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NASA'S SPACE SCIENCE PROGRAMS: REVIEW OF FISCAL YEAR 2008 BUDGET REQUEST AND ISSUES

HEARING

BEFORE THE

SUBCOMMITTEE ON SPACE AND AERONAUTICS

COMMITTEE ON SCIENCE AND TECHNOLOGY

HOUSE OF REPRESENTATIVES

ONE HUNDRED TENTH CONGRESS

FIRST SESSION

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NASA'S SPACE SCIENCE PROGRAMS: REVIEW OF FISCAL YEAR 2008 BUDGET REQUEST AND ISSUES

WEDNESDAY, MAY 2, 2007

HOUSE OF REPRESENTATIVES,
SUBCOMMITTEE ON SPACE AND AERONAUTICS,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
Washington, DC.

The Subcommittee met, pursuant to call, at 10:00 a.m., in Room 2318 of the Rayburn House Office Building, Hon. Mark Udall [Chairman of the Subcommittee] presiding.

U.S. HOUSE OF REPRESENTATIVES
COMMITTEE ON SCIENCE
SUBCOMMITTEE ON SPACE AND AERONAUTICS

Hearing on

NASA's Space Science Programs: Review of Fiscal Year 2008 Budget Request and Issues

May 2, 2007
10:00 a.m. – 12:00 p.m.
2318 Rayburn House Office Building

WITNESS LIST

Dr. S. Alan Stern
Associate Administrator,
NASA Science Mission Directorate

Dr. Lennard Fisk
Chair
Space Studies Board
National Research Council

Dr. Garth Illingworth
Chair
Astronomy and Astrophysics Advisory Committee

Dr. Daniel Baker
Director
Laboratory for Atmospheric and Space Physics
University of Colorado, Boulder

Dr. Joseph Burns
Vice Provost
Physical Sciences and Engineering
Cornell University

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HEARING CHARTER

SUBCOMMITTEE ON SPACE AND AERONAUTICS
COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

NASA's Space Science Programs: Review of Fiscal Year 2008 Budget Request and Issues

WEDNESDAY, MAY 2, 2007
10:00 A.M.—12:00 P.M.
2318 RAYBURN HOUSE OFFICE BUILDING

Purpose

On Wednesday, May 2, 2007 at 10:00 am, the House Committee on Science and Technology, Subcommittee on Space and Aeronautics will hold a hearing to examine the National Aeronautics and Space Administration's (NASA) Fiscal Year 2008 budget request and plans for space science programs including heliophysics, planetary science (including astrobiology), and astrophysics, as well as issues related to the programs.

Witnesses:

Witnesses scheduled to testify at the hearing include the following:

Dr. S. Alan Stern
Associate Administrator,
NASA Science Mission Directorate

Dr. Lennard Fisk
Thomas M. Donahue Distinguished University Professor of Space Science
University of Michigan, and
Chair, Space Studies Board, National Research Council

Dr. Garth Illingworth
Professor
University of California Observatories/Lick Observatory,
University of California, Santa Cruz, and
Chair, Astronomy and Astrophysics Advisory Committee

Dr. Daniel Baker
Professor, Astrophysical and Planetary Sciences
Director,
Laboratory for Atmospheric and Space Physics
University of Colorado, Boulder

Dr. Joseph Burns
Irving Porter Church Professor of Engineering and Professor of Astronomy, and
Vice Provost, Physical Sciences and Engineering
Cornell University

BACKGROUND

Potential Issues

The following are some of the potential issues that might be raised at the hearing:

- **Impact of Budgetary Cutbacks on NASA's Space Science Programs—** In the three years since the President's *Vision for Space Exploration* was announced in early 2004, the Administration has reduced NASA's Science Mission Directorate outyear funding by a total of $4 billion. As a result, missions have been delayed or deferred, supporting activities such as technology development have been decreased and the prospects for new activities have been pushed out into the future. At the same time, some missions in development are costing more than anticipated, placing further stress on Science Mission Directorate programs. How serious a problem is the budgetary situation fac-

ing NASA's Science Mission Directorate? What should be done to ensure NASA has a sustainable and robust science program?

- **Role of Space Science in the President's American Competitiveness Initiative and Innovation Agenda—**Research funded through NASA's space science program exemplifies the types of research highlighted in the National Academies report, *Rising Above the Gathering Storm,* and in the President's American Competitiveness Initiative. Specifically, the Academies' recommendations for long-term basic research and "special emphasis on physical sciences, engineering, mathematics, and information sciences"; high-risk research; research grants to early career researchers; and funding for advanced research instrumentation and facilities also apply to NASA. Given that, why hasn't NASA space science been included in the President's American Competitiveness Initiative? Moreover, why has the NASA-funded research that most directly applies to the goals of the ACI been declining at a time when the focus on and funding for long-term basic research at other agencies is increasing under the ACI? What message does the exclusion of NASA research from the ACI send to the community of space scientists that performs that research? How does a strategy that promotes basic research at some government R&D agencies while cutting funding for the same type of research at other agencies help the Nation meet the ACI goals of strengthening research in the physical sciences, engineering, and mathematics and building the foundation for innovation? What, if anything, should be done to address NASA's absence from the ACI?

- **Lack of Adequate Balance—**Administrator Griffin testified at the March 15, 2007 Committee on Science and Technology hearing on the NASA FY08 budget request that NASA has attempted to balance its science programs. However, a number of advisory committees, including, the National Academies and the Astronomy and Astrophysics Advisory Committee, have raised concerns about the lack of balance in NASA science programs. In its report, *An Assessment of Balance in NASA's Science Programs* (2006), the National Academies found that:

  > *"The program proposed for space and Earth science is not robust; it is not properly balanced to support a healthy mix of small, medium, and large missions and an underlying foundation of scientific research and advanced technology projects."*

  According to the *Assessment of Balance* report, lack of balance, sustainability and robustness in NASA's science programs affects the ability to make progress on the Decadal Surveys (research priorities for the next ten years in specific space science disciplines that represent a consensus of the science community); to follow a plan or sequence of missions, to meet commitments to international partners; to develop advanced technology; to nurture a research and technology community; and to train and educate future space scientists and engineers. What is NASA's definition of balance? What, if anything has NASA done in response to findings of the advisory committees? What does a properly balanced program look like?

- **Cuts to smaller science mission opportunities—**Cutbacks in small- and medium-sized mission opportunities, such as are offered by the Explorer program, are cited in advisory committee reports as indicators of a science program lacking balance. Explorer missions, which are highly rated in the decadal surveys, are competitively awarded missions that are led by a scientist principal investigator (PI) who is given responsibility for the scientific, technical, and management success of the mission. Explorers examine focused science areas not addressed by NASA's larger, agency-led, strategic missions. They provide flight opportunities in the gaps between strategic missions and are critical opportunities for the much-needed training of the next generation of scientists and engineers. That the Nobel Prize in physics for 2006 was awarded to two U.S. researchers whose work relied on data from the Cosmic Background Explorer (COBE) exemplifies the scientific potential of these small spacecraft. Should funds be restored to increase the flight rate of Explorer and other small- and medium-sized missions, and at what cost to other missions or science activities? What is the appropriate frequency of small- and medium-sized missions needed to sustain the scientific activities and researcher base that relies on such flight opportunities? Should future budgets fence off a certain percentage of resources for small- and medium-sized missions such as Explorer?

- **Cuts to Research and Analysis—**According to advisory committee reports such as *An Assessment of Balance in NASA's Science Programs* and the *Annual Report of the Astronomy and Astrophysics Advisory Committee, March 16, 2006–March 15, 2007,* a properly balanced science program is defined, in part, by the support provided for research grants, largely through NASA's research and analysis (R&A) accounts. R&A grants fund theory, modeling, and the analysis of mission data; technology development for future science missions; the development of concepts for potential future science missions; scientific investigations using aircraft, balloons, and sub-orbital rockets; the training of the next generation of scientists and engineers, among a host of other supporting research and technology activities. The FY06 NASA operating plan cut R&A accounts by about 15 percent across the science programs, reducing support for graduate students, post-doctoral students and junior faculty. The FY07 request did not restore those cuts, and the FY08 request largely continues the previous levels of funding for R&A. What is a healthy level of R&A funding within the NASA science programs? How long can the research community sustain lower levels of activity before attrition occurs, along with a loss of expertise that cannot be easily recovered? What, if anything, should be done about the current level of R&A funding? Should measures be instituted to protect R&A funding against future cuts, and if so, what would those measures be?
- **Cost Growth in Missions—**Several of the increases in NASA's FY08 budget request provide funds for science missions that have run over budget or schedule, or that run the risk of doing so. In addition, cost growth in some of the planned space science missions in recent years, coupled with constrained budgets, has wound up squeezing other science activities. The factors contributing to cost and schedule growth are not easy to pinpoint, but can include underestimates in the technology development required for mission readiness; increases in launch vehicle costs; internal decisions to delay missions or alter budget profiles; project management difficulties; and delays in contributions from international or interagency partners. Lack of clarity in the communication of what is included in those costs (e.g., technology development, mission development, operations) has also contributed to the problem. Mission cost growth can lead to delays, cancellations, or reduction in funds for other NASA science missions and activities. What, if anything, can be done to control cost growth on missions? Is there adequate understanding of the cost growth contributors or is more information needed to come up with solutions to the cost growth problem?
- **Role of Space Science in Human and Robotic Exploration of the Solar System—**Robotic exploration of the solar system is called out in the President's *Vision for Space Exploration* as being important to achieving the Vision. The Report of the President's Commission on Implementation of United States Space Exploration Policy states that "science in the space exploration vision is both enabling and enabled." What should be the role of science activities in the context of the *Vision for Space Exploration*? Should science that supports the Vision have a higher priority?
- **Future Availability of the Delta II Launch Vehicle—**The Delta II has been a highly reliable workhorse for space science missions. Over the next two years, eight missions are scheduled to launch on Delta IIs, however, NASA has expressed uncertainty about the availability of the Delta II launch vehicle after 2009 and is studying alternatives. What is the status of the Delta II availability for science payloads after 2009? If the Delta II is not available, what is the plan for launching Delta-class science missions? What are the alternatives to the Delta II and what are the likely impacts of using an alternative vehicle? If launch costs increase, does NASA plan to alter the levels of cost-capped missions?
- **Technology Development and Supporting Programs—**missions proposed with immature technologies can be a root cause of cost growth. The Academies report on *Principal-Investigator-Led Missions in the Space Sciences* states that ". . .project technology development efforts often lag planned progress owing to unexpected design failures, fabrication or testing issues, or other glitches. . . .attempts by mission projects to using promising but immature technology is a frequent cause of PI-led missions (and others) exceeding the cost cap." The FY08 budget request decreases funding for the New Millennium Program and the research and analysis programs both of which enable technology development for future missions. In light of the cost growth and

technical challenges encountered by several science missions, will reductions in technology development programs increase the risk of cost growth on future missions? Have technology development programs been an adequate and effective means of understanding technical risks and mission costs? If not, why and what other mechanisms are available to prepare for technical challenges on future missions?

- **International Partnerships—**NASA has a successful history of international cooperation in science and involves non-U.S. partners on some two-thirds of its science missions, and also provides instruments, science support, and other in-kind contributions to non-U.S.-led space and Earth science missions. Successful cooperative missions can increase the scientific content of a mission and build mutually beneficial relationships. At the same time, cooperation can lead to delays and added mission costs. Among the factors that have made international cooperative missions harder in recent years is ITAR. Pursuant to 22 U.S.C. 2778 of the Arms Export Control Act, the International Traffic in Arms Regulations (ITAR) regulates the export of defense articles on the U.S. Munitions Control List. The Department of State has responsibility for administering the regulations. In 1999, scientific satellites were added to the Munitions Control List (USML). ITAR often poses significant challenges for space science missions, many of which involve international partners. The time required to manage licenses or agreements can threaten mission schedules. ITAR can be especially problematic for U.S. universities, which typically attract a large percentage of foreign graduate students to their programs. Is increasing international cooperation on planned and future missions feasible, given recent experiences with ITAR? What factors associated with ITAR must be considered before agreeing to international collaborations?

Overview

Over the past five decades, NASA has fostered a world-class space science program that has led to such discoveries as new planets outside our solar system, the presence of dark energy and the acceleration of an expanding Universe, the signs of possible recent liquid water flows on Mars, and more knowledge of the Sun's interior structure and activity. NASA missions have also improved our understanding of the effects of solar activity and space radiation on ground-based electrical power grids and wireless communications systems, on orbiting satellites, and also on humans in space. The space science program's technical achievements are equally stunning as demonstrated in the successful landing and operation of Mars rovers Spirit and Opportunity; the recent deployment of five spacecraft to study the causes of the changing auroras at the North Pole, and Deep Impact's successful penetration of the comet Tempel 1. In 2006, Dr. John Mather and Dr. George Smoot were awarded the Nobel Prize in physics for their work with the NASA Cosmic Background Explorer. [Dr. Mather is the first NASA civil servant to receive the Nobel prize.]

This hearing will examine NASA's space science programs within NASA's Science Mission Directorate (SMD) and their status within the context of the Fiscal Year 2008 budget request. The space science programs include the following theme areas:

- Heliophysics, which seeks to understand the Sun and its effects on Earth and the rest of the solar system;
- Planetary science, which seeks to understand the origin and evolution of the solar system and the prospects for life beyond Earth; and
- Astrophysics, which seeks to understand the origin, structure, evolution and future of the Universe and to search for Earth-like planets.

Earth science is also an SMD theme area. It will be the topic of a separate Subcommittee hearing.

It should also be noted that Dr. Stern has informed the Subcommittee that he has gotten agreement to move NASA's Near-Earth Objects (NEO) program, and its associated budget, from the Exploration Systems Mission Directorate to the Science Mission Directorate.

NASA's space science programs involve the following types of activities:

- space missions that take measurements and collect data to investigate high priority science questions;
- the analysis of that mission data, which leads to new knowledge;
- research on theories and models;

- the development of new technologies to enable future science investigations; and
- the use of balloons, sounding rockets, and sub-orbital flights to take measurements and test technologies.

Stakeholders in the NASA space science programs include academic institutions; industry; NASA field centers, predominantly the Goddard Space Flight Center (GSFC) and the Jet Propulsion Laboratory (JPL); and other government laboratories. There are a number of advisory panels that provided guidance on NASA's space science programs and activities, including the NASA Advisory Council (NAC) and the NAC Science Subcommittees, the National Academies, and the Astronomy and Astrophysics Advisory Committee (AAAC).

Fiscal Year 2008 Budget Request

The President's FY08 budget requests $4.019 billion to fund NASA's space science programs—heliophysics, planetary science, and astrophysics. The budget represents a $16.5 million increase (or about 0.4 percent) over the President's proposed FY07 budget. (Appendix A presents the President's FY08 budget request for NASA space science programs.) Space science programs represent 23.2 percent of the President's total FY08 budget request for NASA. Within the proposed FY08 budget for space sciences programs, heliophysics represents 26 percent, planetary science represents 35 percent and astrophysics represents 39 percent of the total space science funding.

Comparing the President's FY08 budget request with the funding requested for FY08–FY11 in the President's FY07 proposal (and under full cost simplification) shows that planetary science gains $87M, while heliophysics loses over $300M and astrophysics is decreased by about $125M. The FY08 budget request shows the following cumulative results for individual science missions, over the FY08–FY11 period, relative to the President's FY07 budget request:

- NASA adds funding to support the development of several key missions and mission areas, including (in millions of dollars):

James Webb Space Telescope	+ 95.6
Stratospheric Observatory for Infrared Astronomy	+344.9
Hubble Space Telescope	+ 3.5
Gamma Ray Large Area Space Telescope	+ 17.0
Kepler	+ 53.2
Astrophysics research missions (in operation)	+134.9
Planetary science research	+378.8

- However, there are significant funding cuts to other space science activities, activities over the same period, such as (in millions of dollars):

Navigator (missions on extrasolar planets)	-819.6
Mars missions/ exploration	-264.7
New Millennium (tech validation missions)	-124.5
Solar Terrestrial Probes (heliophysics mission)	- 84.6
Living with a Star (space weather missions)	- 83.5
Discovery cost-capped planetary program	- 51.6
Beyond Einstein program	-33.9
Heliophysics research	-13.6

In 2008, the Science Mission Directorate plans to launch Kepler, Interstellar Boundary Explorer, Solar Dynamics Observatory, conduct a fourth Hubble servicing

mission; and complete contributions to international and interagency partner missions that are planned for launch in 2008.

Heliophysics

The President's FY08 budget request for NASA includes $1.057 billion for the Heliophysics theme, which seeks to understand the Sun and its effect on the Earth, the rest of the solar system, and the conditions in the space environment and their effects on astronauts; and to develop and demonstrate technologies to predict space weather.

Programs within the Heliophysics theme include:

- *Heliophysics Research*—research and analysis; space missions; sounding rockets and other scientific platforms; science data and computing technology;
- *Living with a Star*—investigations to understand solar variability (space weather), its effect on the Earth and the rest of the solar system, and the implications for ground-based systems such as electric power grids and wireless communications, and for on-orbit spacecraft and astronauts. Space missions under the Living with a Star program include:
  - Solar Dynamics Observatory (SDO) to understand the structure of the Sun's magnetic field and how magnetic field energy forms the solar wind, energetic particles, and fluctuations in solar irradiance. SDO will help acquire data to enable space weather predictions. SDO is slated to launch in 2008.
  - Radiation Belt Storm Probes (RBSP) to investigate solar storms and their interaction with charged particles, fields, and radiation in the Van Allen radiation belts. The results of the mission will be used to develop models that assist engineers in designing systems to withstand radiation effects and to alert pilots and crews of potentially hazardous solar storms or radiation. RBSP is estimated to launch around 2012.
- *Solar Terrestrial Probes*—missions to investigate the Sun, the heliosphere, and planetary environments as an interrelated system. Missions within the Solar Terrestrial Probe program include:
  - Magnetospheric Multi-scale (MMS) is proposed as a system of four spacecraft to investigate processes such as magnetic reconnection, which involves the transfer of energy from the solar wind to the Earth's magnetosphere, and is an important factor in predicting space weather. The estimated launch date for MMS is 2013.
- *Heliophysics Explorer Program*—small and medium-class competitively-selected missions that endeavor to provide frequent flight opportunities to investigate focused research. Explorer programs are cost-capped and awarded to individual principal investigators who have sole responsibility for the scientific and technical success of the mission.
- *New Millennium*—a program to validate technologies for use in future space science missions. The program reduces the risk of new technologies that have not yet been flown in space.
- *Deep Space Mission Systems*—telecommunications and navigation services (e.g., the Deep Space Network) to support human and robotic exploration of the solar system. [This program is located in Heliophysics as a bookkeeping function in the FY08 request.]

Issues

- "Flagship" missions including the James Webb Space Telescope, which is under development in the Astrophysics Program, and the Cassini mission which is currently investigating Saturn, for the Planetary Science program, represent long-term, high priority scientific investigations for those disciplines. The National Academies decadal survey for solar and space physics recommended in 2003 the Solar Probe as a flagship mission to measure the heating and acceleration of the solar wind. According to NASA's Science Plan for 2007–2016, "a flagship mission cannot be supported within the available funding resources." What are NASA's plans for Solar Probe and why are flagship missions being pursued in other science disciplines but not in Heliophysics? How does the absence of a Solar Probe mission affect the balance of the Heliophysics program?

Planetary Science

The President's FY08 budget request provides $1.396 billion to fund NASA's Planetary Science theme, which seeks to understand:

- the history and evolution of the solar system;
- whether life existed or exists beyond Earth.

The FY08 budget represents a decrease of $15.4 million or one percent cut relative to the President's FY07 budget request for planetary science.

The Planetary Science program includes the following elements:

- *Mars Exploration*—several mission projects aimed at exploring Mars for indicators of life, helping to understand the history of the solar system, and to improving our understanding of the potential hazards to humans in future Mars explorations.
  - Mars Scout 2007 (Phoenix) is a mission to help understand the chemistry, mineralogy and composition of gases in surface and subsurface soils at areas in the northern latitudes of Mars. The Mars Scout line is led by a principal investigator, a scientist who is selected competitively to lead the development of a mission and ensure its scientific and technical success. Mars Scout missions are cost-capped at $475M (FY06 dollars). Phoenix is scheduled for launch in August, 2007.
  - Mars Science Laboratory is a NASA strategic rover mission designed with a new entry, descent and landing system to take measurements focused on identifying possible Martian habitats for life. Mars Science Laboratory is scheduled for launch in 2009.
- *Discovery Program*—a program of missions that offer scientists opportunities to form a team and submit a proposal to design and develop innovative, medium-sized, missions that address focused science objectives. Proposals are competed; NASA awards funds to the scientist, as principal investigator, leading the selected proposal. Principal investigators are responsible for the scientific, technical and managerial success of the mission. Discovery missions are cost-capped at $425M, according to the Announcement of Opportunity issued in 2006. Discovery missions under development include Dawn—a mission whose purpose is to visit and study Vesta and Ceres, the two largest asteroids in the solar system. Dawn is scheduled for launch in June 2007.
- *New Frontiers*—offers opportunities for scientists to form a team and propose to design and develop innovative, medium-sized missions that focus on understanding the origin, evolution, and formation of the solar system. New Frontiers missions are led by principal investigators and have a cost-cap up to $700M in FY03 dollars, as of 2006. New Frontiers missions include:
  - New Horizons, launched in 2006, which is en route to Pluto where it will collect data about the geology and atmosphere of Pluto and its moon, Charon.
  - Juno, a mission that is being planned to investigate several aspects of Jupiter including its interior structure and its atmosphere. Juno is being planned for launch in 2011. Juno is a high priority mission of both the National Academies' solar system exploration and solar and space physics decadal surveys.
- *Technology*—a program to develop Radioisotope Power Systems such as radioisotopic thermoelectric generators and In-Space Propulsion technologies such as solar electric propulsion and solar sail propulsion that enable solar system exploration missions to reach distant outer planets at lower costs, with less mass, and for shorter travel times.
- *Planetary Science Research* includes research and analysis, lunar science and funding for existing missions and planetary data archiving. Specific program elements include:
  - Research and Analysis programs involve the development of theory and instrumentation to enable future planetary science missions as well as research on specific interdisciplinary areas such as astrobiology and cosmochemistry (research on the origins and evolution of planetary systems and for study of the atmospheres, geology, and chemistry of planets in the solar system).
  - Lunar Science is a new program in the FY08 request, which provides funds for the archiving of lunar science data, lunar science instruments

and payloads that are selected through peer review, analysis of data from lunar missions, and technology development for lunar science missions.

The planetary science research program also supports planetary data systems and astromaterials curation; the Cassini Huygens mission; U.S. involvement in non-U.S. missions such as the European cometary mission, Rosetta, and the Japanese cometary sample return mission, Hayabusa.

Issues

- NASA created the interdisciplinary field of astrobiology in the late 1990s to increase knowledge on the origin and evolution of life on Earth and beyond Earth. Two National Academies decadal surveys strongly support Astrobiology, and Astrobiology contributes to NASA's own strategic goal to "Advance scientific knowledge of the origin and history of the solar system, the potential for life elsewhere, and the hazards and resources present as humans explore space," as stated in the 2006 NASA Strategic Plan. According to the January–March 2007 Newsletter of the National Academies' Space Studies Board, over the last two years, NASA cut the budget for Astrobiology by 50 percent, from approximately $65 million to $31 million. In FY07, reductions in the astrobiology budget reduced the number of research institutions participating as part of the NASA Astrobiology Institute from 16 to 12, and the funding for those 12 teams was reduced. [The Astrobiology Institute is a consortium of institutions that have been competitively selected and provided seed funding for astrobiology research programs.] No new research has been provided in the Astrobiology Science and Technology for Exploring Planets program or the Astrobiology Science and Technology Instrument Development program since 2004. Funding for grants in the exobiology and evolutionary biology program has been delayed. The cuts to the research program have affected graduate students, post-doctoral students and junior faculty, who rely on grant funding for their research. The decrease in available funding and research opportunities is expected to discourage younger scientists from entering the field.
- The FY08 budget request adds $27 million of new content in FY08 through the creation of a lunar science research program in the Planetary Science Research line. The total funding budgeted for lunar science through FY 2012 is $350 million. The goals for the lunar science program over the next five years include archiving of data from the lunar precursor robotics missions; launching missions of opportunity for scientific instruments on lunar precursor robotic missions or international lunar missions and funding the analysis of data from those missions. Plans for the lunar science program also involve providing opportunities for developing instruments and technologies to support lunar science studies and investigations. What priority will the new lunar science program have relative to other space science research activities? Is it intended to support the human lunar exploration program, or is it independent of that initiative?

Astrophysics

The President's NASA FY08 budget request includes $1.566 billion to fund NASA's Astrophysics program, which seeks to improve our understanding of the origin, structure, evolution and future of the Universe and to search for Earth-like planets. The FY08 request represents a $2.8 million or .02 percent increase over the President's FY07 budget proposal.

The Astrophysics program includes the following elements:

- *Astrophysics Research* includes managing operating missions; managing, archiving, and disseminating mission data; funding science research and data analysis; and technology development
- *Gamma-ray Large Space Telescope* (GLAST) is a mission being conducted with NASA and the Department of Energy. The mission will take measurements of high-energy gamma rays in an effort to understand their sources and behavior. GLAST is scheduled for launch in November 2007.
- *Kepler* is a competitively-selected principal investigator-led mission in the Discovery program that will search for Earth-like planets. Kepler is scheduled for launch in November 2008.
- *James Webb Space Telescope* (JWST) is an infrared observatory involving a 6.5m aperture mirror and sunshade that will unfold upon deployment in space. JWST will enable scientific study of the early Universe and of the de-

velopment of galaxies, stars, planetary systems and the elements required for life. JWST is the top-ranked mission from the last National Academies decadal survey in astronomy and astrophysics and is considered the successor to the Hubble Space Telescope. JWST is slated for launch in 2013.

- *Hubble Space Telescope* is a space observatory currently utilized to study and understand the formation, structure, and evolution of stars and galaxies in the visible, near infrared and ultraviolet wavelengths. The Hubble was designed to be serviced from space. The fourth Shuttle servicing mission is scheduled for September 2008 to replace batteries, gyroscopes, and other systems necessary for operating capabilities and to add new scientific instruments. Hubble was launched in 1990.
- *Navigator Program* involves several projects aimed at the search for habitable planets beyond the solar system:
  - Space Interferometer-PlanetQuest (SIM) is a mission to conduct a census of planetary systems and to identify the location and masses of targets for potential further study. SIM is a technology development project.
  - Terrestrial Planet Finder (TPF) is a concept for a space mission that would detect planets similar to Earth in the areas of nearby stars that are considered possible for the formation of Earth-like planets. TPF would collect and analyze data on the spectra of planets it identified for possible signs of life. TPF is a technology development project.
  - The Keck Interferometer (KI) is a ground-based effort currently under development to measure the dust and gas around stars, especially the inner region of stars where Earth-like planets may form.
  - Large Binocular Telescope Interferometer (LBTI) in under development and will take measurements of the dust and gas surrounding stars, including the outer ranges of disks around stars where it is thought that Jupiter-like planets might form and evolve.
- *Stratospheric Observatory for Infrared Astronomy* (SOFIA) is an astronomical observatory to help understand the birth and death of stars, how new solar systems form, among other astrophysical questions. The SOFIA observatory includes a 2.5 meter telescope, provided by the German Aerospace Center (DLR), that will be mounted on a customized Boeing 747 aircraft.
- *Astrophysics Explorer Program* provides opportunities for researchers to assemble a team and propose to design and develop a focused science mission. Explorer missions are led by principal investigators and are cost-capped. The program is intended to offer frequent flight opportunities and to conduct focused science investigations that complement larger, NASA-developed strategic missions. Astrophysics Explorer missions in development include Wide-Field Infrared Survey Explorer (WISE) which seeks, as a main objective, to find the brightest galaxies in the Universe. WISE is slated for launch in 2009.
- *International Space Science Collaboration,* which involves the U.S. contribution of instruments, subsystems, and U.S. investigators to two European-led missions.
- *Beyond Einstein,* a program including space missions, research and theory work, and technology development aimed at improving our understanding of proposed missions to help understand Einstein's theory of general relativity and its predictions about the Big Bang, black holes, and dark energy. NASA has commissioned a National Academies study to recommend which Beyond Einstein mission should be developed and launched first. The Beyond Einstein program, as described in NASA's FY08 budget request documentation, includes:
  - Laser Interferometer Space Antenna (LISA), a collaborative mission with the European Space Agency to measure gravitational waves.
  - Constellation-X Observatory (Con-X), a mission that will harness the collective power of several x-ray telescopes to investigate black holes, Einstein's theory of general relativity, the formation of galaxies, and the nature of dark matter and dark energy, among other science goals.
  - Joint Dark Energy Mission, which will study the nature of dark energy in the Universe and the expansion of the Universe.
  - Beyond Einstein Future Missions, which include an Inflation Probe to study the causes of the inflation of the Universe and Black Hole Finder Probe, which will conduct a census of black holes to identify where they are and when and how they form.

Issues

- The Navigator Program, a project within the Astrophysics theme, seeks to understand how planets and planetary systems form, search for planets around other stars, and characterize those planets and their environments for signs of potential life. The Space Interferometer-PlanetQuest (SIM) mission along with the Terrestrial Planet Finder (TPF) mission are integral components of the Navigator Program. The 2001 astronomy and astrophysics decadal survey recommends SIM for completion and TPF as a technology development project. The President's FY07 request for NASA delayed SIM to a potential 2015 or 2016 launch and deferred TPF development indefinitely. The FY08 request cuts $800M from the Navigator Program between FY08 and FY11. The FY08 request does provide funds ($35.5M) for reinstating technology development work on TPF. What is the appropriate path for the Navigator program? Should funding be restored to put SIM back on track for mission development? Should funding for TPF technology development be increased? Should both the SIM and TPF missions be deferred until they can be reconsidered in the next decadal survey?
- As can be seen in the chart below, a large number of highly recommended astrophysics missions have been delayed, canceled, or deferred. At the same time, the recent *National Academies Assessment of NASA's Astrophysics Program* noted that: "Although six astrophysics Explorer missions have been launched in the current decade, those launches are the result of development work performed mostly in the 1990s. At this point it appears that only one Explorer mission will be developed and launched in this decade, and at most one Explorer will begin development in this decade for launch in the next." What is the outlook for the Astrophysics program if current trends continue, and what should be done?

Summary of NASA Plans for Recommended Large and Moderate Astrophysics Missions

MISSION	Recommended by	Launch Date 2003 Plan	Launch Date 2006 Plan	Status
Hubble Space Telescope Servicing Mission-4	1980s, 1990s, 2001 decadal surveys	2004	2008	DELAY
Space Infrared Teles. Facility (SIRTF)	1990s, 2001 surveys	2003	2003	LAUNCHED
Stratospheric Observatory for Infrared Astronomy (SOPHIA)	1990s, 2001 surveys	2005	Canceled	REINSTATED
Space Interferometry Mission (SIM)	1980s, 1990s, & 2001 surveys	2005-2010	NET 2015	DELAY
Keck Telescope Outriggers		2003	Canceled	Canceled
Herschel/ Planck	European Space Agency	2007	2008	DELAY
Gamma-ray Large Area Space Telescope (GLAST)	2001 survey	2007	2007	
Kepler (Discovery)	2001 survey	2007	2008	DELAY
James Webb Space Telescope	2001 survey	2005-2010	2013	DELAY
Constellation-X	2001 survey, Q2C	NET 2011	NET 2016	DELAY
Terrestrial Planet Finder	2001 survey	2010-15	NET 2018	DELAY
Laser Interferometer Space Antenna	2001 survey, Q2C	NET 2011	NET 2016	DELAY

Summary of NASA Plans for Recommended Large and Moderate Astrophysics Missions

MISSION	Recommended by	Launch Date 2003 Plan	Launch Date 2006 Plan	Status
Black Hole Finder Probe	2001 survey	NET 2012	Deferred	DEFERRED
Single Aperture Far Infra-Red Observatory	2001 survey, Q2C	Deferred	Deferred	DEFERRED
Inflation Probe	Q2C	NET 2012	Deferred	DEFERRED
Joint Dark Energy Mission	Q2C	NET 2012	Deferred	DEFERRED
Large Binocular Telescope Interferometer	2001 survey	2005	2009	DELAYED

Source: Modified from National Research Council, *A Performance Assessment of NASA's Astrophysics Program*, National Academies Press, Washington, D.C., 2007.

Note: Q2C is an abbreviation for National Research Council, *Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century*, National Academies Press, Washington, D.C., 2003.

- The President's FY08 budget request includes an estimate for a Space Shuttle servicing mission of the Hubble Space Telescope in May 2008, and the budget proposes funding to support that date. An updated Shuttle manifest moved the mission to September 2008, leaving a gap of four months or $40 million ($10 million a month in costs). The current tentative Shuttle manifest has moved the mission forward to an August 2008 launch, although further changes and launch delays could widen the funding shortfall. It is not yet clear where NASA will find the $40 million to fill the gap.

APPENDIX A

FY 08 NASA Budget Request

(Budget authority, $ in millions)	FY 2007	FY 2008	FY 2009	FY 2010	FY 2011	FY 2012
SCIENCE	**4,002.3**	**4,018.8**	**4,009.5**	**4,080.5**	**4,245.7**	**4,449.5**
PLANETARY SCIENCE	**1,411.2**	**1,395.8**	**1,676.9**	**1,720.3**	**1,738.3**	**1,748.2**
Discovery	179.9	184.9	320.7	370.2	355.2	341.1
New Frontiers	158.1	147.3	296.0	277.5	267.9	274.5
Solar System Technology	73.4	67.6	62.6	63.9	62.7	64.2
Planetary Science Research	278.8	370.5	402.9	416.2	428.5	402.9
Mars Exploration	721.1	625.7	594.8	592.5	624.0	665.5
HELIOPHYSICS	**1,028.1**	**1,057.2**	**1,028.4**	**1,091.3**	**1,241.2**	**1,307.5**
Heliophysics Research	221.2	206.1	188.0	201.5	192.8	207.5
New Millennium	89.6	66.2	33.0	36.0	92.1	95.9
Near Earth Networks	63.7	66.0	65.2	67.2	65.6	66.9
Deep Space Mission Systems	254.2	263.0	272.1	277.7	276.5	282.4
Living with a Star	232.5	253.0	269.2	261.4	266.1	286.7
Solar Terrestrial Probes	88.7	126.8	125.3	114.4	181.3	181.5
Heliophysics Explorer Program	78.3	76.1	75.6	133.1	166.8	186.5
ASTROPHYSICS	**1,563.0**	**1,565.8**	**1,304.2**	**1,268.9**	**1,266.2**	**1,393.8**
Navigator	124.7	57.1	58.4	59.5	61.0	62.5
James Webb Space Telescope	468.5	545.4	452.1	376.9	321.1	285.9
Hubble Space Telescope	343.0	277.7	165.2	152.8	151.4	151.3
Stratospheric Obserbatory for Infared Astronomy		77.3	89.1	88.6	89.9	92.1
Gamma-ray Large Space Telescope	90.7	42.2	28.3	28.3	29.3	30.2
Discovery (Kepler)	105.0	93.0	25.7	16.3	16.2	17.6
Astrophysics Explorer	69.4	99.1	88.8	28.2	11.7	5.7
Astrophysics Research	319.8	315.2	306.1	331.9	378.5	491.4
International Space Science Collaboration	19.8	26.5	39.1	38.7	36.5	35.2
Beyond Einstein	22.1	32.3	51.5	147.6	170.6	222.1
Year to Year Increase		**0.4%**	**-0.2%**	**1.8%**	**4.0%**	**4.8%**

Chairman UDALL. Good morning. This hearing will come to order. I would like to begin by welcoming all of our witnesses to today's hearing. We have a distinguished panel that can provide this subcommittee with important perspectives on the state of NASA's space science activities. In particular, I would like to welcome Dr. Alan Stern, the new Associate Administrator of NASA's Science Mission Directorate. I got to know Dr. Stern when he was at the Southwest Research Institute, and I look forward to working with him in his new role.

As Chairman Calvert reminded me, he is also a constituent of mine and I am glad to have Alan here.

I would also like to welcome Dr. Dan Baker, who is the Director of University of Colorado's laboratory for Atmospheric and Space Physics in Boulder, Colorado, also a constituent. Dr. Baker, great to have you.

