-demographic studies in order to supplement and complement satellite data.
8. A comprehensive information system to meet the challenge of production, distribution, and stewardship of observational data and climate records. To ensure the recommended observations will benefit society, the mission program must be accompanied by efforts to translate raw observational data into useful information through modeling, data assimilation, and research and analysis.

Further, the committee is particularly concerned with the lack of clear agency responsibility for sustained research programs and the transitioning of proof-of-concept measurements into sustained measurement systems. To address societal and research needs, both the quality and the continuity of the measurement record must be assured through the transition of short-term, exploratory capabilities, into sustained observing systems. The elimination of the requirements for climate research-related measurements on NPOESS is only the most recent example of the nation's failure to sustain critical measurements. Therefore, our committee recommends that the Office of Science and Technology Policy, in collaboration with the relevant agencies, and in consultation with the scientific community, should develop and implement a plan for achieving and sustaining global Earth observations. This plan should recognize the complexity of differing agency roles, responsibilities, and capabilities as well as the lessons from implementation of the Landsat, EOS, and NPOESS programs.

Mr. Chairman, the observing system we envision will help establish a firm and sustainable foundation for Earth science and associated societal benefits through the year 2020 and beyond. It can be achieved through effective management of technology advances and international partnerships, and broad use of satellite science data by the research and decision-making communities. Our report recommends a path forward that restores U.S. leadership in Earth science and applications and averts the potential collapse of the system of environmental satellites. As documented in our report, this can be accomplished in a fiscally responsible manner, and I urge the committee to see that it is accomplished.

Thank you for the opportunity to appear before you today. I am prepared to answer any questions that you may have.

February 13, 2007
Statement of Berrien Moore III, Ph.D.
University Distinguished Professor
Director of the Institute for the Study of Earth, Oceans, and Space
University of New Hampshire
Co-Chair, Committee on Earth Science and Applications from Space
National Research Council, The National Academies

Mr. Chairman, Ranking Minority Member, and members of the committee: thank you for inviting me here to testify today. My name is Berrien Moore, and I am a professor of systems research at the University of New Hampshire and Director of the Institute for the Study of Earth, Oceans, and Space. I appear today, like Dr. Anthes, in my capacity as co-chair of the National Research Council (NRC)'s Committee on Earth Science and Applications from Space.

As you know, the NRC is the unit of the National Academies that is responsible for organizing independent advisory studies for the federal government on science and technology. The NRC has been conducting decadal strategy surveys in astronomy for four decades, but this is the first decadal survey in Earth science and applications from space.

On March 2, 2006, I testified before this committee at a hearing entitled, *NASA's Science Mission Directorate: Impacts of the Fiscal Year 2007 Budget Proposal*. At that hearing, I showed the table below, which is taken from the 2005 Interim Report of our study. This table shows the effects of the FY '06 budget. I then discussed my concerns about the proposed cuts in the FY '07 budget, especially the continuing reductions in funding for Research and Analysis, which I believed was having a very negative effect on a program already pared to the bone.

With this as background, I will now turn to the questions posed to me in advance of this hearing.

1. How did the Decadal Survey committee determine the priorities that it recommended the nation pursue in Earth and climate science research and applications?

As noted in testimony of my co-chair, Dr. Richard Anthes, the decadal survey's vision, which was first expressed in the committee's 2005 Interim Report, is for a program of Earth science research and applications in support of society. The present report reaffirms this vision, the fulfillment of which requires a national commitment to a program of Earth observations from space in which practical benefits to humankind play an equal role with the quest to acquire new knowledge about the Earth.

The Interim Report described how satellite observations have been critical to scientific efforts to understand the Earth as a system of connected components, including the land, oceans, atmosphere, biosphere, and solid-Earth. It also gave examples of how these observations have served the nation, helping to save lives and protect property, strengthening national security, and contributing to the growth of our economy through provision of timely environmental information. However, the Interim Report also identified a substantial risk to the continued availability of these observations, warning that the nation's system of environmental satellites was "at risk of collapse." As noted above, in the short period since the publication of the Interim Report, budgetary constraints and programmatic difficulties at NASA and NOAA have greatly exacerbated this concern. At a time of unprecedented need, the nation's Earth observation satellite programs, once the envy of the world, are in disarray.

The decadal survey was led by an Executive Committee that drew on the work of seven thematically-organized study panels:

1. Earth science applications and societal needs.
2. Land-use change, ecosystem dynamics, and biodiversity.
3. Weather (including space weather5 and chemical weather6).
4. Climate variability and change.
5. Water resources and the global hydrologic cycle.
6. Human health and security.
7. Solid-Earth hazards, resources, and dynamics.

As described in Chapter 2 of our final report, each of the panels used a common template in establishing priority lists of proposed missions (see Table 1 below). The potential to deliver tangible benefits to society was an overriding consideration for panel deliberations.

Because execution of even a small portion of the missions on the panels' short lists was not considered affordable, panels worked with each other and with members of the Executive Committee to pare the number of missions; they also developed synergistic mission "rollups" that would maximize science and application returns across the panels while keeping within a more affordable budget. Frequently, the recommended missions represented a compromise in an instrument or spacecraft characteristic (including orbit) between what two or more panels would have recommended individually without a budget constraint.

All the recommendations offered by the panels would merit support—indeed, the panels' short lists of recommendations were distilled from the over 100 responses that we received in response to a request for mission concepts, as well as other submissions—but the Executive Committee took as its charge the provision of a strategy for a strong, balanced national program in Earth science for the next decade that could be carried out with what are thought to be realistic resources. Difficult choices were inevitable, but the recommendations presented in this report reflect the committee's best judgment, informed by the work of the panels and discussions with the scientific community, about which programs are most important for developing and sustaining the Earth science enterprise.

The recommended NASA program can be accomplished by restoring the Earth science budget in real terms to the levels of the late 1990s.

2. What are the practical benefits of the research and applications activities that your Decadal Survey recommended?

Our report presents a vision for the Earth science program; an analysis of the existing Earth observing system and recommendations to help restore its capabilities; an assessment of and recommendations for new observations and missions needed for the next decade; an examination of and recommendations concerning effective application of those observations; and an analysis of how best to sustain that observation and applications system. *A critical element of the study's vision is its emphasis on the need to place the benefits to society that can be provided by an effective Earth observation system on a par with scientific advancement.*

The integrated suite of space missions and supporting and complementary activities that are described in our report will support the development of numerous applications of high importance to society. Expected benefits of the fully-implemented program include:

- **Human Health** More reliable forecasts of infectious and vector-borne disease outbreaks for disease control and response.
- **Earthquake Early Warning** Identification of active faults and prediction of the likelihood of earthquakes to enable effective investment in structural improvements, inform land-use decisions, and provide early warning of impending earthquakes.

- **Weather Prediction** Longer-term, more reliable weather forecasts.
- **Sea Level Rise** Climate predictions based on better understanding of ocean temperature and ice sheet volume changes and feedback to enable effective coastal community planning.
- **Climate Prediction** Robust estimates of primary climate forcings for improved climate forecasts, including local predictions of the effects of climate change; determination in time and space of sources and sinks of carbon dioxide.
- **Freshwater Availability** More accurate and longer-term precipitation and drought forecasts to improve water resource management.
- **Ecosystem Services** More reliable land-use, agricultural, and ocean productivity forecasts to improve planting and harvesting schedules and fisheries management.
- **Air Quality** More reliable air quality forecasts to enable effective urban pollution management.
- **Extreme Storm Warnings** Longer-term, more reliable storm track forecasts and intensification predictions to enable effective evacuation planning.

3. How consistent is the President's FY 2008 budget request for NASA and NOAA with the recommendations of the Decadal Survey Committee?

It is important to note we were, of course, not privy to the details of the President's fiscal year 2008 budget, which was developed prior to the release of our final report. The NRC report is a forward-looking document and therefore focuses primarily on the new missions; whereas, the Interim Report dealt with the difficulties and challenges of the Earth observing programs at NASA and NOAA, as they existed in early 2005.

Let me address first the President's FY '08 budget request for NASA Earth science. It is a mixture of some good news and bad news. The primary good news is the small bottom line increases for 2008 and 2009. These increases address the needs of currently planned missions already in development, the completion of which is consistent with the decadal survey's baseline set of assumptions.

Unfortunately, the out-year budgets reveal fundamental flaws in the budget and NASA's Earth science plans—the budgets are totally inadequate to accomplish the decadal survey's recommendations. In 2010, the Earth science budget begins to decline again and reaches a 20-year low, in real terms, in 2012. This decline reflects that the 2008 budget contains no provision for new missions, nor does it allow us to address the significant challenges facing our planet. These disturbing broad trends are captured in Figure 1.

Before turning to NOAA, I want to emphasize that the problems in the out-years appear to be due entirely to the lack of adequate resources. In fact, at a NASA town hall meeting that followed the release of our report on January 15, 2007 at the 2007 annual meeting of the American Meteorological Society, the head of NASA's Earth Science program stated that the recommendations in our report provided the roadmap for the Earth Science program we *should* have.

The NOAA NESDIS budget picture is also a mixture of some good and bad news. In this case, the budget takes a small downturn in FY08, followed by significant growth in FY09–FY10, before turning down again in FY11 (Figure 2). It remains to be seen whether this ~$200 M/year growth in FY09 and FY10 can enable restoration of some of the lost capabilities to NPOESS and GOES-R. There appears to be no budgetary wedge for new starts. Finally, for a variety of reasons, the NOAA NESDIS budget is far from transparent, especially in the out-years, and the level of detail that is readily available makes it difficult to respond adequately to Committee's question.

4. What will be the impact if present trends in Earth and climate science research and applications investments continue?

As detailed in our report and as summarized by my co-chair, between 2006 and the end of the decade, the number of operating U.S. missions will decrease dramatically and the number of operating sensors and instruments on NASA spacecraft, most of which are well past their nominal lifetimes, may decrease by some 35 percent. If present trends continue, reductions of some 50 percent reduction are possible by 2015.

Were this to pass, we would have chosen, in effect, to partially blind ourselves at a time of increasing need to monitor, predict, and develop responses to numerous global environmental challenges. Vital climate records, such as the measurement of solar irradiance and the Earth's response, will be placed in jeopardy or lost. Measurements of aerosols, ozone profiles, sea surface height, sources and sinks of important greenhouse gases, patterns of air and coastal pollution, and even winds in the atmosphere are among the numerous critical measurements that are at risk or simply will not occur if we follow the path of the President 2008 budget and the proposed out-year run out.

Taking this path, we will also forgo the economic benefits that would have come, for example, from better management of energy and water, and improved weather predictions. Again, as my co-chair notes in his comments and testimony, without action on the report's recommendations, a decades-long improvements in the skill in which we make weather forecasts will stall, or even reverse; this may be accompanied by diminished capacity to forecast severe weather events and manage disaster response and relief efforts. The nation's capabilities to forecast space weather will also be at risk, with impacts on commercial aviation and space technology.

The world is facing significant environmental challenges: shortages of clean and accessible freshwater, degradation of terrestrial and aquatic ecosystems, increases in soil erosion, changes in the chemistry of the atmosphere, declines in fisheries, and the likelihood of significant changes in climate. These changes are occurring over and above the stresses imposed by the natural variability of a dynamic planet, as well as the effects of past and existing patterns of conflict, poverty, disease, and malnutrition. Further, these changes interact with each other and with natural variability in complex ways that cascade through the environment across local, regional, and global scales. In summary, absent a reversal of the present trends for Earth observation capabilities, we see the following:

- **Weather forecasts**: After decades of steady improvement, weather forecasts, including those of severe weather such as hurricanes, may become less accurate, putting more people at risk and diminishing the proven economic value of accurate forecasts.
- **Earthquakes, tsunamis, landslides, and volcanic eruptions**: We risk missing early detection of these and other hazards. We also lose our ability to assess damage and mitigate the loss of further human life once they have occurred. Satellite monitoring of volcanic plumes, for example, has a very real impact on air traffic control.
- **Water resources**: We lose many of the needed observations to monitor the health of our water storage reservoirs, and predict droughts with sufficient time to mitigate their impact.
- **Oceans**: Sea level is rising and ice around the world is melting, yet there is uncertainty in how fast these are occurring and whether or not they are accelerating or decelerating. We will become less able address these issues, and assess their implications for our coastal communities.
- **Climate**: We are losing critical observations of the Earth system, the atmosphere, oceans, land, and ice needed to verify and improve the climate models. These models will be increasingly important to the U.S. economy because they best capture the likely patterns of future climate change and variability.
- **Ecosystems**: We lose the ability to assess the health of our forests, wetlands, coastal regions, fisheries, and farmlands and to determine the impact and effectiveness of regulations designed to protect our food supply.
- **Health**: Land-use, land cover, oceans, weather, climate, and atmospheric information observations, now used by public health officials to determine the effects of infectious diseases, skin cancers, chronic and acute illnesses resulting from contamination of air, food, and water are all at risk. As an example, air quality forecasts, which use the global perspective of satellites to identify pollution transport across borders, will become less accurate, with negative implications for both human health and urban pollution management efforts.

I would like to thank the Committee for inviting me to testify, and I would be delighted to answer any further questions.

6.2 National Imperatives for Earth Science Research

STATEMENTS BEFORE SENATE COMMITTEE ON COMMERCE, SCIENCE, AND TRANSPORTATION SUBCOMMITTEE ON SPACE, AERONAUTICS, AND RELATED SCIENCES

March 7, 2007
Statement of Berrien Moore III, Ph.D.
University Distinguished Professor
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Co-Chair, Committee on Earth Science and Applications from Space
National Research Council, The National Academies

Mr. Chairman, Ranking Minority Member, and members of the committee: thank you for inviting me here to testify today. My name is Berrien Moore, and I am a professor of systems research at the University of New Hampshire and Director of the Institute for the Study of Earth, Oceans, and Space I appear today in my capacity as co-chair of the National Research Council (NRC)'s Committee on Earth Science and Applications from Space: A Community Assessment and Strategy for the Future.

The National Research Council is the unit of the National Academies that is responsible for organizing independent advisory studies for the federal government on science and technology. In response to requests from NASA, NOAA, and the USGS, the NRC has recently completed a "decadal survey" of Earth science and applications from space. ("Decadal surveys" are the 10-year prioritized roadmaps that the NRC has done for 40 years for the astronomers; this is the first time it is being done for Earth science and applications from space.) Among the key tasks in the charge to the decadal survey committee were to:

- Develop a consensus of the top-level scientific questions that should provide the focus for Earth and environmental observations in the period 2005-2020; and
- Develop a prioritized list of recommended space programs, missions, and supporting activities to address these questions.

The NRC survey committee has prepared an extensive report in response to this charge, which I am pleased to be able to summarize here today. Over 100 leaders in the Earth science community participated on the survey steering committee or its seven study panels. It is noteworthy that this was the first Earth science decadal survey, and the committee and panel members did an excellent job in fulfilling the charge and establishing a consensus—a task many previously considered impossible. A copy of the full report has also been provided for your use.

The committee's vision is encapsulated in the following declaration, first stated in the committee's April 2005 Interim Report:

"Understanding the complex, changing planet on which we live, how it supports life, and how human activities affect its ability to do so in the future is one of the greatest intellectual challenges facing humanity. It is also one of the most important challenges for society as it seeks to achieve prosperity, health, and sustainability."

As detailed in the committee's final report, and as we were profoundly reminded by the latest report from the International Panel on Climate Change (IPCC), the world faces significant and profound environmental challenges: shortages of clean and accessible freshwater, degradation of terrestrial and aquatic ecosystems, increases in soil erosion, changes in the chemistry of the atmosphere, declines in fisheries, and above all the rapid pace of substantial changes in climate. These changes are not isolated; they interact with each other and with natural variability in complex ways that cascade through the environment across local, regional, and global scales. Addressing these societal challenges requires that we confront key scientific questions related to ice sheets and sea level change, large-scale and persistent shifts in precipitation and water availability, transcontinental air pollution, shifts in ecosystem structure and function in response to climate change, impacts of climate change on human health, and occurrence of extreme events, such as hurricanes, floods and droughts, heat waves, earthquakes, and volcanic eruptions.

Yet at a time when the need has never been greater, we are faced with an Earth observation program that will dramatically diminish in capability over the next 5–10 years.

The Interim Report described how satellite observations have been critical to scientific efforts to understand the Earth as a system of connected components, including the land, oceans, atmosphere, biosphere, and solid-Earth. It also gave examples of how these observations have served the nation, helping to save lives and protect property, strengthening national security, and contributing to the growth of our economy through provision of timely environmental information. The Interim Report documented that NASA had cancelled, scaled back, or delayed at least six planned missions, including a Landsat continuity mission. This led to the main finding in the Interim Report: "this system of environmental satellites is at risk of collapse."

Since the publication of the Interim Report, the Hydros and Deep Space Climate Observatory missions were cancelled; the flagship Global Precipitation Mission was delayed for another two and a half years; significant cuts were made to NASA's Research and Analysis program; the NPOESS Preparatory Project mission was delayed for a year and a half; a key atmospheric profiling sensor planned for the next generation of NOAA geostationary satellites was canceled; and cost overruns led to the NPOESS program undergoing a "Nunn-McCurdy" review. The recertified NPOESS program delays the first launch by 3 years, eliminates 2 of the planned 6 spacecraft, and de-manifests or de-scopes a number of instruments, with particular consequences for measurement of the forcing and feedbacks that need to be measured to understand the magnitude, pace, and consequences of global and regional climate change.

It is against this backdrop that I discuss the present report.

The Decadal Survey presents a vision for the Earth science program; an analysis of the existing Earth observing system and recommendations to help restore its capabilities; an assessment of and recommendations for new observations and missions needed for the next decade; an examination of and recommendations concerning effective application of those observations; and an analysis of how best to sustain that observation and applications system. *A critical element of the study's vision is its emphasis on the need to place the benefits to society that can be provided by an effective Earth observation system on a par with scientific advancement.*

The integrated suite of space missions and supporting and complementary activities that are described in our report will support the development of numerous applications of high importance to society. The expected benefits of the fully-implemented program include:

- **Human Health** More reliable forecasts of infectious and vector-borne disease outbreaks for disease control and response.
- **Earthquake Early Warning** Identification of active faults and prediction of the likelihood of earthquakes to enable effective investment in structural improvements, inform land-use decisions, and provide early warning of impending earthquakes.
- **Weather Prediction** Longer-term, more reliable weather forecasts.
- **Sea Level Rise** Climate predictions based on better understanding of ocean temperature and ice sheet volume changes and feedback to enable effective coastal community planning.
- **Climate Prediction** Robust estimates of primary climate forcings for improved climate forecasts, including local predictions of the effects of climate change; determination in time and space of sources and sinks of carbon dioxide.
- **Freshwater Availability** More accurate and longer-term precipitation and drought forecasts to improve water resource management.
- **Ecosystem Services** More reliable land-use, agricultural, and ocean productivity forecasts to improve planting and harvesting schedules and fisheries management.
- **Air Quality** More reliable air quality forecasts to enable effective urban pollution management.
- **Extreme Storm Warnings** Longer-term, more reliable storm track forecasts and intensification predictions to enable effective evacuation planning.

I will now turn to a brief discussion of the budgetary implications of our recommendations.

The President's FY '08 budget request for NASA Earth science is a mixture of some good news and bad news. The primary bit of good news is the small bottom line increases for 2008 and 2009. These increases address the needs of currently planned missions already in development, the completion of which is consistent with the decadal survey's baseline set of assumptions.

Unfortunately, the out-year budgets reveal fundamental flaws in the budget and NASA's Earth science plans—the budgets are totally inadequate to accomplish the decadal survey's recommendations. In 2010, the Earth science budget begins to decline again and reaches a 20-year low, in real terms, in 2012. This decline reflects that the 2008 budget contains no provision for new missions, nor does it allow us to address the significant challenges facing our planet. These disturbing broad budgetary trends are captured in Figure 1.

Before turning to NOAA, I want to emphasize that the problems in the out-years appear to be due entirely to the lack of adequate resources. In fact, at a NASA town hall meeting that followed the release of our report on January 15, 2007 at the 2007 annual meeting of the American Meteorological Society, the head of NASA's Earth Science program, who appears today with me as a witness, stated that the recommendations in our report provided the roadmap for the Earth Science program we *should* have.

The NOAA NESDIS budget picture is also a mixture of some good and bad news. In this case, the budget takes a small downturn in FY08, followed by significant growth in FY09–FY10, before turning down again in FY11 (Figure 2). It remains to be seen whether this ~$200 M/year growth in FY09 and FY10 can enable restoration of some of the lost capabilities to NPOESS and GOES-R. There appears to be no budgetary wedge for new starts. Finally, for a variety of reasons, the NOAA NESDIS budget is far from transparent, especially in the out-years.

As detailed in our report, between 2006 and the end of the decade, the number of operating U.S. missions will decrease dramatically and the number of operating sensors and instruments on NASA spacecraft, most of which are well past their nominal lifetimes, may decrease by some 35 percent. If present trends continue, reductions of some 50% are possible by 2015.

Were this to pass, we would have chosen, in effect, to partially blind ourselves at a time of increasing need to monitor, predict, and develop responses to numerous global environmental challenges. Vital climate records, such as the measurement of solar irradiance and the Earth's response, will be placed in jeopardy or lost. Measurements of aerosols, ozone profiles, sea surface height, sources and sinks of important greenhouse gases, patterns of air and coastal pollution, and even winds in the atmosphere are among the numerous critical measurements that are at risk or simply will not occur if we follow the path of the President 2008 budget and the proposed out-year run out.

Taking this path, we will also forgo the economic benefits that would have come, for example, from better management of energy and water, and improved weather predictions. Without action on the report's recommendations, a decades-long improvements in the skill in which we make weather forecasts will stall, or even reverse; this may be accompanied by diminished capacity to forecast severe weather events and manage disaster response and relief efforts. The nation's capabilities to forecast space weather will also be at risk, with impacts on commercial aviation and space technology.

The world is facing significant environmental challenges: shortages of clean and accessible freshwater, degradation of terrestrial and aquatic ecosystems, increases in soil erosion, changes in the chemistry of the atmosphere, declines in fisheries, and the likelihood of significant changes in climate. These changes are occurring over and above the stresses imposed by the natural variability of a dynamic planet, as well as the effects of past and existing patterns of conflict, poverty, disease, and malnutrition. Further, these changes interact with each other and with natural variability in complex ways that cascade through the environment across local, regional, and global scales. To cope responsibly with these challenges requires information about our planet; it requires us to expand our scientific basis for foreseeing potential changes and patterns, and this science is dependent upon expanded space-based observation. The needed new missions are set forth in the Decadal Survey; these missions need to be implemented in the coming decade.

I would like to thank the Committee for inviting me to testify, and I would be delighted to answer any questions.

March 7, 2007
Statement of Otis B. Brown, Ph.D.
Dean, Rosenstiel School of Marine and Atmospheric Science, University of Miami
Member, Committee on Earth Science and Applications from Space
National Research Council, The National Academies

Mr. Chairman, Ranking Minority Member, and members of the committee: thank you for inviting me here to testify today. My name is Otis Brown, and I am Dean of the Rosenstiel School of Marine and Atmospheric Science, University of Miami. I am also a member of the National Research Council's Committee on Earth Science and Applications from Space.

As dean of the Rosenstiel School, I have first-hand experience how satellite observations provide real-world results. Following Hurricane Katrina, imagery from our Center for Southeastern Tropical Advanced Remote Sensing

(CSTARS) assisted relief and recovery efforts in New Orleans, tracking to see when and where flood waters had receded to increase the effectiveness of rescue efforts. Also pertinent to the environmental challenges presenting themselves in the Gulf states, we employed satellite imagery that identifies the rate of subsidence in the Mississippi Delta and New Orleans—equally invaluable information when making decisions about the reality and requirements of rebuilding in this area and long-term environmental challenges. This same imagery is what we use to monitor water levels in the Everglades and outbreaks of red tide. And, uses for satellite data only continue to grow as we learn to "see" phenomena like changes in sea surface temperature, sea level, and the size of polar ice caps. I cannot emphasize enough how vital satellite imagery has become to earth observation and consequently our ability to predict, plan, prepare, and respond.

I've been asked to discuss my perspectives on the "National Imperatives for Earth Sciences Research." This topic includes areas relevant to many parts of the federal government. My testimony today focuses on the roles of NASA and NOAA. It also addresses some resource and coordination issues for these two agencies.

As you may know I have been part of the team that recently produced a decadal plan for Earth observations from space, which provides a prioritized roadmap. Our vision is captured in the following declaration:

Understanding the complex, changing planet on which we live, how it supports life, and how human activities affect its ability to do so in the future is one of the greatest intellectual challenges facing humanity. It is also one of the most important challenges for society as it seeks to achieve prosperity, health, and sustainability.

As detailed in the NRC report, and further emphasized by the latest report of the Intergovernmental Panel on Climate Change (IPCC), our planet is faced with a number of significant scientific and societal challenges and their impacts on key parts of our society, economy, and health. The two-year study contained in the NRC report delineates how NASA's Earth science budget has declined 30 percent since 2000, with more funding reductions planned as its priority missions of manned trips to Mars and a station on the Moon take further hold. The National Ocean and Atmospheric Administration (NOAA) likewise faces funding challenges with its National Polar-Orbiting Operational Environmental Satellite System (NPOESS)—now three years behind schedule and $3 billion over budget. Additionally, many of the satellite system's advanced weather and climate instruments have been dropped to address cost and schedule challenges. Meanwhile, current satellites continue to age, and many of us foresee major shortcomings in satellite observations by the end of this decade that will undo much of the progress we have made in earth observation and weather prediction.

So, at a time when our need for understanding the Earth system and the need for Earth observations have never been greater, we are faced with declining investments in Earth science, and, an Earth observation program that will significantly diminish in capability over the next decade.

The first question the National Research Council committee had to address was the national capabilities for Earth observations. We were troubled by the answer.

We found that the current investment strategies had led to a system at risk of collapse. That assessment was based on the observed decline in funding for Earth-observation missions in NASA and the consequent cancellation, downsizing, and delay of a number of critical missions and instruments in both agencies. Since the interim report, matters have only worsened, with further cancellations, descopings and delays of NOAA and NASA satellite plans. This will result in an overall degradation of the network of Earth-observing satellites.

There are many potential consequences. Some examples are:

- Weather forecasts and warnings may become less accurate, putting more people at risk and diminishing the proven economic value of accurate forecasts—this is particularly important to this country since we must cope with many forms of extreme weather, be it in the form of hurricanes, tornadoes, drought, floods or winter storms.
- Climate variability and the rate of change need to be better quantified. Earth is warming because of a small imbalance between incoming solar radiation and outgoing radiation from Earth. Without the recommended measurements, we will not be able to quantify how this net energy imbalance is changing, or when or if the planet will stop warming.
- Climate models have improved steadily over the years, but are far from perfect and must be improved if we are to intelligently cope with climate change. Satellites provide unique observations of the Earth system and validate and improve these models.
- Sea level is rising and glaciers and ice-fields around the world are melting, but we just don't know how fast these are occurring. Without continuing quantitative observations provided via satellites, we can't know how these rates change or the implications for coastal communities.

- Satellite observations could well be pivotal in resolving a controversy about whether the frequency and intensity of hurricanes are increasing; observations of the atmosphere and oceans are essential.
- The limited signals of cataclysmic activity come through vigilant observation. That means the risk of missing early detection of earthquakes, tsunamis, and volcanic eruptions will increase.
- The bottom line is: Earth science is based fundamentally on observations. While it is impossible to predict what scientific advances will not occur without the observations, or what surprises we will miss, we can be sure the rate of scientific progress will be greatly slowed—perhaps even undone to some degree. Without a doubt, it takes us backwards rather than forwards.

Significant advances in hurricane forecasting over the past three decades have come from orbiting satellites that take timely, high-resolution pictures and provide improved estimates of surface wind over the ocean. The satellite images are all over the TV for the public to view, but scientists, dissect them further. From sea level, sea surface temperatures and winds to red tide outbreaks and oil spills, satellite observations afford us a better, informed view of our Earth.

The climate debate has been driven by debate over model capabilities and the lack of long-term critical observations relevant to climate. Many of the capabilities to make such observations exist in the research domain, but have not been transitioned into an operational setting. Our NRC report noted the difficulties in transferring NASA and NOAA research into operational use. That is because there is currently no process to include the necessary scientific input, resources and exploitation capabilities to either facilitate or to define this transition. Thus, we are seeing the winding down of the NASA Earth Observing System and its broad Earth-observing capabilities and information delivery systems, with no apparent way for our nation to harvest the fruits of this multi-billion dollar investment, or, to continue prototype research systems with proven operational value. The follow-on NOAA system, NPOESS, is late and more than likely will not overlap the NASA systems, and, most of the climate-related capabilities are not in its baseline. Put succinctly, much needed long-term time-series of Earth processes required for decisions in this changing world will be lost. This is due to the lack of a functional relationship between research (NASA) and operations (NOAA) for Earth observing systems, and, a lack of resources in NOAA to address all its Earth observing requirements.

The challenge in Earth sciences is that the breadth of study is so large that it's difficult to develop a set of priorities across disciplines. This is the first ever report to provide an integrated set of national priorities for Earth observing from space. It's equally difficult for anyone to imagine how it affects them individually. Often times, it seems we speak in a foreign language about solar irradiance, vector sea surface winds, limb sounding of ozone profiles and water vapor soundings from geostationary and polar orbits—perhaps this is not the clearest way for the public to understand how humans have become dependent on tools that reside in outer space.

What is important to understand about the plan our committee recommended is that its financial requirements are NOT astronomical. In fact, implementing all of the recommendations requires only that we bring the program up to funding levels comparable to the year 2000. The plan we recommend calls for undertaking 17 new NASA and NOAA missions in the period 2008-2020, as well as restoring some of the capabilities lost on NPOESS and GOES, and revitalizing a few delayed NASA missions like GPM and Landsat. Our recommendations for NASA can be implemented in an extremely cost-effective manner. The committee understood the financial constraints and therefore had to find missions capable of tackling several scientific questions simultaneously. The result is that we reduced the number of possible new missions from more than 100 down to 17 broad-ranging, high-value, multipurpose missions. But to accomplish this, NASA's Earth science budget must be restored to year 2000 funding levels. We think this is very reasonable given the obvious societal needs and benefits.

The truth of the matter is that this field of science is inextricably linked to our daily life and that of future generations. Climate variability and natural disasters are taking a significant toll on our economy, our environment, and our well being. And, that is why we must sustain the Earth observations that underpin national preparedness and response. Implementing these missions will not only greatly reduce the risk of natural disasters of all kinds to the people of our country and the world, they will also support more efficient management of natural resources including water, energy, fisheries, and ecosystems, and support the economy. Thus, the cost of the program is repaid many times over.

The observing system we envision is affordable and will help establish a firm, sustainable foundation for Earth science and real societal benefits through the year 2020 and beyond.

Thank you for the opportunity to appear before you today. I would be pleased to answer any questions that you may have.

6.3 Overview Hearing: Balance of Funding at NASA

TESTIMONY BEFORE THE HOUSE COMMITTEE ON APPROPRIATIONS SUBCOMMITTEE ON COMMERCE, STATE, JUSTICE AND RELATED AGENCIES

March 9, 2007
Lennard A. Fisk
NRC Space Studies Board

Mr. Chairman, members of the subcommittee, thank you for inviting me here to testify today. My name is Lennard Fisk and I am the Thomas M. Donahue Distinguished University Professor of Space Science at the University of Michigan. I also served from 1987 to 1993 as the NASA Associate Administrator for Space Science and Applications. I appear here today in my capacity as the Chair of the National Research Council Space Studies Board. The views that I will express, however, are my own.

There are three issues that I would like to discuss today. First, I would like to share my thoughts on what will—or shall we say could be—accomplished in space and Earth science. I am dealing here with the traditional space science disciplines: astrophysics, planetary exploration, heliophysics, and Earth science. Second, I want to discuss what I perceive is the future of human exploration and, finally, I will consider the balance of funding at NASA between space and Earth science and human space exploration.

I want to emphasize that as in any discussion of the future there is a difference between what will be and what could be. There are certain trends in the space and Earth science program, and in human space exploration, which if allowed to continue portend a less than optimum future. Conversely, there are actions that can be taken that will improve our prospects for success.

The Future of Space and Earth Science

Let me begin with the future of space and Earth science. To make this projection, we need first to discuss how science is accomplished. Science is about making discoveries—they can be profound discoveries that alter the concepts we hold of our place in the cosmos, or they can be minor discoveries that reveal some new aspect of a previously studied process. Discoveries lead to insight, insight to knowledge, and in some cases knowledge yields immediate applications that benefit society. Knowledge almost always benefits society in the long run.

A measure then of the health of a science discipline is the pace at which discoveries are being made. Similarly, the prospects for the future of a science discipline can be measured by whether there are any factors that limit the pace of discovery.

Space and Earth science is primarily an observational science. Our discoveries thus come from observations. We do not have much success in predicting in advance what we will observe. Nature appears to be more imaginative than we are. Rather, its secrets are revealed first by observing, and then by constructing a theory or a model that explains the observations.

In the beginning of the space program, when all of space was new to us, every observation was a discovery. There might be concern, I suppose, as the various disciplines of space and Earth science have evolved, and we have observed so much, that the pace of discovery will diminish. I can assure you that it need not.

Each of the disciplines in space and Earth science has demonstrated that there are continuing discoveries to be made, and has positioned itself to be able to do so. Technology has continued to advance to where more detailed and revealing observations can be made. And our understanding of prior observations has improved to where we can search intelligently for new knowledge.

For each discipline of space and Earth science we can cite a breakthrough that awaits us. In astrophysics there is the fundamental question of what is the “dark energy” that appears to be powering the expansion of the universe. In planetary exploration, the fundamental question is whether there is or was life elsewhere in the solar system. In Earth science—what are the consequences of global climate change and how should we as a civilization adapt. In heliophysics—how does the Sun work and influence the Earth and its space environment. To be sure, in each discipline there are sub-disciplines that have been well worked over and discoveries will be at best incremental. These are the exceptions, compared with the overwhelming opportunities that await us to profoundly alter our understanding of the Earth, the solar system, and the universe beyond.