As can be seen by the title of today's hearing, we are going to focus on a subset of NASA's science activities, mainly its astrophysics, planetary science, and heliophysics programs. Obviously, NASA's Earth science program is an important element of NASA's overall science program, but it will be the focus of a separate hearing that will expand on the Full Committee hearing we held earlier this year.

In addition, while not currently part of the Science Mission Directorate, NASA' life and microgravity research programs are also important research endeavors that will be scrutinized by this subcommittee in the coming months, particularly in light of the deep—and many would say, unwise—cuts that NASA has made to those programs. To paraphrase Dickens, it is both the best of times and the worst of times for NASA'S space science programs.

We have witnessed a whole series of exciting events in recent months, whether it be the discovery of possible recent liquid water flows on Mars, stereo images of solar activity, or Nobel Prizes awarded for research enabled by NASA's cosmic background explorer. These are just a few of the accomplishments of NASA's space science enterprise over the last few years.

In short, NASA's space science programs are highly productive and exciting in addressing compelling scientific questions. That is the good news.

What is the bad news? The bad news is that while those accomplishments were enabled by the Nation's past investments in NASA's science activities, the outlook for the needed future investments is not good if present trends are any indication.

For example, the five-year funding plan for NASA's science mission directorate has been reduced by a total of $4 billion since fiscal year 2005, which is a significant disruption. In addition, the impact of those cuts to NASA'S out year science funding is magnified by cost growth that has occurred within some science missions under development, cost growth that is putting additional stress on the overall space science program.

Another example: the Explorer Program, which has enabled major scientific discoveries, has seen new mission opportunities dramatically curtailed. Funding for research and analysis which helps to enable scientific research and train the next generation of scientists and engineers was cut by 15 percent in fiscal year 2007.

Those cuts were also applied retroactively to fiscal year 2006, and that reduced R&A funding level was maintained in the '08 request.

Moreover, that 15 percent R&A cut was an average cut with some disciplines suffering much deeper cuts.

In short, at a time when NASA's science programs offer the promise of major advances in our understanding of the Sun, the solar system, and the universe beyond, we risk long-term damage to the health of those programs if we are not careful. That is why I look forward to hearing from Dr. Stern and the rest of our expert panel today. We need to get their best assessment of the challenges facing NASA's space science program, and the likely consequences of inaction, and most importantly, their recommendations for addressing those challenges.

At the end of the day, however, it is clear to me that if we are going to ask our nation's space science program to undertake challenging and meaningful initiatives, we are going to need to provide the necessary resources.

In closing, again, I want to welcome our witnesses, and I now yield to my colleague, my good friend Ranking Member Calvert, for any opening remarks he would like to make.

[The prepared statement of Chairman Udall follows:]

PREPARED STATEMENT OF CHAIRMAN MARK UDALL

Good morning. I'd like to begin by welcoming all of our witnesses to today's hearing. We have a distinguished panel that can provide this subcommittee with important perspectives on the state of NASA's space science activities.

In particular, I would like to welcome Dr. Alan Stern, the new Associate Administrator of NASA's Science Mission Directorate. I got to know Dr. Stern a bit when he was at the Southwest Research Institute, and I look forward to working with him in his new role.

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To paraphrase Dickens, it is both *"the best of times and the worst of times"* for NASA's space science programs. We have witnessed a whole series of exciting events in recent months, whether it be the discovery of possible recent liquid water flows on Mars, stereo images of solar activity, or Nobel prizes awarded for research enabled by NASA's Cosmic Background Explorer.

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In short, at a time when NASA's science programs offer the promise of major advances in our understanding of the sun, our solar system, and the universe beyond, we risk doing long-term damage to the health of those programs if we are not careful.

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In closing, I again want to welcome our witnesses, and I now yield to my colleague, Ranking Member Calvert, for any opening remarks he would like to make.

Mr. CALVERT. Thank you, Mr. Chairman. I would like to thank you for scheduling today's hearing on NASA's space science program, and my sincere thanks to our witnesses for taking time from their busy schedules to join us this morning and share their views and recommendations.

I am glad our Chairman is here today. He was out running this morning and did a great job, that is why he is a little sweaty in here, though the room is a little warm, Mr. Chairman. Let me just point that out.

As everyone in this room well knows, NASA is an extraordinary agency that, at a relatively small cost to the taxpayer, has produced science discoveries that have transformed man's view of the universe around us, and has also demonstrated that man can live and work in space. The pace and scope of science discoveries over the last decade have been breathtaking. Dark energy, dark matter, extra solar planets, evidence of water, as the Chairman mentioned, on Mars, just to name a few.

Despite the fact that funding for NASA's science mission is roughly 32 percent of the Agency's budget, including Earth science, hovering near a historical high relative to the overall Agency budget, the tempo of new discoveries and capabilities that we recently enjoyed are at serious risk of tapering off for a variety of well-understood reasons.

One, mission costs have far exceeded early projections.

Two, until Mike Griffin's arrival as Administrator, NASA was developing too many missions for the resources they had available, forcing the Agency to stretch out schedules, stay within budget, and delaying the pace of new starts.

Three, cost uncertainties of launching small and medium-sized payloads after Delta 2 is retired, and mission assurance and accounting changes.

Everyone in this room understands that severe budget challenges are also confronting NASA in its manned space flight and aeronautics research programs, forcing the Agency to remove future budget growth from the science mission directorate in order to ad-

dress more pressing needs. I don't fault NASA for making the tough choices it did, but it shouldn't be that way.

I have stated before and I will say it again, that the Administration must provide NASA with realistic budget requests to match resources with program content, otherwise, the balance among NASA's programs becomes imperiled as the Agency moves resources around to fund priorities and invites Congress—and this is often not a good thing—to begin imposing its own preferences.

NASA Administrator Mike Griffin is doing an exceptional job, in my opinion, leading the Agency. He has set priorities, and while everybody in this room may not agree with his decisions, he has not attempted to be disingenuous or hasn't disguised his decisions.

NASA's science enterprise leads the world in the quest for human understanding of the cosmos, our solar system, and indeed, our home planet. The strength of the Agency's science program is rooted in its close working relationship with the science community. Our witnesses today will provide us with the best guidance on how NASA and Congress can address the challenges confronting the science community to ensure a return to a robust mission tempo and to ensure a strong cadre of scientists and engineers to propose and design future missions.

With that, Mr. Chairman, my thanks, and again, thanks to our witnesses.

[The prepared statement of Mr. Calvert follows:]

PREPARED STATEMENT OF REPRESENTATIVE KEN CALVERT

Thank you, Mr. Chairman, for scheduling today's hearing on NASA's Space Science program, and my sincere thanks to our witnesses for taking time from their busy schedules to join us this morning and share their views and recommendations.

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Yet despite the fact that funding for NASA science missions is roughly 32 percent of the Agency's budget (including Earth Science), hovering near an historical high relative to the overall agency budget, the tempo of new discoveries and capabilities that we've recently enjoyed are at serious risk of tapering off for a variety of well understood reasons—

- mission costs have far exceeded early projections;
- until Mike Griffin's arrival as Administrator, NASA was developing too many missions for the resources it had available, forcing the Agency to stretch out schedules to stay within budget, and delaying the pace of new starts;
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Our witnesses today will provide us with their best guidance on how NASA and Congress can address the challenges confronting the science community to ensure a return to a robust mission tempo and ensure a strong cadre of scientists and engineers to propose and design future missions.

Thank you, Mr. Chairman, and my thanks again to our witnesses.

Chairman UDALL. Thank you, Mr. Calvert.

I want to do a little housekeeping at this point before we begin the testimony. If there are Members who wish to submit additional opening statements, your statements will be added to the record. Without objection, so ordered.

In addition, we would also like to include a statement for the record from the Planetary Society in today's hearing. Again, without objection, so ordered.

[The information follows:]

STATEMENT OF THE PLANETARY SOCIETY

Restoring the Vision

NASA is a great agency achieving great things. NASA brings out the best in us, a society using some of its great wealth to help people around the globe understand our place in the Universe and to inspire generations of explorers. NASA's images, its heroes, along with its scientific and engineering achievements, have changed the world for all humankind. This statement criticizing both the proposed NASA budget and current NASA operating plan is in support of space exploration and the process of scientific discovery and engineering achievement that NASA represents to the world.

The Vision for Space Exploration—which is supposed to be guiding NASA's program and budget—has become distorted. Its mantra, "go as you pay," has become "go as you cannibalize other programs." Its scientific underpinnings have been removed, leaving it suspended with uncertain public support and public interest.

This statement of The Planetary Society, is designed to represent that public interest. The Society is the largest public space-interest group in the world, a nongovernmental organization that represents no particular profession, but instead represents the interest of citizens who believe in the value of space exploration to the Nation and to the world.

The original Vision for Space Exploration's first goal was *"a sustained and affordable human and robotic program to explore the solar system and beyond."* Instead, the robotic program has been undercut and the solar system is nearly unmentioned in the human program.

The original Vision for Space Exploration was to *"Undertake lunar exploration activities to enable sustained human and robotic exploration of Mars and more distant destinations in the solar system."* Instead, Mars robotic exploration in the next decade has been almost eliminated, and lunar exploration activities have been diverted to constructing a permanent lunar base with macro-engineering projects in place of exploration objectives.

The original Vision for Space Exploration directed NASA to *"Conduct robotic exploration of Mars to search for evidence of life, to understand the history of the solar system, and to prepare for future human exploration; Conduct robotic exploration across the solar system for scientific purposes and to support human exploration. In particular, explore Jupiter's moons, asteroids and other bodies to search for evidence of life, to understand the history of the solar system, and to search for resources; and Conduct advanced telescope searches for Earth-like planets and habitable environments around other stars."* Instead, Mars exploration has been cut, the mission to Jupiter's moon Europa and the Terrestrial Planet Finder mission have been eliminated, and the search for extraterrestrial life has been cut in half.

Instead of "for scientific purposes," the program has seen $3 billion eliminated from four years of space science planning, and science research and data analysis—the "seed corn" that allows NASA to reap future benefits from its exploration programs—was cut 15 percent across the board.

These contradictions between the conduct of the NASA program and the originally stated Vision for Space Exploration explain why The Planetary Society supports the Vision but opposes its current implementation plan. The word "exploration" has been hijacked and is now used to mean human space vehicle development, instead of missions and discoveries in the solar system.

Not only do we still support the Vision, we also support the NASA Administrator in his incredibly difficult effort to, at long last, redirect human space flight beyond Earth orbit. Mike Griffin is not against science, but he has been given too few resources and too many constraints to properly administer either the Vision or space science and exploration.

NASA cannot juggle limited resources and overburdening constraints without dropping a few balls. NASA's budget should be increased as was originally envisioned, and as this committee particularly supported, to restore the Vision's scientific underpinnings and to prepare for human exploration of the solar system. If such a realistic budget increase is not possible, then the Vision's timetable should be stretched. In fact originally the Vision was said to have no timetable. Most of the current dislocations in the Vision's Constellation program are being driven by arbitrary dates having only political objectives. There is no national security or economic driver that requires its current timetable.

"Save Our Science" has become a rallying cry for The Planetary Society—we submitted thousands of petitions to Congress last year, and thousands more to the President this year, from citizens asking to restore the science funding that was cut from the NASA plan. Intellectually, science and exploration are inextricably linked, but the "firewall" that once helped protect science needs to be restored.

We fully recognize that space science is not an entitlement program and that it can proceed at a slower pace. Our call to restore the scientific underpinnings to the Vision and to NASA's budget is not a statement of special interest for scientists. Too often, NASA is forced to make decisions in order to bolster one or another part of its work force because of some special interest. Our call is dominated by the public interest and by public support for the great ventures of space exploration—the ventures that for the past decade brought such extraordinary credit and support for NASA in the U.S. and around the world.

Consider just three examples: the remarkable, continuing three-year odyssey of the Spirit and Opportunity rovers on Mars; the complementary discoveries about water being made from spacecraft in orbit around Mars; and the thrilling international Cassini/Huygens mission in the Saturn system. The fantastic discoveries from these explorations—the watery history of Mars and the possibility of liquid water on its surface today; water geysers in the Saturnian system; and hydrocarbon lakes on Titan, to name a few—are only part of the rewards that the U.S. has accrued. The adventures of roving on Mars, probing Titan, and voyages through the solar system have enthralled the public, motivated a generation of students and their teachers, and have advanced American technology. And, of course, we should mention the Hubble Space Telescope and the Voyager probes. Their decades-long explorations have inspired generations of students to strive for excellence, and yet NASA was ready to abandon them both just a few years ago.

The FY 2007 budget damaged the future of NASA. Science missions were delayed or canceled; technology funding was slashed, as was research funding—astrobiology, in particular. The slash in research and technology funding put at risk the ability to develop future missions and to adequately analyze data from existing ones; it will drive many young people from the field, thereby mortgaging the future of NASA science and exploration.

Congress recognized these problems last year, the FY 2007 Appropriations Bill passed by the House would have partially rectified these problems. The Senate was also working to correct the situation. But all that work was lost when no budget was passed last year. We urgently ask you to support restoration for some of the losses in NASA science, technology, and flight missions.

There is one additional thing that you could do to help open the box in which NASA has been placed—the box defined by too much politics and not enough resources. That is international cooperation. Four nations, besides the U.S., are planning lunar missions: Japan and China this year, India next year, Russia soon afterwards. In fact, these countries are not just sending single lunar missions, but each has a lunar program with orbiters to be followed by landers and rovers. Europe is also planning lunar missions as part of its Aurora exploration program. For the U.S.

to plan a lunar base completely independent of these missions is not just wasteful, it lacks rationale. It lacks vision.

The Planetary Society has called for an International Lunar Decade in which the space-faring nations of the world can cooperate to advance their exploration objectives, and in which the developing world can share in the benefits of space science and exploration. The U.S. could return to its original Vision for Space Exploration, looking forward to Mars. We have already landed humans on the Moon. We can work with other nations as they now reach for the Moon, and in that way, build a rationale that serves more than just a space program, but global cooperation as well.

This statement has focused on exploration, the goal enunciated in the Vision for Space Exploration to extend human presence into the solar system. More specifically, we have focused on the planets. That isn't too surprising—we are after all, The *Planetary* Society. However, we were founded on the premise that one of the chief goals of planetary exploration is to learn about ourselves, and about our own planet. The very first observations and models of global climate change came from planetary missions to Venus, and then later, to Mars. The most basic scientific work of our co-founders Carl Sagan and Bruce Murray was about comparative planetology, studying other worlds to understand the processes at work on our own planet. Never in our history has understanding the Earth been so important. Congress should, along with addressing all other science concerns, restore the programs and missions in NASA to observe the Earth.

This past year, NASA dropped "understanding the Earth" from its mission statement. The Planetary Society picked it up, and added it to our own mission statement.[1] But we cannot pick up the budget for the planetary and Earth science that has been cut from the NASA budget. Congress must do that. We urge Congress to help NASA achieve the goals articulated in the Vision for Space Exploration, for the benefit of our future, and our children's future. Save our future; Save Our Science.

STATEMENT OF J. CRAIG WHEELER
PRESIDENT, AMERICAN ASTRONOMICAL SOCIETY

I appreciate the opportunity to comment on NASA's 2008 science budget from my perspective as President of the American Astronomical Society (AAS).

The AAS believes that NASA's Science Mission Directorate (SMD) should be part of the American Innovation Agenda, which seeks to bolster funding for the National Science Foundation (NSF), the Department of Energy's (DOE) Office of Science, and the National Institute for Standards and Technology (NIST). These agencies have been identified as vital to America's leadership in innovation, by training a highly-skilled workforce and fostering the discovery and development of new ideas. NASA Science is a partner in these endeavors. Specifically, we advocate for increasing NASA SMD's FY 2008 budget to $5.566 billion, which is six percent over the final FY 2007 amount and a modest increase over the President's FY 2008 request.

The AAS is the major organization of professional astronomers in the United States. The basic objective of the AAS is to promote the advancement of astronomy and closely related branches of science. The membership, numbering approximately 7000, includes physicists, mathematicians, geologists, and engineers whose interests lie within the broad spectrum of modern astronomy. AAS members advise NASA on scientific priorities, participate in NASA missions, and use the data from NASA's outstanding scientific discoveries to build a coherent picture for the origin and evolution of the Earth, the solar system, our Galaxy, and the Universe as a whole.

In the recent past, the astronomical community, working together with NASA, has produced a remarkable string of successes that have changed our basic picture of the Universe. Observations with the *Hubble Space Telescope (HST)* of exploding stars whose light has been traveling for half the age of the Universe, combined with the exquisite map of the glow from the Big Bang itself from the *Wilkinson Microwave Anisotropy Probe* and information from other observatories, shows that the Universe we live in is not the Universe we see. Mysterious Dark Matter makes the ordinary particles clump together to form stars and galaxies. Even more mysterious Dark Energy makes the expansion of the Universe speed up. Both of these concepts challenge our understanding of the nature of matter and energy in the Universe and

[1] To inspire the people of Earth to explore other worlds, understand our own, and seek life elsewhere.

open up broad new vistas for future work. An ambitious set of Great Observatories, now including *Spitzer* in the infrared and *Chandra* at X-ray wavelengths, is hard at work, enriching our understanding of how the Universe works.

Similarly, exploration of the solar system has been a resounding success for NASA, with exciting missions to Mars and to Saturn revealing a beautiful and intricate history that is interwoven with the history of our planet Earth. The discovery of planets around other stars has been a great triumph of the past decade, raising hopes for seeing planets like our own Earth, and placing our own solar system, and life itself, in a new context.

NASA's key role in these discoveries makes its science program of deep interest to AAS members. In the past, NASA has worked with the astronomical community to find the most promising paths forward. The *James Webb Space Telescope (JWST)* is a large program that was endorsed by the National Academy of Sciences (NAS) Decadal Survey in astronomy. When completed in the next decade, it will help expand the frontier of knowledge to the deepest reaches of space and time and into the hidden places where stars and planets are formed. The astronomical community also recommended, and NASA plans to execute, a wide range of other programs—some of moderate scope and others that nourish the infrastructure for a healthy and vibrant community. This balanced approach has proved best—with a range of opportunities carefully crafted to get the best science from NASA's Science budget.

Recognizing the current challenging budget climate, in which federal non-security, discretionary spending is declining by about one percent, the current NASA budget for science is nonetheless cause for concern. The continuing resolution (CR), now Public Law 110–005, provided funding for many federal agencies including NASA for FY 2007. NASA Science has suffered a $78.8 million shortfall from the President's FY 2007 request. The President's FY 2008 budget request represents a 0.9 percent increase in NASA Science spending over the FY 2007 request; however, with inflation currently around two percent, the FY 2008 request still represents a decline in real dollars available for research in science compared to the President's FY 2007 request. A key question is what will become the new baseline for NASA Science funding, the FY 2007 request or the CR. If the CR is adopted as the new baseline, this could represent a loss to NASA Science in the outyears of $1 billion or more.

The AAS therefore recommends that Congress increase the FY 2008 budget for NASA Science by six percent over the CR level. This modest increase over the President's FY 2008 request will help maintain balance within the science portfolio, which is critical to our community. It is important to support small missions and research grants to individual investigators. Otherwise, many exciting programs to explore the solar system, to detect planets around other stars, to measure gravitational waves from astronomical events, to explore black holes in all their manifestations, and to seek the nature of the dark energy may be threatened. In particular, we advocate for restoring funding to the Explorer program and protecting the Beyond Einstein mission.

We further advocate that NASA Science should be part of the American innovation agenda. Maintaining and strengthening American innovation in science and technology has broad bipartisan support, both in Congress and the Administration. Our recommended increase of six percent in NASA Science is smaller than the increases proposed for the science component of other agencies identified as strategically important for innovation. These include an 8.7 percent increase for NSF, a 16 percent increase for Department of Energy's Office of Science, and nearly 21 percent for NIST (all increases over the CR levels). For AAS members, the cuts in NASA's support for science threaten to offset or overwhelm the increases that have been aimed at improving America's innovation through the NSF, DOE, and NIST. A real effort to improve science and engineering in the U.S. should treat NASA's science program as part of the solution. NASA's science missions inspire new generations of young people to pursue careers in science, engineering, and mathematics and train these students and young scientists to become the innovators of the future.

Finally, the AAS applauds the Administration and Congress for upholding the priorities of the NAS Decadal Survey in astronomy. We are pleased that the development of *JWST* and *HST* servicing mission are priorities in the new budget, but we stress that balance is critical in the Science portfolio.

NASA Science has been and continues to be a beacon of innovation and discovery by inspiring generations of young people, capturing the imagination of the public, developing new technologies, and discovering profound insights into the nature of our Universe.

The AAS and its members are prepared to work with Congress and with NASA to help find the best way forward. We will give you our best advice and we will work diligently to make the most of NASA's investment in science.

Chairman UDALL. I would like to acknowledge the presence of Eddie Bernice Johnson, Congresswoman Johnson. She is a Member of the Full Committee, and she is here today with us. Also, I would like to acknowledge, Dr. Stern, with your forbearance, an esteemed and highly accomplished American, Dr. John Mather, who is a winner of the Nobel Prize and the new NASA chief scientist, so Dr. Mather, we are honored to have you here as well.

At this time, I would like to introduce our panel of witnesses, and I will go across and introduce each one of you, and then we will come back and start with Dr. Stern.

As I mentioned earlier, we have a constituent, a friend of mine, Dr. Alan Stern, who in addition to serving as the principal investigator on NASA's New Horizons mission to Pluto, has now become the new Associate Administrator of NASA's science mission directorate.

Next to him is Dr. Lennard Fisk, who is the Thomas M. Donahue distinguished Professor of Space Science at the University of Michigan, currently serving as the Chairman of the National Research Council Space Studies Board.

Next to him is Dr. Garth Illingworth, who is a Professor of Astronomy and Astrophysics at the University of California, Santa Cruz, and is the Chair of the Astronomy and Astrophysics Advisory Committee.

Dr. Daniel Baker, who I mentioned earlier, is Professor of Astrophysical and Planetary Sciences and Director for the Laboratory for Atmospheric and Space Physics, fondly known as LASP, and it is located in my home district at the University of Colorado, Boulder.

Finally, we have Dr. Joseph Burns who is the Irving Porter Church Professor of Engineering and Professor of Astronomy, and currently serves as Vice Provost for Physical Sciences and Engineering at Cornell.

Again, welcome to all of you. We really are appreciative of you taking time today.

You will each, as I think you know, have five minutes for your opening remarks, and after which the Members of the Subcommittee or Members of the Full Committee, as it may be, will have five minutes to ask questions.

Dr. Stern, the floor is yours. Welcome.

STATEMENT OF DR. S. ALAN STERN, ASSOCIATE ADMINISTRATOR, NASA SCIENCE MISSION DIRECTORATE

Dr. STERN. Thank you. Good morning, Chairman Udall, Ranking Member Calvert, Congresswoman Johnson. I appreciate the opportunity to appear before you today.

I am excited and humbled by the task I assumed four weeks ago, leading NASA's Space Science Mission Directorate, the world's premiere space and Earth science effort, without doubt.

The President's budget for NASA fiscal year 2008 provides $5.4 billion for science in that year alone. This allows us to operate a fleet of 52 orbital and interplanetary missions while simultaneously developing another 41 new missions for launch over the next seven

years. That is an impressive total, 93 space missions in development or flight.

Within this budget, we also support a modest sub-orbital research program, and more than 3,000 scientist-led research projects across the entire spectrum of Earth and space sciences.

More complete details of NASA's fiscal year 2008 science budget request are included in my written testimony, but you asked me to comment on how I see my role at NASA, and I want to turn heads while I am here. I want to produce landmark scientific achievements and to make my Directorate and its various projects run more efficiently and stay within their cost boundaries. I see this as a requirement for my being an agent for change.

I want to highlight two examples of change that have already taken place since I began work at NASA four weeks ago. The first is the establishment of an Office of Chief Scientist, or OCS. This office will provide an independent technical analysis and advice regarding scientific matters within our portfolio, particularly on issues of prioritization within and between each of the four scientific disciplines. As I said, we opened this office on my first day at NASA, and it is a signal of our renewed commitment to scientific excellence. OCS is led by one of the most impressive and experienced space scientists in the United States, cosmologist and 2006 Nobel Laureate Dr. John Mather from NASA's Goddard Space Flight Center. John is with us here today, and he is ably supported by two deputies, one for Earth sciences and one for the space sciences.

I have also created a position called a senior advisor for research analysis within the leadership of the Directorate, and I have appointed Dr. Yvonne Pendleton to that position. She is also here just behind me. Yvonne? Dr. Pendleton was formerly the Space Science and Astrobiology Division Chief at NASA's Ames Research Center where she set an outstanding record about science program management and achievement. Dr. Pendleton is charged with guiding our research and analysis program and making recommendations for ways that we can both improve the processes and the content of our core research and analysis effort. This really is a core part of our entire science. Never before has NASA's scientific leadership included a position focused solely on improving our research and analysis programs.

Now, let me turn to the four specific questions you asked of me. The first question asked about my top goals, I have three, and they are first, to make stronger progress at all four of the Decadal surveys. Secondly, to get more from our existing and planned budget, and third, to help the Division for Space Exploration succeed.

Your second question concerning the top three management risks as I see them, these are the cost of launches to space for science missions, cost growth in science mission development, and the sometimes immature cost realism and the resulting unrealistic expectations that have been set by some recent Decadal surveys.

Your third question concerned how we will prioritize and balance our objectives across the portfolio. The answer is that we must balance with four considerations: science impact, affordability, development risk, and technological readiness. I have chartered Chief Scientist Mather to make a fair and deterministic process that

takes these four factors into account to balance our priorities within each portfolio element and between the four portfolios.

The final question concerns strategic investments that I would like to make. These will be in three areas: research and analysis, data analysis from space flight missions, and sub-orbital programs. To say a little bit more specifically, I want to make scientists more efficient and productive and increase the funding to research and analysis so we can better achieve our research objectives. Regarding data analysis, an increase in data analysis would provide the taxpayers and decision-makers like yourself with an enhanced value for the investments that we make in the missions to actually get the goods out at the other end, to the analysis and make the discoveries after the data is collected.

Finally, regarding sub-orbital programs, using rockets and high altitude balloons, I intend to provide opportunities to train space scientists in the art of space flight, to bridge the 2010 to 2012 desert in orbital and planetary mission launches, and to provide opportunities for technology development and demonstration through this sub-orbital program.

I will close now by thanking you again for inviting me, and I look forward to answering your questions and working with you in the future.

[The prepared statement of Dr. Stern follows:]

PREPARED STATEMENT OF S. ALAN STERN

Mr. Chairman and Members of the Subcommittee, thank you for the opportunity to appear today as the new Associate Administrator for NASA's Science Mission Directorate (SMD). The four weeks I have spent on the job at NASA Headquarters have been personally rewarding, and I look forward to continuing that experience in appearing before the Subcommittee today to discuss NASA's plans for the future of SMD's space—and Earth—science portfolio, as represented in the President's FY 2008 budget request for NASA, and to highlight my vision for this organization. I appreciate this opportunity to address your questions and concerns.

First, permit me to note that although my scientific background and expertise is in astrophysics and planetary science, I serve as the Associate Administrator for all four of our Earth and space science disciplines, and that I look forward to learning more about Earth Science and Heliophysics in order to further advance these important programs in SMD's science portfolio.

The President's *Vision for Space Exploration* calls upon NASA to conduct robotic and human exploration of the Moon, Mars and other destinations, to conduct robotic exploration across the solar system, and to conduct advanced telescope searches for Earth-like planets around other stars. Other Presidential directives and legislative mandates instruct NASA to conduct Earth observation and scientific research and to explore the origin and destiny of the universe. With enactment of the *NASA Authorization Act of 2005* (P.L. 109–155), the Congress provided a fresh legislative mandate for this charge, calling for a balanced program of science, exploration, and aeronautics.

I am committed to implementing this direction, and bringing to NASA and the Congress the best possible slate of programs and program success within the significant resources already available. This includes programs synergistic with NASA's Exploration Systems Mission Directorate and also research that both enables, and is enabled by, human exploration plans for the Moon and Mars. I am an enthusiastic advocate of human exploration and believe that a strong science program associated with this exploration is important to maximizing the benefits to the Nation of such human exploration.

Vision for SMD

Before I outline the recent scientific achievements of NASA's space science program and the President's request to further advance that program in FY 2008, I would like to share with the Subcommittee several guiding principles I am instilling in SMD, as well as an important change to the way matters of scientific prioritization are analyzed and debated within SMD.

Below are my three guiding principles for SMD, each is extremely important and of equal priority:

1. To make strong progress advancing the priorities of all four decadal surveys[1], for example by increasing our international collaboration efforts;
2. To get more from our existing and planned budgets, for example by better managing flight missions and by ensuring that data analysis from missions is sufficiently funded to "get the promised goods out;" and
3. To help the *Vision for Space Exploration* succeed, for example by fostering a lunar science community.

As stated above, I also have made an important change to the way matters of scientific prioritization are analyzed and debated within SMD. That change is both to our processes and to our senior leadership in SMD. On my first day with NASA, one month ago today, I established a new office, the Office of the Chief Scientist (OCS), reporting directly to me as the Associate Administrator for SMD. The primary function of this new office is to provide independent technical analysis and advice regarding scientific matters in the SMD portfolio. In particular, this includes issues of prioritization both within, and between, each of the four scientific disciplines in SMD's portfolio. Previously, no strong, formal, independent advice function was in place. To ensure the highest quality of advice, I asked cosmologist and Nobel Laureate Dr. John Mather to lead this effort as the SMD Chief Scientist, and he has accepted. John is ably supported by two deputy Chief Scientists, one for the Earth Sciences and one for the Space Sciences. I believe Dr. Mather and his team, coupled with the strong role they are chartered to play in mission prioritization, selection, and science management decisions, will produce increasing benefits as we go forward.

Scientific Achievements

Now I will turn to some of the recent scientific achievements of NASA's science program.

I am proud to be leading a world-class effort that consistently returns historic scientific results. This past year alone was truly remarkable for scientific discovery about our Earth, the Sun, our solar system, and the universe. This is exemplified in part by the fact that NASA alone was responsible for 11 percent of *Science News* magazine's top stories—covering all fields of science—for 2006; this is an all-time record in the 34 years that this metric has been tracked.

Important findings resulting from our program ranged from new observations of familiar phenomena like the ozone hole, hurricanes, and rainfall, to the discovery of lakes of organic hydrocarbons on Saturn's planet-sized moon Titan, to the identification of new classes of planetary abodes across our galaxy, to the study of the Sun's magnetic field, showing it to be more turbulent and dynamic than previously expected.

As these and other results about our world and the universe pour in, NASA also continues to develop and launch our next generation of missions, and to support a vigorous scientific community via research and data analysis funding. In total, I note, NASA currently is developing or flying a total of 93 space and Earth science missions—far more than all of the other space agencies of the world combined. NASA also supports over 3,000 separate space and Earth science research investigations in our Research and Analysis programs, spending approximately $600 million annually on scientific data analysis, modeling, and theory across the four disciplines of Earth and space science spanned by SMD.

I intend for SMD to continue to turn heads across the world by developing space missions and supporting scientific research that rewrites textbooks in all of our science disciplines.

At present, NASA is operating 52 space and Earth science missions and, simultaneously, developing 41 new flight missions. These new missions range from modest Principal Investigator-led efforts like the Interstellar Boundary Explorer (IBEX) currently planned for launch in 2008 and the Phoenix Mars lander about to launch this summer, to the flagship NASA space science missions like the James Webb Space Telescope (JWST) mission in development for launch in 2013.

In 2006, NASA launched four new science and technology demonstration missions: New Horizons, Solar Terrestrial Relations Observatories (STEREO), CloudSat, and

[1] The term "decadal survey" refers to a regular series of reports conducted by the National Research Council of the National Academies on behalf of NASA and its partner agencies. Each of SMD's science disciplines has its own decadal survey, representing community consensus in each field. These surveys assess proposed activities and recommend investment priorities over a ten-year timeframe.

Space Technology (ST)–5. We also partnered with other Federal and international agencies to launch five other science and technology missions: Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO), Two Wide-Angle Imaging Neutral-Atom Spectrometers (TWINS)–A, Hinode (Solar-B), ST–6, and the NOAA GOES–N satellite. Below Is more detail on this impressive list of newly launched missions.

In January 2006, NASA launched the New Horizons mission to the planet Pluto and the ancient Kuiper Belt in which it orbits. New Horizons, the fastest spacecraft ever launched, will begin its reconnaissance of these bodies eight years hence, in 2015, following a three billion-plus mile crossing of our planetary system. I am very proud to have been since its inception, and to continue to be, the Principal Investigator of this mission. Just 13 months after launch, this February, New Horizons flew by Jupiter, making important new observations of a wide variety of exotic phenomena in the Jupiter system, including, for example, the eruption of the gargantuan Tvashtar volcano on Jupiter's moon, Io.

Following on the launch of New Horizons with the April 2006 launch of the CloudSat and CALIPSO spacecraft, NASA added two important assets to the "A-train" of satellites flying in close proximity polar orbits around the Earth to gain a better understanding of key factors related to climate change.

NASA has also been very active this past year launching new heliophysics missions. The agency collaborated on the Japanese Aerospace Exploration Agency's new Hinode (Solar-B) mission, which was successfully launched in September 2006. Early results have already provided new insight on solar magnetic processes operating in the Sun's atmosphere.

Then in October 2006, NASA's twin STEREO spacecraft were launched to help researchers construct the first-ever three-dimensional views of the Sun's atmosphere. This new view will improve our abilities in space weather forecasting and greatly advance the ability of scientists to understand solar physics, which, in turn, enables us to better protect humans living and working in space.

Already this year, on February 17, we launched all five THEMIS (Time History of Events and Macroscale Interactions during Substorms) microsatellites on a single rocket to study the genesis of Earth's aurora. On April 25, the Aeronomy of Ice in the Mesosphere (AIM) mission was launched to study ice clouds in the polar regions of Earth's upper atmosphere. We also remain on track to launch both the Dawn mission to explore fascinating and important Ceres and Vesta in the main belt of asteroids between Mars and Jupiter, and also the Phoenix Mars lander by late this summer.

From across the solar system, NASA's spacecraft have provided startling new insights into the formation and evolution of the planets. Images from the Mars Global Surveyor have revealed recent deposits in gullies on Mars, evidence that suggests water may have flowed in these locations within the last several years. The Mars Reconnaissance Orbiter, which began its primary science phase in November 2006, has not only taken extraordinary high resolution images of Mars at resolutions greater than any other mission to-date, but has taken incredible images of Opportunity and Spirit on the surface, and helped the Phoenix lander find a safe landing area. From its orbit around Saturn, the Cassini spacecraft recently found unexpected evidence of liquid water geysers erupting from near-surface water reservoirs on Saturn's moon, Enceladus.

Additionally, the Wilkinson Microwave Anisotropy Probe (WMAP) Explorer mission was able to gather new information about the first second after the universe formed, while the Chandra X-ray Observatory provided new and strong evidence of dark matter, and the Hubble Space Telescope identified 16 candidate planets orbiting other stars near the center of our galaxy.

In late October 2006, NASA Administrator Mike Griffin announced plans for a fifth and final Space Shuttle servicing mission to the Hubble Space Telescope (HST) to extend and dramatically improve its capabilities for the future. The repaired and revitalized HST will boast two new major scientific instruments with capabilities that will make it 10 times more powerful than the HST we have today.

In Earth Science, researchers are using Tropical Rainfall Measuring Mission (TRMM) data to provide a complete picture of low-latitude precipitation and storms around the entire world; in 2006, researchers used eight years of continuous data from the TRMM lightning Imaging Sensor to identify the regions on Earth that typically experience the most intense thunderstorms.

Using instruments flying closer to Earth, NASA investigators flew 29 separate scientific instruments to 60,000 foot altitudes aboard NASA's WB–57F Canberra aircraft in the Costa Rica Aura Validation Experiment (CAVE). These airborne measurements, coupled with measurements from the orbiting Aura spacecraft, shed light on how ozone-destroying chemicals get into the stratosphere over the tropics and

how high-altitude clouds affect the flow of water vapor—a powerful greenhouse gas—in this critical region of the atmosphere.

Additionally, scientists using nearly a decade of global ocean satellite data were able to demonstrate a strong relationship between warming climate and a decline in the microscopic marine plant life (phytoplankton) at the base of the marine ecosystem.