Given that abundant discoveries await us, if we are only bold enough to make the observations, the primary determinant of a bright future for space and Earth science is the rate at which we make new observations; that is, the rate of new space missions. And here the trends are very disturbing.

The NSF evaluates the health of its various science sub-disciplines by considering proposal pressure. If a field is healthy, the NSF receives many proposals from the community. If the field is funded at the correct level the very best of these proposals can be supported. In space and Earth science, we have a similar measure of discipline health. In the decadal survey process, the community proposes missions that the survey evaluates, recommending funding only for the very best that fit in a funding model that is consistent with the expected funding for that discipline within NASA. A case in point occurred in our recent Earth science "decadal survey," *Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond*. The community proposed more than 100 mission concepts, which was culled to a list of 35 missions that were important to pursue, of which 17 could fit in the funding model, and then only by arguing that Earth science funding should increase by a third.

In fact, for each science discipline in NASA there is a sobering downward trend in missions and thus opportunities for discovery. In the mid-1990s there were an average of 7 launches per year for missions in space and Earth science. In the last few years, the rate is more like 5 per year. In 2010-2012 the rate is projected to be under 2 per year.

There are some disciplines for which the downward trend in opportunities for discovery is clearly unacceptable. In Earth science, society is demanding to know the consequences of global climate change in order to plan our future. In the other disciplines of space science, it is a grating waste of the nation's capabilities to reduce our pace of discovery. We have painstakingly built the infrastructure to make the nation foremost in the scientific exploration of space, and to allow it to atrophy borders on neglect.

The Administration and Congress will need to decide what rate of discovery in space and Earth science they want, and that will set an appropriate budget level. At a minimum it needs to be a budget that does not reduce the pace of discovery; that is, one that at least retains current funding levels adjusted for inflation. That is not an insurmountable goal. It requires that funding that was removed from space science in the last few years be restored, and that out-year budgets increase with inflation. For Earth science the problem is more severe, since even the minimum acceptable program requires a 33% increase over the current funding level. In all disciplines, the optimum funding level for space and earth science, in which discoveries are being made at a rate limited only by our ingenuity, is much higher still. Budgets could easily be doubled without concern that the potential for fundamental discoveries from each mission would be compromised.

There is another limiting factor on the rate of discovery in space and Earth science, and that is the vitality of our disciplines. Science, as with any pursuit that requires excellence, depends on competition to ensure vitality and quality. Here, there are some interesting corporate analogues.

Large corporations can become complacent, secure in their size and past records of accomplishment. The large corporations can be jarred out of their complacency by the introduction of so-called disruptive technologies that revolutionize the products and the markets served. These revolutionary technologies can be introduced by small start-up companies, or by competitors. The result can be a healthier, expanding economy.

The issue for space and Earth science is how do we ensure the continual infusion of our equivalent of revolutionary technologies, so that we too maintain our quality and vibrancy, and have a healthy, expanding space program. How do we ensure the infusion of new and better observing techniques, new minds, new ideas that challenge the established concepts?

It is in fact very difficult to ensure the infusion of revolutionary technologies and concepts in budgets that are not growing. Available funds support the established technologies and the established researchers. In our corporate analogy, the funds to introduce revolutionary technologies do not come from existing funds, but rather from new investments. The question in space and Earth science is then where do our new investments come from.

There is a need to maintain or better yet optimize the pace of discovery. There is a need to maintain the quality and vibrancy of the NASA science program through the introduction of revolutionary technologies and concepts. Both requirements demand a budget for space and Earth science that is growing. I remind you that the projected budget for space and Earth science in NASA grows at only 1% per year, which is a declining budget when inflation is included. There needs instead to be real growth.

The Future of Human Space Exploration

Let me turn now to the issue of human space exploration. We are embarking on a bold plan to return to the Moon, and resume human expansion into the solar system. We have said, rather explicitly, that the nation made a mistake in 1972 when we abandoned the Apollo program, and the vast infrastructure we had built to place humans on the Moon. I personally agree. It is indeed time to resume the journey. My opinion is based on a simple expectation. When I imagine our civilization centuries from now, I find it inconceivable that we will not have become a true spacefaring civilization, with routine human flights throughout our solar system, and perhaps beyond. The question then is simply when do we start and who leads. And I would argue that now is a good time, and we should be the leaders.

Having said that, I am concerned that we are failing in our efforts to resume the human journey into space. In evaluating our prospects, it is worthwhile to ask why did we abandon the Apollo program in 1972? That was a time early in my career as a scientist, but I do not recall a public outcry that abandonment was a mistake. Rather, the American people were proud and satisfied that our nation had won the so-called space race, and they no longer perceived that there was a foreign threat to our security from, in this case, the Russians being more capable than we were in space. Moreover, there was a major public distraction as we moved to extract ourselves from the Vietnam War.

Why is the situation today different from what it was in 1972? The nation does not appear to be overly concerned with foreign threats from space. We are perhaps concerned about potential military threats, but far less so about who will get to the Moon. And we are certainly preoccupied as a nation with extracting ourselves from the Iraq War.

The proof that the situation today is not more favorable for human exploration than it was in 1972 can be found in the budgets. Not since FY2005, when the Vision for Space Exploration was first announced, has the budget provided by the Administration or the Congress been equal to the projected cost of this program. Many of us believe that the initial projected costs for returning to the Moon were grossly inadequate, and we have not realized even this level of funding.

We should remind ourselves of the lessons of the development of the Shuttle in the late 1970s. It too was underfunded, which forced technical decisions that resulted in a vehicle that although capable was less reliable and far more costly to operate than initially anticipated.

Under the adage that something is worth doing only if it is worth doing well, I suggest that we are at a major decision point in the human exploration program to return to the Moon. We need to decide if we are truly committed at this time to human expansion into space, which I hope we are, and commit the resources necessary to be successful.

And I would argue that the required resources should be provided because human expansion into space by itself is a worthy adventure for our nation, not because there are other parts of the NASA budget that can be cannibalized to provide even minimal funding.

The Balance of Funding Between Space and Earth Science and Human Space Exploration

Let me turn finally to the coupling between space and Earth science and human exploration. In many ways these are separate efforts. They can be coupled, however, through their funding needs.

In the beginning of the space program, the generous funding that was available for the Apollo program was shared with space science, and built the vast infrastructure for the pursuit of science that we enjoy today. Throughout much of the subsequent history of NASA, the space and Earth science budget tracked the agency's budget, rising and falling with the success the agency had as a whole in obtaining funding. In the mid-1990s, however, the coupling between the funding for space and Earth science and for human space fight was removed. Space and Earth science funding was allowed to grow in proportion to the growth in non-defense discretionary spending, whereas NASA as a whole, and in particular human space flight, was not allowed to grow at this rate. Human space flight was judged to be less in the nation's interest, compared to space and Earth science. As a result, the space and Earth science budget grew to be a much larger share of the NASA budget than in the past.

With the implementation of the Vision for Exploration, funding for space and Earth science in NASA and human space exploration has again been coupled. Science has been perceived as growing at a rate too rapid, compared to other agency needs. The growth that science was to receive, had it followed its historical trend of growth throughout the 1990s and early 2000s, has been used as a bank to provide the funding that human exploration was not able to obtain on its own.

The American Competitiveness Initiative has resulted in increases in funding for such scientific endeavors as the National Science Foundation and the Office of Science in the Department of Energy. It is hard to argue that the science pursued by the NSF and the DoE Office of Science is fundamentally different from the science pursued by space and Earth science in NASA. All yield fundamental knowledge important for U.S. competitiveness. It is interesting to note that had space and Earth science in NASA received the same percentage increases as the NSF in FY2007 it would have followed its historical rate of growth in funding.

There is an important policy decision to make. Should funding for space and Earth science be tied to the fortunes of the human exploration program of NASA and be funded in proportion to what the nation is prepared to spend on space? Or should space and Earth science be viewed as in integral part of the scientific fabric of the nation, and be funded in proportion to what the nation is prepared to invest in science? I personally would argue for the latter.

Thank you very much.

6.4 NASA's Space Science Programs: Review of Fiscal Year 2008 Budget Request and Issues

STATEMENTS BEFORE THE HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY SUBCOMMITTEE ON SPACE AND AERONAUTICS

May 2, 2007
Statement of Lennard A. Fisk
NRC Space Studies Board

Mr. Chairman, members of the subcommittee, thank you for inviting me here to testify today. My name is Lennard Fisk and I am the Thomas M. Donahue Distinguished University Professor of Space Science at the University of Michigan. I also served from 1987 to 1993 as the NASA Associate Administrator for Space Science and Applications. I appear here today in my capacity as the Chair of the National Research Council (NRC) Space Studies Board. The views I share with you today, however, are my own and not necessarily those of the NRC.

You have asked me to testify on the top three goals for NASA's Science Mission Directorate (SMD); the top three programmatic risks facing SMD; the top three strategic investments that should be made in SMD; and also to comment on the balance among the various science themes within SMD. The first three items are of course interrelated. The goals in part should be to eliminate the major risks, and identify the strategic investments needed to do so. I will thus answer these three questions as an interrelated set. I will then comment on the balance among NASA's space science disciplines.

Before considering the questions, I would like to comment on the recent history of SMD, since this context determines the goals, the risks, and the investments required. Throughout much of the history of the space program, space and Earth science in NASA was considered to be a fixed fraction of the NASA budget. In the mid-1990s, however, that rule was discarded, and the budget for space and Earth science was allowed to grow at the same rate as non-defense discretionary spending. Human space flight was not permitted this growth, and so the budget for space and Earth science became an increasingly larger fraction of the overall NASA budget. Whether deliberate or accidental, the result was that science in NASA was considered to be part of the Nation's investments in science, not simply as a fixed part of the investments in space. This rapid growth in science, however, was not uniform. The traditional space science disciplines—astrophysics, planetary sciences, and heliophysics—did very well. However, even in these times of growth in science funding, Earth science was kept at a constant budget, and then in FY2000 it began a steep decline in funding.

With the advent of the Vision for Exploration in FY2005, to extend human presence first to the Moon and then beyond, dramatic changes have occurred in the funding for SMD. Initially, the overall funding for space and Earth science, taken together, was projected to do well. Some disciplines, favored in the Vision, did very well, in some cases at the expense of other disciplines; but summed together, the funding for space and Earth science continued to increase. However, it became increasingly obvious that NASA was not being provided with the funds required to execute the Vision; return the Shuttle to flight, and complete and use the International Space Station; maintain a healthy science program; and support its other missions such as aeronautics research. And so the squeeze was on. One by one, the funding for the various missions that NASA is responsible for have been reduced to a sub-optimum and, in some cases, critically inadequate funding level.

In the case of the funding for SMD, some $3 billion was removed from the run-out budget primarily to pay for the cost of the return to flight of the Shuttle and the completion of the International Space Station. There is no way to remove that much money from a budget without causing disruptions in ongoing programs and distortions in the balance among programs. Ongoing major flight programs, well into development, have priority; new flight programs—the future of the program—are seriously delayed or in effect cancelled. Small flight missions and basic research support—for technology development, the training of students, theory, data analysis, and new mission planning—all become vulnerable when there is a sudden and unanticipated change in the expected growth in funding.

To understand the inadequacies in the SMD budget, we need to consider how science is conducted. Science is about making discoveries—they can be profound discoveries that alter the concepts we hold of our place in the cosmos, or they can be minor discoveries that reveal some new aspect of a previously studied process. Discoveries lead to insight, insight to knowledge, and in some cases knowledge yields immediate applications that benefit society. Knowledge almost always benefits society in the long run.

A measure, then, of the health of a science discipline is the pace at which discoveries are being made. Similarly, the prospects for the future of a science discipline can be measured by whether there are any factors that limit the pace of discovery.

Space and Earth science is primarily an observational science. Our discoveries thus come from observations. In each of the disciplines in space and Earth science there are, in fact, extraordinary opportunities to make discoveries. Technology is advancing to where more detailed and revealing observations can be made. And our understanding of prior observations has improved to where we can search intelligently for new knowledge.

Given that abundant discoveries await us, if we are only bold enough to make the observations, the primary determinant of a bright future for space and Earth science is the rate at which we make new observations; that is, the rate of new space missions. And here the trends are very disturbing. For each of the disciplines in SMD there is a sobering downward trend in missions and thus opportunities for discovery. In the mid-1990s there was an average of 7 launches per year for missions in space and Earth science. In the last few years, the rate is more like 5 per year. In 2010-2012, the rate is projected to be under 2 per year.

There are some disciplines for which the downward trend in opportunities for discovery is clearly unacceptable. In Earth science, society is demanding to know the consequences of global climate change in order to plan our future. In the other disciplines of space science, it is a grating waste of the nation's capabilities to reduce our pace of discovery. We have painstakingly built the infrastructure to make the nation foremost in the scientific exploration of space. To allow it to atrophy borders on neglect.

There is another consequence of the inadequacies of the SMD budget, and that is the vitality of our disciplines. The issue for space and Earth science is how do we ensure the infusion of new and better observing techniques, new minds, new ideas that challenge the established concepts? It is in fact very difficult to ensure the infusion of revolutionary technologies and concepts in budgets that are not growing. Rather, there needs to be new investments.

There is a need to maintain or, better yet, optimize the pace of discovery. There is a need to maintain the quality and vibrancy of the NASA science program through the introduction of revolutionary technologies and concepts. Both requirements demand a budget for space and Earth science that is growing. I remind you that the projected budget for space and Earth science in NASA grows at only 1% per year, which is a declining budget when inflation is included. There needs instead to be real growth.

Strategic Goals, Risks, and Investments for the Science Mission Directorate

The first strategic goal of the Science Mission Directorate (SMD) might well be stated—get back the money that was lost. A more constructive way to make this statement would be to note how inadequately NASA as an agency is currently funded. The agency is being asked to do too much with too little, and as a result all components of the agency, including science, are sub-optimally funded. We all need to recognize that without major relief to the total funding for NASA this nation does not have a viable space program capable of meeting the broad national needs that have been assigned to it. And we should all make it a strategic goal to provide NASA with the funding that is required.

The risk to SMD from inadequate funding is that it cannot perform its assigned tasks. The charge to the space and Earth science program in NASA is to explore the universe and lay down the foundational knowledge for the human expansion into space. It is to determine the future of the Earth, so sound policy decisions can be made to protect the future of our civilization. It is to contribute to the capability of the United States to compete in the world, whether it is through new knowledge, new technology, or a new workforce. The funding for space and Earth science in NASA, particularly the growth in funding in the years ahead is inadequate to perform this job, and failure to address this problem is a fundamental risk to the success of SMD in being able to fulfill its obligations to the scientific excellence of the nation.

The investment required in SMD is the same investment that the nation is prepared to make in the American Competitiveness Initiative. ACI has resulted in increases in funding for programs in fundamental science in, e.g., the National Science Foundation and the Office of Science in the Department of Energy. These programs were among only a few that saw increases beyond their FY2006 budget level in the enacted FY2007 budget. It is difficult, in fact, impossible, to distinguish between the fundamental science conducted by NASA in SMD and the fundamental science conducted by the NSF or the DoE Office of Science. It is interesting to note that had the funding for SMD been allowed to increase in the same proportion as the NSF it would have followed the pattern of growth it had enjoyed in the late 1990s and the early 2000s, and would have provided funding that was better able to support the needs of the space and Earth science program.

The second strategic goal is for SMD to make more cost-effective use of the funds that have been provided to it. There is a disturbing upward trend in the cost of flight missions. The problem seems to be most egregious in the case of moderate and small flight missions. We seem unable to execute a mission of comparable complexity today for anywhere near the cost that was required in the previous decade. The cost of launch vehicles has increased. The cost of management oversight is increasing. We take actions that are perceived to reduce risk, but may not be cost effective. Whatever the reason, it should be a strategic goal to get the maximum science for the minimum funding, and, in my judgment, the most likely place to realize cost savings is in the execution of moderate and small flight missions.

There is a risk to SMD should it fail to improve the cost-effectiveness with which it executes moderate and small flight missions. Under any circumstance, funding will be limited. We need to get the maximum science for the minimum available funding, if for no other reason than to introduce flexibility into the SMD budget to fund new missions and needed investments.

Investments are required to achieve the strategic goal of improving the cost-effectiveness of small and moderate missions. Investments may be required in new launch vehicles so that the cost of access to space is reduced, particularly with the planned retirement of the Delta-II launch vehicle. Investments will be required in innovative management procedures and new technologies. There needs to be a concerted effort made to make full use of the best of the nation's vast infrastructure to conduct cost-effective space missions. We have great talent in this country for space hardware. We need to ensure that we are using this talent properly; that our processes ensure good engineering solutions and not simply someone's perceived reduction in risk.

If new funds for SMD can be provided, if missions can be executed more cost-effectively, or preferably both, *the third strategic goal should be to use the funds realized to rebalance the program*. When the funding in the out-years for SMD was reduced, the large flight programs under development were protected. It is the future that has been sacrificed. Missions still in technology development were halted. The pipeline that is essential to the development of technology and human capital—the Research and Analysis programs, sounding rockets, small flight missions—have been seriously disrupted. The portfolio of activities in SMD needs to be rebalanced so that we complete what we have begun, while at the same time we recognize that the scientific exploration and utilization of space is a long-term effort that will extend into the indefinite future. The investments that we make now, in people, in technology, in balloons and sounding rockets, in small flight missions, in the planning for future flight missions, will determine the vibrancy and the success of the scientific exploration and utilization of space in the decades ahead.

The risk of failing to meet the strategic goal of rebalancing the SMD program is, in my judgment, the most serious risk. The pipeline of human capital and technology has been disrupted, and the future of the space and Earth science program is at risk. Consider a case in point. Almost every experimental space scientist currently practicing learned his/her trade in the sounding rocket or balloon programs. Yet with recent budget cuts, these programs are unable to perform this task. Small flight missions are the next step in the natural evolution of experimental capabilities, whether it is the development of new technology or the development of experienced scientists and engineers. And yet with recent budget cuts, the flight rate of small missions has been diminished compared to its previous rate.

It follows, then, given the importance of rebalancing the SMD program to protect the future of space and Earth science, that *an investment that ensures a proper pipeline in human capital and technology will have the highest return*. Research and analysis funding, sounding rockets and balloons, and small flight missions all need to be restored to their proper place in the SMD program.

The Balance Among the Science Disciplines in the Science Mission Directorate

Each of the science disciplines in SMD—astrophysics, planetary sciences, heliophysics, and Earth science—has important tasks to perform, ranging from providing fundamental knowledge of the universe, to, in the case of Earth science, providing knowledge that is a direct and immediate benefit to society. Each of the disciplines has need of more funding, more cost-effective use of its funding, a rebalanced program, and the investments required to achieve these goals, as we discussed above.

In the case of Earth science, however, no amount of efficiencies, no internal rebalance within the discipline, no modest investment will provide the resources necessary. There is not adequate funding for Earth science in NASA to accomplish the mission that it has been assigned—to use the global vantage point of space to provide information on the immediate future of Earth, on which we can base sound policy decisions to protect our future. This deficiency is the result of a downward trend in the funding for Earth science that has persisted for a decade, and which has been in serious decline since FY2000. The recent NRC decadal survey for Earth science outlined the measurements and flight missions that NASA needs to accomplish, to provide society with the knowledge that is required. And the survey pointed out that these measurements can be made only if the Earth science budget, over the next several years, is increased back to at least the level of funding that was available in FY2000, an approximately $500 million increase over the current budget.

This is not a rebalancing question, in the sense that Earth science should grow at the expense of other science disciplines. Nor should it grow at the expense of other programs within NASA. All of NASA's programs are currently inadequately funded. And all have a role to play in the national priorities. Rather, it is time for a new initiative, a specific directed task to NASA, with requisite funding provided, to pursue a vigorous Earth science program, in which the required measurements on the future of Earth are all made.

We need to consider NASA as an agency with many important tasks to perform. It is not just the agency that is to return us to the Moon, and all else is a secondary priority. Space is integral to the fabric of our society. We depend on it in our daily lives; we protect our nation through our space assets; we use space to learn about our future; we enrich our society with knowledge of our place in the cosmos; we are moving our civilization into space; we expect the next generation of scientists and engineers to be versatile in the utilization and exploration of space. NASA has an essential role to play in each and every one of these national pursuits, and its role in each pursuit needs to be properly funded.

Thank you very much.

May 2, 2007
Statement of Daniel N. Baker
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Introduction

Mr. Chairman, Ranking Minority member, and members of the committee, I want to thank you for the opportunity today to address key issues that face the NASA science enterprise. I want specifically to address the impacts of the proposed FY2008 budget on the NASA Heliophysics program. My name is Daniel Baker and I am a professor of astrophysical and planetary sciences at the University of Colorado. I am also the Director of the Laboratory for Atmospheric and Space Physics at CU-Boulder. The Laboratory is a research institute that has over 60 teaching and research faculty in the several disciplines of space and Earth sciences. My institute, which we call LASP for short, receives some $50-$60 million per year to support experimental, theoretical, and data analysis programs in the Space and Earth Sciences. The vast majority of these resources come from NASA. Other strong support comes from NSF, NOAA, and other federal agencies. LASP presently supports some 120 engineers, dozens of highly skilled technicians, and over 20 key support personnel. We are very proud, as well, that LASP has over 60 graduate students and another 60 undergraduate students who are pursuing education and training goals in space science and engineering.

I myself am a space plasma physicist and I have served as a principal investigator on several scientific programs of NASA. I am now a lead investigator in the upcoming Radiation Belt Storm Probe (RBSP) mission that is part of NASA's Living With a Star program. I am also an investigator on NASA's Cluster, Polar, MESSENGER, and Magnetospheric Multi-Scale (MMS) missions. Presently, I serve as Chair of the National Research Council's Committee on Solar and Space Physics. By virtue of that position, I also am a member of the Space Studies Board, chaired by my colleague, Dr. Len Fisk. The views I am presenting here are my own, however.

First, and foremost, I would like to begin by commending the American people, and you as their representatives, for the significant investment made in NASA science. The scientific community is well aware of how difficult it has become to find funding for the many worthy programs that you must consider. We sincerely appreciate continued support from Congress and from the American public. It is a major and lasting achievement of our nation that it finds the means and the will to look beyond the pressure of present-day concerns, to focus on questions about humanity's place in the universe, our relationship to our Sun and the nearby planets, how the Earth and its environment have functioned in the past, and how they may change in the future. I believe—as do you, I suspect—that the United States has benefited greatly from investment in space research. Not only is the technological base of our country strengthened by NASA innovations, but our prestige and competitiveness in the world and our educational investment in the future technical workforce are greatly enhanced by NASA science leadership.

Overview of FY2008 Budget Impacts to the Heliophysics Program

The National Research Council's (NRC's) 2003 solar and space physics (SSP) decadal survey, *The Sun to the Earth—and Beyond: A Decadal Strategy for Solar and Space Physics*, laid out a clear, prudent, and effective program of basic and applied research. The envisioned program would address key science objectives such as: understanding magnetic reconnection—the physical process underlying much of space physics; discovering the mechanisms that drive the Sun's activity and produce energetic particle storms in the heliosphere; determining the physical interactions of the Earth's ionosphere with the atmosphere and magnetosphere; as well as addressing a host of other questions that are essential to understanding our local space environment. The Decadal Plan would also have allowed an end-to-end view of the connected Sun-Earth system through NASA's Living With a Star (LWS) program, thereby enhancing greatly the ability to provide realistic specification and forecasts of space weather. Through both its basic research component and its applied component, the Heliophysics Program would therefore contribute substantially and directly to national needs and to the Vision for Space Exploration.

At present, the Heliophysics Division (HPD) of NASA has a number of exciting projects that have been launched or are ready for launch. The dual-spacecraft STEREO mission is being commissioned and is returning amazing new 3-dimensional views of the Heliosphere. Detailed images of the Sun are also being provided by the newly-launched Hi-node mission, a joint Japan-U.S. venture. The five-spacecraft THEMIS mission was successfully launched in February 2007 and is already providing remarkable multi-point measurements in Earth's magnetosphere. Because of our large role in the program, we at LASP are very excited about the successful launch just last week of the upper atmospheric AIM spacecraft as part of the Explorer program. The first LWS mission, Solar Dynamics Observatory (SDO), is well into development preparing for launch in 2008. Thus, the HPD program has several highly capable new space assets that are joining the Heliophysics Great Observatory constellation of operating spacecraft.

Beyond this good news, however, there are significant concerns. Beginning with the FY2005 NASA budget plan, and continuing through the FY2008 budget and its 5-year run-out, the future Heliophysics program has been significantly compromised. The Solar-Terrestrial Probes (STP) line of missions has had over half of its budget content removed, resulting in at least a 6-year gap in STP launches. Within the current NASA budget horizon extending to 2015, the STP line is now down to a single mission launch, the Magnetospheric Multi-Scale (MMS) mission. The venerable and highly successful Explorer mission line (managed by HPD for all of NASA) has had over $1 billion of budget authority removed in the run-out from FY2005 onward. As shown in the figure below, the Explorer budgets in the FY2008 and its run-out are about half of what they would have been expected to be based on the FY2004 budget and its run-out.

As Principal Investigator (PI)-led missions with a rapid development time, Explorers have proven invaluable for investigating the broad range of Heliophysics science. The drastic funding reduction in this line has greatly reduced HPD's ability to respond effectively to new science/technology advances. The sounding rocket program (and, indeed, the entire suborbital program) is at a dangerously low, bare-bones resource level. The research and analysis (R&A) program was deeply cut last year and no funding restorations seem likely at present. The impact of these cuts will be felt for many years since R&A, Explorers, and Suborbital programs are key elements in capitalizing on the investments that have already been made and for attracting and training the next generation of space scientists and engineers. Moreover, the high priority "Flagship" mission for Heliophysics, the Solar Probe Mission, is not presently contained in NASA's plan.

The other major component of the Heliophysics program is Living with a Star (LWS). The funding profile for LWS as defined by the FY2005 and FY2006 budgets allowed for a robust program. In the FY2008 budget plans, however, LWS funding is stretched out so that simultaneity between missions such as Radiation Belt Storm Probes (RBSP) and Ionosphere-Thermosphere Storm Probes is lost. Alarmingly, and rather inexplicably, the previously-budgeted funding for the RBSP Missions of Opportunity is eliminated from the FY2008 plan. Such reductions to LWS are threatening the success of the immediate program as well as the timely implementation of missions such as Sentinels, which are necessary to fulfill the President's 2004 Vision for Space Exploration. These reductions are impeding progress in understanding the origins of the severe space weather events that have the potential to disrupt civil and military satellite communications, applications that rely on the Global Positioning System (GPS), and power generation and transmission systems. Given the large investments that NASA will make to fulfill the Vision for Space Exploration and the investments that the nation, as a whole, is increasingly making in space-based technology, it seems ill-considered to decrease support for LWS, the NASA program that is most closely directed toward protecting those investments.

To be sure, some of the fiscal problems in Heliophysics and elsewhere are related to mission cost growth. Much of this problem, however, lies in non-technical issues that the science community and the Decadal Survey could not have anticipated, including substantial increases in launch vehicle costs, the effects of full-cost accounting, and mandates for additional layers of oversight and review. As noted above, the problems with the Heliophysics program started well before the FY2008 budget plan, but the trends have been perpetuated in the FY2008 budget and its 5-year run-out.

Specific Questions Concerning Heliophysics

I present here my detailed answers to the questions addressed to me by the Chairman in his letter of 11 April 2007:

1. Perspective on the balance of the NASA Heliophysics program and its mix of program elements.

Considerable anxiety is being caused in the science community due to the anticipated and extraordinary reductions in the smaller mission opportunities and sustaining research programs that form the support for much of the university-based research (in which students and early-career scientist are involved). Small missions, such as those in the Explorer and Earth System Science Pathfinders programs, provide projects in which new concepts are tested for a modest investment and where students first learn the space science and engineering trade. This particularly applies to sounding rockets, balloons, and aircraft flights that provide opportunities on a time scale that falls within the educational horizon of a graduate student. Since 2000, the historical sounding rocket launch rate has dropped more than half (from about 30 to 10 missions per year), with anticipated further reductions as a result of the FY2008 budget. The present run-out budget places even the regular launch facilities, such as those at Poker Flat in Alaska, in danger by 2008. Staff reductions may be necessary at the Wallops Island Flight Facility in a matter of months if additional funds are not forthcoming to the sounding rocket program. I am delighted that Dr. Alan Stern, the new Science Mission Directorate (SMD) Associate Administrator, is taking actions now to remedy the suborbital situation.

The Explorer program is another prime example of the severe impacts in the Heliophysics program. Explorers are the original science missions of NASA, dating back to the very first U.S. satellite, Explorer I. They are universally recognized as the most successful science projects at NASA, providing insights into both the most remote parts of our universe and the detailed dynamics of our local space environment. The Advanced Composition Explorer (ACE) now stands as our sentinel to measure, in situ, large mass ejections from the Sun and the energetic particles that are a danger to humans in space. Two relatively recent Explorers, TRACE and RHESSI, study the dynamics of the solar corona where large solar storms originate, storms that often threaten satellites and other technological assets on which we depend. The recently launched THEMIS constellation and the AIM mission were both done under the Explorer program aegis. Explorers are among the most competitive solicitations in NASA science, and offer opportunities for all researchers to propose new and exciting ideas that are selected on the basis of science content, relation to overall NASA strategic goals, and feasibility of execution. As noted in the figure above, the FY2008 proposed run-out for Explorers will mean a program that is reduced by over half from its proposed FY2004 guidelines. I am again encouraged by the fact that a new Announcement of Opportunity for Small Explorers will be released, thanks to Dr. Stern, by October 2007.

A specific continuing concern to university-based scientists is the impact on the sustaining Research and Analysis (R&A) budgets. The R&A program initiates many of the new, small scientific efforts that eventually lead to the major missions that NASA pursues. R&A grants are highly competitive, maximize the science investment of on-going missions by allowing all scientists to use available data, and are heavily geared toward student and young faculty participation. These are moderate-duration efforts, usually lasting three to four years, where new hardware and theoretical approaches are explored. NASA was forced last year by budget realties to propose an across-the-board reduction of 15% in these programs. This may not appear catastrophic at first sight, but a sudden reduction in such a long-term program can have huge effects. If the budget were allowed to grow once again, the R&A program would slowly recover over the next few years. However, with the present budget prospects, there is skepticism about such future restoration. There is widespread recognition that these realities will inevitably reduce the number of new students who enter university programs such as mine.

2. Does the Heliophysics program reflect the priorities of the NRC Decadal Survey in solar and space physics?

Whereas NASA is attempting to implement some of the highest priority programs from the NRC's 2003 Decadal Survey, the pace and balance of activities seems highly unlikely to achieve the Decadal goals. In 2004, an NRC committee was tasked to assess the role of solar and space physics in the Vision for Space Exploration—*Solar and Space Physics and Its Role in Space Exploration*. This committee stated that:

> NASA's Heliophysics program depends upon a balanced portfolio of spaceflight missions and of supporting programs and infrastructure. There are two strategic mission lines—Living With a Star (LWS) and Solar-Terrestrial Probes (STP)—and a coordinated set of supporting programs. LWS missions focus on observing the solar activity, from short-term dynamics to long-term evolution, that can affect the Earth, as well as astronauts working and living in a near-Earth space environment. Solar-Terrestrial Probes are focused on exploring the fundamental physical processes of plasma interactions in the solar system.

Solar and Space Physics and Its Role in Exploration examined the 2003 Decadal Survey and made the following three recommendations:

> 1. To achieve the goals of the exploration vision there must be a robust program, including both the LWS and the STP mission lines, that studies the heliospheric system as a whole and that incorporates a balance of applied and basic science.
>
> 2. The programs that underpin the LWS and STP mission lines—MO&DA [Mission Operations and Data Analysis], Explorers, the suborbital program, and SR&T [Supporting Research and Technology]—should continue at a pace and level that will ensure that they can fill their vital roles in Heliophysics research.
>
> 3. The near-term priority and sequence of solar, heliospheric, and geospace missions should be maintained as recommended in the Decadal Survey report both for scientific reasons and for the purposes of the exploration vision.

These recommendations remain valid today and the mission priorities within the basic (STP) and applied (LWS) science mission lines as listed in the original Decadal Survey are basically reflected in the Heliophysics budgets for these two mission lines. Where NASA has deviated from the Decadal Survey is in putting greater weight on Living With a Star missions and losing the balance between applied and basic science. Such a priority of emphasizing short-term capability of predicting space weather over the long-term goal of understanding the underlying physical principles may have some practical expedience. A more critical issue, however, is the fact that small missions and supporting research have not kept pace. If these budgets are allowed to decline greatly, Heliophysics will quickly cease to be a robust, viable discipline. It now appears that with mission cost growth and reduced Heliophysics funding, it is very unlikely that most Decadal Survey missions will be completed within the decadal window.

The Sun to the Earth—and Beyond was the first Decadal Survey conducted by the solar and space physics community. The Decadal Survey involved hundreds of scientists in discussions that spanned nearly two years. The scientific priorities set out in the survey remain valid today and there is no community movement to change them. But Decadal Surveys are not just a list of science priorities. To design a coherent program across a decade it is essential to have a realistic budget profile as well as reasonably accurate estimates of both technical readiness and costs of each mission. The Decadal Survey committee worked hard with engineers and NASA management to develop realistic mission costs and a program architecture that fit within budget profiles anticipated in the FY 2003 budget. But changes to the budget profile beginning in FY 2005 necessitated a substantial stretching of the mission schedule. Furthermore, under-costing of just a few missions wreak havoc with even the best-laid plans. The scientific community needs to work with NASA to find ways to cost missions accurately, particularly large missions (for example, by applying lessons learned from management of smaller, PI-led missions as appropriate, and insisting upon greater accountability).