Examples of important successes in our data analysis programs are also diverse. Astronomers combining data from the Hubble Space Telescope with data from ground-based and other space-based telescopes have created the first three-dimensional map of the large-scale distribution of dark matter in the universe. NASA researchers also found organic materials that formed in the most distant regions of the early solar system preserved in a unique meteorite that fell over Canada in 2000. And, using a network of small automated telescopes, astronomers have discovered a planet orbiting in a binary star system, showing that planet formation very likely occurs in most star systems. In our home solar system, scientists predicted that the next solar activity cycle will be 30–50 percent stronger than the previous one and up to a year late. Accurately predicting the Sun's cycles will help plan for the effects of solar storms and help protect future astronauts. And a breakthrough "solar climate" forecast was made with a combination of computer simulation and groundbreaking observations of the solar interior from space using the NASA/ESA Solar and Heliospheric Observatory (SOHO).

The list of achievements resulting from NASA's space and Earth science portfolio is much longer than these examples alone. I am excited to tell you that lack of time here today rather than lack of results, causes me to have to move on from this topic to discuss the President's FY 2008 budget request for space science.

Highlights of the Science Mission Directorate's FY 2008 Budget Request

NASA's FY 2008 budget request for the Agency's science portfolio is $5.5 billion. This represents an increase of $49.3 million (or one percent) over the FY 2007 request, adjusted for NASA's new, simplified full cost accounting system. It will enable NASA to launch or partner on 10 new missions, operate and provide ground support for more than 50 spacecraft, and fund a wide array of scientific research related to and based on the data returned from these missions.

The Planetary Science budget request of $1.4 billion will advance scientific knowledge of the solar system, search for evidence of extraterrestrial life, and prepare for human exploration of the Moon and Mars. NASA will get an early start on Lunar science when the Discovery Program's Moon Mineralogy Mapper (M3) launches aboard India's Chandrayaan-1 mission in March 2008. Also aboard this mission will be Mini-RF, a technology demonstration payload, supported by NASA's Exploration Systems and Space Operations Mission Directorates which may glean evidence for water in the Moon's polar regions. In support of expanded opportunities for pursuing lunar science, the President's request includes $351 million from FY 2008–2012 for a Lunar Science Research budget line within the Planetary Science Division. The Science Mission Directorate is already hard at work creating synergy with the programs of the Exploration Systems Mission Directorate. After the Lunar Reconnaissance Orbiter completes its prime mission for the Exploration Systems Mission Directorate, the Science Mission Directorate plans to fund extended mission operations through this budget line in order to maximize scientific return from the spacecraft. In addition, the new Lunar Science Research Initiative includes Missions of Opportunity, technology development, data archiving, and expanded basic lunar research. The Discovery and New Frontiers programs also provide opportunities for the science community to propose missions to accomplish lunar science investigations, and one such mission is under study. We have tasked the National Research Council (NRC) to conduct a study on the scientific context for the exploration of the Moon. Their preliminary report is in hand, and their final report is due this summer. That report will help us mature our lunar science plans in the months ahead. We have also begun similar coordinating steps for Mars, where SMD already has a mature and robust program of scientific exploration.

The FY 2008 budget also supports the Mars Exploration Program by operating five spacecraft at Mars, flying the Phoenix lander, scheduled for launch in August 2007, and continuing to develop the Mars Science Laboratory for a launch scheduled in 2009. The Discovery Program's Dawn Mission dual asteroid orbiter will be operating en route to the asteroid belt, and the Mercury Surface, Space Environment, Geochemistry and Ranging (MESSENGER) spacecraft will make its first flyby of Mercury. Last year, three Discovery mission proposals and three Discovery Missions of Opportunity were selected for Phase A studies which will culminate late this year in new mission and instrument selections. The Discovery Program plans to again invite proposals for additional new missions in 2008. Additionally, the New Fron-

tiers Program's Juno Mission will undergo both a Preliminary Design Review and a Non-Advocate Review in FY 2008 in preparation for entering development towards a 2011 launch to study Jupiter's interior, aurora, and magnetosphere. Like Discovery, the New Frontiers Program plans to release a new Announcement of Opportunity (AO) in 2008.

The Heliophysics budget request of $1.1 billion will support 14 operational missions and six missions in development to better characterize and understand the Sun and its effects on Earth, the solar system, and space environmental conditions that will be experienced by astronauts, and to demonstrate technologies that can improve future operational systems. Additionally, during FY 2008, the Explorer Program will launch both the Interstellar Boundary Explorer (IBEX) mission, focused on the detection of the very edge of our solar system's heliosphere and the Coupled Ion-Neutral Dynamics Investigation (CINDI) Mission of Opportunity. The Solar Dynamics Observatory (SDO) to study the Sun's magnetic field is also scheduled for launch in late 2008 or early 2009. The Geospace Radiation Belt Storm Probes (RBSP) mission, presently in formulation, will undergo a Preliminary Design Review and a Non-Advocate Review in FY 2008 in preparation for entering development in early FY 2009. RBSP will improve the understanding of how solar storms interact with Earth's Van Allen radiation belts. We remain on track to release the next Explorer Announcement of Opportunity in very early FY 2008 and we hope to select three new astrophysics and heliophysics missions, as well as one or more Missions of Opportunity, as a result of that call for proposals.

The Astrophysics budget request of $1.6 billion will support continued operation and data analysis from NASA's orbital astronomical observatories, including the Hubble Space Telescope (HST), Chandra X-Ray Observatory, and the Spitzer Space Telescope, and to build more powerful instruments to peer deeper into the cosmos. HST is scheduled for a final servicing mission in August 2008 using the Space Shuttle. Along with repairs and service life extension efforts, two new instruments will be installed during the servicing mission that will dramatically extend HST's performance and enable further discoveries, including Wide Field Camera 3 (WFC3), which will re-enable some science observations that have been affected by the recent failure of the Advanced Camera for Surveys. After the servicing mission, HST is planned to have six fully operational instruments (including a suite of cameras and spectrographs that will have about 10 times the capability of older instruments) as well as other new hardware planned to support another five years of world-class space science. Additionally, the Gamma-ray Large Area Space Telescope (GLAST) will launch in FY 2008 to begin a five-year mission mapping the gamma-ray sky and investigating gamma-ray bursts, and the Kepler mission development will be near completion in preparation for launch in FY 2009, to determine the frequency of Earth-like planets. Further, the James Webb Space Telescope astrophysics flagship mission will undergo its Preliminary Design Review and a Non-Advocate Review in FY 2008, in preparation for entering hardware development.

As the Subcommittee is aware, the SOFIA airborne observatory, which we have been developing with the German Aerospace Research Center (DLR) has been reinstated. I am pleased to report that SOFIA had its first functional check-out flight last week; it is scheduled to undergo an ambitious program of flight testing that begins this year and will continue in 2008. Though we know of no technical showstoppers in regard to the airworthiness of the aircraft or operation of the telescope, this program has some remaining hurdles to overcome and so remains subject to a careful management review later this spring chaired by the NASA Associate Administrator. The SOFIA program baseline will be finalized at that time.

Also in our Astrophysics program, ESA's Herschel and Planck missions are planned for launch in FY 2008; both of these missions include important contributions and scientific participation from NASA.

While the focus of this hearing is on space science, I would also like to briefly address the FY 2008 President's Budget request for Earth Science. The Earth Science budget request is $1.5 billion, an increase of $27.7 million over the FY 2007 request, to better understand the Earth's atmosphere, lithosphere, hydrosphere, cryosphere, and biosphere as a single connected system. This request includes additional funding for the Global Precipitation Measurement (GPM) mission in response to the high priority placed on GPM in the National Research Council (NRC) Decadal Survey. As the follow-on to the highly successful Tropical Rainfall Measuring Mission, GPM's Core satellite is planned for launch no later than 2013, followed by a Constellation spacecraft the following year. Other satellites in the GPM constellation will be provided by NASA's international partners or domestic operational partners. The Earth Science budget also includes increased funding for the Landsat Data Continuity Mission and for the Glory mission, and provides funds for the National Polar-orbiting Operational Environmental Satellite System (NPOESS) Pre-

paratory Project (NPP) to reflect instrument availability and launch delays. Funds are requested for continued development and implementation of the Ocean Surface Topography Mission to launch in 2008, the Aquarius mission to measure the ocean's surface salinity to launch in 2009, and the Orbiting Carbon Observatory mission planned for launch in 2008. NASA will continue to be the largest contributor to the Administration's Climate Change Science Program by collecting global data sets and improving predictive capabilities that will enable advanced assessments of the nature, causes, and consequences of global climate change. Over the coming months, NASA will evaluate strategies for implementing the recommendations of the National Research Council's Earth Science Decadal Survey and responding to challenges to the continuity of climate measurements resulting from the Nunn-McCurdy recertification of the NPOESS program. By working together, NASA and NOAA have already been able to initiate the restoration of one of the de-manifested sensors to (the Ozone Mapping and Profiling Suite limb instrument) to the NPP satellite, which will help continue the record of high vertical resolution ozone profile measurements into the next decade. I am personally committed to continuing and continually improving the working relationship between NASA and NOAA, and met with NOAA executives on my first week in office to transmit this message.

Looking Forward

With that overview of the FY 2008 budget request as a backdrop, I turn now to addressing the specific questions raised in the letter of invitation to this hearing. The Subcommittee's first question concerns my goals for SMD over the next five years.

I view my role as the Associate Administrator for SMD as being an agent for change, to make SMD work better and more efficiently, and to turn heads by producing landmark scientific accomplishments. With that in mind, as outlined earlier in my testimony, I have three goals for the organization that I want to share with you today. The first is to make strong progress advancing all four decadal surveys, which we will attack as vigorously as possible, for example by increasing our international collaboration efforts. The second is to get more science accomplished from our budget. I believe that by looking for ways to increase efficiency within our organization, and within the way we manage missions, we can make new funding available within the President's budget that will enable us to do significantly more. My third objective is to help ensure that the *Vision for Space Exploration* is successful by increasing the scientific yield it will produce. There are many ways that SMD and the scientific community will help support the Vision, such as through a robust lunar science research program. By providing increased opportunities to conduct lunar science, I believe that we can grow a strong lunar community, just as the Mars community increased once regular flight opportunities were made available in the mid-1990s.

The Subcommittee's second question concerns SMD's top three programmatic risks. The first is the rising cost of launches to space. The Delta-II launch vehicle has been the reliable workhorse for launching science missions to Earth orbit or in the inner planets neighborhood across SMD disciplines. However, the supplier of that launcher is getting out of the Delta-II business in favor of larger and more expensive Evolved Expendable Launch Vehicles (EELVs). NASA's Space Operations Mission Directorate (SOMD) acquires launch services for SMD, and we are working with SOMD on their assessment of options for the future. These options include: design of the future medium-class mission set to fit either larger or smaller ELVs; planning to co-manifest more missions to optimize the use of larger ELVs, and working with SOMD to qualify new and as yet unproven alternate launch vehicles to be offered by new entrants into the market. A second risk area is cost and schedule growth as SMD pursues its challenging flight missions. At both the Agency and SMD level, we are putting in place better cost-estimating tools and capturing lessons learned from recent missions. We are also carefully examining the readiness of new technologies before we confirm missions that use them, and we are introducing new experience-based standards for the selection of Principal Investigators. This ties into the third risk, which is uncertainty in mission development risk. SMD will work harder to understand and reduce risks, rather than waiting for problems to appear when missions are deep in development when cost impacts are most severe. I note that these kinds of emphasis on good management will be key to getting more from our budget so that future missions are not delayed or canceled to pay for problems on existing mission developments.

The Subcommittee's third question concerns prioritization and balance. NASA's approach to setting the balance of investment among science areas is based on the following considerations: science value, mission affordability, mission risk, and mission readiness. The SMD makes a commitment to progress on each of the four SMD-

assigned science objectives in the 2006 NASA Strategic Plan and each of the four decadal surveys produced for us by the National Academy. Long-term outcomes are science-based, not solely mission-based; thus sub-orbital and research and analysis (R&A) programs are also part of this. We assess progress against community roadmaps laid out for each science area. The pace of progress can be influenced by ties to other NASA and Federal programs, e.g., the U.S. Climate Change Science Program and NPOESS in the case of Earth Science, and human exploration time lines in the case of the Mars Exploration Program. Many science objectives can be accomplished using a mix of small, medium and large missions, international collaboration, and innovative missions of opportunity; others require large missions that are more difficult to initiate. NASA begins in each science area with the priorities defined in decadal surveys of the NRC, then generally sponsors science community-led teams to develop 'roadmaps' to plan implementation of survey research and mission priorities. We then pass these through the filter of budget availability to set final priorities that are affordable and at an appropriate stage of technological readiness and risk reduction. Within each science area, the challenge is to find the proper balance among large, medium and small missions, research and analysis in all its forms, data analysis, and technology development. At the Directorate level, as I previously highlighted, I have charted an Office of the Chief Scientist and appointed Dr. John Mather to lead that office in making recommendations for the best way to balance priorities with in and among each of our four portfolio areas.

The Subcommittee's fourth question concerns strategic investments in space and Earth science I would like to make as the Associate Administrator. I must preface by noting that my analysis of the SMD portfolio is not yet complete and that there are many areas that likely warrant attention or refocus; I address a few here. I believe that, within the SMD five-year budget profile put forward in the President's FY 2008 request, SMD can make modest investments in three key areas that will yield profound and lasting improvements to our bottom line that will increase in our understanding of the Earth, the Sun, the solar system, and the Universe. The first area in which I would invest is Research and Analysis (R&A). This investment would, in part, focus on process improvements to make scientists more efficient and productive; it would also seek new research funding initiatives offered to members of the scientific community. I have appointed a Senior Advisor for R&A, Dr. Yvonne Pendleton, to oversee SMD's efforts in this area and to make recommendations for ways we can improve R&A processes and program content. Dr. Pendleton will work closely with Dr. Mather and the office of Chief Scientist in this regard. The second investment I hope to make is in mission data analysis, so that the taxpayer gets the best value for the investment we make in science missions. Too often, data analysis efforts are curtailed as a result of rising mission development and operations costs. This problem will be addressed beginning this year. The third area in which I would invest is our Sub-orbital programs. Sub-orbital flight using rockets and balloons, as well as aircraft, provide opportunities to train new space scientists in the art of space flight, to bridge the 2010 to 2012 gap in orbital and planetary mission launches, and to produce some exciting science as well. I would also like to see sub-orbital opportunities expanded. Again, I believe it is possible to make progress within the SMD five-year budget profile put forward in the FY 2008 President's request.

Conclusion

In summary, let me say that the President's FY 2008 budget request funds an exciting, productive, and balanced portfolio of Space and Earth science missions, and presents a program that will yield even better results than formerly anticipated though increased efficiencies. This exciting program of research is described in the Science Plan for NASA's Science Mission Directorate (2007–2016), recently submitted to this Subcommittee as directed in the *NASA Authorization Act of 2005* (P.L. 109–155). I look forward to working with this Subcommittee to implement this Plan, as well as my plans to help shape SMD for the years to come. I would be happy to respond to any questions the Subcommittee may have regarding SMD, SMD's portfolio, and the exciting scientific results NASA Is achieving.

BIOGRAPHY FOR S. ALAN STERN

Dr. S. Alan Stern is the Associate Administrator for NASA's Science Mission Directorate.

He directs a wide variety of research and scientific exploration programs for Earth studies, space weather, the solar system and the universe beyond. In addition, he manages a broad spectrum of grant-based research programs and spacecraft projects to study Earth and the universe.

Stern is a planetary scientist and an author who has published more than 175 technical papers and 40 popular articles. His research has focused on studies of our solar system's Kuiper belt and Oort cloud, comets, satellites of the outer planets, Pluto and the search for evidence of solar systems around other stars. He has worked on spacecraft rendezvous theory, terrestrial polar mesospheric clouds, galactic astrophysics and studies of tenuous satellite atmospheres, including the atmosphere of the Moon.

Stern has had a long association with NASA, serving on the NASA Advisory Council and as the principal investigator on a number of planetary and lunar missions, including the New Horizons Pluto-Kuiper Belt mission. He was the principal investigator of the Southwest Ultraviolet Imaging System, which flew on two Space Shuttle missions, STS85 in 1997 and STS–93 in 1999.

He has been a guest observer on numerous NASA satellite observatories, including the International Ultraviolet Explorer, the Hubble Space Telescope, the International Infrared Observer and the Extreme Ultraviolet Observer.

Stern joined NASA in April 2007 from the Southwest Research Institute's Space Science and Engineering Division, Boulder, Colo., where he had served as Executive Director of the Space Science and Engineering Division.

He holds Bachelor's degrees in physics and astronomy and Master's degrees in aerospace engineering and planetary atmospheres from the University of Texas, Austin. In 1989, Stern earned a doctorate in astrophysics and planetary science from the University of Colorado at Boulder.

He is an instrument-rated commercial pilot and flight instructor, with both powered and sailplane ratings. Stern and his wife have three children.

Chairman UDALL. Thank you, Dr. Stern.

Dr. Fisk.

STATEMENT OF DR. LENNARD A. FISK, CHAIR, SPACE STUDIES BOARD, NATIONAL RESEARCH COUNCIL

Dr. FISK. Thank you very much, Mr. Chairman and Members of the Subcommittee. Thank you for inviting me here to testify.

I was asked to testify on the top three goals for NASA's Space Science Mission Directorate, SMD, top three programmatic risks, the top three investments that should be made, and also to comment on the balance among the various science themes within SMD.

As you well know, within the last few years, there has been dramatic changes in the funding that has been provided to SMD. Some $3 billion to $4 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, as was noted in opening statements, there is no way to remove that much money from the budget without causing disruptions in ongoing programs and distortion to the balance among programs, and this is the context in which these strategic goals and risks and investments required for SMD should be evaluated.

The first strategic goal for SMD might be stated get back the money that was lost. A more constructive way to make that statement would be to note, as again was made in the opening statements, how inadequate NASA as an agency is currently funded. It 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 should all make it a strategic goal to provide NASA with the funding that is required.

The risk to SMD for inadequate funding is simply that it can't perform its assigned tasks. The charge to SMD is to explore the universe, lay down the foundation of knowledge required for the human expansion into space. It is to determine the future of the

Earth so that we can make sound policy decisions, and it is to contribute to the capability of the United States to compete in the world, whether it is through knowledge, new technology or new workforce. The funding for SMD is currently inadequate to perform these tasks.

The investment required is the same investment the Nation is prepared to make in the American Competitiveness Initiative. It is difficult, in fact, it is impossible in my judgment to distinguish between the fundamental science being conducted by the NSF and the DOE Office of Science, and the fundamental science that is being conducted by SMD. Those agencies, of course, saw increases—major increases in support through ACI.

The first strategic goal—excuse me, the second strategic goal for SMD is 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, particularly—and I would like to focus on the moderate and small flight missions. The cost of launch vehicles has increased, the cost of management oversight has increased. Whatever the reason, it should be a strategic goal to get the maximum science for the minimum funding. There will be investments required to achieve this goal, whether it is in new launch vehicles, new technology, or new management practices.

Finally, if the funds for SMD can be provided, if the missions can be executed more cost effectively, or preferably both, the third strategic goal should be to use these funds to rebalance the program. When the funding for the out years in 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 of human capital, the research and analysis program, the sounding rocket program, small flight missions, they are the ones that were seriously disrupted. The portfolio of activities in SMD needs to be rebalanced so that we can compete—complete what we have begun while at the same time recognizing that the investments that we need make now, whether it is in people or it is in technology in the planning of future flight missions, will determine the vibrancy and the success of the scientific exploration utilization of space in the decades ahead.

The final question that was asked was the balance among science disciplines at SMD, and I included all four in answering these questions, astrophysics, planetary science, heliophysics, and Earth science. Each has an important task to perform and each has need of more funding, more cost effective use of its funding, a rebalance program, and the investments required to achieve these goals as we talk.

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 in Earth science in NASA to accomplish the goals that have been assigned to it, which is to use the global vantage point of space to provide information on the immediate future of Earth.

This is not a rebalancing question. It is 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 to pursue a vigorous Earth science program.

Thank you.

[The prepared statement of Dr. Fisk follows:]

PREPARED STATEMENT OF LENNARD A. FISK

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 FY 2005, 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 runout 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 canceled. 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 seven launches per year for missions in space and Earth science. In the last few years, the rate is more like five per year. In 2010–2012, the rate is projected to be under two 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 one percent 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 FY 2006 budget level in the enacted FY 2007 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 & 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 FY 2000. 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 FY 2000, 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.

BIOGRAPHY FOR LENNARD A. FISK

Lennard A. Fisk is the Thomas M. Donahue Distinguished University Professor of Space Science at the University of Michigan, where from 1993–2003 he was Chair of the Department of Atmospheric, Oceanic, and Space Sciences. Prior to joining the University in July 1993, Dr. Fisk was the Associate Administrator for Space Science and Applications of the National Aeronautics and Space Administration. In this position he was responsible for the planning and direction of all NASA programs concerned with space science and applications and for the institutional management of the Goddard Space Flight Center in Greenbelt, Maryland and the Jet Propulsion Laboratory in Pasadena, California.

Prior to becoming Associate Administrator in April 1987, Dr. Fisk served as Vice President for Research and Financial Affairs and Professor of Physics at the University of New Hampshire. In his administrative position, he was responsible for overseeing the University's research activities and was the chief financial officer of the University. Dr. Fisk joined the faculty of the Department of Physics at the University of New Hampshire in 1977, and founded the Solar-Terrestrial Theory Group in 1980. He was an astrophysicist at the NASA Goddard Space Flight Center from 1971 to 1977, and a National Academy of Sciences Postdoctoral Research Fellow at Goddard from 1969 to 1971.

Dr. Fisk is the author of more than 185 publications on energetic particle and plasma phenomena in space. He is a Member of the National Academy of Sciences (NAS) and the International Academy of Astronautics (IAA); he is a Foreign Member of Academia Europaea and a Fellow of the American Geophysical Union. He currently serves as Chair of the NAS Space Studies Board; he is a co-founder of the Michigan Aerospace Corporation and a Director of the Orbital Sciences Corporation. He is the recipient of the NASA Distinguished Service Medal in 1992, the AIAA Space Science Award in 1994, and the IAA Basic Science Award in 1997.

He is a graduate of Cornell University. In 1969, he received his doctorate degree in Applied Physics from the University of California, San Diego.

Chairman UDALL. Thank you, Dr. Fisk.

Dr. Illingworth.

STATEMENT OF DR. GARTH D. ILLINGWORTH, CHAIR, ASTRONOMY AND ASTROPHYSICS ADVISORY COMMITTEE (AAAC)

Dr. ILLINGWORTH. Thank you, Chairman Udall, Ranking Member Calvert, Members of the Committee and Subcommittee. Thank you for the opportunity to testify today on NASA's astrophysics program.

I am the Chair of the Congressionally chartered committee, the Astronomy and Astrophysics Advisory Committee. This committee was established to assess and make recommendations regarding the coordination of the astronomy and astrophysics programs of NSF, NASA, and DOE, and to assess progress on the National Academy's Decadal Survey on Astronomy and Astrophysics. While we deal extensively with all three agencies, NASA has been a particular focus of our attention recently because of the contrast with NSF and DOE science. The decreasing budget in real terms for NASA science contrasts very substantially with ACI led growth in the other agencies, and this is very unfortunate, given that NASA's science missions play such a central role in scientific advances in the last two decades. NASA missions have dramatically changed our understanding of the universe, of our own solar system, and of our planet Earth. And so, NASA's science program has been an extraordinarily successful enterprise.

As we look at the suite of missions that are now available to the science community, as Alan emphasized, we have a wide array of capabilities. For astrophysics, Hubble, Chandra, Spitzer are all returning remarkable data, while several medium-sized missions, GLAST, Kepler, WISE, will be launched over the next few years.

Yet, this leadership in the scientific and technological arena with the visibility that it brings to our nation's technological and scientific achievements is clearly at risk in the coming years. The rate at which new missions are being launched drops dramatically in 2009, and continues at a low level for many years well into the foreseeable future.

Furthermore, during the first part of the decade, the number of operating astrophysics missions, of course, will decease, such as current missions near the end of their life. The three great observatories will be replaced by one. JWST, of course, will be a remarkably powerful observatory, but it cannot encompass the full breadth of science areas that three great observatories do now, Chandra, Hubble, and Spitzer.

SOFIA will become operational, and a possible small Explorer. These are the only new capabilities in the first part of the next decade. Furthermore, the decline in the astrophysics budget in real terms, by 25 percent starting 2009 and throughout for several years after that, greatly reduces the opportunities for new missions following the next Decadal Survey report, which will be released in 2010.

Though NASA has had extraordinary successes over its last decade from its challenging, ambitious science missions, it produced stunning science return. In 10 to 15 years, as we stand and look back, will we be able to make the same statement? I am concerned that we are on a track that will make it very difficult and will maybe preclude such a positive outlook at that time.

The next question is how do we recover from this? More resources are clearly needed, but I would like to emphasize it is my view that it is neither wise nor productive to expect that they will come from NASA's human space flight program. No discussion of the budget challenges of the science of NASA can take place without acknowledging the challenges that face the Agency overall. It has become widely recognized that NASA is significantly underfunded to the mandate that it has been given to implement the *Vision for Space Exploration.* The lack of growth in the NASA budget is stressing all of the Agency's activities.

AAAC is deeply concerned about the growing impact on the space and Earth science program, and strongly endorses efforts to increase NASA's budget to allow it to undertake the transformation and vision without imparting serious damage to the science program.

The issues faced by NASA are so challenging that they really require broad consensus between the Administration and Congress on the Nation's goals for its space endeavors. I hope that some form of higher level discussion forum such as recently been proposed both in the House and the Senate does come to fruition and provides clear guidance for NASA and enhancement of its budgetary framework.

I would like to comment, though, that in all conscience I cannot ask for additional resources for the science programs without commenting on the undercosting that has occurred over the last decades, in fact, in our programs. The cost growth in missions both moderate and large has been substantial and clearly indicates the need for better cost estimates for each of the project phases by both NASA and the Decadal Survey. We need to work together on this, and the need to use life cycle costs for planning and roadmapping instead of just construction costs.

What counts, of course, is what we are going to be spending on a mission over the 10- to 20-year lifetime of that program, and not just what it takes to build a mission. When mentioning cost growth, JWST is the immediate program that comes to mind, but SIM and SOFIA are comparable examples. All of these programs have suffered huge growth with their budget over the numbers that were given in our Decadal Surveys. SIM and SOFIA were both $250 million missions in 1990. Both are now $3 billion programs. JWST went from $1 billion to $4.5 billion. But this is not new. Chandra, the current mission we are flying in x-ray astronomy, was a $500 million mission in 1980 and when re-costed in current dollars, it is $3.4 billion. So we clearly do need to understand much more carefully and fully the programs that we are putting forward.

So it is really clear that we need to develop reliable and robust life cycle cost estimates. I think it is to the credit of both NASA and the community that there is recognition of this and much more open discussion of these issues, and it is my view that we will do better, but it will take significant effort.

I would just like to note on a couple of the questions that came from the Chairman, since I am running out of time on this, was that I would like to note the three risks that were mentioned in the question, and they, in my view, are the lack of small and medium missions beyond 2009; the inability to respond to the 2010

Decadal Survey, I think we will do a very serious effort of putting forward an incredibly vibrant science program that is much more realistically costed, but it would be tragic if we, in fact, were not in a position to respond to that; and the current lack of technology development and mission development funding is a serious concern as well, because this obviously impacts mission cost and readiness. And the three strategic investments that I would make would be R&A funding—I think there is unity on the importance of this across the community and probably amongst the speakers here; technology development for missions; and the importance of competed cost-capped missions at the small and medium level as well.

So in closing, I would like to emphasize the remarkable productivity of the current program, but the dramatic changes—and not for the better, unfortunately, lie ahead if we continue with the budget of this declining substantially in real terms.

Thank you again for the opportunity to testify. I am happy to respond further.

[The prepared statement of Dr. Illingworth follows:]

PREPARED STATEMENT OF GARTH D. ILLINGWORTH

Mr. Chairman, Members of the Subcommittee, thank you for inviting me to testify. I am a Professor and Astronomer at the University of California, Santa Cruz and the University of California Observatories/Lick Observatory. I am the Chair of the Congressionally-chartered FACA committee, the Astronomy and Astrophysics Advisory Committee (AAAC). This committee was established to assess and make recommendations regarding the coordination of astronomy and astrophysics programs of NSF, NASA and DOE and progress on the National Academy National Research Council's (NRC) Astronomy and Astrophysics Decadal reports. As required by the enabling legislation, the AAAC generates an Annual Report in March of each year (the 2007 AAAC report is at *http://www.nsf.gov/mps/ast/aaac.jsp*). As Chair of the AAAC, the recommendations of that committee underpin this testimony.

In addition to responding to the questions from the Chairman, I would also like to highlight some issues that were a concern of the AAAC and will increasingly impact science at NASA unless rectified. Arguably science is the crown jewel of NASA. The science missions give NASA great return through their frequent and exciting results that capture the imagination of the public. They are equally a frequent demonstration of our nation's scientific and technical capabilities. However, that jewel is becoming tarnished by the effective reductions in the NASA Science Mission Directorate (SMD) budget.

SCIENCE AT NASA AND THE CURRENT NASA BUDGET PROJECTIONS

Science at NASA: NASA's science program has been an extraordinarily successful enterprise. The scientific productivity of its diverse suite of science missions has made many of them household names. Missions like the Hubble Space Telescope (HST), the Mars Rovers, the very successful Explorer missions like the Wilkinson Microwave Anisotropy Probe (WMAP), the remarkable outer planet images in our Solar System from Cassini-Huygens and Galileo, along with numerous other remarkable missions and projects, are a demonstration of U.S. technological leadership. NASA has shown time and time again that novel technology, driven by great science goals, can dramatically expand our horizons and bring exploration of the cosmos beyond our Earth within the reach of all. NASA's missions have dramatically changed our understanding of the universe—its origin, evolution and structure, the existence of massive black holes, when and how galaxies formed, and the birthplaces of star and planets—our solar system, and our home planet Earth. The value of these science missions is widely recognized for generating enthusiasm for science and engineering and for stimulating the interest of the Nation's youth.

Yet this leadership in the scientific and technological arena—with the visibility that it brings to U.S. technological and scientific achievements—is clearly at risk in the coming years. The breadth and balance within NASA's science program is a major factor in this visibility. The substantial budget changes envisaged for the coming five years are already having a major impact on the future science program. The resulting major restructuring of the long-term science program is a great concern to the science community and will, over time, significantly change NASA's per-

ceived value to the Nation. NASA has had extraordinary successes over the last decade from its challenging, ambitious science missions, combined with continuing, broadly-based research support that produces stunning science return from a diverse portfolio of programs. In ten years as we look back, will we be able to make the same statement? There will be highlights, but will we feel that NASA's science program has had its golden era? I feel very strongly that we all do not want that to be the case, but if we are to explore our universe and our Earth through the unique capabilities that NASA brings, then we must step up to the plate and commit the resources needed.

The problems that are visible in SMD flow not just from NASA trying to implement the *Vision for Space Exploration,* but also from the recovery from the loss of *Columbia* and major impacts such as Katrina. Science at NASA suffered a major hit when ~$3B was removed from SMD in the FY07 five-year projected budget request. The SMD budget is now down seven percent in inflation-adjusted FY06 dollars by 2012 in the FY08 request, instead of growing as in the FY06 request. The reduced SMD budget stems from the overall problems of the NASA budget and it's disconnect with its current mandate. This is discussed further below, after the discussion of the role of NASA science in the American Competitiveness Initiative (ACI).

Innovation, Competitiveness, ACI and NASA: Research is essential to innovative activities and underpins a technologically-competitive society, as enunciated in the NRC report, *Rising Above the Gathering Storm.* The inclusion of ACI increases in the FY07 budget request for NSF, DOE Science and NIST was a very strong response to the challenges faced by the Nation in staying at the forefront of scientific and technological development. The continuation of the ACI in the FY08 budget request demonstrated the Administration's commitment to building a robust R&D base in the physical sciences. Congressional support for NSF research and DOE science in the FY07 appropriation through the Joint Funding Resolution was a further key step in strengthening science and technology through the Congressional Innovation and Competitiveness effort. However, the exclusion of NASA science from the ACI contrasts with the inclusion of DOE science. There is no question that NASA is at the cutting-edge of science and technology research. This exciting and highly visible research contributes to the vitality of the national skill set and has encouraged young people to move into science and engineering. The Congressional interest in Innovation and Competitiveness enables a fresh opportunity for enhancing NASA science. The AAAC in its Annual Report strongly encourages Congress to consider enhancing the support for science at NASA explicitly to improve innovation and competitiveness, as has been done for NSF and DOE science.

Funding for NASA for the Vision for Exploration: Before discussing the science program further I would like to comment on the overall context in which the NASA science budget is developed. It has become widely recognized that NASA is significantly underfunded for the mandate that it has been given to implement the Vision for Space Exploration. No discussion of the budget challenges for science at NASA can take place without acknowledgement of the challenges that face the Agency overall. The challenges of transitioning within the current NASA budget to a new generation of space capabilities in the framework of the Exploration Vision, with no new funding, are obvious. NASA's overall budget has remained essentially unchanged through the last three budget requests. Yet in that timeframe the real costs of the transition to a new human space flight structure have been recognized. As a result, the balance among the needs of Space Shuttle (STS) operations and ramp-down, International Space Station (ISS) completion and operation, Exploration Systems development and a robust Space and Earth Science program has come under great strain. I recognize the support that the Administrator has to transition the Agency from being driven by the vestiges of its past program—one that was devised in the 1970s—into a new, forward-looking set of objectives. Broadly I support the goals of transitioning the human space flight program into a new set of capabilities. A nation as technically-advanced as ours, with such human, technological and fiscal resources, should be able to explore beyond the Earth. Furthermore, these new capabilities will benefit science missions and scientific "exploration." But to ask NASA to transition and develop these new capabilities, while undercutting its most innovative and productive component, its science program, is unwise. NASA needs more resources if it is to explore in a "feet on the ground" sense through a human space flight program, and to explore our universe by unearthing its secrets through a vibrant science program.

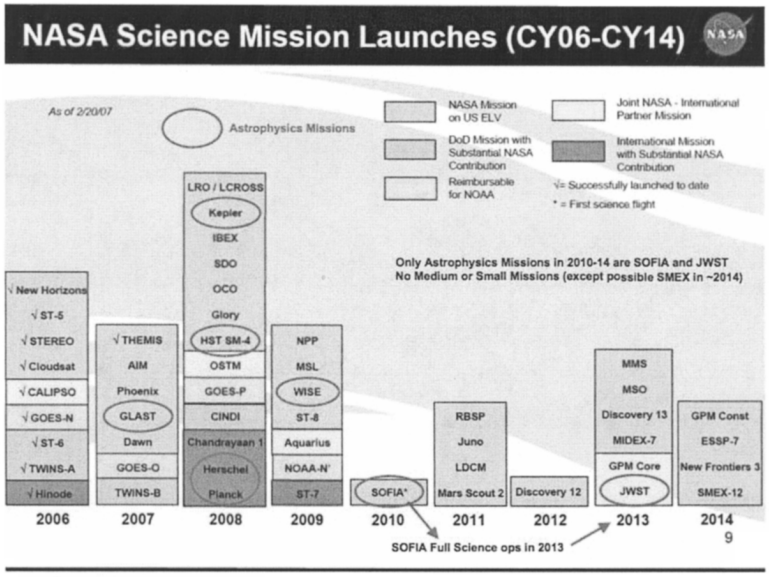
The AAAC hopes that Congress can work to rectify this problem, since the recent fiscal year requests have not provided the resources to enable NASA to carry out its mandate in the Vision. Adequate funding is critical over the next few years when

NASA is trying to support Shuttle operations and ramp-down, completing the ISS, initiating new launch and transportation capability, and carrying out a comprehensive science program. Long-term impacts to both science and human space flight will accrue if the funding is not adequate during this period. The AAAC recognized the issues with its highest-priority recommendation in its Annual Report in the discussion re NASA: **"The lack of growth in the NASA budget to respond to the Exploration Vision is stressing all the Agency's activities. The AAAC is deeply concerned about the growing impact on the space and Earth science program."** The AAAC was also concerned about the potential out-year impact of the reduced funding for NASA overall in the FY07 appropriation if this funding is used as a base for the FY08 appropriation. The science community appreciated that the FY07 Joint Resolution budget for NASA explicitly designated and made statutory only a small cut (~1.5 percent) to science compared to the FY07 budget request level, but remains concerned that further cuts may arise if the FY07 base is used. The AAAC noted: **"The AAAC is concerned that the appropriation for FY08 and beyond may lead to a further cut by using the FY07 appropriation as the base for future budgets, and recommends that the FY08 request be the base to preclude added impacts on science at NASA."**

The issues faced by NASA are so challenging that they really require broad consensus between the Administration and Congress on the Nation's goals for its space endeavors. I hope that some form of high level discussion forum, such as has been recently proposed both in the House and Senate comes to fruition, and provides clear guidance for NASA and enhancement of its budgetary framework.