3. What are the three top risks facing the Heliophysics program over the next 5 years?

Heliophysics, like most of the NASA science enterprise, is significantly affected by some very basic, systemic issues. These issues spread throughout all programs, projects, and missions. A continued forward propagation of these problems ultimately represents a huge level of risk for the subdisciplines of the SMD and for the Agency as a whole:

- **Prudent Management of Risk**. Getting into space, working in space (either for humans or for machines), and returning appropriate data from space is an inherently "risky" business. Despite highly competent people exercising all sensible and prudent care, there can be failures of space missions. For those programs involving humans and human life, truly heroic measures must be employed and extraordinary efforts must be extended to assure that missions do not fail: In the human space flight realm, failure is not an option.

In the robotic exploration realm, there are a wide range of mission sizes and costs. Very large, high-profile missions of great complexity, international prominence, and resource investment may have to be safeguarded by many levels of review and hardware redundancy. Such approaches tend to drive up program costs tremendously. However, for smaller missions, there is a proper level of redundancy, scrutiny, and oversight that matches the program scale. To do more than this "due diligence" drives costs for even small-end missions to extraordinary levels. Such fear of failure, or undue "risk aversion" is having very detrimental effects on Heliophysics missions.

What we really need to focus on is the management of risk. Since the first Explorer, almost 50 years ago, NASA science projects have been extraordinarily successful. But over the years, the management procedures and quality assurance burden for robotic science projects has grown to an almost unsustainable level—commensurate with human spaceflight missions—without any quantifiable impact on improving the ultimate reliability of science missions (as far as many scientists can discern). In my view, the American people accept the idea that the space business is risky, especially during launch and re-entry. Given launch risks, it makes no sense to spend hundreds of millions of dollars on procedures that might improve the reliability of payloads far beyond, say, the 98 percent or 99 percent reliability level.

There is considerable debate whether present reliability approaches are actually achieving more assurance than this. We have all learned that unnecessary risk in human spaceflight programs has tragic consequences and clearly more must be done to minimize that risk. It is equally true that *not* taking risks in leading-edge robotic science projects has undesirable results. Not only must science continue to push the technological envelope where failure is a risk that accompanies new ideas, but these projects provide opportunities for training staff and students in an environment where failure is not life-threatening, and where a student can gain hands-on experience in the real work of building state-of-the art instrumentation. Having gained this expertise, these students can go on to form the workforce of future operational robotic science missions and human spaceflight missions.

- **Lack of affordable access to space.** A major hallmark of the past science program of NASA has been the regular, frequent launches of a balanced portfolio of small, medium, and large missions to address key science questions and to test new enabling technologies. "Balance" in this context does not mean equal dollars in all mission categories, but rather it means appropriate investment in small-end missions targeted toward specific science questions and toward workforce development, as well as investments in major flagship programs. In my view, there should be heavy emphasis on smaller spacecraft and suborbital missions. (This idea has been endorsed by last year's NRC report *An Assessment of Balance in NASA's Science Programs*).

Unfortunately, the cost of launching missions into space has grown out of all proportion to the cost of small scientific satellites and payloads. This imbalance between payloads and launch costs is destroying the ability of the Heliophysics Division to develop and maintain its regular, frequent launches of Small Explorers, University-Class Explorers, and even Solar-Terrestrial Probe missions. The risks associated with increasing costs of access to space, in my view, are threatening to sink the entire carefully-laid plans for Heliophysics science.

There are some disturbing recent signs in the access to space arena. One of the longest-serving launch vehicles for NASA missions, the Boeing Delta II vehicle, is being eliminated as an option for future science programs. Much of the NASA medium-lift needs for Earth-orbiting and planetary missions was carried out using the Delta II. Losing the "sweet spot" around which so many NASA launches were planned will, I fear, propagate in highly detrimental ways throughout the space science enterprise.

I have also mentioned above the removal of funding for the RBSP Missions of Opportunity. It is hard to imagine a more cost-effective investment that NASA can make than to launch instruments on commercial or partner-nation spacecraft. For a relatively small NASA investment, the science enterprise gains access to a highly leveraged program and can often provide a complementary science capability that lends a robustness and insurance that could not be afforded any other way. I am very encouraged that Dr. Stern has voiced strong public support for MoOs.

- **Erosion of trained workforce**. A key to the success of NASA as a whole, and Heliophysics in particular, is the availability of hardware-educated scientists and "hands-on" trained engineers. Nearly all space projects require a great deal of technical competence, and a correspondingly competent workforce. There has been a steady erosion of that workforce, not only at NASA but across the entire country, and this fact has been decried from many quarters. The NRC report, "*Rising Above the Gathering Storm*," makes this case most emphatically. Other technical industries have been able to compensate somewhat by tapping the pool of highly-trained immigrants and foreign students, and they often outsource work abroad. But spacecraft are ITAR sensitive items, so this pool is not available to NASA or to its outside space-enterprise partners, even to universities, because of the constraints of the law. All the space programs at NASA, DOE, NOAA, and the DOD feel this shortage acutely. And the situation will probably just get worse unless something is done.

NASA commissioned the NRC to study how the workforce necessary to carry out the Vision for Space Exploration can be maintained given the impending retirement of much technical talent. The report, released earlier this week, cites the need for more highly skilled program and project managers and systems engineers who have acquired substantial experience in space systems development, and identifies limited opportunities for junior specialists to obtain hands-on space project experience as one of the impediments to NASA's ability to execute the Vision. The report recommends that NASA place a high priority on recruiting, training and retaining skilled program and project managers and systems engineers, and that it provide hands-on training and development opportunities for younger and junior personnel (*Building a Better NASA Workforce: Meeting the Workforce Needs for the National Vision for Space Exploration,* p. 7).

It is clear that there is a shortage of engineers and scientists who have actually built space hardware, and know how that hardware can be integrated and function within larger, more complex systems. NASA science programs are a critical source of this needed native talent, whether they remain in NASA science programs or move out into the larger industrial base. Education at its very best is a process of discovery and of trial-and-error: the efficacy of learning-by-doing has been proven over many years.

NASA needs to maintain its investment in space science programs that allow universities to attract and engage undergraduate and graduate students in all aspects of mission development and deployment—from proof of concept studies, to proposal submittal, to prototype development, to launch, data analysis, and publication. Whether these programs have short or long time horizons, there are ways to allow the next generation of space scientists to participate in all aspects of an exciting NASA mission.

4. What would be the top three investments that could be made to benefit the Heliophysics program over the long-term?

The Heliophysics Division would benefit substantially in the long-term from several immediate investments. These include not only dollars, but "intellectual capital" and renewed commitments to a properly balanced experimental, theoretical, and modeling program.

- **Lower cost and frequent access to space**. In my view, the single greatest impediment to a healthy and vigorous Heliophysics program is the uncertainty and cost of getting spacecraft and suborbital missions launched. Obviously, the Heliophysics Division cannot, and should not, pay for developing new launch vehicles. But HPD, NASA in general, the Congress, and other stakeholders should work together to make sure that every avenue for launching space hardware is made readily available to research teams. This should include less expensive domestic launch vehicles, "military" launchers (such as the Minotaur rocket), secondary launch capabilities on commercial and U.S. military vehicles, and unfettered access to non-U.S. launch vehicles. In the latter category are launches on European, Indian, Japanese, and other launch systems that can offer very attractive prices for access to space. A secondary launch on an Ariane 5 vehicle, for example, could be obtained for as little as $1 million or so.

In this category of access to space, I would also place Missions of Opportunity (MoOs). Launching NASA instruments or payload suites on commercial or military vehicles, or onboard foreign spacecraft, can provide tremendous "bang for the buck." I know from public statements by Dr. Stern that he recognizes the power and benefits of MoOs and I hope this avenue to space can be pursued aggressively. The MoO component should certainly be restored explicitly to the Radiation Belt Storm Probe program.

- **Regular cadence and more frequent small-end missions**. As pointed out above, the key to a healthy, robust Heliophysics program is to have more and better opportunities for Small Explorer (SMEX), University-Class

Explorer (UNEX), and suborbital missions. This emphasis is wholly consistent with the Decadal Survey recommendations and it fulfills a wide variety of programmatic, educational, and workforce training goals that I have alluded to above. The investment necessary to achieve the desired outcome in this arena could be readily accomplished (I believe) by restoring the Explorer mission line to the budgetary level that existed in the FY2004 budget plan (~$350 million per year). The combination of sound management approaches, reasonable launch costs, sensible numbers of reviews, and appropriate levels of risk tolerance would, I maintain, allow a very vigorous small-mission capability within Heliophysics for a very modest amount of new budgetary authority.

- **Improve management of mission costs**. As has been alluded to above, the Heliophysics missions—as with most of NASA programs—have increased in cost to well above the levels planned in the 2003 Decadal Survey. Much of this has been due to factors touched on earlier: access to space has become prohibitively expensive and "risk aversion" has increased mission development costs to extraordinary heights. I believe that Heliophysics should invest time and money now into developing an approach to mission management that uses prudent levels of reviews and much wiser risk mitigation strategies. Some years ago—perhaps a decade or so—"best practices" were developed for PI-led missions and I firmly believe those practices could and should still serve as the basis for managing essentially all Heliophysics instrument and spacecraft programs. A small investment now in improved management approaches both at NASA Headquarters and NASA Centers would pay tremendous future dividends.

Summary

Fortunately, smaller-end programs such as R&A, sounding rockets, and the Explorer mission line could be restored to the levels anticipated in the FY2004 budget by infusions of modest amounts of budget authority. For the larger Heliophysics programs (Solar-Terrestrial Probes and Flagship missions), comparatively higher levels of resources are required. Better management of programs and containment of cost growth is clearly necessary to stretch available dollars. However, absent a restoration of more balanced budgets to levels planned as recently as FY2004, it will not be possible to have a robust program that is capable of meeting high priority national needs.

Thank you very much for your attention.

May 2, 2007
Statement of Joseph A. Burns
Irving P. Church Professor of Engineering and Astronomy
Vice Provost for Physical Sciences and Engineering
Cornell University

Mr. Chairman and Members of the Committee:

I appreciate having this opportunity to testify before you today. For most of my professional life, I have been an active planetary scientist and an unabashed enthusiast for space exploration. I chaired the 1994 National Research Council (NRC) strategy for solar system exploration, and more recently I was a member of the NRC's 2003 decadal panel on planetary sciences. I also served as a panel member on the NRC's 2001 decadal report for astronomy and astrophysics.

We meet at a time when, once again, NASA's planetary missions are returning truly remarkable results. For the last three years, the twin *Mars Rovers* have marched systematically across Mars's arid surface, poking their instruments into assorted rocks. These measurements and observations by several superb orbiting spacecraft have revolutionized our perception of the Red Planet, revealing it to have previously been episodically much wetter and perhaps even hospitable to life. *Cassini*, the most recent planetary flagship mission, is orbiting Saturn, where its broad instrument suite has been surveying this ringed beauty for nearly three years, finding that a disparate pair of Saturnian satellites—Titan and Enceladus—are potentially habitable islands in this frigid world. *Stardust*'s capsule has returned samples of comet Wild-2's dust back to Earth and this material has testified about the turbulent nature of the gas/dust cloud that gave birth to our local planetary system. *New Horizons* peeked at Jupiter as it streaked past on its voyage to Pluto. And just last week, a Swiss team spied the 229th extra-solar planet, and a most special one: the first known so far, but for Earth, to reside in its star's habitable zone, where water—life's requisite ingredient—remains fluid. The early 21st century is truly a time of extraordinary discovery in planetary and other space sciences. The continuing generous and unwavering support of Congress and the American people has made these accomplishments possible.

Starting with Sputnik's launch fifty years ago this October, all Earth's peoples—including you and I—have been privileged to participate as our planetary environs have been "*explored, discovered and understood,*" to invoke NASA's mantra. Scientists believe that this exploration program addresses profound questions about our origins and that it provides unique insights into how our Earth functions as a planet. At the same time the public finds this investigation of Earth's surroundings to be inspiring and meaningful. January's issue of the popular magazine *Discover* listed its top-ranked one hundred findings across all scientific disciplines during 2006. Of these, fully one-seventh came from astronomy, with half concerning solar system objects or extrasolar planets. So what could be better? The reason why we aren't all celebrating is, because, while America's planetary exploration program is indeed doing well currently, its future is quite uncertain.

I submit to you that an appropriate analogy might be that today's planetary program is like a powerful ship that appears to be staunchly cruising along, making good progress as its crew explores and probes a rich, ever-surprising shoreline. But our vessel is sailing so smoothly nowadays principally because of yesterday's investments. Without continued attention, the ship's momentum will inexorably be drained away. In fact, today's craft is running low on fuel, some of its machines are not being properly maintained and upgraded, improved replacement instruments are unavailable, and sadly the boat's crew is aging. Surprisingly, this ship is from the nation that has always led in exploration of the cosmos. Maybe other nations instead will guide humankind's search of the next shoreline, just as four centuries ago England replaced the Portuguese and the Spanish, partway through the exploration and subsequent development of the New World. Only if we are vigilant today will our ship's journey be secure, with it resupplied, its instruments revitalized and its crew replaced.

To carry our nautical analogy one step further, fortunately during these treacherous times NASA's Science Mission Directorate has a new admiral—Alan Stern—and the Planetary Science Division has a new captain—Jim Green. These are excellent choices—enthusiastic, knowledgeable and creative scientists who happily are also experienced and successful managers. They will be energetic advocates for—and tireless workers toward—a productive, healthy and effective planetary program.

I now respond to the topics that you have asked me to address. Please note that my ordering is a little different than yours and that many of these items are linked so that my answers to one may overlap with another topic.

Mission Mix

Here I will restrict my comments to a consideration of missions; these engineering marvels provide us the capability to *"explore"* as NASA's slogan states. Technology development and research funding will be discussed in later sections.

Planetary science's 2003 decadal survey recommends a finely tuned mix of mission sizes, each with its own programmatic purpose, cost cap and launch rate. *Discovery* missions (e.g., *Deep Impact* that slammed into comet Tempel-1 on July 4, 2005) permit rapid response to discoveries across a range of topics; such missions should launch every eighteen months or so. *New Frontiers* spacecraft (e.g., the *New Horizons* mission en route to Pluto and beyond) allow thorough study of pressing scientific questions, with a selection every two or three years. *Flagship* missions (e.g., the *Cassini* spacecraft presently observing the Saturn system)—comprehensive investigations of extraordinary high-priority targets—should be flown at the rate of about one per decade. The separate Mars program has a comparable breakdown of mission classes into large, medium and small (*Mars Scout*) categories.

How do the various missions and their mix fare in the FY08 budget and beyond? The pace of future *Discovery* missions seems about on track, after several years of delayed selections. The *New Frontiers* line has fallen to half the planned rate; the next selection should be made in the next year to get this program back on track.

Once again, no new *Flagships* have been started. The *Europa Geophysical Orbiter* has been indefinitely deferred; it was THE *Flagship* mission recommended for this decade by the decadal study. In fact, at present, no planetary flagship mission is in development, an unprecedented situation that has not happened since the start of the American planetary program. Hence, in view of the necessary preparations and required budget, no major mission will be launched until 2017, and even that schedule will require a significant augmentation to the budget. I am somewhat encouraged that NASA has recently initiated \$1M studies of four potential very capable missions to satellites of Jupiter and Saturn; three of these spacecraft would reconnoiter their targets for their suitability to sustain life. Nonetheless it should be recognized that **no** funds are available in the foreseeable future to actually build and fly **any** *Flagship*, if one were to be selected.

Mars flight missions have been reduced from a nominal two launches per opportunity to just one every two years. To accommodate this change, the number of medium-class missions to the Red Planet is lowered, and two *Mars Scouts* are eliminated. In terms of *Flagships*, during the FY 2006 budget-rebalancing exercise, *Mars Sample Return*, a crucial mission to understand the Martian mineralogy and to develop a Martian chronology, was delayed from "early in the next decade" until at least ~2024.

The reining-in of the aspirations of the planetary program is a direct consequence of fewer dollars being available. The agency budget has not grown to accommodate the President's exploration vision, and so NASA has covered its shortfall by draining $3 B from the science program, 97 percent of that coming from solar system exploration, especially Mars. Thus the planetary program has become a source of funds to support other demands for NASA's needs. I am puzzled that NASA would chose to lessen robotic solar system studies, especially investigations of Mars, given the ultimate destination for the President's vision. The NRC's Space Studies Board has been steady in its belief that robotic exploration and human exploration are complementary ventures to understand and exploit Earth's neighbors.

At the time when the American solar system exploration program is slowing down, our international partners (and competitors) are expanding theirs. The European Space Agency has very capable spacecraft orbiting each of Earth's planetary neighbors, as well as another well-instrumented craft on its way to land on a comet. And soon yet more European spacecraft will be exploring the Moon, where it will join scientific missions from Japan, China and India. Now, when other nations have improved capabilities, we should be pursuing increased interactions with them. However, ITAR regulations hamper international cooperation on existing and planned space missions.