Astrophysics—an overview: If one takes a near-term view, and looks forward with a horizon around 2009–2010, the mission mix in Astrophysics looks fairly good. Over the next ~5 or so years Astrophysics will have a reasonably well-balanced program, i.e., one with a mix of small, medium and large missions in operation covering a diverse range of scientific areas. The launch of a mid-size mission, the Gamma Ray Large Area Space Telescope (GLAST—in late 2007), a Discovery mission Kepler (in 2008), an Explorer mission, the Wide-Field Infrared Survey Explorer (WISE—in 2009), and participation in two powerful European Space Agency (ESA) missions Herschel and Planck (2008–9) strengthens the program. Astrophysics is operating three Great Observatories, Chandra, Hubble and Spitzer, and providing significant funding for data analysis for those missions. The next Hubble Servicing Mission (SM4) and the instrument upgrades will rejuvenate Hubble. The Stratospheric Observatory for Far-Infrared Astronomy (SOFIA) is moving towards its first science demonstration in 2010 and full science operation in 2013. NASA is progressing on an extremely powerful Great Observatory-class mission, the James Webb Space Telescope (JWST). NASA is also planning for a possible Beyond Einstein mission that would begin to be funded for development in the same time frame (though its launch would not be until the middle of the decade or beyond). These elements of the program are consistent with community-developed strategic plans such as the National Academy Astronomy and Astrophysics Decadal Survey.

So why is the astronomy and astrophysics community so concerned? And why is this concern reflected so strongly in the AAAC annual reports, and the reports and discussions of the NASA Advisory Council (NAC) Astrophysics Science Subcommittee, and the NRC committees (Space Studies Board—SSB; Board on Physics and Astronomy—BPA, Committee on Astronomy and Astrophysics—CAA)? First, the cuts that have occurred in the Research and Analysis (R&A) funds are a very serious issue for the community. R&A funds support theory and modeling, training of students and postdocs, and development of new technologies, and so are of great future value to NASA as well as the community. Second, it is when we look up from the immediate future and look down the road that we see that the new mission pipeline is strikingly empty beyond 2009. This is a major issue. This can be seen in the Figure below. The next few years look good because we are benefiting from the achievements of the past decade, or even longer. The missions from the 1990s and early 2000s are operating or coming to fruition—but the dearth of new small and medium missions initiated in the last few years is reflected in the next decade. SOFIA does not come into full science operations until 2013. JWST, when it launches in 2013, will be an amazing observatory, as dramatic in its way as Hubble was in 1993 (when its optics were corrected), or when the Hubble Advanced Camera was installed in 2002. In contrast to these major programs there is nothing else in the years 2010–2014, except for a possible Small Explorer (SMEX) in Astrophysics in ~2014 (from the recent SMEX announcement of opportunity—AO).



How limited the options are for Astrophysics can be seen in the Table below. In real terms the Astrophysics Division suffers a precipitous decline in FY09 (down by 23 percent in constant dollars relative to 2006) that worsens in the outyears. Even though a number of important and productive missions will be operating into the next decade, the long lag between inception and launch will lead to a period with far fewer operating missions by the middle of the next decade, unless this budget trend is reversed.

	FY06	FY07	FY08	FY09	FY10	FY11	FY12
Actual Year $M	$1,553	$1,540*	$1,566	$1,304	$1,269	$1,266	$1,393
% change**		-0.8%	1.0%	-16%	-18%	-19%	-10%
Inflation Adjusted***	$1,553	$1,495	$1,476	$1,193	$1,127	$1,092	$1,167
% change**		-2.3%	-5.0%	-23%	-27%	-30%	-25%

*1.5% decrease from FY07 request **relative to FY06 ***in FY06 dollars with 3% annual inflation

Need for better cost estimates and the use of "life cycle" costing: I have emphasized the impact of the projected budget decreases for the science program at NASA, with particular emphasis on the situation in Astrophysics. But I think we all recognize that there is another aspect that has impacted our ability to plan ahead—and that is the unrealistic and incomplete costs estimates that have been used in the past for science projects by NASA and the community. The AAAC has strongly encouraged the adoption of a consistent and common approach to mission costing by the community and NASA, and advocated that the baseline be "life cycle" costs (from conceptual development through the end of operations—from pre-Phase A through Phase E). Doing so would eliminate some of the uncertainty that has surrounded cost numbers in community discussions and lead to more realistic costs. In addition, better cost estimation is needed for the Phases, utilizing independent cost estimates as a cross-check. The transition to full-cost accounting at the NASA Centers also is resulting in more realistic cost estimates for missions.

The Decadal Survey recommendations are typically implemented over 10–15 years. This is therefore the timeframe over which we should be costing our missions if we are to match our recommended mission suite to likely budgets. The full costs of JWST, the Space Interferometry Mission (SIM), and SOFIA over that 10–15 year timeframe were not appreciated because the costs used for planning in the Decadal Survey and elsewhere were typically construction or Phase C/D costs (and also were

not subject to an independent cost study). This "undercosting" (to use the NASA Administrator's very appropriate word) has led to a gap between what we wanted to do and what we can do. Fortunately, both NASA and the astronomy community have recognized the problem that this approach has caused. We do not want to repeat this mistake and so ways to improve the mission and project budgets are under serious discussion for the next Decadal Survey.

One important step in being more realistic about mission costs is to ensure that we understand the "life cycle" costs of our currently operating missions. These estimates have significant uncertainty, given the very different situations under which the missions were developed. Nonetheless, they will allow us all to compare new, current and old missions in a more uniform way. Some examples are (for life cycle costs in current dollars in a full-cost accounting environment, including design and technology, construction, launch and operations): HST: ~$7.5–9B (including SM4 plus five years of added operations); Chandra: ~$3.4B (15 years of operations); Spitzer: ~$1.3B (with operations through 2011); Cassini-Huygens: ~$3B (including ESA and DOE contributions); JWST: ~$4.5B (assuming 2013 launch and 10 years of operations); SIM: ~$3B (uncertain since launch date unclear—12 years of operations); SOFIA: $3.4B (with 20 years of operations).

The Decadal Survey numbers were traditionally "construction" costs. These were typically under-estimated and this, in combination with the change to life cycle costs, led to some dramatic increases. JWST (2000 survey as NGST) has gone from $1B to $4.5B, but such cost growth is not rare. Chandra (1980 survey as AXAF) went from $500M to $3.4B. SOFIA (1990 survey) went from $230M to $3.4B. SIM (1990 survey as AIM) grew similarly $250M to ~$3B. Correcting for inflation changes the factors a little, but the growth is still very large. The examples of SIM and SOFIA, both of which were moderate-size ~$250M missions in the 1990 Survey, but which grew to be $3B programs life cycle, have made us aware of the challenges. JWST was a major surprise when it grew to $4.5B life cycle, but given that we now understand that, in current dollars, with full-cost accounting, Chandra is a $3.4B program and HST is over double that, the life cycle cost of JWST, while high, is not extraordinary compared to other major programs.

The discrepancies clearly indicate the need for better cost estimates for each of the project Phases by both NASA and the Decadal Survey, and the use of life cycle costs for planning. Great cost detail is not necessary (nor is it possible), but knowing that JWST would be an ~$4B program life cycle instead of a $1B program, or that SIM and SOFIA would be ~$3B life cycle instead of $0.25B, would certainly help the development of a more robust Decadal Survey, and subsequent planning and roadmapping. It is already clear that developing reliable life cycle mission cost estimates is considered to be very important for the next Decadal Survey—both NASA and the community are learning from our previous mistakes.

Summary

The key points from this discussion are:

- NASA's science program has been an extraordinarily successful enterprise. NASA has shown time and time again that novel technology, driven by great science goals, can dramatically expand our horizons and bring exploration of the cosmos beyond our Earth within the reach of all.
- The exclusion of NASA science from the ACI contrasts with the inclusion of DOE science; the AAAC encourages Congress to consider enhancing the support for science at NASA explicitly to encourage innovation and competitiveness, as has been done for NSF and DOE science.
- The lack of growth in the NASA budget to respond to the mandate of the Exploration Vision is stressing all the Agency's activities. The AAAC is deeply concerned about the growing impact on the space and earth science program and strongly endorses efforts to increase NASA's budget to allow it to undertake the transformation envisaged in the Vision, without imparting serious damage to the science program.
- The decline in the Astrophysics budget in real terms by ~25 percent (from 2009) greatly reduces the opportunities for new missions following the next Decadal Survey in 2010. Even though a number of important and productive missions will be operating into the next decade, the long lag between inception and launch will lead to a period with far fewer operating missions, with scientific and productivity impacts, by the middle of the next decade, unless this budget trend is reversed.
- The cost growth in missions, both moderate and large, clearly indicates the need for better cost estimates for each of the project Phases by both NASA and the Decadal Survey, and the need to use life cycle costs for planning and

roadmapping. It is already clear that developing reliable and robust life cycle mission cost estimates is considered to be very important for the next Decadal Survey—both NASA and the community are learning from our previous mistakes.

I would also like to add that the changes in SMD under the new Associate Administrator Alan Stern are being viewed very positively. His efforts to add to the many very experienced people in SMD with new people to strengthen the scientific focus of the Directorate is being well received in the community.

RESPONSES TO THE QUESTIONS FROM THE CHAIRMAN

1. What are the AAAC's concerns and recommendations with respect to NASA's astrophysics program?

The AAAC noted a number of concerns in its report. The broadest issues concerning the NASA budget ("too small for the mandate it has been given") and ACI ("NASA science is equally as important for the Nation as DOE, NSF, and NIST science") were discussed above. The AAAC is very concerned that the NASA science program has been seriously impacted and that further stresses lie ahead for a science program that has been such an effective demonstration of U.S. science and technology leadership. These broad concerns led directly to two of the AAAC's 2007 recommendations: **"NASA's science funding outlook should be restored. Doing so would be entirely consistent with the commitment to innovation and competitiveness already demonstrated by the Administration and Congress for the NSF and the DOE Office of Science"** and **"The AAAC strongly encourages Congress to consider enhancing the support for science at NASA explicitly to improve innovation and competitiveness, as has been done for NSF and DOE science."**

Beyond the budget question (but obviously related) the central issue is the trend in the mission mix in Astrophysics. It is clear that Astrophysics at NASA is living off the past and the mission pipeline will, with the exception of JWST, largely run dry post-2009. JWST will be a remarkably powerful observatory, as dramatic in its impact as Hubble was in the 1990s, but astronomy and astrophysics encompasses much more than the science enabled by JWST. The only other new opportunities are SOFIA, a possible SMEX by 2014 and a possible Beyond Einstein mission by the middle of the decade. Serious problems with cost growth, both from underestimates and from not using life cycle costs, have occurred in a wide range of programs from Explorers through Discovery to large missions like SIM, SOFIA, JWST and HST SM4. The cost growth has combined with the budget changes to leave the future looking bleak.

Other areas of concern and recommendations in the AAAC 2007 Annual Report are summarized here (and discussed in more detail in the 2006–2007 AAAC report at *http://www.nsf.gov/mps/ast/aaac.jsp*):

Research and Analysis (R&A) funding. The widespread concerns in the community about the cuts and trends in R&A funding were reflected in the report. R&A encourages creative extension of archived data, theoretical studies that can cross traditional disciplinary boundaries, laboratory studies that provide fundamental measurements, and new instrumentation and sensor technologies that pave the way for new science initiatives. With its strong academic emphasis R&A is a key factor in the scientific training and development of younger members of the community—reductions will certainly impact their involvement and run counter to the overall goals of ACI. The R&A program is broader than mission-specific data analysis, and has significant direct value to NASA for science planning and future flight opportunities. A strong R&A program will result in greater productivity from the mission investment at NASA.

The AAAC would very much like to see recovery (and enhancement) of the very valuable R&A program. However, we recognized the great strains on the Astrophysics budget in the near-term due to SOFIA reinstatement, HST SM4 delays, preparing for GLAST, Kepler and WISE launches and ensuring JWST stays on track, so we were reluctant to recommend an "unfundable activity." In the end we recommended that R&A be given high priority if any additional funds became available in the near-term, and if not, that R&A be considered for recovery in the 2009–2010 timeframe as part of the "wedge" that opens up as HST servicing mission activities are ramped down and as JWST construction funding ramps down. We recognize that incrementing R&A competes with the "Beyond Einstein" and the "Decadal Survey" wedges, but that exemplifies the very serious problems faced by Astrophysics.

Competed, cost-capped missions. The Explorer and Discovery mission lines have been very productive. The AAAC believes that a similar program of larger cost-

capped missions, the Einstein/Origins Probes (analogous to the Planetary Division's New Frontiers line), would be particularly valuable for Astrophysics. Several concepts for Probes are being discussed, including the Joint Dark Energy Mission (JDEM). The AAAC felt that development of this concept and discussion with the Decadal Survey about their potential broad value to Astrophysics would be a valuable step and recommended that the Probes be discussed as a mission line for Astrophysics.

Current major programs in Astrophysics. The AAAC discussed a number of the major activities in its report because of their importance to the Astrophysics program.

- The AAAC was very encouraged by the results of the JWST Technology Non-Advocate Review. Technically, JWST appears to be in excellent shape, with all major technologies at TRL–6 (flight readiness). The added contingency provides a better buffer too. JWST is a major, cutting-edge project and we are not naive enough to expect a completely smooth progression to launch, but the committee, like the community at large, hopes that its cost-growth problems are now in the past.
- The committee is very supportive of HST SM4, even more so now that the ACS has failed. A modern camera is needed to restore Hubble's imaging capability. Accommodating the costs of servicing remains a major challenge, especially budgeting for the four-month launch delay in 2008. This further reduces the flexibility within the Astrophysics program.
- The Navigator program is under stress, with two large missions, TPF and SIM, given the recognition that SIM is in reality a ~$3B program. Guidance from the ExoPTF and the Decadal Survey is needed on how to move forward on the study of other planetary systems.

Major mission technology and conceptual development. It is crucial that programs under consideration for implementation by the Decadal Survey process reach a level of maturity that is characterized by a well-defined architecture with well-vetted costs. The AAAC has emphasized that consistent support, roughly at the $10M level, would make a significant difference in the robustness of the mission selections in the next Decadal Survey. The AAAC recommended that early phase development funds for the major missions in Beyond Einstein (Constellation-X; Con-X and the Laser Interferometer Space Antenna; LISA) and in Navigator (Terrestrial Planet Finder; TPF) should be continued if possible until the Decadal Survey re-evaluates the mission suite in the Astrophysics arena.

SOFIA. The SOFIA program underwent dramatic changes in the last year: the project was first reduced to $0 and effectively terminated. SOFIA then underwent a recovery and is now part of the Astrophysics budget. SOFIA has had a troubled and costly development history and will not reach full operations until 2013, more than 15 years after the project began. SOFIA has a distinctly different operational model, akin to ground-based telescopes, in that its instruments can be developed to take advantage of ongoing technological developments. Because of this the science opportunities can be high. SOFIA is a major mission, with a full life cycle cost for 20 years of operations that is $3.4B (FY08 budget request). From FY09 its yearly cost is estimated to be $90M, including Institutional costs, broadly comparable to Hubble (excluding servicing costs) and JWST. When fully operational, SOFIA is estimated to provide ~900 hours of on-target time per year for science observations—space missions average significantly more (HST ~2500 hrs.; JWST ~6000 hrs.). The cost-per-hour of on-target operation is comparable to Hubble and several times JWST, and so the AAAC considers that it is crucial that SOFIA operates as efficiently as possible and fully involves the science community to provide high science returns.

Advisory structure. The AAAC expressed great concern last year in our 2006 report about the lack of an advisory process at NASA, and were very encouraged when the new NASA advisory committees were established. The new structure has, however, lost a valuable role that was once provided by the Space Science Advisory Committee (SScAC). That structure encouraged dialogue, on wide-ranging issues that cut across the SMD divisions, between SMD and a broadly-representative group from the science community. While the AAAC welcomed the re-establishment of the advisory structure at NASA, we noted our concern that dialogue between SMD and a broadly-representative group from the science community is missing in the new structure. The AAAC (and the community more broadly) would welcome an evolution of the current advisory structure that would provide more dialogue with

SMD through a more scientifically-diverse group, even as formal recommendations are channeled through the NAC to the Administrator.

Task forces. The agencies have responded very supportively to the AAAC's requests for community-based task forces to advise the agencies on implementation approaches for key scientific areas. NASA's recent support for two interagency activities, the Dark Energy Task Force and the ExoPlanet Task Force was appreciated (in addition to its earlier support for the Task Force on the Cosmic Microwave Background). With the substantial advances on the ground and the recognition of the challenges and cost of major space missions for planet search projects like SIM and TPF, the AAAC recommended last year that NSF and NASA constitute a Task Force to develop a strategic framework for how to move forward on the detection and characterization of planets around other stars. The AAAC greatly appreciates that the agencies responded positively and quickly; the ExoPlanet Task Force (ExoPTF) has been formed and has begun its deliberations. Its report is expected late in 2007. The AAAC also welcomed the decision by SMD last year to ask the NRC to carry out a study to determine which Beyond Einstein mission should go forward if funding became available in a possible FY09/10 funding "wedge" as HST SM4 is completed and JWST passes the peak of its spending curve. The selection of three JDEM mission concept studies for conceptual development by NASA Astrophysics, and the joint support of the NRC Beyond Einstein Program Assessment Committee (BEPAC) study by DOE were also highly welcomed by the AAAC.

National Virtual Observatory (NVO). While this is a very small program, it was considered to be of particular importance in the 2000 Decadal Survey. It is a joint NASA–NSF activity. The agreement on a joint NASA–NSF solicitation for management of the NVO operation has been moving forward at a very slow pace, and the AAAC would like to see this come to closure to minimize the disruption to a small but important activity.

2. What are your perspectives on the balance of the NASA astrophysics program in terms of the mix of mission sizes, R&A, theory, modeling and technology development? What if any adjustments are needed in your view?

A balanced program within Astrophysics has been a consistent goal of the astronomy community. Such a program provides the most cost-effective way to address the great science issues of our time. Some can be addressed through smaller missions like COBE and WMAP (the cosmic microwave background), others require medium missions like Kepler (planet searches), GLAST (the gamma-ray universe) and JDEM (dark energy), while the largest missions (the Great Observatories like Hubble, Chandra, Spitzer and JWST) can address some of the most challenging scientific questions that cannot be answered any other way. The versatility of such Observatories also allows them to be used for follow-up of discoveries with very little time lag. However, where the Observatory capabilities cannot address a particular high-priority science objective the relatively rapid response with small missions provides a means of doing so. The last three astronomy and astrophysics Decadal Surveys have all emphasized the importance of a balanced program of small, medium and large missions, and have given particular emphasis to the Explorer program and to a healthy program of research support (Data Analysis—DA, and Research and Analysis—R&A).

In the near-term, over the next few years, as noted above, Astrophysics will have a range of missions including an Explorer (WISE), a Discovery mission (Kepler) and a medium class mission (GLAST). Data Analysis (DA) funds from the ongoing Great Observatories are supporting a very wide variety of science objectives. The biggest immediate concern is the cut in R&A, which, while modest, had great impact because cuts in a multi-year program are immediately felt by the new or renewing investigators. Another concern that is also vitally important for the future of the Astrophysics program is the current low level of technology development funding. This gets less attention, but it is the "seed corn" for future missions.

However, the clouds on the horizon portend a more dismal future. The future program is dominated by JWST and SOFIA, both of which are large programs (in $ terms). As can be seen in the Figure above, the dearth of small and medium missions post-2009 is a great concern for the vitality of the field in the next decade. The continuing effective reductions in the R&A budget (in the FY08 budget and by inflation) will further impact the community, unless the trend is reversed. As Spitzer, Hubble and Chandra approach the end of their lives the community will also see reductions in data analysis funds. The DA and R&A funds and smaller-scale missions serve a critical role in supporting the broad fabric of research needed

for realizing the science from future missions and in enabling the development of the necessary personnel and skills.

The program is clearly unbalanced in the future beyond 2009. There are no small-medium Astrophysics missions for many years after 2009. The first mission might be a Small Explorer (SMEX) in ~2014. The unbalance across Astrophysics is but one aspect. There is a need for balance within the very broad areas encompassed by Astrophysics—a single large program in one broad science area and only small missions in another also indicates unbalance. For example, searches for and research on exoplanets will benefit from an ensemble of small-to-large missions complemented by ground-based facilities. A broad, systematic cost-effective approach is needed. The same could be said of a broad science program to explore our universe from its earliest moments to the present day (Origins), and the Beyond Einstein program. Both have very broad goals that together encompass most of the "great questions" within astronomy and astrophysics, and need a suite of missions of different scales to address those fundamental questions.

As much as possible it would be good to not have all our eggs in one basket—especially for space missions. Whole areas of science could be drastically undercut if problems occur. Realistically, there are high priority science objectives where there is no other way than by doing a large space mission, as with JWST's search for the earliest galaxies in the early days of the universe. However, as much as possible, we should try to accommodate a diverse range of mission and project scales (and to give particular attention to complementing ground-based studies, and collaborations both with other agencies and internationally).

R&A funding needs to improve since it is essential for providing the research base and the development of skills on which future return from missions will depend. Funds for technology development are needed to ensure that optimal choices are made when selecting missions and that the mission options available are broad. There is a crucial need to encourage and support technology development in the science community, as well as at NASA Centers. Core capabilities are required in the NASA Centers, but the Centers might be encouraged to involve the academic community more routinely and directly, possibly through R&D funding that supports more technology development.

I would give particular focus on strengthening the theory and modeling program in R&A. This is remarkably inexpensive for its value to the scientific enterprise. I am not a supporter of acquiring reams of data without concurrent theoretical development. Results drive theoretical efforts and give them relevance, but it is a synergistic and two-way effort, where theoretical developments also help focus observational efforts. It is crucial to have the challenge that comes from having theory observations confront each other, and challenge and test each other.

In summary, in my view adjustments are needed to provide a more balanced mission suit across the whole program and also within broad scientific areas, along with support for technology development, and increased support for R&A, particularly for theory.

3. Does the program, in your view, reflect the priorities of the National Academy of Sciences' decadal survey for astronomy and astrophysics? If not, where does the program diverge from the decadal survey?

As discussed above, the Astrophysics program in the near-term, does have a number of launches and a suite of operating missions, and so looks fairly balanced and productive. There are very real concerns, however, about R&A funding, the frequency of small missions (Explorers) and the very limited funds for technology development. The concern grows substantially as one looks further into the future. Moreover, as one takes a longer-term view the program increasingly moves away from the goals of the Decadal survey. The mission mix becomes very unbalanced. JWST will be a remarkably powerful mission, but the mission suite is devoid of other space missions. SOFIA should be operating on the ground, and hopefully a Beyond Einstein program will be under development early in the decade, but launch would be many years away (5–7?). An Astrophysics Small Explorer (SMEX) may be operational by 2014, but other launch opportunities may not arise for years. This is not a balanced program, either scientifically or by mission scale (small-medium-large), and will become increasingly unbalanced as the current Great Observatories begin to end their useful life. This unbalance will be accentuated as the missions launched in 2007, 2008 and 2009 start to approach their end of life towards the early-middle of the decade. The lack of scientific breadth and limited numbers of operating missions will be a serious departure from the breadth of the program envisaged in the Decadal Survey. This will be compounded if the problems with R&A funding and technology development continue.

4. What do you regard as the top three risks facing NASA's astrophysics program over the next five years and how should those risks be addressed?

The challenge of dealing with a reduction and a dramatic change of slope in the Astrophysics budget, combined with recognition of the costs of the current mission suite have resulted in great concern about future opportunities in Astrophysics. I am assuming that we will develop processes that ensure that we have more realistic cost estimates and that we will use life cycle costs for programs for planning and roadmapping. I then see the top risks from a scientific perspective as:

1) The lack of small and medium missions beyond 2009. The dramatic drop in the small-medium launch rate beyond 2009 is a major concern. The recently announced Small Explorer SMEX call for proposals later this year could lead to an opportunity for Astrophysics, but the earliest likely launch date would be around 2014. The contrast with the next few years, and with the early part of this decade (when many small and medium Explorers were launched) is dramatic. SOFIA will not reach full operations until 2013. JWST will be a superb scientific mission with wide-ranging capabilities but it alone cannot encompass the science goals of the astronomy and astrophysics community. This becomes especially so since Spitzer, Chandra and Hubble will all be nearing or past their end of life (Spitzer will lose a lot of its science capability by mid-2009 as its cryogen is exhausted). The risk is of greatly reduced scientific returns in the coming decade. An associated risk is that of launch vehicle availability at reasonable cost. This is a serious issue for mission frequency if a substantial fraction of the cost of an Explorer or SMEX is the launch vehicle cost.

2) Inability to respond to the 2010 Astronomy and Astrophysics Decadal Survey. The funding for Astrophysics drops by ~25 percent in real terms around the time when the new Decadal Survey is released and so the opportunities for ramping-up on the recommended missions will be quite limited. The Decadal Survey will be discussing and making recommendations on many high priority programs that have been under development or discussion for some time. For example, Con-X, LISA, SIM, TPF and SAFIR will all be discussed, as well as a variety of Einstein Probe missions that are being considered in the current BEPAC study. The AAAC ExoPlanet Task Force will likely identify additional areas for development and missions. Some hard choices face the community in the upcoming Decadal Survey. The natural outcome of the more realistic costing that will be part of the next Decadal Survey will be a reduced program, better matched to the available funding. However, the lack of a significant funding opportunity will limit the ability to initiate a strong effort following the survey. This translates to a risk of significantly diminished scientific returns on the highest-priority science questions of the decade. The next generation of missions will also be at risk if technology development cannot be initiated because of the same funding problems.

3) The current lack of technology development and mission development funding and its impact on mission costs and readiness. The very limited funding available in recent times has severely limited the technology development efforts for both current missions in early development (like Con-X and LISA, and now TPF), and also more innovative and speculative technologies for future opportunities. This will have far-reaching implications for mission opportunities in the next decade and is significant risk to future astrophysics missions and competed opportunities. It also increases the risks of cost growth if conceptual development and technology development have been unable to progress steadily.

These areas are identified as risk areas because of two problems. The first is the dramatic change in the budget situation for Astrophysics over the next few years, particularly the cuts in FY09 and beyond. Second, the poor cost estimates in the past have exacerbated our current problems. The agency and community together did not deal very well with the cost estimates and budgets of the missions and programs that we jointly developed. However, it is my view that this situation has changed dramatically with the much more realistic and open approach of the new Administrator and with a more sophisticated and realistic view of project costs and the costs over the life cycle of missions by the community and the Agency. While I think we are now working to deal collectively with the undercosting problem, a solution to the budget problem for science is a more challenging concern for the future. If we are to have a strong, productive and broadly-based science program, additional funding is needed. Recognition is needed that NASA science plays a role as important as that of DOE science, NSF and NIST in the Nation's science enterprise.

5. If you could make three strategic investments that could benefit the astrophysics program over the long-term, what would those investments be?

Strategic investments are key to positioning the Astrophysics Division, the astronomy and astrophysics community, the NASA Centers and industry partners to be able to extend the limits of scientific endeavor and scientific understanding. To meet the science goals of the community, NASA and the community need to be able to move forward on a variety of missions from large Flagship missions to medium and smaller scale missions, while returning cutting-edge science results from the current missions. I think the following three areas would be excellent strategic investments to position the Agency and the community for a cost-effective program of science missions. The first two are relatively low cost (though still very difficult to fund in the current budget environment), while even the third could be carried out in an Astrophysics budget that is constant at the FY06 dollar level.

1) R&A funding is a strategic investment. This is particularly so for theory, modeling and cutting-edge technology development to complement mission specific data analysis. Clearly R&A and mission-specific DA maximizes the science return from current programs and also maximizes the "return on investment" in space science. Support for such activities is also a strategic investment from NASA's perspective. A key aspect of an implementable long-range plan is knowing what are key science questions, why they are important, and whether answering them is doable. Exploiting current data, along with theory allows us to set science priorities. Furthermore, exploring novel technologies and strengthening the technological base amongst graduate students and postdoctoral researchers is an investment for the future.

2) Technology development for missions. Astrophysics missions utilize state-of-art technology, and it is essential that that technology be developed and demonstrated to flight readiness levels before a mission enters construction. Retiring technological risk early helps to minimize the likelihood of cost growth. There is another aspect as well. The science community must make strategic choices on how to spend limited funds as wisely and effectively as possible. For this to happen we must understand the level of risks and costs at the time we undertake our Decadal Surveys. We cannot afford to have moderate scale missions at the few hundred million dollar level grow into multi-billion dollar programs. Modest (by comparison with the final costs) expenditures on technology development and on establishing a strong science and management team early in the planning and development process would be money very well spent.

3) Competed, cost-capped missions. These missions, at the medium scale (Einstein and Origins Probes—like New Frontiers), along with the smaller Explorers and Discovery-class missions have a valuable role. Having been a strong proponent of large "Flagship" missions (through personally spending a great deal of my career working to make Hubble a success and NGST—now JWST—a reality in its early years) I do not want to downplay the central role that large missions play in the Astrophysics science enterprise. Flagships, however, are rare and it is essential for the vitality of the field for frequent launch opportunities at the medium and small scale. Cost-capped, competed missions have many attractive features (e.g., focused science opportunities, community involvement, responsive to more current science goals, controls on cost-growth). However, heavy reliance on such quick response, "bottom-up" missions may undercut the benefits of strategic planning through the Decadal Survey. This can be rectified if the Decadal Survey gives guidance on broad areas that the community sees as important and ready for investigation (e.g., searches for planets around other stars—exoplanets; the early universe; dark matter and dark energy). A additional major concern for such missions could be the cost of launch vehicles with the demise of the Delta 2 launchers. This has the potential to be a serious issue for the small-medium scale missions.

As noted, an Astrophysics budget that is constant in FY06 dollars, with the FY06 base, could accommodate all these recommendations. Any growth as part of the legislative Innovation and Competitiveness agency would enable, for example, a new large Flagship mission in the next decade as well.

BIOGRAPHY FOR GARTH D. ILLINGWORTH

POSITIONS HELD

1988– Astronomer, University of California Observatories/Lick Observatory

1988– Professor, Department of Astronomy and Astrophysics, University of California, Santa Cruz

1985–1987 Research Professor, Department of Physics and Astronomy, Johns Hopkins University

1984–1987 Deputy Director, Space Telescope Science Institute

1978–1984 Astronomer, Kitt Peak National Observatory

1976–1977 Miller Fellow, Department of Astronomy, University of California, Berkeley

1974–1975 Postdoctoral Fellow, Kitt Peak National Observatory

MAJOR ACTIVITIES/ACHIEVEMENTS (LAST SIX YEARS)

1. Major ongoing programs on galaxy evolution in clusters at z~1, and galaxy formation and evolution at high redshift (from z~2~7 and beyond): four graduate students and postdocs plus a number of major HST, Spitzer, Keck, VLT and Magellan collaborations. Many talks at international workshops on high redshift galaxies in the first 1–2 billion years.
2. Chair, Astronomy and Astrophysics Advisory Committee. Editor, AAAC Annual Report to NSF, NASA and DOE, and to Congress and OSTP.
3. Deputy PI, HST Advanced Camera (ACS): Successful completion and launch of most powerful instrument yet on the Hubble Space Telescope. Improved HST's performance by 10 times.
4. Co-organizer major workshop ("Hubble's Science Legacy") on science issues and technical challenges for a large space telescope successor to HST.
5. Chair, for four years, of Space Telescope Institute Council, STIC.
6. Search for planets using space coronagraph/nuller. Member TPF–C STDT committee.
7. HST Key Project, "Determining the Hubble Constant to 10 percent." Achieved 10 percent goal.

ACADEMIC HISTORY

1965–1968 B.Sc. (Honors) 1st Class (Physics), University of Western Australia

1969–1973 Ph.D. (Astrophysics) Australian National University, Mount Stromlo and Siding Spring Observatory

2007 Invited Speaker, EU ASTRONET Worshop—Status of U.S. Astronomy Program

2006 Invited Plenury Speaker, SPIE, "Large Telescopes" Meeting—Astronomy and the Decadal Survey

2005– Editor, with AAAC committee, *AAAC Annual Report for Congress and Agencies*

2004– Chair, AAAC, Astronomy and Astrophysics Advisory Committee

2004–2006 TPF–C Science Technology Definition Team

2004 Chair, Spitzer TAC (GO Time Assignment Committee)

2004–2007 Nominating Committee, Aspen Center for Physics

2003 Elected General Member, Aspen Center for Physics

2003 PI, Visions proposal for >20-m UVOIR Telescope

2002–2005 SScAC, NASA Space Science Advisory Committee

2002–2003 NAAAC, National Astronomy and Astrophysics Advisory Committee

2000–2007 AURA Board of Directors

1999 NGST Instrument Study Team

1999–2000 HST Second Decade Study Committee

1998–2002 Chair, Space Telescope Institute Council

1997–2003 Member Representative, AURA (University of California Representative)

1996–2002 Space Telescope Institute Council

1995–1996 AURA Board of Directors (University of California Representative)

1995 NRC SSB "Task Group on BMDO New Technology Orbital Telescope"

1995– Deputy PI, HST Advanced Camera

1994–1995 NASA HQ UVMOWG

1993–1999 Co-Chair, Keck Telescope Science Steering Committee

1992 HST Second Generation Instrument Review Team

1991–1993 JPL special review panel for HST camera, WFPC–2, chair Charles Townes

1991–1999 Member, Keck Telescope Science Steering Committee

1990–1991 Co-chair, Keck Telescope Science Steering Committee

1990–1992 Chair, NGST SEWG (Next Generation Space Telescope Science-Engineering Working Group) to oversee technology development program for future large space telescope

1989–1990 Chair, "UV–Optical In Space" Panel of Astronomy and Astrophysics Survey Committee

1988–1989 Chair, Scientific Organizing Committee for Workshop on "The Next Generation: A Successor to Hubble Space Telescope" sponsored by NASA HQ and STScI

1987–1990 Keck Telescope Science Steering Committee

1987–1990 Chair, Keck Telescope Segment Acceptance Committee

1987–1989 NASA HQ UV–Visible-Relativity Management Operations Working Group

1987 Executive Committee, HST Maintenance and Refurbishment Workshop, Goddard Space Flight Center, Greenbelt, Maryland

1986–1987 Co-Chair, HST Science Certification Review

PUBLICATIONS (recent selections from 295 papers total)

196. "Spectroscopic Confirmation of a Substantial Population of Luminous Red Galaxies at Red shifts $z >\sim 2$," P.G. van Dokkum, N.M. Forster Schreiber, M. Franx, E. Daddi, G.D. Illingworth, I. Labbé, A. Moorwood, H.-W. Rix, H. Röttgering, G. Rudnick, A. van der Wel, P. van der Werf and L. van Starkenburg. *ApJL,* 587, L83–L87, 2003.

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224. "Stellar Populations and Kinematics of Red Galaxies at $z >2$: Implications for the Formation of Massive Galaxies," P.G. van Dokkum, M. Franx, N.M. Förster Schreiber, G.D. Illingworth, E. Daddi, K.K. Knudsen, I. Labbé, A. Moorwood, H.-W. Rix, H. Röttgering, G. Rudnick, I. Trujillo, P. van der Werf, A. van der Wel, L. van Starkenburg and S. Wuyts. *ApJ,* 611, 703–724, 2004.

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DEPARTMENT OF ASTRONOMY AND ASTROPHYSICS

April 28, 2007

The Honorable Mark Udall, Chairman
Subcommittee on Space and Aeronautics
Committee on Science and Technology
House of Representatives
Washington, DC 20515

Dear Mr. Chairman:

As requested of witnesses appearing before your Subcommittee I would like to submit the following financial disclosure regarding my research support from NASA.