Much of the slowdown in America's exploration of the solar system is not presently apparent because most of pain has been deferred to beyond 2011 . . . to the next administration. But planetary missions require extended advanced planning, especially if we are to collaborate with international partners. For example, the Cassini-Huygens mission to Saturn, on which I am a member, started planning in the early 1980s, selection of payload instruments and team members took place in 1990, launch in 1997, arrival in 2004. Scientific results were not returned until more than twenty years after the mission was initially devised.

The reduced run-out budget for the planetary division, coupled with growth in the cost to mount each of these mission classes, means that the planetary survey's plan is not attainable. New flight projects, especially for outer planet (see below) and Mars exploration, will not be started. The reduction in missions can be painlessly accommodated in the short term because the affected missions occur beyond 2011. However, if the workforce drifts away to other areas and if technology development lags, the loss to the U.S. planetary program will become increasingly irreversible. Analysts suggest that a minimum of at least $200 M more annually would be needed in the PSD budget in order to bring it in line with the strategic plans of the decadal survey.

Research and Analysis Funds

Now I will address the support for research and analysis (R&A) and technology development. The 2003 planetary survey recommended "an increase over the decade in the funding for fundamental research and analysis programs at a rate above inflation…[till it reaches] closer to 25 percent of the overall flight-mission budget." Instead R&A funding has fallen one-quarter from its FY05 level. The budget that you are considering today recommends that this budget line continue to slip further behind the inflation rate, in clear contradiction to the decadal report. Yet it is only through these studies that the American populace "*understands*" the data being returned from Mars, Saturn and other scientific stations.

This continuing decline in R&A funding is troubling for several reasons. Improved understanding and answers motivate our visits to other solar system bodies; to accomplish these goals requires follow-up studies. When funds for supporting research are tight, scientists who are early in their careers are most affected. I know several young scientists who are contemplating career changes because they perceive bleak prospects with space missions. Moreover, any shortfall in the science and engineering workforce will damage the long-term technical and scientific capabilities that underpin the solar system exploration program. Finally, with few academic posts as yet in this emerging discipline and with limited interest to date from the defense/commercial sectors, a higher fraction of the planetary community is supported by soft money than in other astronomical disciplines. Taking a bigger view, I am surprised that NASA's science program has not been considered part of the America's Competitive Initiative, for this program has drawn many to engineering and science as careers.

NASA's goal to *"discover"* becomes somewhat problematic if only limited opportunities exist to analyze mission results. Funding for data analysis should increase in proportion to the growing data volume and the diversity of targets, now including solar wind samples, comet dust, remote-sensing data obtained by dedicated missions at terrestrial and giant planets and measurements taken at academic laboratories.

Top Risks for Next Five Years

The future U.S. space enterprise is jeopardized by the loss of core competencies (both technology development and personnel) as a consequence of inadequate base-program resources. Furthermore, the rapid growth in mission costs limits the nature and number of flights that can be flown. Finally the lack of long-lived power sources will prevent missions to the outer solar system.

Monies for technology development are limited. Nonetheless the American planetary program needs more capable instruments to perform more effectively in more difficult environs. For example, dollars could be saved and mission opportunities expanded if in-space advanced propulsion and more efficient radioisotope power systems were available. Future missions will require that samples be returned from inhospitable places and/or that on-site analytical tools be accessible. A healthy funding level would support new instrument development through spaceflight qualification. A limited budget causes a chicken-and-egg problem: present-day funds cannot support both capable missions and the technology that makes those missions as worthwhile as they might be.

Mission costs are rising quickly for several reasons. For some years NASA has been risk-averse and, in today's litigious society, this tendency has only increased. This leads to unnecessary oversight and documentation, with attendant costs, both financial and programmatic. The absence of an adequate technology development program requires either the costly ab initio development of new instruments or flying last year's technology. ITAR, which considers satellite technology to automatically be munitions under State Department rules, hamstrings spacecraft operations and complicates international space programs. Expendable launch vehicle costs are growing faster than inflation, because of the limited market. *Discovery* has a separate problem: the imminent phase-out of the Delta-II expendable launch vehicle, which will require future flights to be flown aboard the more-expensive and too-capable EELV (evolved extended launch vehicle) fleet, namely Delta-IVs and Atlas-Vs. Given *Discovery*'s fixed cost cap, substantial increases in launch-vehicle costs erode the science that these missions can achieve.

The usual power supply for missions beyond Jupiter—RTGs containing plutonium-238—is increasingly scarce, meaning that new starts to outer solar system are no longer feasible. Unless this issue can be resolved to provide power on distant flights, the solar system no longer extends to comet belt, but rather it stops at Jupiter, something similar to halting Henry Hudson at the Azores. This is especially troubling as many of the discipline's highest priority targets—Jovian and Saturnian satellites plus Neptune/Triton—are very distant. These power generators are also preferred for energy-intensive explorations of Mars.

Especially Beneficial Strategic Investments

Investments in core technologies, science instruments and infrastructure will be most fruitful for the long-term health of the planetary exploration program. Such investments are likely to also benefit other parts of NASA, additional federal agencies that have space platforms and the commercial sector.

The overall budget for solar system exploration should be reinstated so as to allow a continuing reasonable rate of *Discovery* and *New Frontier* flights, but also a new *Flagship* mission, since all classes play important roles in any balanced plan. A sharp increase in R&A funds is essential to a healthy program.

The Human Exploration program needs to be stabilized in order to minimize its potentially adverse impact on science programs. The Shuttle should be retired by 2011 to obviate serious concerns about its safety. Moreover, the operational costs of the Shuttle are eating NASA's lunch (and dinner!).

Place of NASA's Proposed Lunar Science Initiative

In spite of the current drought in new mission starts, humankind's exploration of the Moon is reasonably robust, thanks in part to significant international involvement. At the Moon, or soon to be launched, are six lunar missions:

four from other nations (Europe, China, Japan and India) as well as a U.S. Lunar Reconnaissance Orbiter and a U.S. Lunar Crater Observation and Sensing Satellite. With this expansion of information about the Moon, it may be time to reassess the adequacy of the current lunar research budget line to benefit fully from the returned results about the surface and interior of Earth's natural satellite.

In addition to these more focused missions, one of the decadal study's recommended *New Frontiers* was to return samples from a deep lunar crater, partly to learn what the lunar interior can tell about the Moon's origin, but also to develop technology that may be deployed at Mars and Venus as well as on comet nuclei. This mission has not yet been selected, but it undoubtedly will be a candidate in the next round. In the more distant future, we have the prospect of human exploration of the moon beginning as early as 2020. All told, these programs form a sustainable initiative of lunar science exploration.

Concluding Remarks

These are exciting times for the planetary program. Unfortunately budgetary constraints are jeopardizing the future of this program. If the United States is to *"explore, discover, understand"* Earth's surroundings, as NASA claims it wishes to do, more attention and additional funding seem to be required. The planetary science community believes that, with Congressional support, and new very capable leaders at the helm of our ship of discovery, our nation's exploration of the solar system will continue to make great progress in understanding our neighboring worlds.

Mr. Chairman and Members of the Committee, I thank you for your attention today, but most of all for your continuing support to NASA's planetary exploration program.

6.5 NASA's Earth Science and Applications Programs: Fiscal Year 2008 Budget Request and Issues

STATEMENTS BEFORE THE HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY SUBCOMMITTEE ON SPACE AND AERONAUTICS

June 28, 2007
Statement of Richard A. Anthes, Ph.D.
President of the University Corporation for Atmospheric Research and
Co-Chair, Committee on Earth Science and Applications from Space
National Research Council, The National Academies

Mr. Chairman, Ranking Minority Member Calvert, and members of the subcommittee: thank you for inviting me to testify on this important subject. My name is Richard Anthes, and I am the President of the University Corporation for Atmospheric Research (UCAR), a consortium of 70 research universities that manages the National Center for Atmospheric Research, on behalf of the National Science Foundation, and additional scientific education, training and support programs. I am also the current President of the American Meteorological Society. I appear today in my capacity as co-chair of the National Research Council (NRC)'s Committee on Earth Science and Applications from Space: A Community Assessment and Strategy for the Future.

The National Research Council is the unit of the National Academies that is responsible for organizing independent advisory studies for the federal government on science and technology. In response to requests from NASA, NOAA, and the USGS, the NRC has recently completed a "decadal survey" of Earth science and applications from space. ("Decadal surveys" are the 10-year prioritized roadmaps that the NRC has done for 40 years for the astronomers; this is the first time it is being done for Earth science and applications from space.) Among the key tasks in the charge to the decadal survey committee were to:

- Develop a consensus of the top-level scientific questions that should provide the focus for Earth and environmental observations in the period 2005-2020; and
- Develop a prioritized list of recommended space programs, missions, and supporting activities to address these questions.

The NRC survey committee has prepared an extensive report in response to this charge. Over 100 leaders in the Earth science community participated on the survey steering committee or its seven study panels. It is noteworthy that this was the first Earth science decadal survey, and the committee and panel members did an excellent job in fulfilling the charge and establishing a consensus—a task many previously considered impossible. A pre-publication version of the report was published in January 2007 and is available at <http://www.nap.edu/catalog/11820.html>.

The committee's vision is encapsulated in the following declaration, first stated in the committee's interim report, published in 2005:

> Understanding the complex, changing planet on which we live, how it supports life, and how human activities affect its ability to do so in the future is one of the greatest intellectual challenges facing humanity. It is also one of the most important challenges for society as it seeks to achieve prosperity, health, and sustainability.

As detailed in the committee's final report, and as we were forcefully reminded by the latest set of reports from the International Panel on Climate Change (IPCC), the world faces significant and profound environmental challenges: shortages of clean and accessible freshwater, degradation of terrestrial and aquatic ecosystems, increases in soil erosion, changes in the chemistry of the atmosphere, declines in fisheries, and above all the rapid pace of substantial changes in climate. These changes are not isolated; they interact with each other and with natural variability in complex ways that cascade through the environment across local, regional, and global scales. Addressing these societal challenges requires that we confront key scientific questions related to ice sheets and sea level change, large-scale and persistent shifts in precipitation and water availability, transcontinental air pollution, shifts in ecosystem structure and function, impacts of climate change on human health, and occurrence of extreme events, such as hurricanes, floods and droughts, heat waves, earthquakes, and volcanic eruptions.

As a result, one way or the other, our international neighbors and we will undoubtedly be taking steps in an effort to deal with the climate changes we will confront. And as we do so, policy makers and others will want to know if such steps are actually making a difference in addressing climate change. Yet at a time when the need for that kind of information has never been greater, we are faced with an Earth observation program that will dramatically diminish in capability over the next 10-15 years.

Between 2006 and the end of the decade, the number of operating missions will decrease dramatically and the number of operating sensors and instruments on NASA spacecraft, most of which are well past their nominal lifetimes, will decrease by some 35 percent, with a 50 percent reduction by 2015 (Fig. 1). Substantial loss of capability is likely over the next several years due to a combination of decreased budgets and aging satellites already well past their design lifetimes.

In its report, the committee sets forth a series of near-term and longer-term recommendations in order to address these troubling trends. It is important to note that this report does not "shoot for the moon," and indeed the committee exercised considerable restraint in its recommendations, which were carefully considered within the context of challenging budget situations. Yet, while societal applications have grown ever-more dependent upon our Earth observing fleet, the NASA Earth science budget has declined some 30 percent in constant-year dollars since 2000 (Fig. 2). This disparity between growing societal needs and diminished resources must be corrected. This leads to the report's overarching recommendation:

> The U.S. government, working in concert with the private sector, academe, the public, and its international partners, should renew its investment in Earth observing systems and restore its leadership in Earth science and applications.

The report outlines near-term actions meant to stem the tide of capability deterioration and continue critical data records, as well as forward-looking recommendations to establish a balanced Earth observation program designed to directly address the most urgent societal challenges facing our nation and the world (see Fig. 3 for an example of how nine of our recommended missions support in a synergistic way one of the societal benefit areas—extreme event warnings). It is important to recognize that these two sets of recommendations are not an "either/or" set of priorities. *Both* near-term actions *and* longer-term commitments are required to stem the tide of capability deterioration, continue critical climate data records, *and* establish a balanced Earth observation program designed to directly address the most urgent societal challenges facing our nation and the world. It is important to "right the ship" for Earth science, and we simply *cannot* let the current challenges we face with NPOESS and other troubled programs stop progress on all other fronts. Implementation of the "stop-gap" recommendations concerning NPOESS, NPP, and GOES-R is important—and the recommendations for establishing a healthy program going forward are equally as important. Satisfying near-term recommendations without placing due emphasis on the forward-looking program is to ignore the largest fraction of work that has gone into this report. Moreover, such a strategy would result in a further loss of U.S. scientific and technical capacity, which could decrease the competitiveness of the United States internationally for years to come.

Key elements of the recommended program include:

- Restoration of certain measurement capabilities to the NPP, NPOESS, and GOES-R spacecraft in order to ensure continuity of critical data sets.
- Completion of the existing planned program that was used as a baseline assumption for this survey. This includes (but is not limited to) launch of GPM in or before 2012, securing a replacement to Landsat 7 data before 2012.
- A prioritized set of 17 missions to be carried out by NOAA and NASA over the next decade (see Tables 1 and 2 below). This set of missions provides a sound foundation for Earth science and its associated societal benefits well beyond 2020. The committee believes strongly that these missions form a minimal, yet robust, observational component of an Earth information system that is capable of addressing a broad range of societal needs.
- A technology development program at NASA with funding comparable to and in addition to its basic technology program to make sure the necessary technologies are ready when needed to support mission starts over the coming decade.
- A new "Venture" class of low-cost research and application missions that can establish entirely new research avenues or demonstrate key application-oriented measurements, helping with the development of innovative ideas

and technologies. Priority would be given to cost-effective, innovative missions rather than ones with excessive scientific and technological requirements.

- A robust NASA Research and Analysis program, which is necessary to maximize scientific return on NASA investments in Earth science. Because the R&A programs are carried out largely through the Nation's research universities, such programs are also of great importance in supporting and training the next generation of Earth science researchers.
- Suborbital and land-based measurements and socio-demographic studies in order to supplement and complement satellite data.
- A comprehensive information system to meet the challenge of production, distribution, and stewardship of observational data and climate records. To ensure the recommended observations will benefit society, the mission program must be accompanied by efforts to translate raw observational data into useful information through modeling, data assimilation, and research and analysis.

Further, the committee is particularly concerned with the lack of clear agency responsibility for sustained research programs and the transitioning of proof-of-concept measurements into sustained measurement systems. To address societal and research needs, both the quality and the continuity of the measurement record must be assured through the transition of short-term, exploratory capabilities, into sustained observing systems. The elimination of the requirements for climate research-related measurements on NPOESS is the most recent example of the failure to sustain critical measurements. Therefore, our committee recommends that the Office of Science and Technology Policy, in collaboration with the relevant agencies, and in consultation with the scientific community, should develop and implement a plan for achieving and sustaining global Earth observations. This plan should recognize the complexity of differing agency roles, responsibilities, and capabilities as well as the lessons from implementation of the Landsat, EOS, and NPOESS programs.

In your invitation, Mr. Chairman, you asked me to explicitly address a number of issues and I am pleased to do so:

1. What, in your perspective, should be the top three priorities for the NASA Earth sciences program over the next five years, and what, if any, are the most significant challenges in meeting those priorities?

This is a somewhat difficult question to answer. Five years from now is well into the period covered by the Decadal Survey, and the Survey has recommended a balanced set of 15 high priority missions for NASA. This set of 15 missions was derived from over 100 proposed missions, so a great deal of priority setting has already taken place by the community. It is therefore important to make progress on all of these missions during the next five years, with greater attention paid to the recommended missions early in the queue (the 2010 to 2013 timeframe as described in the report). Thus my answer to this question will focus on the highest priorities to begin in FY08 in order to lay the foundation for implementing the full set of recommendations during the next decade.

- First, NASA should commit to and begin to implement its recommended Decadal Missions. Although, the NASA budget for Earth Sciences is not now adequate to implement the survey recommendations (see next question), a useful start can be made with modest resources. The survey's initial seven missions (2010-2013) should begin in 2008; the first four (CLARREO, SMAP, ICESat-II, and DESDynI) should begin intensive Phase A activities and the next three (for the time period 2013-2016—HyspIRI, ASCENDS, and SWOT) should begin pre-Phase A studies. Increment needed beyond President's Request in FY08: \$90 million.
- Second, NASA should increase its suborbital capabilities. NASA's airborne programs have suffered substantial diminution and should be restored. In addition, NASA should lead in exploiting unmanned aerial vehicles (UAV/ technology). Both conventional and UAV aircraft are needed for instrument development, and hence risk reduction and technology advancement, and for their direct contribution to Earth observations. Increment needed beyond President's Request in FY08: \$10 million.
- And third, NASA should increase support of its Research and Analysis (R&A) program and in Earth System modeling. Improved information about potential future changes in climate, weather, and other environmental conditions is essential for the benefit and protection of society. This improvement will come from: a) better observations (the recommended missions and enhanced suborbital capabilities); b) more capable models of the Earth System; and c) a vigorous research program to use the observations in models and interpret the results. The R&A program has

suffered significant cuts in recent years and these should be reversed. R&A investments are among the most cost-effective as they directly exploit on-going missions, advance knowledge to better define what is needed in the future, and sustain and develop the requisite scientific and engineering workforce. Increment needed beyond President's Request in FY08: $20 million.