I have been the recipient of a number of competitively-awarded grants from NASA for scientific research support in my university department. These grants have been used to fund personnel for research and research activities (data analysis and publications). One of my research contracts was awarded for being part of the team that won the contract to build and utilize an instrument on the Hubble Space Telescope (the HST Advanced Camera). In addition, I have been awarded funding through peer-review competition for telescope time on the Spitzer Space Telescope and the Hubble Space Telescope.

Sincerely yours

Garth D. Illingworth,
Professor, Department of Astronomy and Astrophysics
Astronomer, University of California Observatories/Lick Observatory
Chair, Astronomy and Astrophysics Advisory Committee

Chairman UDALL. Thank you, Doctor.
Dr. Baker.

STATEMENT OF DR. DANIEL N. BAKER, DIRECTOR, LABORATORY FOR ATMOSPHERIC AND SPACE PHYSICS, UNIVERSITY OF COLORADO, BOULDER

Dr. BAKER. Mr. Chairman, Ranking Minority Member, and Members of the Committee, I want to thank you for the opportunity to address the impacts of the proposed fiscal year 2008 budget on the NASA program and heliophysics.

In addition to my roles at the University of Colorado, I am also the Chair of the National Research Council's Committee on Solar and Space Physics, and a member of its parent body, the RC Space Studies Board.

Part of the views I express today are my own.

Let me begin by thanking you for your continuing and substantial support for NASA science. We in the science community sincerely appreciate the support, and fully recognize the difficulty of funding NASA science in a constrained budget environment.

Heliophysics division at NASA has a number of exciting missions that have been launched recently. Stereo, NODI and DEMES are already providing remarkable new measurements. Because of our large role in the program, we at LASP are very excited and proud of the successful launch just last week for the upper atmospheric AIM spacecraft as part of the Explorer program.

However, beginning with the fiscal year 2005 NASA budget plan, and continuing through the fiscal year 2008 budget in its five year run out, the future heliophysics program has been significantly compromised. For example, the solar terrestrial probes, or STP line, has had over half of its budget content removed, resulting in at least a six year gap in STP launches. A highly successful Explorer mission line has had over $1 billion of budget authority removed in the run out from the fiscal year 2005 budget onward.

As shown in the figure here, the Explorer budget in the fiscal year 2008 plan is about half of what would have been expected, based on the fiscal year 2004 budget, which greatly reduces our ability to respond effectively to new science and technology advances. Noted by others, the sounding rocket program, and indeed the entire sub-orbital program is also at a dangerously low bare bones resource level.

In the fiscal year 2008 budget plan, the space weather oriented living with the STAR program also sees its funding stretched out so that it substantially—missions have been reduced, and the radiation belt storm probes and atmosphere/thermosphere probes have—no longer have simultaneity. Alarmingly, and rather inexplicably, the previously budgeted funding for the RBSP missions of opportunity is eliminated in the fiscal year 2008 plan.

In response to your questions about my perspectives on the balance of the NASA heliophysics program and its mix of program elements, I must say that considerable anxiety exists in the science community due to anticipated reductions in the smaller missions and sustaining research programs that perform the support for much of the community based research.

I am delighted that Dr. Stern is taking actions now to remedy the sub-orbital situation. I am also encouraged by the fact that a new announcement of opportunity for small explorers will be released, thanks to Dr. Stern and his team, by October 2007. There is widespread recognition as well that R&A cuts are harmful and will inevitably reduce the number of new students who enter university programs. This definitely needs to be addressed.

As for how the heliophysics program reflects the priorities of the decadal survey in solar and space physics, NASA is attempting to implement some of the highest priority programs from the 2003 survey, but the pace and balance of activity seems highly unlikely to achieve the decadal goals. It now appears that with mission cost growth and reduced heliophysics funding it is very unlikely that most survey missions will be completed within the decadal window.

The three top risks facing the heliophysics program over the next five years, in my opinion, are first, fear of failure. There is a proper

level of redundancy, scrutiny and oversight that matches the risk of a robotic mission failure, and balances that with the program's scale. To do more than this due diligence drives costs for even small end missions out to extraordinary heights. I fear this is paralyzing the space science program at present, this fear of failure.

Lack of affordable access to space is the second. Unfortunately, the cost of launching missions into space has grown out of all proportion to the cost of small scientific payloads and satellites. This imbalance is destroying the ability of the heliophysics to develop and maintain a regular and frequent launch of all class submissions.

The third risk is the erosion of trained work force. The NRC has recently issued a report on the NASA work force, and it confirms my view that NASA needs to invest in space science programs that allow universities to attract and engage undergraduate and graduate students in all aspects of mission development and deployment.

Finally, the top three investments that could be made to benefit the heliophysics program over the long-term are, first, I would say, lower costs and frequent access to space. Congress and other stakeholders should work together to make sure that every avenue for launching space hardware is made readily available to research teams.

In this category of access to space I would also place missions of opportunity. Launching NASA instruments or payload suites on commercial or foreign spacecraft can provide tremendous bang for the buck.

Secondly, would be a regular cadence and more frequent small end missions. This echoes what other speakers have said. The key to a healthy, robust heliophysics program is to have more and better opportunities for small explorer, university class explorer and sub-orbital missions.

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 fiscal year 2004 budget plan. It was about $350 million per year.

Finally, and I can't stress this strongly enough, is improved management of mission costs. 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.

Thank you very much for your attention. I look forward to answering questions.

[The prepared statement of Dr. Baker follows:]

PREPARED STATEMENT OF DANIEL N. BAKER

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 FY 2008 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 sup-

port 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.

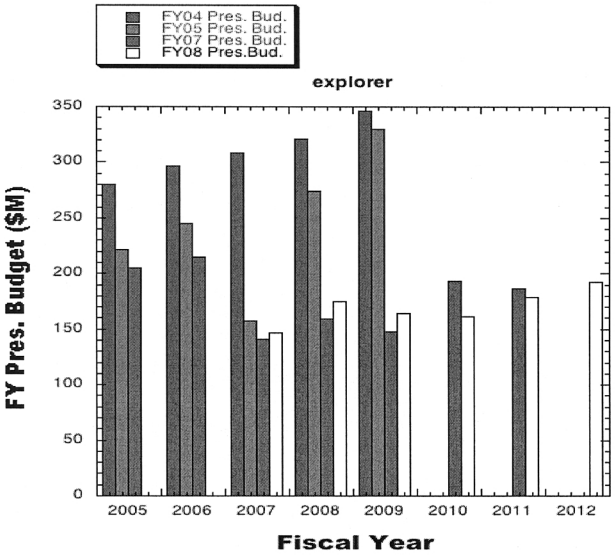
Overview of FY 2008 Budget Impacts to the Heliophysics Program

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 three-dimensional views of the Heliosphere. Detailed images of the Sun are also being provided by the newly-launched Hinode 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 FY 2005 NASA budget plan, and continuing through the FY 2008 budget and its five-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 six-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

FY 2005 onward. As shown in the figure below, the Explorer budgets in the FY 2008 and its run-out are about half of what they would have been expected to be based on the FY 2004 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 sub-orbital 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 Sub-orbital 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.[1]

The other major component of the Heliophysics program is Living with a Star (LWS). The funding profile for LWS as defined by the FY 2005 and FY 2006 budgets allowed for a robust program. In the FY 2008 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 FY 2008 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

[1] The Solar Probe mission was the highest priority large-class mission in the NRC solar and space physics decadal survey. An early start of Solar Probe would have required resources beyond those anticipated at the time the survey was completed; however, the anticipated budgets supported a start in FY 2010. Long a priority of the heliophysics community, the Solar Probe mission promises to revolutionize our knowledge of the physics of the origin and evolution of the solar wind. Moreover, by making the only direct, in-situ measurements of the region where some of the deadliest solar energetic particles are energized, Solar Probe would make unique and fundamental contributions to our ability to characterize and forecast the radiation environment in which future space explorers will work and live.

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.[2]

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 FY 2008 budget plan, but the trends have been perpetuated in the FY 2008 budget and its five-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 FY 2008 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 sub-orbital 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 FY 2008 proposed run-out for Explorers will mean a program that is reduced by over half from its proposed FY 2004 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-

[2] For example, in 2004, it was reported the economic benefits of providing reliable warnings of geomagnetic storms to the electric power industry alone were approximately $450 million over three years. See, "Solar Storms Cause Significant Economic and other Impacts on Earth," and references therein, in *NOAA Magazine,* available on the Internet at: *<http://www.magazine.noaa.gov/stories/mag131.htm>*.

duration efforts, usually lasting three to four years, where new hardware and theoretical approaches are explored. NASA was forced last year by budget realities to propose an across-the-board reduction of 15 percent 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 space flight 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 sub-orbital 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 five 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 sub-disciplines 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 space flight 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 space flight 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 space flight 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 sub-orbital 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 sub-orbital 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 on-board 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 sub-orbital 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 FY 2004 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 FY 2004 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 FY 2004, 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.

BIOGRAPHY FOR DANIEL N. BAKER

Dr. Daniel Baker is Director of the Laboratory for Atmospheric and Space Physics at the University of Colorado-Boulder and is Professor of Astrophysical and Planetary Sciences there. His primary research interest is the study of plasma physical and energetic particle phenomena in planetary magnetospheres and in the Earth's vicinity. He conducts research in space instrument design, space physics data analysis, and magnetospheric modeling.

Dr. Baker obtained his Ph.D. degree with James A. Van Allen at the University of Iowa. Following postdoctoral work at the California Institute of Technology with Edward C. Stone, he joined the physics research staff at the Los Alamos National Laboratory, and became Leader of the Space Plasma Physics Group at LANL in 1981. From 1987 to 1994, he was the Chief of the Laboratory for Extraterrestrial Physics at NASA's Goddard Space Flight Center. From 1994 to present he has been at the University of Colorado.

Dr. Baker has published over 700 papers in the refereed literature and has edited five books on topics in space physics. He is a Fellow of the American Geophysical

Union, the International Academy of Astronautics, and the American Association for the Advancement of Science (AAAS).

He currently is an investigator on several NASA space missions including the MESSENGER mission to Mercury, the Magnetospheric Multi-Scale (MMS) mission, the Radiation Belt Storm Probes (RBSP) mission, and the Canadian ORBITALS mission. He has won numerous awards for his research efforts and for his management activities including recognition by the Institute for Scientific Information as being "Highly Cited" in space science (2002), being awarded the Mindlin Foundation Lectureship at the University of Washington (2003) and being selected as a National Associate of the National Academy of Sciences (2004). Dr. Baker has been chosen as a 2007 winner of the University of Colorado's Robert L. Stearns Award for outstanding research, service, and teaching. Dr. Baker presently serves on several national and international scientific committees including the Chairmanship of the National Research Council Committee on Solar and Space Physics and membership on the Space Studies Board. Dr. Baker recently served as President of the Space Physics and Aeronomy section of the American Geophysical Union (2002–2004) and he presently serves on advisory panels of the U.S. Air Force and the National Science Foundation. He was a member of the NRC's 2003 Decadal Survey Panel for solar and space physics and he was a member of the 2006 Decadal Review of the U.S. National Space Weather Program.

Chairman UDALL. Thank you, Dr. Baker.

Dr. Burns.

STATEMENT OF DR. JOSEPH A. BURNS, IRVING P. CHURCH PROFESSOR OF ENGINEERING AND ASTRONOMY; VICE PROVOST, PHYSICAL SCIENCES AND ENGINEERING, CORNELL UNIVERSITY

Dr. BURNS. Chairman Udall, Ranking Member——

Chairman UDALL. Dr. Burns, if you would turn your microphone on.

Dr. BURNS. That works much better. Let me try that again.

Chairman Udall, Ranking Member Calvert, and Representative Johnson, I sincerely appreciate this opportunity to testify to you today.

Since Sputnik's launch 50 years ago this October, all Earth's peoples have been privileged to participate as our planetary environs have been explored, discovered, and understood, to invoke NASA's mantra.

This continues today. We have two twin Mars rovers that are—carried back the story that there—Mars was once wet. We have a remarkable spacecraft in orbit around Saturn, the Cassini spacecraft. We also have Alan's New Horizons. It has just slipped past Jupiter a few months back.

So this is a—America's planetary exploration program today is indeed doing extremely well, but its future is quite uncertain. I submit that an appropriate analogy might be that today's planetary program is a powerful ship that appears to be staunchly cruising along, but our vessel is sailing so smoothly nowadays principally because of yesterday's investments. Without continued investment and attention, the ship's momentum will inexorably drain away.

Today's craft is running low on fuel. Some of its machines are not being properly maintained. Upgraded, improved replacement instruments are unavailable, and sadly, to me, the boat's crew is aging.

Fortunately, to deal with these treacherous times, we have a new Admiral, Alan, and a new Captain to our ship, Jim Green. These are excellent choices, and we are very pleased to be able to work with them.

I would like to move to your question—the questions that you asked me. The first concern, mission mix. Missions are, of course, the engineering marvels that provide us the capability to explore, as NASA's slogan states. So how do the various missions and their mix fare in the fiscal year 2008 budget and beyond?

The pace of the future Discovery missions seem about on track right now, after several years of delayed selections. The New Frontiers line, the middle line, seems also on track, roughly. I am sorry, has fallen to half of the plan grade. The next selection should be made in the next year to get this program back on track.

Once again, there are no new flagship missions in the planetary area, and the fact 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.

So the reigning 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 NASA has covered its shortfall by draining three or $4 billion from the science program, much of that coming from solar system exploration, especially the Mars program.

I am especially perplexed that NASA should—would choose to lessen robotic solar system studies, especially investigations of Mars, given the ultimate destination for the President's vision.

Much of the slowdown in America's exploration of the solar system is not presently apparent, because most of the pain has been deferred to past 2010. Planetary missions require technological development, an educated work force, an excited work force, advanced planning, especially if we are to collaborate with international partners.

What about research and analysis funds? Research and analysis funds have dropped by one-quarter since fiscal year 2005. The budget that you are considering today recommends that this 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 population will understand the data that is being Mars, Saturn and our other outposts. We can only plan for the future wisely if we have sufficient R&A funds.

Similarly, if we are to discover things, that whole process becomes problematic if there are only limited opportunities exist to analyze the mission results. Funding for data analysis should increase in proportion to the growing data volume and the diversity of targets that we are visiting.

What are the top risks for the next five years? The future U.S. space enterprise is jeopardized by the loss of its core competencies, both in technology development and personnel, and this is a consequence of inadequate base program resources. Furthermore, the rapid growth in mission costs limits the nature and number of flights that we can fly. And finally, the lack of a long lived power sources will prevent any missions to the outer solar system.

What are especially beneficial strategic investments? I believe investments in core technologies, science instruments and infrastruc-

tures, such as the Deep Space Network, will be most fruitful for the long-term health of the planetary exploration program.

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 of mission size play important roles in any balanced plan. A sharp increase in R&A funds are essential to a healthy program.

In conclusion, 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 and understand Earth's surroundings, as NASA claims it wishes to do, more attention and additional fundings are—funding are required.

Mr. Chairman and Members of the Committee, I thank you for your attention today, but most of all for your continuing support of the planetary exploration program.

[The prepared statement of Dr. Burns follows:]

PREPARED STATEMENT OF JOSEPH A. BURNS

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 extra-solar 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 re-supplied, 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 1980's, 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 space flight 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.

Outline of Joseph A. Burns's remarks to the U.S. House Science Committee 5/2/07

The U.S. planetary program is producing extraordinary scientific results across the solar system as a result of long-term support from Congress. However, the proposed FY08 budget i) is insufficient to allow the mix and pace of flight missions that was recommended by the 2003 planetary decadal survey; ii) should be augmented to support more data analysis; and iii) falls far short of the funds that would adequately strengthen the necessary associated Research and Analysis. The top risks faced by NASA's Planetary Science Division are inadequate funding of technology development, lessened availability of suitable flight and power systems, rising mission costs and the dwindling supply of plutonium to allow missions to the outer solar system. Additional strategic investments in infrastructure, core technologies and scientific personnel would prove especially valuable for the long-term vitality of the U.S. solar system exploration program. The lunar exploration program is reasonably sound, principally because of international missions. Without augmented funding, it is questionable whether NASA will be able to fulfill its stated goal of *"explore, discover, understand."*

BIOGRAPHY FOR JOSEPH A. BURNS

Joseph A. Burns is the Vice Provost for Physical Sciences and Engineering, the Irving Porter Church Professor of Engineering and Professor of Astronomy at Cornell University. Joe received a B.S. from Webb Institute of Naval Architecture in 1962; Cornell awarded his Ph.D. in space mechanics in 1966. In addition to his activities in Ithaca, Burns has held year-long appointments at two NASA facilities (Goddard Space Flight Center and Ames Research Center), at UC–Berkeley and at the University of Arizona. Burns has also spent extended leaves in Moscow, Prague, and Paris. He is a member of the imaging teams for the Cassini (Saturn) and Rosetta (European comet) missions, and was an associate of the Galileo imaging team.

Burns has written more than two hundred papers—both original research and extensive review articles—in the refereed literature. His current research concerns the orbital and rotational evolution of solar system bodies, especially planetary rings and the small bodies of the solar system (dust, satellites, comets and asteroids). Using ground-based telescopes and spacecraft, his students and he have discovered dozens of irregular satellites and several planetary rings.

Burns edited *Icarus,* the principal journal of planetary science, between 1979–1997. He edited two books, *Planetary Satellites* (1977) and *Satellites* (1986). He currently sits on the editorial boards of *Science, Icarus* and *Celestial Mechanics & Dynamical Astronomy.* Joe has served on many NASA scientific advisory groups and two terms on the Space Studies Board of the National Research Council (NRC), chairing its Committee on Planetary and Lunar Exploration; the latter wrote the NRC's first planetary exploration strategy in 1994. He also sat on the executive committee for the 2003 planetary decadal report and was a panel member for the astronomy community's 2001 decadal strategy. He has been Vice President of the American Astronomical Society; earlier he led its Divisions for Planetary Science (DPS) and on Dynamical Astronomy (DDA). He chairs the International Astronomical Union's Commission on celestial mechanics and dynamical astronomy. Burns is a fellow of the American Geophysical Union and of the AAAS, a member of the International Academy of Astronautics, and a foreign member of the Russian Academy of Sciences. He has received the DPS's Masursky Prize, the USSR's Schmidt medal and several NASA awards for research achievements.

Funding. Professor Burns's current personal research support comes solely from NASA. He has held a Planetary Geology and Geophysics grant for theoretical and dynamical modeling for many years. He is funded as an imaging team member of the Cassini mission at Saturn by the Jet Propulsion Laboratory. These grants pay for two post-doctoral associates and a graduate student, and part of Burns's summer salary. His work as an imaging team member on the European Rosetta comet mission is unfunded. Burns has previously received grants from the NY Council on the Arts, NATO, the National Research Council' Soviet Exchange Program and the NSF. As Vice Provost for Physical Sciences and Engineering, Burns is Cornell's cognizant administrator over about a dozen interdisciplinary research centers, most of whose primary grants are from the NSF.

DISCUSSION

Chairman UDALL. Thank you, Dr. Burns. I want to thank the panel in general. We will move now to the period where we will ask a series of questions. I think we are going to at least have a couple of rounds, and perhaps a third round, depending on what is happening on the Floor. And Dr. Stern, I am going to start with the rest of the panel, but I want you to know that we will come back to you. And since we have the panel here, I would like to ask each of the witnesses, you heard Dr. Stern testify about his vision priorities. I would like ask each of you what, in your opinion, is the most important issue that Dr. Stern needs to address as head of the—Science Mission Director.

I should say I am yielding myself the five minutes here, and we will move to Mr. Calvert. So we will start with Dr. Fisk and move across.

MOST IMPORTANT ISSUE FOR SMD

Dr. FISK. First of all, let me state that I have great faith that Alan's going to make really good decisions, and the reason is, I believe he understands from his experience, having been a working P.I. and having been from the community, understands the issues that we are facing in the community.

In very simple terms, the issue is—and the one we have flagged throughout this—these statements, in fact, is the balance what we are doing at the moment and what we—and how to protect the future of this program. And the future of the program is in people, the future of the program is in new technologies. The future of the program is in new missions, and we have to create the right investments in that future in this current budget in order to make sure that the space program goes on and is productive for the decades to come.

We are not ending this adventure. This adventure is only beginning, and the question is, how do we make sure that we are doing today protects, enhances and makes possible that future?

Chairman UDALL. Dr. Illingworth.

Dr. ILLINGWORTH. Yes, thank you.

I think all of us who have been involved in space missions recognize that there are very long lead times, often decades or more. I actually was involved in one of the first meetings that was organized for JWST in 1988. It would be 25 years before we launched that program, and this is not uncommon.

And so, a lack of funding profile in the future, unfortunately, eats the seed corn for the future as well. That if we are at the position where we are not building a strong people base, a strong technological base, we are placing our future program at risk in ways that are not immediately obvious.

Fortunately, I think Alan and his group, because of his recognition of this, having been a working P.I., is strongly concerned about this, and the statements that he has made over the last month, and moves that he has done in bringing in new people, I think have been very good. But it will be a challenge, because it also requires resources to do this. So it is recognition one, and then resources two. Thank you.

Chairman UDALL. Dr. Baker.

Dr. BAKER. I believe that the amount of money that is available for science in NASA, $5.4 billion or so, is a tremendous amount of money, and can be used more effectively than it is being used.

I believe that number—the steps that Alan has outlined in his testimony and in his public statements, I think, has the potential for utilizing those resources very, very effectively.

I think that taking steps at the smaller end of the spectrum requires less dollars, but has dramatic effects, the sub-orbital program, the research and analysis. Smaller missions such as Explorers or systems—science pathfinders, these are things that can be worked on, can be relatively readily remedied, compared to some of the larger, more challenging flagship missions in the—so I strongly support what my colleagues have said, and I believe that, from what I have seen, Alan is taking some very good initial steps in this direction.

Chairman UDALL. Thank you, Dr. Baker. Dr. Burns.

Dr. BURNS. I am going to say much the same. Mainly, we need to support the core infrastructure, especially R&A, research and analysis funds. Making those funds difficult to obtain affects, especially people who are early in their careers, and so we are seeing youth drift away.

And that is something—the, you know, if the work force drifts away to other areas, and if technology development lags, the loss to the program will become increasingly irreversible.

Chairman UDALL. Thank you, panelists. I am going to, at this point, yield five minutes to the Ranking Member, Mr. Calvert.

MEASURES TO REDUCE MISSION COSTS, SPECIFICALLY, MANAGEMENT, OVERSIGHT AND RISK REDUCTION

Mr. CALVERT. Thank you, Mr. Chairman. One thing that we heard, I think, consistently through the panel was reducing mission costs.

I think I will start with you, Dr. Stern. From the perspective of management and oversight and risk reduction, what measures can NASA take that would provide meaningful help to reduce mission costs? And maybe you can provide some management examples of, you know, is there too much risk reduction work, do you believe there is unnecessary paperwork, other costs that are imposed upon these programs that make the investment impractical? And after you answer the question, I will ask the panel to add to the answer.

Dr. STERN. Yes, sir. Well, you and the panel members had pointed this whole problem out, had your finger on something very important. We would, in fact, despite how ambitious, with 93 missions in development or flight, and our program is, we would, in fact, have more missions in development were we better able to control costs on the same budget. And so, I am setting out to do that.

Really, our missions in the space science directorate fall in two categories. There are principal investigator led missions, and then those larger missions that are done strategically at the centers.

With regard to the centers, Administrator Griffin has wisely put in place a new policy that our cost estimating will be done at the 70 percent confidence level, a much higher confidence level than in

the past. This causes us to have a greater degree of realism as we budget for missions.

We have to marry that with stronger controls so that we stay within that, but at least we are going to be able to begin now with a much more realistic view of what missions cost and don't have unrealistic expectations that are dashed.

With regard to principal-investigator led missions, some of those have also run into problems. And we put in place a couple of things that I think will help.

First, in this new Explorer announcement of opportunity that we have just called for, and which will be out later this year, we are calling for a minimum experience level for the principal investigators themselves. These are the project leaders, the scientist that runs the project.

Previously, there was no minimum experience level, so a scientist who had not been involved in space flight could write a sufficiently good proposal and lead a team to a win, and sometimes that gets you in trouble. You know, you may wake up in the morning and want to do brain surgery, but it doesn't mean that you can do it. Space flight is an art, and I think this is an important new step that we are taking.

Let me mention just one other—we are going to be willing, in the future, when missions get into trouble, and a principal investigator is not controlling the cost of their mission, to consider and then execute on changing the principal investigator.

And this would be a very strong feedback loop, because to the principal investigator, and I speak as one myself, having been involved in 24 space flight missions, that the only incentive for the scientist leader of the project is to collect and analyze the data and make discoveries, not to carry out the project.

And so, the control mechanism that we will put in place, where the principal investigators know that their job is on the line as the leader if they can't perform, if their view of a P.I. led mission is that the P.I. is led around, then they are at risk, and we will find somebody who can do it better, close and finish on schedule and on cost.

Dr. FISK. Just sort of a corollary statement, perhaps. It is kind of two different directions that you can go at when you think about what missions are going to cost. You can worry about what they are likely to cost in advance, you can cost improperly. We haven't done that very well in the past, and I think there are a lot of things in the works at the moment, even in the future decadals and so forth, which will do a better job on that.

But then the question is, does it have to cost that much? Even if it is estimated correctly, did it have to cost that much? And the question is, were there things that we could have done in the management of the program, or the execution of it, not only to control the cost—we have an estimate, we try and reach the estimate cost.

You say, was that a success? Well, it was a success. We reached the—we got the cost right. But perhaps there was a way to do the mission more efficiently, and that would be even a better victory. Not only did we come in on cost, but the cost that we thought it was going to be, we either executed it for less, or we found a way

to manage this program in such a way that the cost was reduced. We got more science for our dollars.

I think more emphasis on that latter point needs to be made, and it comes down, particularly in this area of small and moderate missions. The question is, are we doing things that actually do reduce risk, or are we, in fact, managing in such a way that we are comfortable? We have reviewed it, we have paperwork. We are sure that nothing will go wrong, but we wasted money in deciding that because it either didn't—it didn't add to our risk reduction.

And I guess what the community is sort of asking of NASA, choose experienced P.I.s, that is a good thing. But if they are really experienced, let them do the program in such a way that they can produce this in the most cost effective way possible.

And so, there is an issue, then, of sort of driving—getting a partnership with NASA that we get the missions for the least cost, maximum security, minimum risk. Find the sweet spot.

Mr. CALVERT. Thank you, Doctor. My time has expired. I will come back for the second round.

Chairman UDALL. Thank you, Ranking Member Calvert. I would like to turn to Dr. Stern at this point. Doctor, in your testimony you talked with a lot of enthusiasm about being an advocate of human exploration, and then you went on to state that one of your three guiding principles for SMD is to help the *Vision for Space Exploration* succeed.

PLANNED CHANGES IN THE SCIENCE MISSION DIRECTORATE

In specific terms, what changes do you plan to make to the goals, priorities and plans of the science mission directorate to help the vision for exploration succeed?

Dr. STERN. Yes, sir. Well, I see two things that we should be doing. The first is supporting the *Vision for Space Exploration* by providing the knowledge necessary to return to the Moon and to Mars, particularly issues of astronaut safety.

Whether it is in heliospheric studies, understanding the Sun, the radiation environment, for example, or understanding the properties of lunar—the size and density of the Moon, toxicity of Martian soils, whether Mars is biologically active and presents a threat to our astronauts, et cetera. That is one area.

The other is we need to build a lunar science community. Really, there was a very strong lunar science community during Apollo. And—but when the Apollo program was terminated, the lunar science research and analysis funds that went with that, and the data analysis funds very quickly tapered off. And today, there is only a small remnant of that lunar science community.

The Moon is a fascinating world. An in member silicate planet, it has a kind of tenuous surface boundary exosphere that is the most common type of atmosphere in the solar system. Its origin is intimately tied to the origin of the Earth, and the giant impact that we believe occurred to create the Moon. I could go on and on.

This is a ripe scientific area, waiting for us to help it flower, in the same way that 15 years ago the decimated Mars science community from the 1970's was brought back by a series of a robotic Mars missions, beginning after the demise of the Mars Observer.

And now, we have a very strong Mars science community. I want to do the same with lunar science.

Chairman UDALL. Any of the other witnesses care to comment? Doctor?

Dr. ILLINGWORTH. Yes, thank you. At—maybe at some slight risk of disagreeing with the A.A., but I would like to comment on this.

In the sense that, while I think there are opportunities with regard to lunar science, it is very important when opportunities come up, they are chosen for other reasons, that the science community think about that in the context of its broad goals.

And so, this is obviously been something that has happened recently, as folks have started to think about missions on—that we would put on the Moon versus elsewhere. But it does need to be done in the broad context. I don't think that we want to find that we are driven to do things on the Moon because we are there. It is not of the higher scientific priority.

So, encompassing all solar system objectives, for example, discussing that and choosing the opportunities that arise from having access to the Moon is good, but in context. Thank you.

Chairman UDALL. Dr. Baker.

Dr. BAKER. I would like to just comment—I had the privilege of chairing a recent ad hoc committee for the NRC that was looking at the radiation risk for the human space exploration program. I just want to endorse what Dr. Stern said. I think that the heliophysics community in particular is excited, and I think quite capable of developing new predictive models, and I think would gladly undertake the effort to help provide information that would be very enabling for the *Vision for Space Exploration.*

But as with all things, this has to be balanced against the other basic kind of understanding that we need, and the programs that we have talked about today, I think, can contribute some of that basic knowledge that can then be converted into very effective predictive models.

Chairman UDALL. Dr. Fisk.

Dr. FISK. I just have one comment on the scientific activities that are—that you—on the lunar science, and the exciting things that can be done there. The science mission director some time ago asked the NRC to do a study on science to be done on the Moon, or lunar science to be done in advance of and at the beginning of the human exploration of the Moon.

That report will come out shortly. There was an interim report earlier, and if you—I haven't—I can't comment on the final report, but the interim report pointed out a number of very important scientific topics that involved the Moon that need to be pursued.

Chairman UDALL. Dr. Burns, do you have anything to add?

Dr. BURNS. Well, the science that can be done on the Moon is useful, certainly. I think, on the other hand, that there—the—we need the background knowledge in order to have a successful human exploration program, and I think that if the purpose of being there is to get that knowledge, then probably those funds should come from the exploration program rather than from the science director.

Chairman UDALL. The Chair recognizes Mr. Calvert for five minutes. Thank you.

UNDERSTATING TRUE COSTS

Mr. CALVERT. Thank you, Mr. Chairman. I am going to stick with the mission—for a little bit. Dr. Illingworth, obviously you commented about astrophysics is at risk because of the drop that we may experience down the road, but one of the things that I want you to comment about is the understating, sometimes, of true cost when we get in these large programs, and, specifically, the James Webb Space Telescope which now is, I believe, four times the cost that came up in the decadal survey, four times the cost.

And obviously when we in Congress try to determine what dollars we are going to set aside for future missions, it makes it extremely difficult when we have to, basically, take all the money out of these various programs and fund what you obviously believe is a very important instrument that we are going to put up. Any comment about that? Because I think that is probably the most obvious one out there that is just out of whack right now.

Dr. ILLINGWORTH. Yes, certainly. I think that cost estimation is critical to our credibility as a community, and to NASA's credibility. We work together on these activities, and NASA provides input to the decadal survey. I think the input that was given then was not optimal, and I think that we in the community and the folks in the survey didn't ask the right questions, or think in the right context.

And as I mentioned, JWST was a very significant example, but there are others as well that we are dealing with that has led to major changes in the current program because of those collective cost growth.

And though think the crucial thing here is, of course, getting independent estimates of the cost, thinking about the costs over the full life cycle, over the 10 to 15 years that the decadal survey is referring to, and not just construction costs. Asking questions of the proponents and trying to fully understand what it is they are proposing.

I think if we understood the costs better as we are discussing the science missions, it would also help us frame the resulting priorities much better, that we would not bring forward a program that was so much larger than is likely to be doable in a given decade, with so many attendant problems that come from that.

So there are, I think, ways that this can be done. I think just by asking the right questions, by thinking in the right way, and I get a very strong sense from all the folks who are thinking about the next decadal survey that we are all on the same wavelength here.

We do not want to repeat what we have done in the last two or three Decadal Surveys. We want to get more accurate cost estimates. We want to test them independently. We want to have people involved who have recognition of mission cost development in the process, and we want to have NASA work with us on that and try and give us the best cost estimates based on their very extensive experience.

STATUS AND IMPACT OF DELTA 2 LAUNCHER

Mr. CALVERT. One of the things that we are going to experience here in the near future, obviously, is the discontinuation of the Delta 2 launcher, and what that will do to launch costs, how are we going to manifest payloads, and what are we going to put it on? I would like to hear some comments from you. Obviously, there are some folks out there trying to come up with less expensive ways to get to a low-Earth orbit, but I want to hear from you.

Maybe we will start with Dr. Stern about how are we going to do this here in the next decade?

Dr. STERN. Yes, sir, it is a very important issue. Our Delta 2 inventory allows us to fly out all of the missions through 2012 that we planned to fly, and we do have some smaller launchers, for example Pegasus and Taurus. The Pegasus launched two small explorers this year, including the A mission that Dan Baker spoke about that just launched last week and is doing very well on orbit.

We are additionally—I mean the Agency—looking at some alternatives to, or additions, to those possibilities to give us low-cost access to space again for small and moderate-sized missions. Those decisions have not been made, but I can assure you that it is important, not only to the science-mission directorate, but to the larger agency.

Dr. FISK. We are all encouraged that NASA has got this on their agenda to do because, I mean, there is a very simple problem here that the range of the Delta 2—this has been the workhorse of the science-mission directorate since the beginning of the space program, and so everything is sized for this, whether it is the size of your chambers that you build satellites for. And so the idea is, if you don't have that capability, there is a whole range of things that you will not be able to do, and there will be a whole range of things that you will have to make adaptations to your infrastructure to be able to do in the absence of that vehicle.

So it needs to be a problem that is solved, and I—it has to be a robust solution, and I—you know, this is not something the Nation—and the Delta—it can't be simply, you know, keep the Delta 2 alive, because that will probably be too expensive of a solution. Someone needs to comes up with a less expensive solution, or at least comparably expensive solution, and the assurance needs to be there to have that happen.

Dr. BAKER. I would like to comment I support strongly the idea that the Delta 2 provides the sweet spot for many missions in planetary science and heliophysics. I also believe that if we lost that kind of capability, we are going to—this is going to propagate through the system in a number of ways.

Going to larger launch vehicles immediately adds tens of millions of dollars to the mission cost, and by sort of taking the cap off of mass constraints and things like that, it can also allow for unexpected growth in mission that—just as I say, it compounds itself over and over again, and I think that we would be well advised to try to restore that capability or make sure that we have something that is very comparable to the Delta 2 to enable these missions.

Dr. BURNS. The same thing, especially for the low-cost mission like Discovery, this is a critical issue, because in percentage-basis,

once you start increasing your launch costs by a few ten of missions of dollars, percentage-wise, that is just staggering, very damaging.

Chairman UDALL. I think if the panel is willing to do so we will engage in another round here. It is been very helpful. Dr. Stern, let us talk a little bit about New Horizons, if you would, and I would note for the record that of you many talents, you have also been considered for the astronaut corps, and I don't know whether that is still on the possible list of undertakings that you would pursue. I know you have got——

Dr. STERN. Kind of a busy day job right now.

Chairman UDALL. Yes, you have got a day job, but we will see what we can do. I think there are some people in the country who would like to send Congressman Calvert and myself to Pluto, but that is another discussion topic.

Dr. STERN. I would love to have you bring a sample back.

APPLICATION OF SPACE RESEARCH EXPERIENCE TO NASA SPACE SCIENCE PROGRAMS

Chairman UDALL. Are there any lessons that you have learned from your space-research experience that you want to apply to NASA space-science programs, and if you see some of those, would you outline them a little bit for us?

Dr. STERN. Sure, absolutely. Well, it is been a privilege for my peers and for the Agency, when I was on the other side, on the so-called receiving end of the bureaucracy, to be given the responsibility to lead space missions and space instruments, and I found two things were absolutely required to do a good job: one was a complete commitment of time and resources, personal time and resources, on the part of the PI to the project; and to realize that when you have been entrusted with that kind of responsibility that other aspects of you professional career should be secondary to the very great responsibility of carrying out that mission. For example, I made a conscious and public decision to reduce the rate at which I was writing research papers while we were getting New Horizons built, and I think that PIs would be wise to do that in general and to keep their eyes on the ball, and to get commitments from their university or their institution to remove them from management or teaching responsibilities. After all, we are talking about entrusting those individuals with $100 million to $1 billion project. This is big science. It is a big enterprise by any standard.

The second and other area I will speak to has to deal with being the adult in the room or being able to say no to control requirements, and not just the scientific requirements on a mission, but also the engineers who oftentimes or almost always want to please, and yet sometimes, I found, that we went a little overboard in that regard, and I was able to make a contribution to simplify what we were doing and that always paid off, because in the end, we were always short on money trying to finish, and some of those decisions made early on that were painful really paid off because in the end, what matters is that you get a successful mission out of it, and you know, the best gilded lily that is still a bird on the ground doesn't get you very far in terms of scientific return. I would offer those two things.

Chairman UDALL. Thank you. Those were very insightful, and you certainly come with a great deal of experience, and I think that is why you are the right person for the job at this particular job, given all of your experience as a PI.

I turn back to the panel, and I think this may be slightly redundant, but we really want to drill into this. Over the next couple of months, we are going to be deciding on the '08 appropriation for NASA, and I would like to ask each one of you what is the most important what NASA could do in the '08 NASA appropriation to strengthen the space-science programs?

'08 APPROPRIATIONS PRIORITIES TO STRENGTHEN SPACE SCIENCE PROGRAMS

Okay, we will start over here, Dr. Burns, and we will come back this way. How is that?

Dr. BURNS. I am going to say the same thing again. We have got a consistent message here, and it is we need funding for R&A support, and we need to keep our young people here, and we need to make use of the data that we are getting so that we can understand the places that we are visiting and so that we can better plan our future missions.

Chairman UDALL. Dr. Baker.

Dr. BAKER. Yes, I agree that the research and analysis and suborbital, as we have talked about. I feel very passionately about the Explorer program and like programs. I think that these offer so much. They return wonderful science. They give a chance for hands-on engineering, hands-on science education, and they give the kind of frequency and cadence of missions that really build enthusiasm through the community and through the country, I believe, so I think that is a real place to put resources, if possible.

Chairman UDALL. When I see you, of course, all I think about—well, not all I think about, but a lot of what I think about is the University of Colorado students that I have seen running satellites, being engaged, excited, committed to a long-term career and to the program in general.

Dr. BAKER. We have 60 graduate students, 60 undergraduate students. Many of the undergraduates are operating spacecraft, and it is just a marvelous thing and it builds a cadre of people that go out into industry, and they do marvelous things.

Chairman UDALL. Doctor?

Dr. ILLINGWORTH. Yes, I think I used the word seed corn before. I think R&A is—on the human front, on the people side, is what is critically important here in investing for the future, and this is for NASA's benefit as well. NASA will have its science program, its most striking results, with a strong community behind it. And then I would add to that the Explorer level, the cost-capped, moderate-sized missions as being very important for getting returns quickly, and providing very high scientific leverage for the money.

Chairman UDALL. And Dr. Fisk.

Dr. FISK. I am probably bolder than my colleague here on this issue. Let me come at it from the very top. I hope when the '08 budget is considered, you will keep recognizing how much money NASA has lost from what the President said it could have for its budget when he announced the vision for exploration, and I am

talking several billions of dollars. And then, if you add to the things that were not in the budget, such as the return-to-flight of the Shuttle and new initiatives, such as Earth science, you are way off. And so, if you, then, can recognize at that level, then of course, it feeds down to the science. Science can have some of its funding restored, and the investments that need to be made at that time are the kinds of investments—there are two investments, actually. A lot of things were decelerated that should be accelerated, and the R&A funds and the future of the program needs to be restored.

But I don't think we should lose sight of the big picture here, which is science is only one of the abused parts of the NASA budget, and the whole budget is several billion dollars short. It is several billion dollars short from the authorization amount that your committee put into effect when you authorized the New Vision, and we have to drive it back to those kinds of levels.

Chairman UDALL. Thank you. I am going recognize Mr. Calvert here, again, but I just want to make a note that I was anticipating leaving it to the end of the hearing, but I think this appropriate on the heels of what has been said, that Chairman Gordon and myself have written to the President, asking him to meet with Congress to address the funding challenges facing NASA. Ranking Member Calvert and other Members have made a similar request. We will see where that leads.

It is my pleasure to recognize my friend and the Ranking Member, Mr. Calvert, again, for another five minutes.

Mr. CALVERT. I point out to my friend and Chairman that also we have an appropriations process that we need to support, and I hope that all of you—I know that Dr. Stern is in the position of supporting the President's budget, and I would hope, at the very least, we can hit that mark. But as you know, under the continuing resolution, we took a $500 million hit, and we want to make sure that our friends don't believe that that is the new baseline for the NASA budget, or all of the NASA budget will be deeply impacted even worse than it is today, so we need to hear support to educate Members of Congress to the importance of it.

While we are on the research and analysis activities, and obviously you all believe. I will turn to Dr. Stern for this answer, but he can listen to it. What do you believe is the metric or rule of thumb that should be used to suggest how NASA establish the appropriate amount of money to put into this, and should it be a fixed level, should it be money provided by a certain percentage? What are your suggestions, because that is certainly an ongoing, I am sure, discussion within your committee.

R&A BUDGETING

I will start on the right this time.

Dr. BURNS. Thank you. I really appreciate you always starting with me.

The planetary decadal panel considered this issue, and as a rule of thumb, they felt that something like 25 percent of the budget that is being used for the mission costs would be an appropriate percentage, and we are well below that. We were well below it even before the fiscal year 2005 cut in the R&A funds by 25 percent.

Mr. CALVERT. Dr. Baker, do you agree with that?

Dr. BAKER. Thank you for keeping me to second, but it still hasn't helped me too much to come up with a formula to answer your question but my impression is that we were better a few years back than we are now. I think that the kinds of cuts that have occurred and the kind of bailing out of other things at the expense of the research and analysis across the board in all of the disciplines has been very, very detrimental. But I think we recognize that there is too little now. What would be optimal or what would be healthiest? I don't think that has been totally established. Each of the disciplines treats research and analysis somewhat differently, and I am very encouraged that—again, that Alan Stern is focusing on this and have a person, Yvonne, who is really going to focus on R&A specifically and look at the question that you have asked in a very systemic way. I think it is very important.

Dr. ILLINGWORTH. Yes. Let me add my thoughts on this. I would like to distinguish that there are two components to the R&A area. Very broadly, one is the data analysis that goes explicitly with the operating missions, and NASA has been very good at funding these and with the goal, of course, that one maximized the scientific return from a very substantial investment by the Nation in these projects.

I think that the 25 percent number would be wonderful to have. I think none of our current missions come close to that. I suspect that it would be—I actually think it would be useful to do an assessment of this question and ask what may well be needed. There was one many years ago, in fact, two decades ago, that was done for Hubble, but I am not aware of any more recent assessments, and maybe this is something that NRC might like to do across the various programs.

The second part, of course, is R&A itself, which tends to be a grab bag for a lot of activities, from technology development, to theory, to reworking data sets. And those individual elements, probably, are the areas that need to be considered instead of looking at the overall picture, and trying to assess whether or not the funding that is being put into those areas, like theory, is really adequate for the returns that we are getting for the investment that we are putting in on the mission side, and the data analysis side. Certainly, I think my theory colleague think it is not, but doing it—finding metrics to do that is a challenge, but it may well be that the right approach here is to think about doing this in a somewhat systematic way.

Dr. FISK. I have a similar sort of answer. The mission-operations data-analysis costs are the easiest to figure out because you sort of know what it is that you are costing to operate the mission, and you want to get the maximum number of years out of things, so you get the maximum return on investment. You do that calculation, and you add it up.

One of the questions that is the hardest one is on technology development, training of the next generation, theory and so forth. It all comes together. And there, I think you have to go in sort of discipline by discipline, and you have to ask yourself the question—and this is a question that NASA can perform the analysis—and say what does this community need to have a future? If the community is aging and aging rapidly, and there needs to be an infu-

sion of new talent, that focuses some of that money on this. If it is not, then, maybe less money is needed. You need theory because no one knows what it is this data means. You know you need that, and so on. But you need—it will vary from discipline to discipline. The analysis needs to be made and the funding provided because we all agree on the goal. The question is what is the right way?

Mr. CALVERT. Thank you. My time has expired.

Chairman UDALL. Thank you, Congressman Calvert.

INTERNATIONAL COLLABORATION

Dr. Stern, if I could come back to you, in your testimony you state that you plan to increase international collaboration as a means to advance the priorities of the National Academy's decadal surveys. While international survey—excuse me. While international collaboration can provide scientific and other benefits to both parties, it can also lead to increased costs and delays. What actions do you intend to take to reduce the costs and programmatic risks of such collaborations? And we did talk about this when we met. It is exciting area, and I will give you a chance to talk about that as well.

Dr. STERN. Yes, sir. Well, I do think that there are other ways besides throwing money at problems to increase the productivity of this program, and we are looking at cost controls, as I spoke to earlier. We are conducing a zero-based review across all four divisions to see what may have been important in the past that is not important now, and international collaboration is yet another way to accomplish that. And to be quite frank, any country who is on an acceptable list, who has the space programming capability that could fly our instruments or collaborate on missions is somebody that I want to talk to. And I mean that to be a win-win. Certainly, Asian nations like the Japanese and the Indians, who are space powers, the European Space Agency, the European national space programs, the Canadian Space Agency, and other all come to mind.

One important, new element of international collaboration that addresses what you are asking about is the possibility that we could collaborate, instead, at the hardware level within a mission, where different parties, NASA and a foreign partner, for example, divide up who builds which part of the spacecraft and the payload. Instead, we would collaborate at higher, more strategic level, at a mission level, so that for example, a given foreign partner might want to build an astrophysics missions that we are very interested in and that is close to something in our decadal survey, and yet, we might be able to build a mission, a planetary mission for example, that is of interest to that party. And they would go about their business; we would go about ours; but have a science team formed from both nations or both parties, so that there is very little swapping of hardware, software, technology, but that the science data analysis is win-win for both parties and the field advances more rapidly than might otherwise.

Chairman UDALL. It seems to me there is a great deal of room here in which to maneuver and develop some of these new relationships. I want to commend you for taking a hard look at this and moving ahead.

Would anyone else on the panel care to comment, in particular about the.

Dr. BURNS. I have a couple of comments, actually.

Chairman UDALL. Yes.

Dr. BURNS. One of my concerns is the fact that ITAR automatically considers satellite technology to be munitions under the State Department rules, and that really hamstrings us in interacting with other nations. I have cases of post-docs who have written code, going back to Germany and France, and they can't access the code that they wrote because they are foreigners.

I think another issue is—and this, I know, is a no-no—but a question of whether or not to consider other launch vehicles than American launch vehicles because competition, according to America is a good thing, and I wonder if that might lower the cost in some way.

And finally, of course, we need to be firm and make sure that when we say we are going to do something, we carry it out. At present, we tend to drag things on and then sometimes stop, and that does not make for good international relations.

Chairman UDALL. Anyone else care to comment?

Dr. BAKER. Well, I would just like to add on the ITAR issue that I agree that is seems inappropriate to be stifling what we are able to do with foreign partners, and I know that the NRC is going to have a workshop this fall and kind of look further into what effect this is having on the space program, and perhaps what could be done to remedy it. I couldn't agree more that seizing all possibilities for foreign launchers, for foreign missions of opportunity or a much stronger collaboration, it just seems to me it offers tremendous possibilities of leveraging the resources that we have.

Chairman UDALL. Anyone else? Dr. Illingworth.

Dr. ILLINGWORTH. Yes, just to comment on this, I think it really can be win-win with international partnerships, and I actually was intrigued by Alan's idea of creating, in some sense, different missions, ensuring access. I think that is a very good approach. It probably isn't a way that will work, necessarily, for the very largest and rarest missions, but probably more for medium. And there is actually—from my hat that I war as the AAAC Chair, I also think across the other agencies as well, and we are also trying to encourage active collaborations where practical, and particularly with DOE on missions as well, with their science interests. And something, they are also as challenging because of the different approaches and culture.

Chairman UDALL. Dr. Fisk, has it all been said, or would you like to add?

Dr. FISK. I think it is all been said.

Chairman UDALL. There is an old Washington saying that it has all been said, but not everybody said it, so I don't want to cut you off, but I think you all have given us a homework assignment. Mr. Calvert and I have been speaking up here about ITAR and trying to find a sweet spot to give on our national security concerns, but also, we are actually putting ourselves at competitive——

Dr. FISK. I will add to that. I encourage you to do something. It has become a nightmare, and is probably the single biggest impediment to international cooperation in the science program.

Chairman UDALL. I am happy, again, to recognize Mr. Calvert for five minutes.

Mr. CALVERT. Thank you, Mr. Chairman. I agree that we need to come up with some solution on ITAR, but obviously, we both serve on the Armed Services Committee, also, and as we all know, the technology can be not just for peaceful purposes.

STATUS OF EUROPA MISSION

One question, because I am going to have to go out to a markup, regarding the Europa mission, I want to get all of your thoughts regarding the relative importance of sending a satellite to Europa, versus other moons that may harbor water, such as Enceladus or Titan. I know at one time, the planetary community seemed committed to launching a Europa mission, but with the findings from Cassini, has anyone kind of shifted or changed their mind and want to look at something else? I was just kind of curious. I will start with you again, Dr. Burns.

Dr. BURNS. Man, you are my friend. That is a very difficult question because the results that we have gotten from the Cassini mission on both Enceladus and Titan are of great importance. They show the possibility of conditions that are appropriate for the formation of life, both the presence of water and organics and energy sources, much like Europa. I think that there is a benefit for continuity in the program, however, and the Europa mission continues to be a very exciting mission. I believe that since there—NASA has put forward a million dollars for each of four studies to look at the two missions that you mentioned and two in the Jovian system, including the Europa mission. I think we will have an answer to this question.

Dr. BAKER. Thank you. Planetary science is part of what my laboratory does, and I am just struck by what a target-rich environment planetary science is. Everywhere you turn there is something, there is an object, that would just be wonderful to visit in more detail, and I am not prepared to say which of those—and my planetary friends probably can make a judgment about that—but it is just astonishing. Had we the resources, I think there are just a very large number of objects that would be worth investing, and we would learn a great deal about the universe beyond by studying them.

Dr. ILLINGWORTH. Just to note here, this is really an area which is beyond—well, maybe inside astrophysics. Astrophysics starts outside of the solar system, so I don't have an explicit comment.

Mr. CALVERT. Dr. Fisk.

Dr. FISK. Well, the Europa mission, you know, was a priority in the decadal survey, and I have, sort of, two comments on that. One is it was, and we need to take that into account because the community consensus that is where you should go. The second thing is it probably points out one of the issues that we need to always deal with in decadals, and will have to deal with the next one that is starting up in a year or so, which is as new science comes in, how do you decide whether or not you should stick to your previous recommendations or you should move to some other target because we learned something? Well, I think there is a lesson in the Europa

discussion for the future decadals, and also, that we need to simply make intelligent decisions now as to how to do this.

Mr. CALVERT. Flexibility. Dr. Stern.

Dr. STERN. Yes, if I could speak to this point: it is a very rare and special opportunity to be able to fly a flagship in any field, including in planetary science. Outer-planet flagships come approximately every quarter century. I think it is our responsibility as program leaders in NASA to take a careful look at the new data that is coming in, for example from Cassini, and the excitement that that has generated and not simply genuflect to decision made before that data was available. We might come back to the same conclusion that Europa is the mission to fly, and we might find that the community consensus is that this is just such a special opportunity, we only have one, it should be different target.

But I think that we absolutely have the responsibility to make that decision consciously and not implicitly or simply because of a report written years ago.

Mr. CALVERT. Thank you, Mr. Chairman, I have to go to a markup, so I thank the witnesses for attending today and look forward to seeing you again soon. Thank you.

Mr. WU. [Presiding] Thank the Ranking Member, and our new Ranking Member.

CHINESE COOPERATION

The Chairman recognizes himself for five minutes. We had some discussion of ITAR earlier and perhaps we will return to that subject because it is related to what I intend to ask about. Over three decades ago, at a time when our relationship with the then-Soviet Union was not on the friendliest of terms, we initiated some cooperation in human space flight, which has born significant fruit and perhaps given us an opportunity to find areas of cooperation as well as share some costs. Many of the problems that we face in space exploration today whether they are human exploration or robotic missions, you know, the stress is on matching the resources with the mission. The Chinese have a vigorous space program going as well as a vigorous economy. There are obviously some challenges in developing any human space flight cooperation programs with the Chinese because of the structure of their human space flight program but I would like the panel of witnesses, in particular Dr. Stern, to comment on the potential for cooperation in human space exploration as the Chinese develop their space program and also whether this might potentially help us match mission and budget.

Dr. STERN. Yes, sir. As you well know, the Administrator has said that the return to the Moon and the eventual exploration of Mars by human beings is a program that the United States and NASA is very, very open to international participation. We would like to provide the core infrastructure and have other partners, other nations and multi-national space programs such as the European Space Agency or others come in and participate and add to the value of what we are doing.

Mr. WU. Dr. Fisk or any other of the panelists?

Dr. FISK. This is stretching the limits of my—I suppose like any good university professor, I am supposed to have an opinion on ev-

erything. I mean, it is—I mean, clearly you raise the early issues, which is, you know, how do we cooperate, you know, in ITAR in particular and so that will be an impediment, particularly in dealing with the Chinese. I think the thing the country needs to be most concerned about when we decide whether we are going to do these things cooperatively, how we are going to do them, is the fact that if we choose not to, there are alignments that will take place among other nations whether it is the Chinese and Russians or the Russians and the Chinese, and so various things could take place and we could find ourselves in a space race with the world, and that would probably be an unwise position. So the figuring out how to cooperate in a way that we sort of use the world's resources is probably not only a wise offensive strategy on our part, it is a wise defensive strategy.

Dr. ILLINGWORTH. Let me just comment on this. You know, ITAR is a significant concept for the community and it is right at the working level. Alan mentioned the possibility of doing missions where the primary responsibility was taken by an international partner but there was a joint science team, but that in itself poses problems because the science team that we had that involved international scientists, getting into many of the technology areas may determine whether or not one can carry out the science and those sort of discussions are very challenging to do with ITAR regulations, and it has been an impediment to bringing together international teams, even on smaller programs. So it does have a substantial impact at a level which is often not recognized. It narrows the expertise and the involvement that you would like to use on these programs. So, the hope, of course, is very broadly in the community that there are ways in which this can be changed for a lot of the missions that we would like to carry out.

Mr. WU. Dr. Baker.

Dr. BAKER. I would just say that cooperation done well and wisely can be very cost-effective and can use scarce resources very well. I think forcing cooperation in unnatural ways or ways that are not going to be well thought out could very well drive up the cost to the United States and so I think that it is very, very important to think through thoroughly what is the appropriate role for whatever foreign partner we might have and whatever program we have.

Mr. WU. Thank you.

Dr. Burns.

Dr. BURNS. I will just comment on a sideline of this and that is the fact that it is interesting to note that within the next year we will have three foreign spacecraft in orbit around the Moon. We will have Japan, China and India, and they will provide a very significant part of humankind's understanding of what the Moon is all about and thereby aid our exploration program and I think we need to carry that into other spheres.

Mr. WU. Thank you very much. I see that my time has expired and I yield to the Ranking Member, the gentleman from California.

LESSONS FROM ASTRONOMY

Mr. ROHRABACHER. Thank you very much. I apologize for not being here earlier. I was at another hearing where I am the Ranking Member of that subcommittee, as this often happens. I am

jumping from the human rights report of the State Department to scientific exploration, and that is just the jumps we have to make.

First of all, I have been a big supporter of astronomy and in fact, the more I have been involved with astronomy, the more I have been impressed with what we learn from astronomy is actually important. I always remind everybody that I have this much knowledge about that much but I don't have this much knowledge about anything, and maybe one of you could tell us, the fundamental importance of having an understanding through astronomy of what is going on out in the universe because at one point it was like—it was this great revelation when they told me, well, if you see it out there working out there, we actually then could understand how molecules and how the basic building blocks of the universe at our level down at a molecular level in our bodies and various things here work. Maybe somebody could just give a 30-second or one-minute explanation of that to me. Does anybody want to jump forward? Don't we have anybody that——

Dr. BURNS. I am both a Professor of Engineering and a Professor of Astronomy so let me start as an engineer. As an engineer, you want to know how planets work, and the way you learn how things work is, you look at a batch of them. You don't look at one car, you want to look at a variety of cars in order to see the different characteristics and what those characteristics lead to, and I think it is essential in that sense that we go and explore various planets before we try to understand the Earth. And then my astronomy hat says these are the most profound questions that mankind faces: who are we, where do we come from, and we can only understand that if we see how commonplace the formation of organic molecules and the possibility of life elsewhere might be.

Mr. ROHRABACHER. Are the fundamental principles that we learn from astronomy applicable to the scientific basis for decisions that we make here?

Dr. BURNS. Physics is everywhere. Chemistry is everywhere. I mean, it is the same stuff.

Mr. ROHRABACHER. Well, tell me about that in layman's terms, if someone would like to——

Dr. FISK. Let me try from a slightly different tack here. Astronomy at the moment, real astrophysics, is—and I am a practitioner only——

Mr. ROHRABACHER. So astronomy is astrophysics?

Dr. FISK. They are becoming completely synonymous for the following reasons, that is, we are now—as we look out, you know, and see space and there so—there are very fundamental problems that have been surfaced, even in the last decade. We only see a few percent of the universe. We don't know what the other 99 percent is. There is evidence of a dark energy, as it is called, which is causing the expansion of the universe in a way that we did not anticipate. We don't know what it is. And what that says to you, I mean as a practitioner—I am not a practitioner. I never venture beyond the orbit of Pluto. That is just—that is my domain so I am talking about things that are beyond this point now. And you say these guys are going to find that there is a very basic physics which governs everything that we don't understand. We don't know what it is. And when we do, the laws of physics as we know them will be

revised. Now, I can't think of anything more compelling than to try and figure out what the laws of physics are because the laws of physics concern everything that happens right down to the microscale and the molecules and all this stuff, and so you need to be able to—they are probing the most fundamental questions in the universe about how does it work and we thought it worked the same everywhere, all scales. Now there is something we see that doesn't fit; we don't know what it is. And when we find it out, the world will change.

NUCLEAR ENERGY

Mr. ROHRABACHER. Does this have anything to do with Einstein and the nuclear bomb? How do the physics of understanding what is going on way out there have anything to do with our ability to create nuclear energy?

Dr. FISK. How about if we leave Einstein in but we leave the bomb out?

Mr. ROHRABACHER. Okay. Nuclear energy then. Leave the bomb out.

Dr. STERN. If I might chip in, I think to answer your question, Mr. Rohrabacher, there are a couple of—if you take the long view, there is both economic and a strategic importance to doing astronomy. You know, the guys that were playing around in physics labs in the middle of the 19th century with electricity had no concept of what that would yield for the future of the economy, and the same can be true of quantum mechanics in the 1920s and how that has impacted our view, and more importantly, impacted our ability to build the electronics that we all depend upon today, and the same way the energy sources that we see in the universe may have some application in the future that we can't anticipate and that is the advantage of basic research is that it often yields something far beyond your imagination which completely transforms the world, and from a strategic standpoint, I think it is crucial that the United States has been the Nation that has led in astronomy, astrophysics and planetary science. As we go forward into the future, in future generations and future centuries, when folks are taught science, school children in whatever nation they are in, those discoveries that opened up the universe and that opened up the solar system will always be tagged with American space missions, American facilities, American scientists like Dr. Mather, who is here, and his collaborator, Dr. Smoot, who really put the point on what was only a theory and established the Big Bang with observational evidence and won the Nobel Prize for it.

Mr. ROHRABACHER. Just to note that astronomy is also something that we can—and which I have tried to do as a Member of Congress, find ways of getting young people involved in something that they can actually do. You know, young people can actually have a telescope. Young people can actually go into planetariums and look into this. So I had legislation in the past that passed. That that in itself is of great value in exciting young minds, giving them something that is specific they can do. Let me—something else that I was involved in, in terms of to delve into this, was trying to instruct NASA to conduct a survey of Near-Earth Objects that might hit the Earth, and at this point I understand that there is a tele-

scope in Puerto Rico that the Administration is thinking about shutting that may indeed be contributing to our ability to find and catalog near-Earth objects. Is there something move to shut down this telescope in Puerto Rico and——

ARECIBO RADIO TELESCOPE AND NEAR-EARTH OBJECTS

Dr. STERN. I believe you are speaking about the Arecibo radio telescope that is used for radar purposes, and the radar is used to better determine the orbits of some of these near-Earth objects so that we get a better bead on whether or not they have a potential for hitting the Earth.

Mr. ROHRABACHER. Is there some plan to shut that down?

Dr. STERN. Yes, but it is actually a facility in the budget that is supported by the National Science Foundation that is at risk. The NASA support for that program, the grants, for example, that we have in the science mission directorate, are not affected and that really is an NSF issue.

Mr. ROHRABACHER. So let me note, in order for us to support basic science, quite often we have to try to show people that there is a direct relationship, a cost-benefit relationship to our life and our safety, let us say, as part of our life, of course, and that to have a telescope that one of the services it provides is helping us to track near-Earth objects or to catalog them, I think it is a great disservice to try to shut something like that down and I would hope that you keep that priority in mind.

I would like to ask a question about the study—am I out of time? Can I ask one more question? Would the Chairman indulge me one more question?

I understand that we have determined that there are warming trends going on on Mars and other planets. Maybe Dr. Burns or Baker could let me know. So we have determined this, that the other planets are becoming warmer or at least Mars is becoming warmer, and how does that fit into calculations that our own planet may be becoming a little warmer?

Dr. BURNS. Let me, if you don't mind, step back for a moment because I am quite intimately involved with the Arecibo telescope and in fact maybe I shouldn't be speaking because I am sort of biased. The university that runs that telescope is my university and in fact sits under my domain, and that is a crucial facility, as Dr. Stern said, for determining the orbits and surface characteristics of asteroids and thereby helping us avoid the threat, and the problem that has occurred is that the NSF budget has been cut for the observatory and that has necessitated the loss of the radar capability as of this coming October, and that facility originally received its funding from NASA which NASA decided three, five years ago that they were not willing to fund a ground-based facility and so they shifted it over to the NSF. The NSF now says hey, that is not a problem, we are shifting it back to NASA, and there is a discussion between the two agencies. From the standpoint of the science community, we think this is a unique—we know it is a unique world facility and we don't care who funds it but it needs to be funded.

Mr. ROHRABACHER. I think you put us on notice on that and I think we should make sure we pay attention to that admonition.

WARMING ON MARS

Dr. BURNS. Let me move on to your other question. I mean, certainly there are changes that are occurring on Mars in the upper atmosphere of Mars, in particular that suggests there is a deposition of some energy there but the evidence for that is much less than the evidence that we believe we see here on Earth for the increasing local temperatures and so forth and so on. I will let others address that question actually.

Dr. BAKER. I would just comment that I believe that one of the great strengths of planetary science is comparison back with the Earth so what we see in planets in different stages of development with different processes dominating helps us better to understand our own planet.

Mr. ROHRABACHER. Well, if Mars is indeed warming as I have read in several reports, as some of the other planets may be warming as well, it would indicate to me that the warming trends going on in the universe have little to do with SUVs and humankind unless of course we are talking about SUVs and UFOs are the same thing, which I doubt. So it would seem to me that solar activity may have a lot to do with changes in climate on the Earth and other places.

With that said, I thank you very much, Mr. Chairman.

ITAR AND INTERNATIONAL TECHNOLOGICAL DEVELOPMENT

Mr. WU. I thank the gentleman, and I only have one further question, to return to the ITAR subject. Dr. Burns, you referred to that in your testimony and several of the panelists referred to ITAR and its potential effect on international cooperation in space. I want to look at this in a slightly different way. Are some of you concerned that potentially overrestrictive provisions of ITAR have resulted in giving foreign governments an incentive to developing sensitive space and sensor and other technologies that perhaps they would not have otherwise developed and in fact that they then proceed to market around the world as ITAR-free technology?

Dr. FISK. I think the answer to that is categorically yes and I think there are lots of studies on that point that have been conducted by the AIAA and I think perhaps even the Defense Science Board, and it is a concern whether or not we are in fact protecting ourselves or simply encouraging. The science perspective is probably—I mean, I wouldn't make this a science issue. That is an issue of, you know, American space industry and its ability to market its technologies around the world and its competitors finding incentives and reasons for being able not to do that. That is a much bigger issue than just the science question.

Mr. WU. Any other comments from the panelists?

Dr. BURNS. I would just say it is a very delicate balance that one has to play when dealing with ITAR issues. You obviously don't want to go overboard and allow access to sensitive technologies. On the other hand, if you hamper the science and hamper our own activities, that is detrimental to us as well and I worry too much that, you know, we are so worried about the competition that we are weakening ourselves in many of these avenues.

Mr. WU. Dr. Illingworth.

Dr. ILLINGWORTH. Thank you. Yes, I would like to reinforce that because I think this does come back to hurt ourselves. Science engineering, these areas benefit from dialog from competition in a sense of ideas and actual techniques and so where there is not those opportunities to engage in those activities, we are the ones that lose out as much as anybody else or maybe more so because other people have the opportunities for that dialog and that sort of competitive spirit that we may not be able to carry out.

Mr. WU. Thank you very much. I understand that Mr. Rohrabacher has one quick question.

Mr. ROHRABACHER. Very quick. Just for the record, I have no trouble with cooperation between scientists from free countries and from other democratic countries. I think that we have to be very cautious in training scientists who will then return to dictatorships that may be opposed to our way of life and may actually create a threat to Western civilization. I mean, whether or not is a bomb in Pakistan, I would hate to think that we had Pakistani scientists here and trained them how to make that bomb. I would hate to think that democratic countries like our own would use our science and so indiscriminately provide information that we provide the means for a dictatorship like China to set up a computer system that will spy on its own people and put believers in God in jail and be able to control the Internet in their societies when they couldn't have done it without our help, things such as that. So I just would like to make sure that we balance off the pure science isn't an end in and of itself. If it works with people who are tyrants and negative forces on this world, that science is not a good thing to transmit to those people.

So with that said, I thank you, Mr. Chairman, and I am sorry I was a little late but I am running back and forth.

Mr. WU. I thank the gentleman, and I think we all have this concern about appropriate development of technology and national security.

Before we bring the hearing to a close, I want to thank all of our witnesses for testifying before the Subcommittee today. The record will remain open for additional statements from Members and for answers to any follow-up questions the Subcommittee may ask of witnesses. I also ask unanimous consent to insert into the record additional and extraneous material. Without objection, so ordered. The hearing is now adjourned.

[Whereupon, at 12:58 p.m., the Subcommittee was adjourned.]

Appendix:

Answers to Post-Hearing Questions

ANSWERS TO POST-HEARING QUESTIONS

Responses by S. Alan Stern, Associate Administrator, NASA Science Mission Directorate

Questions submitted by Chairman Mark Udall

Q1. Your testimony refers to new standards for the selection of Principal Investigators on space missions. Could you please elaborate on those standards and how they will help to manage mission costs?

A1. NASA is instituting minimum Principal Investigator (PI) space flight experience standards for the 2007 Small Explorer (SMEX) Announcement of Opportunity and all future PI mission selections in order to reduce the inherent risk in PI-led missions with PIs who have never played a significant role in a space flight mission or instrument development. This risk jeopardizes PI-led mission cost and schedule attainability, and can affect mission technical risk level. Cost, schedule, and technical problems in turn adversely affect the frequency of future missions NASA can mount, and jeopardize the viability of PI-led missions. The PI experience standards we are implementing are designed to significantly mitigate these issues to the benefit of NASA and the science community as a whole. Other senior mission science team personnel such as mission Deputy PI, Project Scientist (PS), Chief Scientist, Science Team Lead, and instrument PIs that may be involved in PI-led missions need not meet the same space flight experience standards as the mission leader—the PI—though their experience level will remain a factor in Technical, Management, and Cost evaluations.

There are three parts to the minimum space flight experience standards for a SMEX and all future PI mission selections—the PI will need senior project experience on a project that went into space. More specifically:

(a) A PI will need to have served previously as the PI, the Deputy PI, the PS, or the Deputy PS on a qualifying space project.

(b) A qualifying space project can be a full mission, an instrument, or an experiment.

(c) The qualifying project must have been a space project that has been launched. A space project is one that goes into the space or near-space environment. Space projects include sub-orbital projects (sounding rockets, scientific balloons), orbital projects, and deep space projects. Within these standards, a large number of U.S. space scientists qualify.

Q2. In your testimony, you state that "by looking for ways to increase efficiency within our organization, and within the way we manage missions, we can make new funding available within the President's budget that will enable us to do significantly more." *What fraction of the space science budget (or, alternatively how much money) do you realistically think you will be able to free up through those efficiencies? What's the basis of that estimate?*

A2. Over the three year period from February 2004 (FY 2005 budget request) to February 2007 (FY 2008 budget request), 28 Science Mission Directorate (SMD) flight missions required budget increases totaling $4 billion in estimated Life Cycle Costs (LCC). This is a major source of inefficiency corresponding to approximately 75 percent of one year's budget for SMD. NASA needs to avoid such LCC growth in the future via stronger program management practices. While we cannot specify the exact amount of future mission LCC growth we will avoid, we expect significantly better performance than in the last three years. We also plan to institute smaller dollar (but important) efficiency gains in the management of grant paperwork.

Q3. Immature technologies have been identified as a key contributor to mission cost growth in the past. However, the FY 2008 budget request reduces the opportunities for technology development through the New Millennium and research and analysis programs.

Q3a. Why is NASA reducing the New Millennium and R&A technology development activities?

A3a. The decision to reduce funding for the New Millennium program was necessary in order to make funding available to achieve a balanced, executable portfolio within the Science Mission Directorate (SMD) and to concentrate more heavily on SMD's main mission: producing scientific results that advance the priorities of the four National Research Council (NRC) Decadal Surveys (Earth science, Astro-

physics, Heliophysics, and Planetary science). Specifically, funding was eliminated for the Space Technology (ST)–9 mission since that early (competitive Phase A) formulation and had only reached the Concept Study Report stage. Furthermore, its priority as a technology flight demonstration was lower than the current science missions vying for funding in this time period.

With regard to funding for the Research and Analysis (R&A) program, total funding in the FY 2008 budget request for technology-based R&A activities represents a slight increase over the funding available in FY 2007. The R&A program includes a number of instrument incubator and technology development elements designed to enable future science missions in each of SMD's four portfolio areas. In addition, SMD is looking for ways to increase opportunities, such as through the addition of four new investigations to the Astrophysics and Heliophysics sounding rockets programs, which not only provide training for investigators, but also help develop the next generation detectors required for future science missions.

Q3b. How important do you think it is to maintain a long-term technology development activity in NASA's space science program? What fraction of overall funding should that technology development activity represent?

A3b. It is very important to maintain a long-term technology development activity in NASA's space science program. The funding reductions for long-term technology development in FY 2008 reflect the relative priority and are temporary as NASA works off the backlog of missions already in formulation and readjusts the phasing of its future mission plans. In the future, technology efforts will be more closely tied to specific mission needs, as has recently been successfully demonstrated by James Web Space Telescope. There is not an optimal single overall fraction of funding that should be allocated for technology development. This decision must be made for each science discipline within the context of the community-based priorities and mission plans. In some cases, the reapplication of technologies that are already in hand can go a long way towards meeting the science priorities identified in the National Research Council (NRC)'s decadal surveys. In other cases, significant technology development will be required in order to achieve these priorities. Each science division within NASA's Science Mission Directorate will work with their communities, including the NRC and the subcommittees of the NASA Advisory Council, to identify critical technology needs for specific missions and their relative priorities in the context of the overall science program.