2. What are your perspectives on how well the FY 2008 budget request and out year projections for NASA's Earth science program align with the recommendations of the Earth science decadal survey?

The FY2008 budget request for NASA's Earth science program is inadequate to meet the recommendations of the decadal survey. Figure 2 compares the request and the requirements to carry out the recommendations. Even with an encouraging increase in the NASA Earth Science request for FY08, it still falls short of what is needed to get a full start on the recommended program. Moreover, the out year projections show a *steady decrease* when the requirements call for an increase to a level of about $2.1 billion by 2010 with a level budget (in real dollars) after that.

This committee's leadership on Earth sciences and the recent actions in the House appropriations process with respect to FY08 are encouraging and greatly appreciated. I am hopeful that the Congress and the Administration will ultimately support the actions taken by this committee and the appropriators in the FY08 appropriations process and continue to build on that momentum into the future.

3. Could you please describe your views on how NASA might begin to implement the recommendations of the National Academies' Earth science decadal survey?

It is a truism that to begin a long journey you have to take the first step. NASA should first commit to implementing the recommendations in a timely fashion, and then begin developing implementation plans and schedules for the recommended missions and supporting research and technology development. I am encouraged that NASA is planning workshops to further analyze the decadal survey recommended missions, but to develop the survey ideas further will require substantial investments.

Implementing the survey results will require modest increments in the NASA Earth Science budget, restoring the budget back to where it was in real dollars in the early part of this decade. This will require NASA to request the necessary resources and for Congress to provide them. Alternatively, Congress could take the lead and require NASA to implement the survey while providing the resources.

My recommended first specific steps for implementation are given in my answer to the first question.

4. What are your perspectives, as an individual researcher, on international collaborations in the Earth sciences, and what value would international collaborations offer in advancing the recommended missions in the decadal survey?

As the survey states, international partnerships can be very important in implementing complex expensive space missions such as recommended in the survey. Collaborations with other nations not only save scarce resources for all the partners, they promote scientific collaboration and sharing of ideas among talented people of all nations. Most of the smart people in the world do not live in the United States! International collaborations increase the brain pool to carry out the challenging proposed missions and use the observations in creative, innovative ways for the benefit of society.

However, international collaborations come at a cost. Any time partners are involved, control must be shared and the success of the mission depends critically on the performance of all the partners. If one partner runs into difficulties (e.g. financial support is withdrawn), the entire mission can be threatened. A successful collaboration also requires assurance that data will be shared and that U.S. scientists are full partners on teams that ensure adequate pre-launch instrument characterization and post-launch instrument calibration and validation. Other issues such as regulations governing the sharing of technologies (e.g. International Traffic in Arms Regulation, ITAR), governance and even language and cultural differences can make international partnerships more difficult and risky than "going it alone." Nevertheless, the potential benefits outweigh the downsides and NASA, NOAA and their U.S. partners in academia and industry should seek opportunities for international partnerships at every turn.

Mr. Chairman, the observing system we envision will help establish a firm and sustainable foundation for Earth science and associated societal benefits through the year 2020 and beyond. It can be achieved through effective management of technology advances and international partnerships, and broad use of satellite science data by the research and decision-making communities. Our report recommends a path forward that restores U.S. leadership in Earth science and applications and averts the potential collapse of the system of environmental satellites. As documented in our report, this can be accomplished in a fiscally responsible manner, and *I urge the committee to see that it is accomplished.*

I close my testimony with a quote from Vice Admiral Richard H. Truly, former NASA Administrator, Shuttle Astronaut and the first commander of the Naval Space Command in a recent report *National Security and the Threat of Climate Change*. Admiral Truly speaks as one of 11 retired senior military officers who wrote this report that describes the serious threat of climate change to the nation's security. Describing his experience in space 25 years ago, Admiral Truly said:

> I have images burned in my mind that will never go away—images of the earth and its fragility. I was a test pilot. I was an aviator. I was not an environmentalist. But I do love the natural environment, and seeing the earth from space was the experience that I return to when I think about what we know now about climate. . . .
>
> When you look at the earth's horizon, you see an incredibly beautiful, but very thin line. That thin line is our atmosphere. And the real fragility of our atmosphere is that there's so little of it. . . .
>
> The stresses that climate change will put on our national security will be different than any we've dealt with in the past. For one thing, unlike the challenges we are used to dealing with, these will come upon us extremely slowly, but come they will, and they will be grinding and inexorable. . . .

Admiral Truly said he was not convinced of the importance of climate change by any person or interest group—he was convinced by the data. We as a nation must continue to provide the data on the Earth, for only the data can reveal the truth that will affect us all.

Thank you for the opportunity to appear before you today. I am prepared to answer any questions that you may have.

June 28, 2007
Statement of Eric J. Barron
Dean, Jackson School of Geosciences and Jackson Chair in Earth System Science
University of Texas at Austin
Member, Committee on Earth Science and Applications from Space
(Chair, Climate Variability and Change Panel)
National Research Council, The National Academies

Mr. Chairman, Ranking Minority Member Calvert, and members of the subcommittee: I appreciate the opportunity to provide this testimony on *NASA's Earth Science and Applications Programs: Fiscal Year 2008 Budget Request and Issues."* My name is Eric Barron, and I am Dean of the Jackson School of Geosciences at the University of Texas at Austin. I was also the Chair of the Climate Variability and Change Panel, which was one of the key components of the National Research Council (NRC)'s Committee on Earth Science and Applications from Space: A Community Assessment and Strategy for the Future.

Our most basic objective is to simultaneously protect life and property, promote economic vitality, and enable environmental stewardship. Regardless of our views on climate change, we all recognize that this objective is a balancing act. It is impossible to have billions of people on a planet and not have an environmental impact. Impact is also clearly associated with individual, regional and national levels of consumption. We also know that nations that have the strongest economies are the ones who are the most capable of adapting to change or mitigating its adverse consequences. Finding the optimum balance is enormously challenging and is in itself a subject of great debate. However, it becomes impossible if we lack sufficient knowledge of how the Earth operates. We need a commitment in two key areas if we are to achieve this most basic objective. First, we need to know how the components of the Earth are changing in response to human activity and natural forces. Second, we need to continue to improve our ability to "anticipate" or predict the future on a variety of time scales. If current climate projections are correct, climate change over the next ten to twenty years will have highly noticeable impacts on society and the demand on climate scientists will begin to broaden substantially. Impacts on agriculture, water resources, human health, and ecosystems are likely to drive a public demand for climate knowledge that is both sector (agriculture, health, water, etc.) and regionally dependent. It will be our ability to anticipate or forecast all of these elements in the future, and then to take appropriate action on these predictions with full understanding of their uncertainties, that can enable us to simultaneously protect life and property, promote economic vitality, enable environmental stewardship, and help assess a broad range of policy options for decision-makers.

This view yields six key tenets that should define the observation systems of the future:

(1) Sustained multi-decadal, global measurements and data management of quantities that are key to understanding the state of the climate and the changes taking place are crucial.

(2) Climate change research, including the observational system, will be increasingly tied directly toward understanding the processes and interactions needed to improve our predictive capabilities and resolve the probabilities associated with different outcomes.

(3) Evaluation and assessment of model capability will increasingly be the focus of future measurement activities. Demonstrating model capability is likely to be a driver for developing and evolving observation systems and field campaigns.

(4) The link between climate research and societal benefit will require a much greater emphasis on higher spatial resolutions in climate predictions, observations, and assessments.

(5) The "family" of climate observing and forecasting products will continue to grow, involving innovative research into societal connections with energy, agriculture, water, human health, and a host of other areas, creating new public and private partnerships.

(6) The demand to understand the connection between climate and specific impacts on natural and human systems will require a more comprehensive approach to environmental observation and modeling in order to integrate the multiple stresses that influence human and natural systems (i.e. climate, land use, and other human stressors such as pollutants).

The importance of climate information is clear. As economic impact from climate change grows there will likely be both a change in research emphasis and a demand for much greater investment in climate research. Yet, the NASA investment in climate research and observation is in serious decline. We will enter the next decade with an observing system that is substantially less capable than we had at the start of the 21st century.

The specific questions provided by the Subcommittee help elucidate this issue and I am pleased to answer them to the best of my ability.

1. What is NASA's contribution to the U.S. Climate Science Research Program in terms of percentage of overall expenditures and percentage of sensors dedicated to studying Earth's Climate? What fraction of the world's effort on climate change research does NASA's contribution represent?

At the start of the U.S. Global Change Research Program, considerable effort was invested in labeling the contributions of each federal agency to the components of global change research including climate. Further, this analysis identified contributions to the observing and modeling components of the investment in climate research. In 1992, NASA contributions were approximately 70 percent of the total USGCRP budget, with more than a third of the total USGCRP budget focused on climate and hydrology observations provided by NASA (about 400 million dollars of a total budget of 1,185 million). A decade later, growth in NASA investments in USGCRP kept pace with the growth in the total budget, and also kept pace in terms of the investment in climate research. In the FY08 request, NASA's investment is about 60 percent of the total Climate Change Science Program (CCSP) and the total CCSP budget request is about 6.5 percent above the 2002 USGCRP budget (figures not adjusted for inflation). The full set of segmented disciplinary topics within the USGCRP set of cross-cuts is combined into one CCSP budget. More telling is an analysis of the out-year budgets with their associated numbers of missions and instruments. Even with the extension of some current missions beyond their nominal life times, by 2010 the U.S. will have a 35 percent decrease in the number of operating sensors and instruments on NASA spacecraft. By 2015, the number will have decreased by more than 50 percent. In real dollars, NASA Earth Sciences has declined by more than a half a billion dollars since the 2002 USGCRP budget.

The total international investment in climate science is difficult to confirm with certainty by the science community, but NASA has always been the international leader in Earth observations. The decrease in research, missions, and numbers of instruments is a real loss of capability. The baton is not being passed to international partners, it is simply being dropped.

2. What are your perspectives on the FY2008 budget request for NASA's Earth Science Program and how well does it position NASA to contribute to the U.S. priorities and plans for climate and related research?

The modest increase in the FY2008 budget request for NASA's Earth Science Program is the first sign that the steady erosion of capability and the lack of a credible program of observations beyond the end of this decade is reversing. However, the FY2008 budget and its out-year projections are simply inadequate. Under current funding and projections, the U.S. will have significant gaps in the long-term observation record, making it more difficult to separate natural and human contributions to climate change and making it more difficult to assess how the Earth is changing. Debates on issues such as the relative importance of solar versus greenhouse causes of warming will continue rather than be solved definitively. Under current funding and projections, the key areas of uncertainty in climate models will very likely continue to languish. Most certainly, the areas of investigation that couple climate change to societally-important areas such as water, health, and food security will be delayed. Stated frankly, our capabilities to address critical questions in climate change in service to society will experience a dramatic decline if the NASA out-year projections are realized.

3. Which missions and observations recommended in the National Academies Earth science decadal survey are most critical for advancing our understanding of climate change and any mitigation and adaptation strategies? What uncertainties in our understanding of change would the observations from those missions help reduce?

In my opinion, a decadal survey in the Earth sciences produced a decade ago would have focused on innovation built upon a robust global observing system. Such a survey would likely have focused on new technologies and new capabilities that would have extended our abilities to address difficult variables, improve the quality of our observations, and demonstrate an increase in forecasting capability. Certainly, we would have debated how to balance the notion of entraining new technologies while still preserving continuity of the observations. Likely, we would have debated the best mechanisms to bring the same "discipline of forecasting" that has resulted in dramatic improvements in weather forecasting to a much broader family of variables of interest to our society. In contrast, the Decadal Survey rarely considered the frontiers that we know are in the realm of the possible. This is not a critique of the Decadal Survey. It is a fact that the NRC effort sought primarily to ensure a reasonable and robust set of observations within a tractable budget, where "tractable" is defined as only restoring the budget to its level in 2001 in terms of real dollars, while ensuring that the most critical needs were addressed.

For climate studies, the list provided in the National Academies Earth Science Decadal Survey is a base set. It is prioritized in time, taking into account the existing instrumentation and international partners, but each element is critical and the list is not sufficient to solve all of the major uncertainties in forecasting the future. It maintains the most basic needs and adds only those missions which are clearly the most crucial priorities in a set of many critical observations. The request for climate research reveals the level of constraint applied within the Decadal Survey.

First, we must have a sufficient set of sustained multi-decadal, global measurements of key variables in order to understand how the Earth is changing, to understand the roles of various natural and human forcing factors, and to assess climate models. Stripped to its fundamentals, the climate is first affected by the long-term balance between incoming and outgoing energy. Both the long-term records of total solar input and the Earth's energy budget are in jeopardy. Other variables that define the state of the atmosphere and ocean and provide a foundation for both weather forecasting and climate are equally critical. These include such fundamental observations as temperature and water vapor soundings, the distribution of snow and ice, ozone profiles, and surface winds. The de-scoping of NPOESS involved each of these key climate variables. Without the Decadal Survey recommendations we do not address these most basic needs of the climate sciences.

Second, current observations and models raise particular concerns about the mass balance and even the stability of the large ice caps. In terms of our capabilities to assess how the Earth system is changing, the ice sheets represent one of the most significant areas of uncertainty and one of the most significant areas in terms of potential societal impact. The Decadal Survey places a high priority on determining ice sheet volume, sea ice thickness, ice sheet surface velocities, and improved estimates of the sensitivity of the ice sheets to climate change.

Third, the Decadal Survey calls for a focus on the two areas that are considered to be the most limiting in terms of our ability to improve climate model predictions. The first area is aerosol-cloud forcing. Aerosol climate forcing is similar in magnitude to carbon dioxide forcing, but the uncertainty is estimated to be substantially larger. The impact of aerosols on cloud formation amplifies their importance to the climate system. The Decadal Survey also calls for a focus on measuring ocean circulation, ocean heat storage and ocean climate forcing. Again, the problems are fundamental, involving the measurement of sea level, the importance of how rapidly heat is being mixed into the oceans, and improvements in our ability to simulate the ocean circulation.

We are more than capable of providing the observations needed to address the specific topics above. Importantly, the climate chapter of the Decadal Survey also calls for us to address much more challenging problems by bringing innovative approaches to the fore and challenging our ability to return to the cutting-edge of Earth observing. The accurate measurement of the surface fluxes of energy, water and momentum at the Earth's surface, and an improved ability to examine atmospheric convection (which governs the transport of heat, water vapor, trace gases, and aerosols and defines cloud formation) would substantially advance our ability to predict the future and to understand critical problems such as sea level variations and changes in the distribution and character of precipitation. Missions dedicated to these two important topics are not a part of the priority set from the Decadal Survey.

4. What role, if any, do NASA's Earth science research and related programs play in validating the accuracy of climate measurements collected from Earth observing satellites and in developing predictive capabilities for climate change and its effects?

The decline in capability is not restricted to missions and instruments. The decline in the observation budget is matched by a significant decline in the Research and Analysis budget in the Earth Sciences. Sub-orbital and land-based studies increase our ability to assess and validate climate measurements. A comprehensive approach to the analysis, distribution and stewardship of observations broadens the base of applications and entrains a broader set of disciplines and a higher level of expertise directed toward increasing our confidence in Earth observations, expanding their value, and improving predictive capabilities.

The loss of capability has the potential to be long-term and particularly costly because of its timing. The lack of missions, the reduced level of opportunities, the lack of innovation, and the weakness in the Research and Analysis budgets are likely to result in a reduction in student interest, and most clearly in the training of graduate students and post-doctoral researchers. This loss of opportunity, with it potential impact on attracting the next generation of scientists and engineers who design sensor systems and analyze data, matches a time in which a substantial fraction of the NASA Earth sciences workforce is able to retire. The FY2008 and out year budgets have the potential to create significant weakness in the capability of the workforce at the same time that society is demanding an increased emphasis on understanding climate and its impacts.

5. What are your perspectives, as an individual researcher, on international collaborations in the Earth sciences, and what value would international collaborations offer in advancing the recommended missions in the decadal survey?

In my opinion, the statements on international collaboration provided in the Decadal Survey are sound. International collaborations have a number of benefits including a reduction in cost and a potential reduction in the likelihood of gaps in key data sets. In addition, collaboration can increase the number of science users and bring a broader array of technologies to bear on a specific problem. NASA has demonstrated success in developing such partnerships, with TOPEX/Poseidon and RADARSAT-1 as good examples. Moreover, it is now relatively common for flight agencies to offer announcements of opportunity to the international science community as the agencies attempt to maximize the payoff of each flight project.