Q4. The science community has commented on the absence of mechanisms by which the community can have input into NASA's space science program through its internal advisory process. A recent National Academies report recommended that "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." *In specific terms, how do you plan to work with the space science community and with internal and external advisory committees?*

A4. NASA restructured the NASA Advisory Council and its Committees and Subcommittees in November 2005 in order to ensure that the Administrator receives advice that is fully integrated across the science, engineering, and business disciplines involved. Over the last year and a half, the communications links between the NAC and its subcommittees and relevant NASA managers remain intact. Each of the science subcommittees of the NAC holds open meetings, which NASA mangers regularly attend and are often asked to make presentations. Further, each subcommittee generates a report for each meeting that, while addressed to the Chair of the NAC Science Committee, is copied to the relevant NASA science division director. Externally, NASA maintains its long-standing relationship with the Boards and Committees of the National Research Council/National Academy of Sciences, as well as the Astronomy and Astrophysics Advisory Committee (AAAC). Members of NASA's Science Mission Directorate (SMD) senior management team are regular participants at NRC Space Studies Board meetings, and at any given time, the Board has several studies underway to provide advice requested by the Agency on science community priorities. The next round of space science decadal surveys will begin formulation next year.

In addition to these formal channels by which the community can provide input to NASA's SMD, the new SMD Senior Management team has begun a series of active townhall meetings with members of the science community as a way to receive informal feedback. These town halls occur across the country, with participants at science conferences and scientists at universities, research laboratories, and NASA centers. Through early June 2007, meetings have taken place in California, Mary-

land, Hawaii and Arizona. SMD has also established an e-mail address to receive questions, concerns, and suggestions from members of the science community.

Q5. The Science Plan for NASA's Science Mission Directorate 2007–2016, *notes that an important question is* "whether the science activities enabled by the human exploration program and identified as compelling by the science community have greater or lesser priority than activities previously planned by [NASA's Science Mission Directorate]." *How do you plan to address this question of priorities?*

A5. NASA will address the question of priorities of science that derives from human exploration activities and capabilities using the results of the upcoming National Research Council (NRC) report on lunar science priorities, "The Scientific Context for Exploration of the Moon." In addition, NASA will ask the next round of Decadal Surveys, beginning with the 2008 kickoff of the 2010 Astrophysics Decadal Survey, to incorporate human exploration-enabled science in their deliberations.

Q6. You have informed the subcommittee that you will move NASA's Near-Earth Object program, along with its associated funding, from the Exploration Systems Mission Directorate to the Science Mission Directorate. Could you please elaborate on your plans for the Near Earth Object program?

A6. The Near-Earth Object Observation (NEOO) program is being transferred back to NASA's Science Mission Directorate (SMD), along with its budget, effective in FY 2008. The Program will be housed in the Earth Science Division (ESD) as ESD studies hazards to the Earth, and NEOs are one such hazard, much like the ozone hole and global warming. While there are currently no plans to alter the budget for the NEOO program, NASA does plan to continue NEOO efforts after 2008 when the current survey down to one-kilometer sizes is completed. That new work will concentrate on reducing orbit uncertainties in catalogued NEOs, finding the few (10 percent) undiscovered NEOs with one km or larger sizes, and locating new but somewhat smaller NEOs. In addition, NASA's Planetary Astronomy grants program supports NEO observations that reveal new physical insights into their nature and our Discovery mission program is evaluating a finalist proposal to visit and sample an NEO.

Q7. Dr. Stern, given the constraints on growth in NASA's space science programs, how do you plan to help ensure the lunar science program is sustainable? Where will the money for it come from?

A7. NASA's FY 2008 budget request includes $351 million over five years for a lunar science research project. This project, part of the Science Mission Directorate's Planetary Science Division, is being designed to provide for the activities outlined below:

1) Ingest and archive of Lunar Reconnaissance Orbiter (LRO) and Lunar Crater Observation and Sensing Satellite (LCROSS) data into the Planetary Data System;
2) competed opportunities for scientific payloads to fly on both international missions;
3) competed opportunities to analyze scientific data from lunar missions and accompanying scientific payloads;
4) competed opportunities to develop technology and instruments to support lunar science studies; and,
5) competed basic lunar science investigation.

Additionally, in 2010, SMD will take over the LRO mission from ESMD for an extended mission of lunar science observations. Funding to create the lunar science project came from the reprogramming of funds from lower priority activities within the Planetary Science Division. SMD is also encouraging the use of its existing orbital assets like the Hubble Space Telescope and Chandra for observing the surface of the Earth's moon.

Q8. In your testimony, you talk about being an enthusiastic advocate of human exploration. You then go on to state that one of your three guiding principles for the Science Mission Directorate is "to help the Vision for Space Exploration succeed." *In specific terms, what changes do you plan to make to the goals, priorities, and plans of the Science Mission Directorate to help the* Vision for Space Exploration *succeed?*

A8. NASA's Science Mission Directorate will support human exploration efforts by funding a developing lunar science community. This effort is included in the Presi-

dent's FY 2008 budget request as a new line item in the Planetary Science Division of SMD, with a total funding of $351 million over five years. That funding will be used to analyze lunar data from the Lunar Reconnaissance Orbiter (LRO) and Lunar CRater Observation and Sensing Satellite (LCROSS) missions, to institute a lunar R&A program, to fund a science-driven mission extension of LRO in FY 2010, and to fund new instruments to fly on future foreign lunar missions.

Q9. NASA has imposed cost "caps" on a number of its small- and medium-sized mission programs, such as Discovery. With the advent of full-cost accounting, are those cost caps still realistic or should they be adjusted? Have the cost caps proven to be an effective tool?

A9. While full cost charges have varied over time as full cost accounting has been implemented at NASA, the cost cap in each Announcement of Opportunity has taken this factor into account. These cost caps have increased in recent years for the purpose of compensating full cost accounting-driven increases in the cost of work done at NASA centers. Additionally, NASA has consistently instructed that all proposed mission costs include full cost accounting.

Cost caps serve an effective role in guiding management decisions both within projects and at NASA Headquarters, and we plan to continue using cost caps on PI-led and other missions for this purpose. Some of the benefits of using cost caps are outlined below.

- Cost caps help bound the complexity of possible missions, thus simplifying the evaluation and selection process by ensuring there is no bias towards those missions with the most complexity.
- Cost caps enable long-range program planning for future solicitations since the maximum cost of each mission is known.
- Cost caps provide clear and strategic limits on how much mission implementation costs can grow after selection without termination.

Questions submitted by Representative Ken Calvert

Q1. Statements made by other witnesses indicate a strong preference for flying frequent small- and medium-sized competed missions, with a flagship mission once every decade. What would you consider to be a healthy tempo for competed, Principal Investigator-led missions?

A1. At our current budget level, the optimum rate for Principal Investigator (PI)-led missions is approximately one to two per year in each of our science disciplines: astrophysics, Earth science, planetary science, and heliophysics. The Agency is taking steps to move towards this level within our available resources. For example, NASA will select three Small Explorer (SMEX) missions, instead of one Medium-Class Explorer (MIDEX) mission, in the next Explorer Announcement of Opportunity (AO), to be released this October. NASA also will release an Earth science PI-led mission AO in the 2008–2009 timeframe. Finally, NASA will increase the rate of sub-orbital PI-led missions, as evidenced by our recent selection of four new PI-led missions in the astrophysics and heliophysics sounding rocket programs. Within the next few months, NASA's Science Mission Directorate (SMD) will select new PI-led missions from the Discovery Program and from the Mars Scout Program. In addition, SMD plans to call for a number of different types of PI-led proposals in 2009.

Q2. With respect to future deep space missions, what is the availability of RTGs (or other forms of nuclear power sources) to power spacecraft? How many would be available over the next decade?

A2. At the present time, the first Multi-Mission Radioisotope Thermoelectric Generator (MMRTG) is being qualified for the Mars Science Laboratory (MSL) rover mission, which is planned for launch in September 2009. Following qualification of MMRTG for MSL, one flight unit and a spare are planned to be ready in time for the scheduled launch opportunity. It is anticipated that use of MMRTG for a large mission to an outer planet destination such as Jupiter would require fabrication of six to seven more MMRTGs, assuming spacecraft power requirements do not exceed about 1 kilowatt at the beginning of the mission, and preferably no more than about 800 watts. NASA would work with the U.S. Department of Energy to determine resource needs and the schedule to meet potential launch dates.

ANSWERS TO POST-HEARING QUESTIONS

Responses by Lennard A. Fisk, Chair, Space Studies Board, National Research Council

Questions submitted by Chairman Mark Udall

Q1. NASA has imposed cost "caps" on a number of its small- and medium-sized mission programs, such as Discovery. With the advent of full-cost accounting, are those cost caps still realistic or should they be adjusted? Have the cost caps proven to be an effective tool?

A1. There have been two principal drivers that have required increases in cost caps: launch vehicle costs, and full-cost accounting, the primary impact of which has been increased management costs of missions. NASA needs to continuously assess the realism of the cost caps, to ensure that quality missions can be proposed and executed. The realism of the assigned cost caps can be validated by the proposals submitted. So long as the science community can propose and subsequently execute exciting missions within the assigned caps, the cap is realistic. As a management tool, the caps have been highly effective. Missions with caps have far fewer, and less-severe cost overruns than do the larger missions, which are generally not initiated with a cap in place.

Q2. The National Academies report, Rising Above the Gathering Storm, *recommends "emphasis on physical sciences, engineering, mathematics and information sciences," as well as high-risk research, grants to early career researchers, and funding for advanced research instrumentation and facilities, among other actions, that can help foster innovation and sustain a strong economy. How relevant are NASA's space science research programs to those recommendations? Can you offer any specific examples? NASA's science program was not included in the President's American Competitiveness Initiative (ACI). Would you advocate NASA's science programs be made part of the ACI in future budgets? If not, why not?*

A2. There are three areas in which NASA science could participate in the ACI: education, direct impact on the economy, and fundamental knowledge:

Education: Currently and historically, NASA's most important contributions to education have come from the Science Mission Directorate (SMD), and, when it existed, the program in physical and biological sciences in microgravity. Hands-on projects have been provided for undergraduates and graduate students, fellowships have been available, and most R&A grants to universities provide support for students. Students are trained by participating in projects of varying complexity, ranging from sounding rockets and balloons, to actual space instrumentation that is being built in university labs.

In recent years, NASA has cut back dramatically on its support for university research labs, particularly those doing space hardware. This is the result of reduced flight opportunities of small and moderate missions, and a retrenchment into the NASA centers. The consequence has been fewer and fewer opportunities to train undergraduate and graduate students in the construction of actual space hardware. The historical role that NASA has had to provide the Nation with a technically competent workforce has been greatly abated.

A useful role for NASA in the ACI would follow from an initiative to enhance the participation of the Nation's research universities in the development of space hardware, thereby, as a direct consequence, providing hands-on experience for students. It is important to note that these are not to be watered-down projects, such as student satellites. There is no reason why university researchers and their students cannot develop the most sophisticated instruments needed for forefront research.

Direct Impact on the Economy. There are certain NASA science disciplines that have direct impact on the American economy. Foremost is Earth science, and to a lesser degree, heliophysics [the influence of the Sun on the climate and space weather]. Knowing what is the immediate future of the climate is essential to a variety of industries, from agriculture to insurance, to the auto industry, to coastal infrastructure, etc.

The current funding for Earth science is not adequate to provide the required information on the future of the climate. There are also deficiencies in the funding for heliophysics, particularly if space weather, and its influence on space or ground assets, is considered important.

Fundamental Knowledge. All of the science disciplines in NASA—astrophysics, planetary, Earth science, heliophysics, life and physical sciences in microgravity—

provide fundamental knowledge. Since this is a thrust of ACI, with the inclusion of the NSF and the DOE Office of Science, each of the NASA science disciplines could be an active participant in ACI.

Q3. A recently released study of the National Academy of Sciences on Building a Better NASA Workforce *recommended 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."*

Q3a. Could you please explain in concrete terms how the sub-orbital programs are used to train students and young workers?

A3a. There is a very powerful statement that can be made about the sub-orbital program: Essentially every major experimentalist currently executing NASA's space and Earth science program learned his/her skills in the sub-orbital program. The projects are of limited duration, and thus can be executed during the time required for a graduate thesis. They involve the essential skills of system management and data analysis, as well as challenging engineering.

The sub-orbital program has been reduced systematically over the years, to where it is now a shadow of its former robustness. With the arrival of Alan Stern as Associate Administrator, there has been some welcome revitalization to the sub-orbital program, but it is still inadequate to meet the needs of training the next generation of experimentalists.

Q3b. What do these sub-orbital programs typically cost and do they produce peer-reviewed research?

A3b. There are certain science disciplines that can use the sub-orbital program effectively. Sounding rockets can study plasma phenomena such as the aurora directly, and sounding rockets and particularly balloons can be used for forefront astronomical observations. There are also aircraft that can be used as appropriate platforms for astronomical and Earth science observations. It would be helpful in Earth science, if there were a more robust program in Unpiloted Airborne Vehicles (UAVs). Other disciplines, such as the study of the heliosphere, where it is necessary for a spacecraft to make in-situ observations, cannot profit from the sub-orbital program for science; however, instrumentation that will ultimately fly on a spacecraft can be tested.

Q4. NASA's Research & Analysis (R&A) programs are mentioned as being critical for developing new mission concepts and advanced technology. What impacts will the cutbacks in R&A have on the opportunities for future missions and programs? If R&A remains at current levels, what are we likely to see, or not see, in the next five years?

A4. Science is an evolutionary process. We make observations, and then we develop theories and models to explain the observations. The theories and models then demand new observations as tests. Similarly, we build our missions on existing technology, but in the process recognize the opportunities that new technology can provide us. The availability of the new technology, plus the demand, results in new missions. The R&A program, with its support of theory and modeling, and technology development, is the lynchpin in this evolutionary process. The program sits at the nexus between what we have done and what we want to do.

Historically in NASA science, when the flight rate was low, the R&A program was enhanced, to increase the demand and the opportunities for new missions. The flight rate and funding for R&A have thus been anti-correlated. The odd part of the recent cuts in R&A is that they occurred when the flight rate is in decline. It thus follows, that at the current reduced funding level, the R&A cannot readily serve its historical role in the evolutionary process of advancing space and Earth science.

Q5. What is the current frequency of Explorer and Discovery missions, and what do you believe should be the frequency of launch opportunities if we want to sustain a healthy space science research program in each of the disciplines?

A5. The Explorer program, which supports the astrophysics and heliophysics program, is NASA's oldest flight program. Over the 49-year history of the space program, there have been more than 100 Explorers, or an average of more than two per year. The flight rate today is a small fraction of this rate, as a consequence of more than $1 billion having been removed from the runout of the Explorer line. The Discovery program has fared somewhat better, being better able to maintain its historic flight rate of about one every other year. Other moderate missions, such as the Solar Terrestrial Probes (STP), have greatly reduced flight rates, since the first

mission in this line has been allowed to grow substantially in cost. In Earth science, the Earth System Science Pathfinder (ESSP) missions are effectively at a stand still, and, so far, it has not been possible to initiate the new missions called for in the recent decadal survey for Earth science. As a consequence of all these reductions and delays, the flight rate for small and moderate missions is greatly reduced. The total fight rate for all NASA science missions will be under two per year in 2010–2012, compared to an historical rate in the 1990s of an average of seven per year.

Clearly, it is necessary to restore the flight rate of small and moderate missions. These missions not only perform excellent science, but they are an essential part of the continuum that is essential for the development of human capital and technology; it begins with R&A and extends through small and moderate missions, to NASA's most challenging flight programs. A reasonable goal would be to return the overall flight rate of science missions to above seven per year, balanced across the disciplines. Since there is no room in the budget for additional large programs, this can be accomplished only by additional small and moderate missions. In addition, a major Earth science initiative to implement the recommendations of the decadal survey should be allowed to increase the flight rate even further.

Q6. In your view, what role does the structure of the advisory system play in ensuring the strength of the space science programs?

A6. Early in the history of the space program, a very effective advisory structure for science developed. The National Academies' National Research Council, primarily through the Space Studies Board, provided strategic advice, and the internal NASA advisory committees, particularly those that advise the Science Mission Directorate and its predecessor offices, provided the tactical advice for implementing the strategies. This advisory system, which was practiced for 40 years, has ensured the quality of NASA science. Recently, NASA has effectively abolished the internal NASA advisory structure, particularly the advice that was given directly to the Associate Administrator for the Science Mission Directorate. This is an unfortunate loss, and removes from the Associate Administrator an effective means to formally interact with the science community. It is possible to work around this deficiency, and there is an expectation that the current Associate Administrator, Alan Stern, will. Nonetheless, in the next NASA administration, the decision to abolish the internal advisory structure for science should be reversed.

Questions submitted by Representative Ken Calvert

Q1. What metric should NASA use to establish an appropriate level of technology development investment across the programs? Should it be a percentage of the overall program funding, or should it be a fixed amount?

A1. This question does not have a simple answer. The technology investment required, as well as R&A funding in general, will vary from discipline to discipline. It will depend on the current state of technology development for the discipline; is it adequate to support future missions or are major breakthroughs required to advance the science? For example, as noted above, there is a logic in making more R&A investments when the flight rate is low, to yield a greater flight rate, and thus funding R&A should not be a simple percentage of the existing program.

This is an area that is worthy of a detailed study to set up appropriate strategies for R&A funding for each science discipline. R&A funding is perceived to be inadequate, particularly with the recent cuts, and it is important to have a detailed defendable strategy to justify the restoration of funds and any future funding level.

Q2. What mission assurance and management requirements imposed by NASA do you believe are counter-productive or impose costs that are disproportionate to the size of the mission, or that offer little added value?

A2. Over the decades, the United States has developed great capabilities to execute space and Earth science missions. The expertise resides in university labs, in industry, as well as in the NASA centers. In the best of worlds, the organizations with expertise and experience are allowed to perform their tasks in the most cost-effective manner possible, with the aim that they achieve mission success. Many of these projects are managed out of NASA centers. With full-cost accounting, and the need to justify the civil service workforce, the number of individuals participating in this management has grown substantially. It is not obvious to experienced scientists and engineers that this additional management oversight, which carries a cost for both the NASA center and the contractor, adds value and reduces risk. Rather, it is more likely that risk is increased since the oversight, if extreme, can reduce the attention paid to good engineering practices. For larger, more complicated missions, highly so-

phisticated management oversight is required. However, the distracting practices are particularly onerous for small and moderate missions, and of no clear benefit.

Q3. How should NASA and the space science disciplines best develop estimated mission costs, at a reasonable level of confidence, during the next round of decadal surveys? Who should perform these estimates? What level of confidence do you believe is appropriate?

A3. In the current Beyond Einstein Program Assessment Committee (BEPAC), the NRC has engaged a subcontractor specializing in cost estimating to perform independent cost estimates for the missions under consideration. If this arrangement proves satisfactory, it will be an appropriate model to follow for the next round of decadals and other NRC priority setting studies. The cost of such studies to the sponsor(s) will increase commensurately, but the final product would be more valuable in that priorities would be set with greater clarity about project costs, and assurance that the cost estimate was developed independently of the project, as well as the Agency overall. The NRC does not take a position on what level of confidence should be used for such cost estimates, and will rely on the sponsor to specify the level of confidence required. NASA's current policy for missions in the Science Mission Directorate is 70 percent, and that is the confidence level being used for BEPAC.

ANSWERS TO POST-HEARING QUESTIONS

Responses by Garth D. Illingworth, Chair, Astronomy and Astrophysics Advisory Committee (AAAC)

Questions submitted by Chairman Mark Udall

Q1. NASA has imposed cost "caps" on a number of its small- and medium-sized mission programs, such as Discovery. With the advent of full-cost accounting, are these cost caps still realistic or should they be adjusted? Have the cost caps proven to be an effective tool?

A1. With the advent of full-cost accounting, are these cost caps still realistic or should they be adjusted? There has been a growing sense that the previous cost-caps were too small for Explorers and Discovery missions. It has become increasingly difficult to undertake cutting-edge science programs within the cost-caps. Several changes have contributed to this, in addition to the impact of full-cost accounting. NASA advocated an increase in contingency to 30 percent for these programs several years ago, given the concerns with continuing overruns in the cost-capped missions. The Space Science Advisory Committee (SScAC) supported increasing the contingency to try and minimize overruns, even though it appeared to limit what the proposing team could do within the cost-cap. A further problem is the increasing cost of launch vehicles for Explorer and Discovery missions, especially with the discontinuation of the Delta launch vehicles. There is another factor also which reflects the increasing maturity of the scientific studies in the Planetary and Astrophysics programs. The "low-hanging fruit" has been picked, i.e., the easiest studies have, in many cases, been done already. This results in future missions needing more sophisticated detection systems and/or larger optics, both of which tend to drive costs higher. While increases in cost caps will trade against mission frequency, unless the budgetary situation improves significantly, it is my sense that many prospective projects that have high scientific value are potentially excluded with the "traditional" level of cost caps and so some reduction in frequency for higher cost-caps might be an appropriate trade-off.

The cost-cap for the last Discovery mission proposals proved to be extremely challenging for the Astrophysics missions that were proposed for planet searches. Increases are needed, at least to accommodate planet search options. The next Explorer call for proposals will need a much larger cost-cap than that used in 2002 to offset the effect of full-cost accounting, appropriate levels of contingency, and the uncertainty and likely higher cost associated with the launch vehicle. The cost-cap associated with the Astrophysics Probes, now being discussed as an analog to the Planetary Division's New Frontiers program, will need careful consideration as well, since all the same elements (full-cost accounting; contingency; launch vehicles) will challenge those $0.6+B scale missions as well.

Have the cost caps proven to be an effective tool? It is my view that cost caps are an effective tool. They are no guarantee that missions will come in on budget, but the caps provide a number of advantages. First, they provide great pressure on the proposing team and the NASA center to rigorously assess the likely cost of the mission before and during the proposal process. The cost caps also provide similar pressure during the Phase A process after preliminary selection and before final selection. Second, they provide some very specific budget points at which the agency and its advisory committees begin to discuss the project performance. This provides additional pressure on the project to work to the budget. There are no guarantees that any project will come in on-budget, but the cost-capped missions do have a number of mechanisms that help this situation. The recent emphasis on the experience of the PI before selection, and on the performance of the PI during development and construction, provides an additional pressure point on the project. Overall it is my view that cost-capped programs are a very desirable component of the mission suite in SMD, but more realistic cost-caps are needed.

Q2. The National Academies report, Rising Above the Gathering Storm, *recommends "emphasis on physical science, engineering, mathematics and information sciences", as well as high-risk research, grants to early career researchers, and funding for advanced research instrumentation and facilities, among other actions, that can help to foster innovation and sustain a strong economy. How relevant are NASA's space science research programs to these recommendations? Can you offer any specific examples? NASA's science program was not included in the President's American Competitiveness Initiative (ACI). Would you advocate NASA's science programs be made part of the ACI in future budgets? If not, why not?*

A2. How relevant are NASA's space science research programs to these recommendations? NASA's science programs are extremely relevant to the issues and concerns raised in the *Gathering Storm* report. A substantial fraction of NASA's science programs relate directly to the areas highlighted in *Gathering Storm* as needing emphasis, namely physical science, engineering, mathematics and information sciences. Furthermore, NASA's science programs bring a largely unique coupling between academia (university and research organizations), industry (aerospace contractors) and government (NASA centers) on cutting-edge, high-technology projects.

Can you offer any specific examples? I think the cost-capped PI missions (e.g., Explorers, Discovery, Astrophysics Probes, New Frontiers) are particularly good examples of programs that have these interfaces and encourage innovative thinking. However, at the other end of the cost scale, I also think that missions like JWST are particularly responsive to these recommendations. They bring together the best people from a wide range of areas, encourage them to work together innovatively and demand cross-cutting skill development that is remarkably valuable for all those involved. The end product of the JWST effort will be a mission like Hubble in its likely impact and thus likely to be the seed from which great public interest in the physical sciences will grow.

NASA's science program was not included in the President's American Competitiveness Initiative (ACI). Would you advocate NASA's science programs be made part of the ACI in future budgets? Given the above aspects and roles of the NASA science program, and its national recognition, the NASA science program should be considered especially deserving of any funding gains that might grow from *Gathering Storm* and, in particular, therefore deserving of inclusion in ACI. I personally advocate, very strongly, that NASA's science programs be made part of ACI in future budgets, and hope that Congress is willing to support and increase funding for NASA's science programs as part of its Innovation Initiative. The AAAC has also endorsed this approach through one of its primary recommendations for NASA in the 2007 AAAC Annual Report:

> *"The American Competitiveness Initiative (ACI) recognized the challenges faced by the Nation in staying at the forefront of scientific and technological development. Research is essential to innovative activities and underpins a technologically-competitive society, as highlighted in the NRC report* Rising Above the Gathering Storm. *The exclusion of NASA science from the ACI, in contrast to the inclusion of DOE science, is inconsistent. There is no question that NASA is at the cutting-edge of science and technology research.*
>
> *This exciting and highly visible research contributes to the vitality of the national skill set and has encouraged young people to move into science and engineering. The Congressional interest in Innovation and Competitiveness enables a fresh opportunity for enhancing NASA science. The AAAC strongly encourages Congress to consider enhancing the support for science at NASA explicitly to improve innovation and competitiveness, as has been done for NSF and DOE science."*

Q3. A recently released study of the National Academy of Sciences on Building a Better NASA Workforce *recommended 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."

a. Could you please explain in concrete terms how the sub-orbital programs are used to train students and young workers?

b. What do these sub-orbital programs typically cost and do they produce peer-reviewed research?

A3. A continuing concern for the science community has been the ability to train young researchers in the complexities and details of space science missions. This has traditionally happened through the sub-orbital programs (rockets and balloons most typically for Astrophysics) and through the Explorer-class missions. However, the Explorer-class missions, even small Explorers (SMEX), happen rarely, involve long timescales from proposal to fruition (typically five years or more), and are now at cost levels (~$300+M for an Explorer and >$100M for a SMEX) that makes student and postdoctoral involvement challenging. The sub-orbital programs appear to be the best mechanism for doing these "training" activities. While I have no direct experience with the sub-orbital program, recent discussions confirmed my view that the rocket program, as an example, allows for direct, end-to-end, hands-on involvement by students and postdocs and so can provide for substantially training that is of great value in building up a cadre of researchers who have developed signifi-

cant experience with doing science in space. The sub-orbital program faces challenges in the present funding environment, like many areas, and it would be valuable for the upcoming Decadal Survey to assess its value for training and science, and to provide some guidance on the role that the sub-orbital program should play in the coming decade in astrophysics.

Could you please explain in concrete terms how the sub-orbital programs are used to train students and young workers? As an example (which surely varies by program in its details), a typical rocket flight can give a student or postdoc insight into most of the elements that constitute a "science" space mission. A typical rocket program might run from instigation to published results in a couple of years. The young researchers involved with a program could be involved in concept development and science experiment "design," to hardware design, through "bread-boarding" to construction and testing in the home institution, to integration at the launch facility (at NASA's Wallops Flight Facility), launch and science data acquisition (even doing real-time control of the experiment), and then to data analysis and publication. If the peer-reviewed publication that results is done by a graduate student, then that publication would constitute a significant part in the Ph.D. thesis of the student. Being involved in this sequence of events, with the interactions with engineering personnel and rocket operations personnel, gives a young researcher valuable insight into the steps involved in doing space mission development. They can build on this subsequently with involvement in more complex and expensive programs and missions.

What do these sub-orbital programs typically cost and do they produce peer-reviewed research? The sub-orbital program is distinguished by being inexpensive (relative to orbital missions). The 2007 budgets for sounding rockets, balloons and aircraft at Wallops are $32M, $22M and $10M. These are modest programs in NASA terms. The flight opportunities for rockets and balloons are typically ~20 per year. A typical rocket mission is thus in the million-dollar range ($1–2M), very low compared to any Explorer. Balloon programs have also been of significant value to the overall science program, and provide similar training opportunities at similar modest cost (by space mission standards). Of course, the science returns from a typical sub-orbital program are less than from an Explorer, as would be expected, but the combination of training and science from the sub-orbital program is of great value. Even though the costs are modest these programs are quite competitive and involve selection through peer-review. The sub-orbital programs have consistently produced peer-reviewed research. It is well recognized in the community that one must publish in peer-reviewed Journals to be seen as successful, and the same is true if one is to be competitive in subsequent competitions for R&A support and access to the sub-orbital facilities. The research from the sub-orbital program has played a significant role in a large number of Ph.D. theses, as noted above, and resulted in publication in peer-reviewed journals. The peer-reviewed research productivity has then been a factor in the peer-reviewed selection of subsequent sub-orbital proposals.

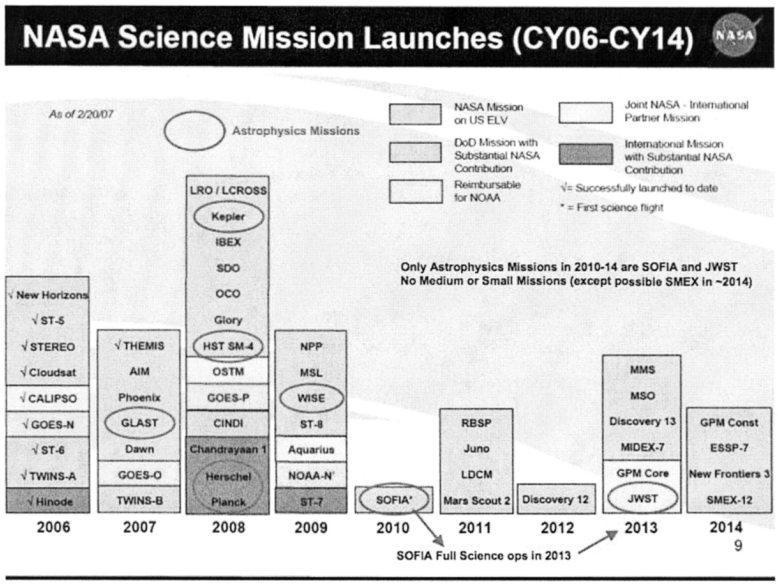
Q4. NASA's Research and Analysis (R&A) programs are mentioned as being critical for developing new mission concepts and advanced technology. What impacts will the cutbacks in R&A have on the opportunities for future missions and programs? If R&A remains at current levels, what are we likely to see, or not see, in the next five years?

A4. Research and Analysis (R&A) funds are used for a very wide range of activities related to the NASA science program. These are directly relevant for science productivity and science preparation for future programs through the theory component, through a variety of diversified research activities that improve the foundations and underpinnings of the scientific framework, and through training and development of young scientists. These young scientists are the future researchers who will both implement and provide the scientific returns. Furthermore, R&A funds have been used for technology development and for mission concept development. Major projects, in particular, typically take decades from concept development to launch. This was true of AXAF (Chandra), which was developed conceptually back in the 1970s and eventually launched in 1999 after 20+ years. The same was true of SIRTF (Spitzer), which also took some 20+ years from initial conceptualization to launch. NGST (JWST) will launch in 2013, but the first international workshop took place in 1989. I personally was a co-organizer on that workshop and know directly the importance of support in the early stages for concept development. During much of the 1980s and 1990s key technologies were developed for these and other missions (including the HST 2nd and 3rd generation instruments) using a variety of funding sources, but many development activities by scientists and organizations were carried out using R&A-like funds. It is hard to quantify the role that R&A funds played, and would require a substantial effort to trace the use of funds in the

early stages of mission development and their ultimate role in early project and technology development. The anecdotal evidence is widespread, however, with many senior scientists noting how support for them allowed for significant efforts on the early phases of major projects, both on hardware development and on concept development.

What impacts will the cutbacks in R&A have on the opportunities for future missions and programs? The level of effort in such areas is now substantially less, even within the NASA centers, and considerable concern exists that we are not investing enough for the missions of the next decade and beyond. I do not see the same level of involvement in future missions and programs that existed in the 1990s. I think that the involvement of the University/academic community in long-range development is not at a healthy level for providing the ground-work for the major missions of the next decade and beyond, nor even for the next round of larger cost-capped missions. Innovative efforts are being limited by R&A funding shortfalls. The level of funds available for small missions, like Explorers, has an impact too. The recent SMEX (Small Explorer) announcement is good, but they provide less opportunities for Astrophysics than for Heliophysics—the Astrophysics science opportunities within the SMEX size, weight and orbital constraints are limited.

If R&A remains at current levels, what are we likely to see, or not see, in the next five years? Unfortunately, since it takes about five years to do even the smallest missions, any changes that we implement now will not have an impact for longer than five years. This is why the dramatic decrease in missions in SMD beyond 2009 (and in Astrophysics in particular) is such a concern (see the Figure below from my testimony to the Subcommittee). To increase the mission frequency by the middle of the next decade will require an increase in the budget for small and moderate missions in the next couple of years.



Q5. In your view, what role does the structure of the advisory system play in ensuring the strength of the space science programs?

A5. I have been involved with, and on, advisory committees for the agencies, and NASA in particular, for some 20 years, and will have chaired the Astronomy and Astrophysics Advisory Committee (AAAC) for four years when my term ends next year. It is my view, based on this extensive experience, that the advisory structure for the science agencies plays an incredibly important role as an interface between the community—which sets the broad goals, defines the mission or project suite, and carries out the scientific research program—and the agency which implements and manages the very complex missions and projects, and enables the science program.

The management and implementation of these activities is clearly an agency responsibility, but the effectiveness of their role is crucially dependent on a continuing dialogue and advice from the community as the implementation realities impact projects, and as agency budgets and goals evolve. This is particularly true of NASA where there is a very complex set of interfaces with the agency, both at HQ and the Centers, the science community and with the contractors, and where the political environment plays such an important role (given the fiscal scale of the missions being undertaken). The AAAC paid particular attention to the lack of an advisory process in 2005 and early 2006 when the NASA advisory process was in abeyance, and wrote extensively about its importance. The AAAC followed up on this in its most recent 2007 report. The AAAC summary statements regarding advisory committees at NASA for the science program are given below:

The AAAC statement regarding the NASA science advisory structure in our 2006 report was: *"For the past year the lack of an advisory structure for NASA—and for science at NASA—has been a deep concern for the community. By the time the new science advisory committees are selected, approved and assembled, a year will have passed without discussions on issues that are critical for the community and for NASA science. During that time, far-reaching decisions were made without any scientific input (e.g., the effective cancellation of SOFIA whose budget was reduced to $0 in FY 2007 and beyond without a review). The AAAC welcomes the creation of a new advisory structure. However, we and others are concerned that this structure may not be as effective as that previously employed. The lack of close coupling of the science subcommittees to the SMD leadership is likely to be a significant impediment. The AAAC has every hope that the new structure will work effectively and in a timely way by providing feedback to SMD quickly with minimal modification. However, if the structure is perceived by either party to be ineffective, the AAAC hopes that the Administrator and the Associate Administrator for Science will evolve the structure to better serve NASA and the community. These committees play an essential role in optimizing the science program within the programmatic and budgetary constraints faced by the agency and thus are of great value both to NASA and to the community. The NASA advisory process has been a mainstay of a productive and mutually beneficial relationship with the space and earth science community, including the astronomy and astrophysics community. The AAAC considers effective advisory committees to be essential for developing consensus and support for an effective science program."*

The AAAC statement regarding the NASA science advisory structure in our 2007 report was: *"The AAAC expressed great concern last year in our report about the lack of an advisory process at NASA. We were very encouraged when the new NASA advisory committees were established. The new structure does differ from that used previously, providing a clearer path for advice to the Administrator. The new structure has, however, lost a valuable role that was once provided by the Space Science Advisory Committee (SScAC). That structure encouraged dialogue, on wide-ranging issues that cut across the SMD divisions, between SMD and a broadly-representative group from the science community. An improved interface with SMD is in the best interests of both NASA and the science community to restore this important two-way communication link that has contributed to the success of NASA science in the past. The AAAC welcomed the re-establishment of the advisory structure at NASA last year, but notes that dialogue between SMD and a broadly-representative group from the science community is missing in the new structure."*

Q6. Your testimony mentions the creation of an ExoPlanet Task Force that will consider the missions and science related to the search for extrasolar planets. What is the charge for this task force? Will it provide advice on the future direction for the SIM and TPF missions?

A6. The ExoPlanet Task Force is a subcommittee of the AAAC. The charge letter is attached separately, along with the request letter from the AAAC. These can also be found on the AAAC website (see: *http://www.nsf.gov/mps/ast/exoptf.jsp*), along with the list of members. The AAAC requested that the agencies consider setting up a task force to assess how to move forward in a coordinated way (ground and space) on extra-solar planet detection and characterization. The core statement of that letter was:

> *"Over the last year there have been discussions at several AAAC meetings about establishing an ExoPlanet Task Force (ExoPTF) to assess approaches and options for extra-solar planet detection and characterization, using both space and ground-based facilities. Planet searches are technically challenging and projects that will enable major advances have long development lead-times and will be costly. Planned space missions and major ground-based instruments will provide*

near-to-intermediate term results, but the way forward on a synergistic, cost-effective approach involving both space and ground-based facilities remains unclear. In the 2006 AAAC Annual Report the AAAC recommended the formation of such a task force later this year so that its report would be available late in 2007 or early in 2008, providing guidance both to the agencies and the upcoming Astronomy Decadal Survey."