However, joint ventures must still be considered with care, particularly for climate data sets. As noted in the Decadal Survey climate chapter, instruments built by one partner may not be designed to the exact requirements of another partner. Although two missions may utilize the same type of instrument—for example an altimeter—and therefore sound like they are duplicative, the differences in design may allow one to resolve ocean eddies and improve our knowledge of the ocean circulation while the other may not achieve this objective. Technology transfer restrictions may also prevent the exchange of important technical details about the instruments. Restrictions on access to data and software vary from country to country, as do approaches to calibration and validation. Joint ventures between government flight agencies and commercial partners can result in serious complications with data cost, availability, and distribution. Missions can also be terminated or significantly altered by host countries, resulting in a greater impact if the other partners had counted on the international partner to provide a key observation or synergistic measurement.

International partnerships should only be fostered where synergy between instrument capabilities and the science requirements is strong, where there is free and easy access to data, and where there is transparency in the process of analyzing data such that analysis algorithms are freely available.

The Decadal Survey includes many examples where priorities were altered based on knowledge of missions proposed by international partners. A case in point is the cloud-aerosol mission (ACE) proposed by the Decadal Survey which, despite its importance in addressing areas of uncertainty in climate models, was placed in phase 2 (2013-2016) because of cloud and aerosol information that would become available from international sources (GCOM-C and EarthCARE).

End Note: An improved ability to predict climate change will allow us to be good stewards of this planet. But few seem to recognize that our ability to better predict the future has benefit far beyond addressing the consequences of increased levels of greenhouse gases. The potential societal benefits are substantial. For example, even modest improvement in seasonal to interannual predictions have the potential for significant societal benefit in agriculture, energy, water, and weather-related management. The Decadal Survey presents a vision that recognizes that the demand for knowledge of climate change and variability will increase. The risk in failing to provide this information is high. However, our ability to serve society through increased observing capability and improved model prediction is far greater than a single issue, even though the issue of climate change is of enormous significance. An improvement in our ability to anticipate the future increases our capability to utilize this knowledge to both limit adverse outcomes and maximize benefits to society.

6.6 U.S. Weather and Environmental Satellites: Ready for the 21st Century?

STATEMENT BEFORE THE SENATE COMMITTEE ON COMMERCE, SCIENCE, AND TRANSPORTATION

July 11, 2007
Statement of Antonio J. Busalacchi, Jr., Ph.D.
Director, Earth System Science Interdisciplinary Center (ESSIC), University of Maryland
Chairman, Climate Research Committee
Chair, Panel on Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft
National Research Council

Mr. Chairman, Mr. Vice Chairman, and members of the committee, thank you very much for this opportunity to testify. I am Dr. Tony Busalacchi, Director of the Earth System Science Interdisciplinary Center and Professor of Atmospheric and Oceanic Science at the University of Maryland. I also serve as the Chair of The National Academies' Climate Research Committee and of the Academies' "Panel on Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft." This latter study is in response to a NASA and NOAA request to the National Research Council (NRC) for a follow-on report to the Decadal Survey in Earth Science that focuses on recovery of lost measurement capabilities, especially those related to climate research, which occurred as a result of changes to the NPOESS and GOES-R satellite programs.

On June 19, 2007, our NRC Panel convened a three-day workshop, "Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft." The workshop attracted some 100 scientists and engineers from academia, government, and industry. The workshop gave the climate community a chance to review and comment on the NASA/NOAA assessments of the climate impacts associated with Nunn-McCurdy descopes of NPOESS, as well as offer input on a variety of suggested mitigation scenarios. A report of the workshop will be available later this summer. Presentations from the workshop are available download at <http://www7.nationalacademies.org/ssb/SSB_NPOESS2007_Presentations.html>. A final report, with findings and recommendations, will be issued in January. As this study is still underway, the views I express today are my own.

As requested, I will use my time this morning to summarize my views on the status and direction of the nation's current and planned constellation of weather and environmental satellites. In particular, I will focus on your request for information on the "budgetary, management, and schedule risks of these [weather and environmental] satellite systems, as well as the potential lost capabilities in climate monitoring, modeling, and forecasting that are possible under the current program."

This hearing takes place against the backdrop of significant developments in NOAA weather and environmental monitoring programs and NASA's Earth Science Program:

- In June 2006, the next-generation National Polar-orbiting Operational Environmental Satellite System (NPOESS) completed its "Nunn-McCurdy" certification. As a result, the planned acquisition of six spacecraft was reduced to four, the launch of the first spacecraft was delayed until 2013, and several sensors were canceled or descoped in capability as the program was re-focused on "core" requirements related to the acquisition of data to support numerical weather prediction. "Secondary" sensors that would provide crucial continuity to some long-term climate records and other sensors that would have provided new data are not funded in the new NPOESS program.
- Costs for NOAA's next generation of geostationary weather satellites, GOES-R, have risen dramatically and late last year NOAA canceled plans to incorporate a key instrument on the spacecraft—HES (Hyperspectral Environmental Suite). HES was to provide GOES-R spacecraft with significantly advanced three-dimensional vertical profiles of atmospheric temperature and humidity, and coastal waters imagery to help scientists monitor events like harmful algal blooms or to assist in fisheries management.
- The 2005 National Research Council report, *Earth Science and Applications from Space: Urgent Needs and Opportunities to Serve the Nation* described the national system of environmental satellites as "at risk of collapse." That judgment was based on the observed precipitous decline in funding for Earth-observation missions and the

consequent cancellation, descoping, and delay of a number of critical missions and instruments. The report also identified the need to evaluate plans for transferring capabilities from some cancelled or scaled back NASA missions to the NOAA-DOD NPOESS satellites. Since the publication of that report, NPOESS and NOAA have experienced the problems noted above and NASA has canceled additional missions, delayed the Global Precipitation Mission (GPM) another 2.5 years, and made substantial cuts in its Research and Analysis program.

This hearing also occurs shortly after the completion of the first National Academies Decadal Survey in Earth Science and Applications from Space and the recent release by the Intergovernmental Panel on Climate Change of their Fourth Assessment Report. In addition, as you are all aware, there have been numerous news accounts in recent days regarding the fate of a particular spacecraft—QuikSCAT, which measures sea surface wind speed and direction.

Sustained Earth Observations for Operations, Research, and Monitoring

Scientific breakthroughs are often the result of new exploratory observations, and therefore new technology missions stimulate and advance fundamental knowledge about the planet. Analysis of new observations can both test hypotheses developed to elucidate fundamental mechanisms and lead to the development of models that explain or predict important Earth processes. The data from these new technology missions sometimes provide early warning of changes in the Earth system that are critical to our well-being, such as declining ice cover in the Arctic Ocean, developing holes in the protective ozone layer, or rising sea level. To determine the long-term implications of the changes or to uncover slowly evolving dynamics, the measurements must be continued, usually with one or more follow-on missions.

Access to uninterrupted space-based global observations of the atmosphere, oceans, and land surface has enabled breakthroughs in predicting natural climate variability beyond the day-to-day weather time scale. Today's coupled climate models, initialized by global satellite observations, now routinely issue short-term climate forecasts from seasons out to a year in advance with the realistic prospect of extension to years and decades. To discriminate between natural climate variability and anthropogenic climate change requires instrument accuracy and stability greater than is normally required to support weather prediction. Interruptions to the continuity of these climate data records without such accuracy and stability can induce uncertainty that may be as large, or larger, than the climate signal being monitored.

Sometimes data from a new technology mission become critical to an operational system, such as the wind speed and direction measurements from NASA's QuikSCAT mission and precipitation measurements from NASA's Tropical Rainfall Measurement Mission (TRMM), both of which are used in weather and climate forecasting. An obvious but often difficult consequence is that these *research* measurements need to be transitioned into *operational* systems and continued for many years. This is a recognized and well-studied challenge, but, the record of transitioning new technology into the operational system is, at best, mixed. More often than not, the operational utility of data from these research missions is realized toward the end of the design life of the instruments. By then however, it is usually too late to begin the planning of a follow-on operational mission if continuity is to be maintained.

The difficulties in combining the climate and weather requirements on NPOESS as well as the problem in executing what is sometimes referred to as the transition from NASA "research" missions to NOAA operations (which, is effectively the source of the current controversy surrounding the aging QuikSCAT spacecraft) are different aspects of an overarching problem: *the United States lacks a coherent strategy to manage its earth observation programs in general and its climate observations in particular*. The Nunn-McCurdy certification of NPOESS exposed the difficulty in sustaining long-term climate observations within a program managed by agencies with different priorities and missions. Whereas NOAA and DOD have complementary priorities with respect to weather prediction, the same does not hold for climate. Moreover, the stability, calibration, and technology refresh requirements for climate observations call for a flexible systems approach consisting of a mix of small climate-specific satellites, formation flying, and single sensor "free flyers," as opposed to the (small school bus) one-size fits all series of "Battlestar Gallactica" NPOESS platforms.

Our ability as a nation to sustain climate observations has also been complicated by the fact that no single agency has the mandate and requisite budget for providing routine climate observations, prediction, and services. As stated in the January 2007 National Research Council pre-publication of the "decadal survey," *Earth Science and Applications from Space*: *National Imperatives for the Next Decade and Beyond:*

> The committee is concerned that the nation's institutions involved in civil space (including NASA, NOAA, and USGS) are not adequately prepared to meet society's rapidly evolving Earth information needs. These institutions have responsibilities that are in many cases mismatched with their authorities and resources: institutional mandates are inconsistent with agency charters, budgets are not well-matched to emerging needs, and shared responsibilities are supported inconsistently by mechanisms for cooperation. These are issues whose solutions will require action at high-levels of the government.

For example, in a recent NRC review of NASA's 2006 Draft Science Plan the committee noted that the "NASA/SMD (Science Mission Directorate) should develop a science strategy for obtaining long-term, continuous, stable observations of the Earth system that are distinct from observations to meet requirements by NOAA in support of numerical weather prediction." Accordingly, the Decadal Survey committee recommended that, "The Office of Science and Technology Policy, in collaboration with the relevant agencies, and in consultation with the scientific community, should develop and implement a plan for achieving and sustaining global Earth observations. This plan should recognize the complexity of differing agency roles, responsibilities, and capabilities as well as the lessons from implementation of the Landsat, EOS, and NPOESS programs."

I will now turn to the specific questions about the programs under consideration:

What are the potential lost capabilities in climate monitoring, modeling, and forecasting?

- **NPOESS**: As noted in a recent NASA-NOAA report, which was performed at the request of the White House Office of Science and Technology Policy, "For more than thirty years, NASA research-driven missions, such as the EOS, have pioneered remote sensing observations of the Earth's climate, including parameters such as solar irradiance, the Earth's radiation budget, ozone vertical profiles, and sea surface height. Maintaining these measurements in an operational environment provides the best opportunity for sustaining the long-term, consistent, and continuous data records needed to understand, monitor, and predict climate variability and change." However, the Nunn-McCurdy certification placed a priority on the continuity of operational weather measurements at the expense of climate measurements. In addition, the post-certification constellation eliminated the "mid-morning" orbit and reduced the planned acquisition of six spacecraft to four. NASA and NOAA have completed their preliminary assessments of the impacts of these changes, focused primarily on the de-manifested sensors. Their assessment is documented in a white paper prepared for OSTP. Rather than go into the details of their assessment or repeat it here, a brief summary of the climate impacts associated with de-manifestation of these sensors is included in the appendix.
- **QuikSCAT**: QuikSCAT continues to function well and provide all-weather observations of ocean surface wind speed and direction, although it is five-years beyond its design lifetime and it is operating on a backup communication system. Should QuikSCAT fail, the United States would have to rely on the ASCAT instrument on the European MetOp system and on data currently provided by the WindSat spacecraft. Both of these systems have drawbacks compared to QuikSCAT—ASCAT has large gaps in coverage compared to QuikSCAT and analyses to date of WindSat data expose serious concerns about the utility of passive polarimetric measurements of surface wind speed and direction in low and high wind regimes for research and operational applications. Further, the capabilities of the successor to Windsat, the MIS instrument planned for NPOESS, are still unknown and NOAA does not plan to incorporate the instrument on NPOESS until launch of the second spacecraft in 2016 at the earliest. The National Academies Decadal Survey recommended a follow-on mission to QuikSCAT—XOVWM—to be launched in the 2013-2016 timeframe. It is my understanding that the survey's choice of this time period, versus one sooner, was based on an examination of expected resources and the need to launch other priority missions.
- **GOES-R**: the loss of HES is of higher priority for numerical weather prediction and monitoring of coastal waters than it is for climate. The impact to climate research is the loss of ability to track changes in the intensity and frequency of extreme events such as hurricanes, floods, wildfires, and harmful algal blooms as modulated by climate variability and change.

Overview of Climate Needs

The climate community has three basic observational needs: (1) sustained (continuous, and often overlapping) measurements of certain key climate parameters critical to monitor long-term climate trends and to validate climate models, (2) observations to initialize and force coupled climate prediction models, and (3) new or improved measurements of additional key parameters to advance climate science and reduce uncertainty in our understanding of climate processes and interactions within the coupled climate system. It is the first and second category of needs which are now threatened by NPOESS, though the third category—and indeed all of Earth science—is implicitly threatened by the cost overruns of NPOESS, which have had great impact on already-tight Earth science budgets. This impact will increase as the agencies attempt to assure continuity of the most critical of climate records by altering upcoming flight manifests, restoring instruments to NPOESS, or designing "gap-filler" missions.

Mitigation Challenges

Any strategy to mitigate the impacts of the loss of these sensors begins with a prioritization of their importance and an assessment of the cost and risk of various recovery options. Such an assessment is the subject of the ongoing National Research Council study that I chair; it is also the subject of an Office of Science and Technology Policy (OSTP)-requested study that is being executed by NASA and NOAA. The range of options under study include re-manifesting selected sensors to the NPOESS platforms, making use of ongoing and planned missions by international partners, launching selected sensors on missions of opportunity or on new spacecraft, and assimilating data from multiple sources to help reconstruct the lost data.

It is also important to recognize the limitations of some of the climate sensors on NPOESS even before the Nunn-McCurdy actions. For example, from its sun-synchronous orbit, altimeter measurements of sea-surface height (SSH) via the ALT instrument would contend with the effects of tidal aliasing. The precise record of SSH that began with the Topex/Poseidon mission (and continues with Jason-1 mission, which should overlap with the 2008 of Jason-2) derives from instruments on spacecraft that are *not* in sun-synchronous orbit. Moreover, as emphasized repeatedly in recent NRC studies, the generation of credible climate records requires investments in pre-launch instrument characterization, on-orbit calibration and validation, and a ground support system that has the requisite resources to archive, disseminate, analyze, and periodically re-analyze the data. Appropriate investments in this critical part of the chain from raw data to climate data record were never part of the NPOESS program. Indeed, their absence is indicative of the problem that arises when the very different needs of the climate community are effectively piggybacked on the needs of numerical weather forecasters from both the DOD and civil communities.

Recovery strategies also must take account of plans for execution of the NRC Decadal Survey. The Decadal Survey was sponsored by NASA (Office of Earth Science), NOAA (NESDIS), and the USGS (Geography). While cognizant that space-based observations were only part of a credible Earth observing system, it was charged with:

- Articulating priorities for Earth system science and the space-based observational approaches to address those priorities.
- Establishing individual plans and priorities within the sub-disciplines of the Earth sciences as well as providing an integrated vision and plan for the Earth sciences as a whole.

The relevance of the recommended Decadal Survey missions mapped against de-manifested NPOESS sensors is shown in Table 1 below. It is important to note that the decadal strategy covers all of Earth science, including but not limited to climate science. Though I am limiting my remarks here to discuss those elements related to climate and of relevance to the NPOESS and GOES-R considerations, I support the report's call for a balanced Earth science program. Of particular interest for NPOESS mitigation strategies in the near-term is the recommendation for an early start of the CLARREO radiance mission. A more capable ocean vector wind follow-on to QuikSCAT—XOVWM—is also called out to start in the period from 2013-2016.

In summary, our climate monitoring capabilities are neither adequate to meet the needs of the climate research community nor the needs of decision-makers. The NPOESS descopes highlight what has for too-long been a precarious and loosely coordinated series of climate observations in which the long-term generation and support of climate data records are left out of key agencies' long-term planning. The Nunn-McCurdy certification of NPOESS has exposed the fact that we do not have an agreed upon national strategy for long-term, continuous, and stable observations of the Earth system. As the recent Decadal Survey committee pointed out, sustained measurements with both research and operational applications do not fall clearly into any one agency's charter. This results in a metaphorical relay race between NASA and NOAA, where no runner is waiting to be passed the baton.

As it pertains to climate monitoring, the relative roles and responsibilities of NASA and NOAA remain uncertain. As a direct consequence, we are faced with a likely gap in critical long-term climate records and a diminished capability to understand and predict climate and related changes on our planet for generations to come. As we seek to mitigate this situation, applying a band-aid if you will, I urge members of this committee to carefully consider how we might avoid having a similar hearing in the not too distant future. Right now, we are in a reactive mode with respect to what can only be referred to as the NPOESS debacle. Our nation needs a deliberate, forward looking, and cost-effective strategy for satellite-based environmental monitoring. The nation requires a coherent strategy for Earth observations which provides for operational climate monitoring and prediction, scientific advances, *and* the continuation of long-term measurements. The nation *deserves* such a strategy. Thank you for the opportunity to appear before you today on this important topic. I am prepared to answer any questions you may have.

7
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