What is the charge for this task force? The charge letter consists of three pages of background, a broad statement of the charge, and ten explicit questions to be addressed by the committee. The broad statement of the charge from the attached charge letter is:

"The ExoPTF is asked to recommend a 15-year strategy to detect and characterize exo-planets and planetary systems, and their formation and evolution, including specifically the identification of nearby candidate Earth-like planets and study of their habitability. The strategy may include planning and preparation for facilities and missions beyond the 15-year horizon. Since future funding levels are uncertain, and project costs are difficult to establish at an early stage, it is important to develop an efficient and adaptable plan. To the extent possible, the recommendations should accommodate a range of funding levels representing conservative and aggressive programs. The ExoPTF will work in cooperation with agency efforts to advance the justification, specification and optimization of planet finding and characterizing opportunities."

Will it provide advice on the future direction for the SIM and TPF missions? The ExoPTF will certainly be thinking about SIM and TPF and their role in the overall suite of missions, telescopes, instruments and projects that will be part of the framework for the next decade or so of extra-solar planetary research. The committee is being asked to provide guidance on the type of capabilities and sequencing of capabilities that are needed to undertake a vibrant program of extra-solar planetary studies. They are being asked to not consider specific missions, but the expectation is that they will provide guidance and recommendations which will bear strongly on the role that SIM and TPF-like missions will play in the coming 1–2 decades.

Q7. Please provide your recommendations for what the highest priority uses of any additional resources should be if they become available for science at NASA in the FY08 appropriations process?

A7. At the highest level I think it is important to enhance the science budget for NASA. The NASA science program has been incredibly effective in its exploration of new frontiers and in its coupling to the American public through a very effective outreach program. The strength of the response to the Hubble cancellation was a testament to the effectiveness of the science program in coupling with the public's imagination. Thus I feel that NASA and SMD should get *at least* the funding level in the FY08 budget request. Furthermore, I would hope, as enunciated above, that NASA SMD is seen in the same light as NSF, DOE science and NIST and gets ACI-like increases which set it on a track for substantial recovery and increases in the FY08 appropriation (through the Congressional Innovation effort, for example) that are above the FY08 request. Increases in the SMD budget at the ~7 percent level, like the ACI increases at the other agencies, would do much to restore the vitality of the science program. These funds would provide a much more robust future for the science program by increasing the mission flight rate in the next decade, and would generate greater science return and output from the current and near-future missions.

I believe that the details of how these funds would best be used would be through discussion by SMD with its advisory committees. In the spirit of the question however, I personally think that additional resources would be most effectively used for (i) recovery and increases in peer-reviewed and competed R&A funding, (ii) a variety of peer-reviewed and competed technology development programs (particularly those that encourage the science community to invest effort on technologies with their students and young researchers—I would be less supportive at this stage of those funds going largely to the NASA Centers and/or contractors for technology development), and (iii) more cost-capped missions of the Explorer, Discovery or Astrophysics Probes scale. I think that relatively modest investments in these areas would return a great deal of scientific results, begin development of new concepts, and provide opportunities for a wider range of science missions on relatively short timescales.

Q8. In your testimony, you highlighted the importance of obtaining both more realistic cost estimates of missions, including the use of life cycle costs. You gave some examples, but it would be useful for the Subcommittee to have a tabulated summary of life cycle mission costs for past and present astrophysics missions

on a uniform basis (as much as is practical) in current dollars including full-cost accounting estimates, with any assumptions given as well. Please provide that information to the Subcommittee, working with NASA to ensure that the cost numbers are developed consistently.

A8. The importance of having life cycle costs for missions has been dramatically demonstrated over the last few years. The use of "construction costs" in community discussions has contributed to "under-costing." For many programs the bulk of the costs are not in construction (Phase C/D) but actually in Phase B and earlier activities, and in operations. An extreme example is SOFIA which stands at $3.4B life cycle (in actual year dollars), but whose Phase C/D costs, while hard to define because of the poor management oversight and structure for this program, is probably around $0.6B. Even for major missions such as JWST and SIM, the Phase C/D costs are about 30–40 percent of the total. For planning it is essential to develop reliable cost estimates and to use life cycle costs (over the lifetime of the mission) in the discussions between NASA and the community. This will ensure that program planning within the astrophysics Decadal Survey, and subsequent agency roadmaps, can be carried out within likely budgets.

The Table below summarizes life cycle mission costs (LCC) in constant 2007 dollars, with a summary of the caveats/comments appropriate for the derivation of these numbers. These numbers are from the NASA Science Mission Directorate (SMD). Since these numbers were derived by NASA, I would hope they become the baseline numbers for subsequent discussions of mission costs. Obviously taking costs from past missions done under very different accounting structures and converting them to present day structures will be uncertain, but they provide a very useful guideline for planning purposes and for setting the scale for missions under discussion. They are estimated as likely to be accurate to better than 10 percent, probably about ±5 percent. I would like to express my deep appreciation to the NASA SMD leadership for providing these numbers and notes for the response to this question.

NASA SMD Lifecycle Costs for Science Missions (in constant 2007 dollars)

Mission (alphabetical)	$B (constant 2007 dollars)	Comments
Cassini	$3.9	Launch included
CGRO	$1.5	Launch included
Chandra	$4.0	Shuttle cost not incl. (IUS incl.)
Galileo	$3.2	Shuttle cost not incl. (IUS not incl.*)
HST	$12.8	Shuttle cost not incl.; Servicing mission costs incl.**
JWST	$4.4	2013 Launch; 10 yrs operations
SIM	$2.6	Nominal 2015/16 Launch; 10 yrs ops***
SOFIA	$2.7	Full science ops 2013; 20 yrs ops
Spitzer	$1.7	Launch included

All costs are lifecycle (LCC), adjusted for full cost prior to FY04 (full cost accounting used since FY04), and converted to constant 2007 dollars (rounded to nearest $0.1B).
*Inertial Upper Stage (IUS) number too uncertain for inclusion (maybe $0.2B?);
**ESMD funding of robotic servicing not included.
***Based on FY07 budget data; currently "indefinitely deferred."

Questions submitted by Representative Ken Calvert

Q1. What metric should NASA use to establish an appropriate level of technology development investment across the programs? Should it be a percentage of the overall program funding, or should it be a fixed amount?

A1. What metric should NASA use to establish an appropriate level of technology development investment across the programs? The level of technology development funding required for a given project will depend on a number of factors. My feeling is that there are three primary factors: (i) the maturity of the technology, (ii) the

maturity of the project, and (iii) the scale of the project. There will be a wide dispersion in the needed funding for technology, but all science projects are demanding of technology and require early technology development to minimize risk and minimize overall cost. During the early phases of a project the cost of developing and retiring technologies can be rather small, though there are disadvantages to working some technologies too far ahead of the mission (they could become outdated). For Discovery and Explorer-class missions, even funding levels around $1M/yr at the pre-proposal and Phase A stage can make a significant difference in the maturity of the technologies and the likelihood that the mission can be selected and implemented effectively. This is not the case with larger missions. Even during their early phases they may require budgets more like $10M/yr to make significant progress, in part because a number of key technologies must be developed and demonstrated. The AAAC in its deliberations and discussions with NASA personnel came to see at least $10M/yr as a figure that was needed for missions like Con-X, LISA, and TPF to develop to the point where useful assessments could be made of their likely cost and readiness for moving ahead for further development.

The funding required when missions transition to Phase B can be significantly higher. Substantial funding was needed for AXAF (Chandra) to develop the mirror technology to the point where the project could convince review boards that the project was ready to move forward. The approach currently being taken by JWST is one that was chosen after it was realized that much of the cost growth in the HST program came because required technologies were being developed during construction (Phase C/D). This can lead to large cost overruns when the marching army pauses because of a problem in a key system. While JWST has had its problems, we should not lose site of the very rational approach that was developed with this mission based on the experience with HST (and others such as AXAF)—that is, all technologies should be at TRL–6 before JWST transitions to Phase C/D. The goal is have this project proceed rapidly through construction (Phase C/D) in ~5 years, with minimal risk of delays from unresolved technology issues.

My view is that the approach should be (i) early, careful assessment of the key technologies, with emphasis on identifying all of them well in advance, (ii) plans to develop them to TRL–6 before Phase C/D, and (iii) assessment of the timing such that they reach maturity at the needed time (before the end of Phase B). In parallel with these developments, the value and power of integrated modeling has been recognized in many industries as providing a (relatively) low cost cross-check on the performance of the final product. Modeling is no substitute yet for extensive subsystem and system level testing, but it provides an extremely powerful cross-check. Integrated models should also be developed early and refined as the system develops.

Should it be a percentage of the overall program funding, or should it be a fixed amount? I think that experience has shown that a fractional amount is more appropriate, though the percentage is hard to establish. As a ballpark I might suggest one percent per year of the expected total program cost in the very early phases, rising to a total expenditure of tens of percent during Phase B, so as to adequately retire risk before construction commences. I would suggest eliciting further input on this topic from others with extensive project experience.

Q2. What mission assurance and management requirements imposed by NASA do you believe are counter-productive or impose costs that are disproportionate to the size of the mission, or that offer little added value?

A2. I recognize and agree with the goal of minimizing mission costs. Clearly, we can do more missions within the available budget if we can lessen the cost of missions. However, in thinking about this I came to the conclusion that much of the oversight within the current mission assurance and management structure is necessary. The missions that we do, even the smaller cost-capped missions, are generally very complex and technologically challenging. The larger missions stretch our collective abilities to manage them. The teams that work these missions consist of project managers, engineers, scientists and support personnel, from the contractors, the NASA centers and the academic and scientific community. They deal with very complex issues, with tight deadlines and with tight budget constraints. The teams hold evaluations and progress assessment meetings on timescales of weeks and months. These meetings deal with detailed project issues and problems as they arise—which tends to be frequently. Fewer meetings might help progress at times, but having fewer will also allow some problems to persist longer than they should and require greater efforts and costs for recovery actions.

While these rather routine meetings are part of the process, there is another aspect that is crucial for our most challenging missions (and possibly even for our less challenging missions, as I will note below). My view is that it is crucial to have inde-

pendent, external oversight boards and teams. These boards should consist of a small number of people who have a great deal of project experience, who are independent of the project and report to NASA HQ above the project and program level. They should meet often enough that they are conversant with the project, but not so often that their independence is lost. From watching the JWST project over the last few years it is my understanding that such groups are now in place and that they appear to be fulfilling a very important role.

By putting such groups in place at an earlier time in major projects I would hope that some of the problems with missions like JWST in its earlier days could be averted. We have now done many large science missions and it would be useful to have a "lessons-learned" assessment at some point before we embark on others. And it is not just the most challenging missions like JWST that develop problems. As an example of when projects go awry, SOFIA should be an excellent case study. It is dramatically behind schedule and over budget, yet it is not one of NASA's most challenging missions. In fact, it is probably at the less demanding end of the range technically. Yet when it finally reaches science operations it will be approaching a decade behind schedule (aircraft completion and science operations were quoted in 1999 as being on track for startup in 2001!). I think it is an example of poor management and inadequate oversight, and would hope that lessons are learned also from this program to lessen the chance of a repeat occurrence.

Q3. How should NASA and the space science disciplines best develop estimated mission costs, at a reasonable level of confidence, during the next round of decadal surveys? Who should perform these estimates? What level of confidence do you believe is appropriate?

A3. There is no doubt that better cost estimates are clearly needed for the upcoming Decadal Survey. The recognition that this was a serious problem for the last Survey has led already to extensive discussion of how to improve the cost estimates. It is clear also that this is a work in progress. The BEPAC study (the Beyond Einstein Program Assessment Committee) is assessing mission costs in its current evaluation of what should be the first Beyond Einstein mission to be carried out early in the next decade. Deriving accurate costs is a challenge at an early stage of mission development, but it is clear that we do need to do better. Even with the clear recognition on the Astrophysics side that mission costs need to be much more reliably assessed and used in the development of a the Decadal program, the recent Earth Sciences Decadal Survey did a rather poor job of costing the programs it discussed and recommended. They appear to be systematically underestimated.

How should NASA and the space science disciplines best develop estimated mission costs, at a reasonable level of confidence, during the next round of decadal surveys? There are two steps that need to be taken. The first is to clearly agree that life cycle costs will be used; the second (below) is to obtain "accurate" costs. The overall or life cycle costs should be at least the costs over a 10–15 year period appropriate for the recommendations from a Decadal Survey group. The examples of SIM and SOFIA, both of which were moderate-size ~$250M missions in the 1990 Survey, but which grew to be ~$3B programs life cycle, provide a sobering lesson. In detail, SIM was $250M in the 1990 Survey (as AIM, the Astrometric Interferometry Mission), or $420M inflated to 2007 dollars, and was costed last year in actual year dollars at $3.4B if launched in 2015–16, or $2.7B if launched in 2011 ($2.6B in constant 2007 dollars for 2015/16 launch—see Table above). SOFIA was listed as a $230M program in the 1990 survey, or $386M inflated to 2007 dollars, and is now, in the FY08 budget request, $3.4B life cycle in actual year dollars ($2.7B in constant 2007 dollars).

JWST and Chandra provide other examples where our initial costs were significant underestimates of what the mission ultimately cost. JWST went from $1B as quoted in the 2000 Survey, or $1.2B in 2007 dollars, to $4.4B life cycle, while Chandra went from $500M in the 1980 Survey, or $1.4B in 2007 dollars, to $4.0B life cycle in 2007 dollars with full-cost accounting. One should note that the costs quoted from the Decadal Surveys were not usually life cycle costs (they were probably closer to construction costs). However, they are so different from the reality of the actual or expected life cycle mission costs that the lesson that we must learn from these comparisons is that we must deal directly and thoroughly with the cost issue and bring the cost estimates closer to reality.

What level of confidence do you believe is appropriate? The second important element, to make the life cycle costs accurate, is that the costs for each of the phases of missions should be derived with minimal systematic underestimation. This is much more important than having costs given to many significant figures. It is worthwhile to add a note of caution against undertaking very elaborate cost studies and expecting too much from them. We are undertaking technically-challenging

projects using cutting-edge technology. Getting costs to two significant figures with small uncertainty at an early stage is a practical impossibility. But if we can get costs to one significant figure with a fair degree of confidence that they are not systematically underestimated, we will be markedly better positioned for reliable planning than in previous surveys. Knowing during the Decadal Survey that a program of the scale of JWST is $4B instead of $2B or $3B would be a major achievement, especially if that cost included a significant contingency. Reliably identifying whether space missions are at <$0.5B, $1B, $1.5B, $2B, $3B would be sufficient granularity, in my view, if the costs were devoid of significant systematic uncertainties.

Who should perform these estimates? A key issue will be to balance the level of cost reliability with what can realistically be done before and during the Decadal Survey process. A realistic goal might be to (i) establish common ground rules (e.g., any cost estimates would most usefully include both full life cycle costs and costs within the coming decade), (ii) provide independent cost estimates (not just cost estimates from the project proponents), (iii) aim to provide costs that are less systematically underestimated, (iv) aim for accuracy and not precision, and (v) include experts in project management and cost assessment in the deliberations. In particular it might be useful to have a panel that is used as a resource by other panels to evaluate costs, and to include at least one person with good project oversight/management experience on each panel to help frame the right questions for the "cost expert" panel. NASA cost estimation emphasizes life cycle costs and thus can provide feedback from considerable in-house experience (e.g., see NASA Cost Estimating Handbook CEH—at *http://www.nasa.gov/offices/pae/organization/cost_analysis_division.html*). The Program Analysis & Evaluation (PA&E) office has provided a more focused role for these issues under the new Administrator (*http://www.nasa.gov/offices/pae/home/index.html*), and provides a web-enabled version of the 2004 NASA CEH or a downloadable pdf version at the above URL for the cost analysis division. Furthermore, if funding allows, it would be very useful to use independent organizations, such as Langley Research Center and Aerospace Systems Design Lab, to provide some separate estimates, even if they were "rough," early-stage estimates.

Appendix:

Re ExoPTF and Question 6: The following letters relate to Question 6 regarding the ExoPlanet Task Force. They are the original request letter from the AAAC to NSF and NASA regarding the ExoPTF, and the Charge letter that the agencies developed in their request to the AAAC to form such a subcommittee.

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UNIVERSITY OF CALIFORNIA OBSERVATORIES/LICK OBSERVATORY
DEPARTMENT OF ASTRONOMY AND ASTROPHYSICS
SANTA CRUZ, CALIFORNIA 95064

June 21, 2006

Dr Rick Howard, Acting Director,
Astrophysics Division, Science Mission Directorate, NASA

Dr. Wayne Van Citters, Division Director,
Division of Astronomical Sciences, MPS, NSF

Dear Dr Howard and Dr Van Citters:

Over the last year there have been discussions at several AAAC meetings about establishing an ExoPlanet Task Force (ExoPTF) to assess approaches and options for extra-solar planet detection and characterization, using both space and ground-based facilities. Planet searches are technically challenging and projects that will enable major advances have long development lead-times and will be costly. Planned space missions and major ground-based instruments will provide near-to-intermediate term results, but the way forward on a synergistic, cost-effective approach involving both space and ground-based facilities remains unclear.

In the 2006 AAAC Annual Report the AAAC recommended the formation of such a task force later this year so that its report would be available late in 2007 or early in 2008, providing guidance both to the agencies and the upcoming Astronomy Decadal Survey. The AAAC Annual report language is given below.

We recognize the concern that was expressed about having a number of such Task Forces running in parallel, but now that the Dark Energy Task Force Report has been completed, we feel that it would be valuable to have more detailed discussions of a possible structure, key elements of the charge and a timetable for the ExoPTF at the AAAC meeting in October.

Sincerely yours, on behalf of the Committee,

Garth D. Illingworth,
Chair, Astronomy and Astrophysics Advisory Committee

Cc:
NSF: Eileen Friel, Dana Lehr
NASA: Mary Cleave, Colleen Hartman, Paul Hertz, Michael Salamon
DOE: Robin Staffin, Kathy Turner
OMB: David Trinkle, John Sloan, Amy Kaminski, Joel Parriott,
OSTP: Rob Dimeo, Jon Morse

AS: David Spergel

AAAC: Neta Bahcall, John Carlstrom.
Bruce Carney, Wendy Freedman,
Katherine Freese, Robert Kirshner,
Daniel Lester, Angela Olinto,
Rene Ong, Sterl Phinney,
Catherine Pilachowski, Abhijit Saha

Excerpts from the 2006 AAAC report re Synergy and the ExoPlanet Task Force

ExoPlanet Task Force (ExoPTF)

ExoPTF: The AAAC notes that substantial progress is being made on ground-based planet searches and that substantial activity has occurred in defining future space-based facilities. The AAAC recommends that the agencies consider the establishment of a task force to develop a roadmap for planet detection and characterization, as well as planetary formation, with consideration of the relative roles and contributions of future ground-based programs and space missions. Such a report, as well as being a guide for agency planning, will also provide very valuable input to the Decadal Survey.

Interest in planet searches, in the characterization of planets, and in the broader scientific issues encompassing planet formation is rapidly growing in the community. The technological challenges associated with planet searching and characterization are formidable. This has led to a number of extremely innovative techniques and approaches being developed and applied on the ground and under consideration for use in future facilities in space. In the near-term a number of space missions, including HST and Spitzer, are being used to address the scientific questions with several missions planned or under discussion for the future, including Kepler, SIM, TPF-C, TPF-I, and Darwin. The science case for current and future large ground-based telescopes with innovative (and very challenging) AO capabilities includes programs that are contributing to this topic or are planned to do so. Given this great interest in the field of ExoPlanet research, and the challenges and high cost of both ground- and space-based experiments and missions, it would be very timely and valuable to undertake a study similar to what has been done recently for the CMB and for dark energy. The results of such a study would also be very valuable input for the next Decadal Survey. Given the dramatic changes that have occurred at NASA in the last two years with regard to

planet searching, the recommendations of such a group could also provide a more stable framework under which a planet search/characterization program could be developed. The AAAC recommends that the agencies consider establishing such a task force this year. Once started, this activity would likely take over a year, and so the availability of a report late in 2007 or early in 2008 would allow the community to build on its findings and recommendations in time for the next Decadal Survey later this decade.

Synergy

Another aspect of the synergy between ground and space has surfaced as a result of developments over the last year. The focus at NASA on the search for other planets (see TPF §5.10) has highlighted the scientific and public interest that is developing in the search for planets around other stars, their characterization and the broader issue of planetary system formation and evolution. Recent developments in adaptive optics (AO—and particularly the potential of what is now called Extreme AO—ExAO) have led many researchers into thinking about the great potential of large telescopes in the GSMT-class for tackling these problems in the upcoming decade. The high resolution available in the infrared with 30-m class, AO-equipped telescopes enables observations of some planets and disks closer to other stars than can be done with space telescopes with their smaller mirrors. Again, these ground-based capabilities will complement the space observatories under discussion and allow synergistic approaches to investigating how planetary systems develop around stars. See §6.9 for a discussion of the AAAC recommendation that the agencies form an ExoPlanet Task Force to evaluate the approaches to planet detection and characterization on the ground and in space.

National Science Foundation
and the
National Aeronautics and Space Administration

DEC 19 2006

Professor Garth Illingworth
University of California Santa Cruz
Lick Observatory
Santa Cruz, California 95064

Dear Dr. Illingworth:

This letter is to request that the Astronomy and Astrophysics Advisory Committee (AAAC) establish an Exo-Planet Task Force (ExoPTF) as a subcommittee to advise NSF and NASA on the future of the ground-based and space-based search for and study of exo-planets, planetary systems, Earth-like planets and habitable environments around other stars.

Background and Purpose

In the past 10 years more than 200 planets have been detected in orbit around nearby stars. During this time, the study of exo-planets and systems has blossomed into a mainstream activity that engages hundreds of astronomers in the U.S. and around the world, and many community teams have successfully competed for federal support to carry out exo-planet research. Progress in the technologies of precision radial velocity measurement has reinvigorated the classical Doppler shift technique, which is steadily improving capability to find smaller planets close to or in the habitable zone. Transit detections are providing increasingly valuable constraints from both ground and space programs. Gravitational micro-lensing is beginning to probe an Earth-mass planet population. Imagery of pre-planetary and planetary debris disk arcs and rings is becoming available to confront theoretical models for planet formation and evolution. The recent and continuing dramatic successes of exo-planet programs strongly validate the search for Earth-like, habitable planets in orbit around nearby stars.

The study of exo-systems is very challenging and remains strongly limited by the scale and performance of the available tools. Nonetheless, impressive efforts are underway from the ground through existing and new federal, private and international facilities. These include dedicated telescopes and ongoing experiments, as well as traditional telescopes accessed through the normal proposal process, some with specialized instruments. Promising approaches under development include extremely large telescopes, extreme adaptive optics with new coronagraphic methods, millimetric and submillimetric imaging with the Atacama Large Millimeter Array (ALMA), and optical interferometry over long baselines. As exo-planet science addresses increasingly difficult questions, scientists are led to more advanced instrument concepts, with higher costs and longer lead times.

Moreover, while much exo-planet research continues to be carried out on the ground, space platforms will offer a unique advantage for the most sensitive measurements, and NASA has responded to this opportunity. In 1995, the NASA report *Roadmap for Exploration of Neighboring Planetary Systems* (a.k.a. "the Townes report"; http://origins.jpl.nasa.gov/library/exnps/) described a program to detect Earth-like planets orbiting the nearest stars and to characterize the atmospheres of the brightest of

these planets. NASA endorsed and responded to this opportunity with the broadly based Origins Theme and Program. More recently, the NASA Navigator Program was established with the prime objective of advanced telescope searches for exo-solar planets and habitable environments. In 2004 the search for exo-planets was featured in the President's Vision for U.S. Space Exploration, and the search is a central element of NASA's Astrophysics Plan. This basic approach to research in exo-planetology has found support in the last two National Research Council Decadal Surveys and has been revalidated and endorsed in three community-prepared Origins Roadmaps. The Kepler transit survey telescope, now in an advanced stage of development, will return measurements of the statistical frequency of Earth-sized planets. Technology development and engineering demonstrations in the Navigator Program have produced mission-enabling technology advances in precision metrology, interferometric nulling, and coronagraphy. The Space Interferometry Mission (SIM-PlanetQuest) is in Phase B (formulation), and the Terrestrial Planet Finder Coronagraph (TPF-C) and Interferometer (TPF-I) missions are in pre-formulation study.

In view of the rapid recent progress, the high scientific and public interest, and the probable large cost, it is timely and appropriate to reassess the national strategy in this area. The task force study will be conveniently timed for consideration by the next Astronomy and Astrophysics decade review.

Charge to the Task Force

The ExoPTF is asked to recommend a 15-year strategy to detect and characterize exo-planets and planetary systems, and their formation and evolution, including specifically the identification of nearby candidate Earth-like planets and study of their habitability. The strategy may include planning and preparation for facilities and missions beyond the 15-year horizon. Since future funding levels are uncertain, and project costs are difficult to establish at an early stage, it is important to develop an efficient and adaptable plan. To the extent possible, the recommendations should accommodate a range of funding levels representing conservative and aggressive programs. The ExoPTF will work in cooperation with agency efforts to advance the justification, specification and optimization of planet finding and characterizing opportunities.

The ExoPTF is asked to address the following specific areas:

1. The key scientific questions and issues, in the context of recent developments in exo-planet science and planet formation;
2. Measurement techniques, their enabling technologies and their implications for future survey and measurement directions and priorities;
3. Specific types of experiments (e.g., radial velocity measurements, transit searches, microlensing, adaptive optics, coronagraphy) with respect to their expected scientific return and contributions;
4. The potential and complementary science return from measurements at different wavelengths;
5. The role of theoretical investigations in defining needed capabilities, constraining measurement requirements, and interpreting results in terms of the overarching scientific questions;
6. Major decision points in the exo-planet study process;
7. Identification of key technologies relevant to the scientific goals of the program;
8. Important steps in the development of instrumentation, R&D, and other work required in preparation for or in support of, these and related experiments and missions;
9. The complementary ground-based and space-based research opportunities, coordination between funding agencies and possible synergistic advances;
10. Opportunities for cooperation, coordination or synergy with international programs.

The ExoPTF is not constituted to review individual proposals to determine their technical feasibility or likelihood of meeting performance goals. However, in recognition of the difficult technological challenges associated ultimately with the direct detection of Earth-like planets, the ExoPTF must address carefully a measured program of technology development that can lead to optimal and affordable facilities and missions.

Composition of the Panel and Community Input

The challenge of finding other habitable planets and searching for life will draw on many fields of science and technology. The Task Force should engage these issues with a broad representation of experience and expertise. Early in its activity, the ExoPTF should solicit white papers from the community, in addition to arranging for invited briefings by groups and individuals active in exo-system research.

Reporting

The ExoPTF Chair is responsible for preparing the final report in consultation with all ExoPTF members. In accordance with Federal Advisory Committee Act (FACA) rules, this report will be discussed independently at the first meeting of the AAAC following completion of the report, before formal presentation to the agencies. We request that the ExoPTF prepare their report for submission to the AAAC with a target date of October 1, 2007.

We thank you for your efforts and wish you success in this important endeavor.

Sincerely,

Tony F. Chan
Assistant Director, Directorate for
Mathematical and Physical Sciences
National Science Foundation

Richard J. Howard
Acting Director, Astrophysics Division
Science Mission Directorate
National Aeronautics and Space Administration

cc: G. W. Van Citters, NSF–AST

P. Hertz, NASA–Science Mission Directorate
M. H. Salamon, NASA–Astrophysics Division

ANSWERS TO POST-HEARING QUESTIONS

Responses by Daniel N. Baker, Director, Laboratory for Atmospheric and Space Physics, University of Colorado, Boulder

Questions submitted by Chairman Mark Udall

Q1. NASA has imposed cost "caps" on a number of its small- and medium-sized mission programs, such as Discovery. With the advent of full-cost accounting, are those cost caps still realistic or should they be adjusted? Have the cost caps proven to be an effective tool?

A1. Capping costs on projects provides one part of the framework for the principal investigator and project manager to actively manage the project. This is healthy for a program. Projects could always use more funding (if available) and so balancing costs with scientific return is part of the management challenge. One of the significant factors in the overall cost of missions is the launch vehicle cost, and factoring this cost in should be one of the key considerations in setting the mission cap.

The effect of the full-cost accounting on the mission cost cap is somewhat dependent on the mission. The new full-cost accounting rules only apply for the NASA centers. Other subcontracts for NASA, such as to LASP for satellite instruments, have been implementing full-cost accounting since the 1990s. Consequently, PI-mode missions where NASA centers primarily provide oversight have seen minor adjustments when NASA centers converted to full-cost accounting. However, missions at NASA centers have seen significant growth in costs. Some growth has just been in where the cost is book kept, but also some real cost growth that would require a higher cost cap. I believe that NASA centers are able to provide more detailed information on the specific cost growths that have resulted.

I believe that full cost accounting has not been helpful to cost-capped projects. Full cost accounting forces everyone in NASA centers to actively charge to a program code. While this might sound like a responsible and sensible approach to managing cost, it is one of the principal reasons for requirements creep in flight missions, making management-to-cost extremely challenging. Although planned as an accounting change that would be revenue-neutral, full cost accounting has, in fact, resulted in a monetary tax on each program that was not previously there. The Agency is struggling with this change, as the culture of NASA has in the past been collaborative and collegial in nature. NASA was never a corporation, and full cost accounting is a poor prescription for improving the Agency's performance. Two impacts are unfortunate: First, individuals within the NASA structure who had not been actively charging to programs are now actively seeking out projects to charge to—whether or not they can actively contribute. This effectively creates a tax on projects. In additional to direct costs rising, cost-capped programs are being forced to absorb more staff who, in turn, need to somehow demonstrate their meaningful participation. Reviews are longer and more numerous, there are more action items—many of which are unnecessary, yet need to be addressed—and consequently more inefficiency as a result of larger teams. Setting the idea of a cost capped mission aside, it is the view of myself and my colleagues at LASP that NASA's ability to team and collaborate in a collegial way has been hurt by the move to full cost accounting.

Q2. The National Academies report, Rising Above the Gathering Storm, *recommends "emphasis on physical sciences, engineering, mathematics and information sciences," as well as high-risk research, grants to early career researchers, and funding for advanced research instrumentation and facilities, among other actions, that can help foster innovation and sustain a strong economy. How relevant are NASA's space science research programs to those recommendations? Can you offer any specific examples? NASA's science program was not included in the President's American Competitiveness Initiative (ACI). Would you advocate NASA's science programs be made part of the ACI in future budgets? If not, why not?*

A2. NASA should be included in the American Competitiveness Initiative (ACI) as NASA's satellite programs are science-based missions that require advanced instrumentation, require highly technical developments in many different engineering areas, develop sophisticated data processing and distribution systems, and offer education and hands-on training for students and early career researchers. LASP's experience with NASA's PI-mode missions (SORCE and AIM most recently) clearly support many ACI objectives, and this really can be said for most NASA missions.

It has often been noted that NASA technology development can—and does—play a big role in U.S. economic competitiveness.

NASA is becoming more aware of the need to support the recommendations of *"Gathering Storm,"* at least as they pertain to supporting education infrastructure and young scientists. NASA has not fully articulated how the Agency would support ACI, but it is clear that the shift in EPO (Education and Public Outreach) from K–12 and informal education now to the inclusion of undergraduate and, in some cases, graduate education is a response to this. There seems to be a growing understanding at NASA that universities need to be playing a primary role in this much-improved EPO effort.

It is unclear why NASA was not invited to join the DOE and the NSF as partners in the ACI. With the emphasis in the ACI of connecting industry, education and government in supporting and sustaining innovation in science and engineering, surely NASA has a track record that merits its participation. I think it particularly crucial that NASA be included in the ACI at a time when the Agency is actively engaged in looking at workforce issues.[1] Targeted NASA ACI resources that support workforce training, particularly in the engagement of universities and sub-orbital programs, would be ideal.

Q3. A recently released study of the National Academy of Sciences on Building a Better NASA Workforce *recommended 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."

a. Could you please explain in concrete terms how the sub-orbital programs are used to train students and young workers?

b. What do these sub-orbital programs typically cost and do they produce peer-reviewed research?

A3. Sub-orbital programs have always had three key elements that differentiate the work from a larger space program: 1) Sub-orbital programs are typically hands-on projects, providing participants with a broad experience that is not possible to get on a larger project. 2) Sub-orbital projects tend to be of shorter duration, allowing participants to see the project from start to finish. It is almost always the case that everyone on a sub-orbital team gets experience in all phases of the effort; and 3) Results are usually immediate. This "instant feedback" goes hand-in-hand with trial-and-error learning that cannot be experienced in larger, more risk-adverse space programs. Sub-orbital programs have been the training ground for engineers and scientists, and keeping these projects supported will add to the vitality of the space program.

Sub-orbital Student Training

A typical sub-orbital program at LASP includes support primarily for an experienced instrument scientist, an experienced system engineer, a graduate student, and a couple of undergraduate students. The students, with the help of close mentoring from the professional staff, are responsible for most of the work. They are also heavily involved with the project from cradle-to-grave, as the same students design instruments, fabricate and assemble the instruments, calibrate and test the instruments, integrate and then launch the rocket payload at a NASA facility, and finally analyze and model the rocket data. All of this work is over a 2–3 year period, which is commensurate with a student's timetable for completing college. In contrast, a typical satellite program at LASP has a duration of about 10 years, and student involvement on such programs is necessarily more limited. Typically, the student will support the professional staff and he/she will not be responsible for major project development milestones.

Sub-orbital Costs

A typical sub-orbital program at LASP costs about $300K per year and has a duration of three years (two years to develop the payload and launch, with data analysis in third year). Because the payload is recovered, it can be flown multiple times, usually with enhancements developed by successive students. The LASP cost for a re-flight is about $150K. In addition to the science payload cost, NASA's cost for its rocket subsystems, launch vehicles, and launch range is about $2M per launch.

[1] See National Research Council, 2007. *Building a Better NASA Workforce: Meeting the Workforce Needs for the National Vision for Space Exploration,* National Academies Press, Washington, D.C.

Peer-Reviewed Research

The NASA sub-orbital program research is peer-reviewed, initially through the research proposals that are submitted to NASA and peer-reviewed by a NASA proposal panel, and later through peer-reviewed research papers as a result of the analysis of the rocket measurements. The LASP graduate students involved in the sub-orbital program are expected to write first-authored research papers, and the results from the rocket flights often lead to a Ph.D. dissertation for the graduate students.

Q4. NASA's Research & Analysis (R&A) programs are mentioned as being critical for developing new mission concepts and advanced technology. What impacts will the cutbacks in R&A have on the opportunities for future missions and programs? If R&A remains at current levels, what are we likely to see, or not see, in the next five years?

A4. Much of the technologies for satellite programs depend on research development prior to program initiation. Therefore, the NASA Research & Analysis (R&A) funds are critical to develop new instrument concepts, detector technology, and satellite subsystems, and then select the best, most-proven technology for the satellite flights. The sub-orbital rocket program is a excellent example where technology is developed quickly to address specific science objectives. These results are then used successfully for more detailed, thorough investigations on satellite missions that can last for years.

If R&A remains at current levels, LASP expects that its staff number will shrink (both science and technical staff) because many employees are still working on multiple-year grants that were established some years ago when the R&A program was more robust. In addition, this effective reduction in grant budgets will force LASP to reduce the number of students involved in the Lab's research projects, and laboratory and facility maintenance is likely to suffer.

It is likely that reducing of R&A programs will increase the competitiveness of the environment: Good researchers will potentially improve, marginal researchers will not be able to make a living in the business, and new people will find increased difficulty in gaining access into space research. As some point the idea of a critical mass of researchers has to enter into policy-makers' thinking. I have great concern that we are not at a healthy mass now: smaller definitely is not better.

Q5. What is the current frequency of Explorer and Discovery missions, and what do you believe should be the frequency of launch opportunities.if we want to sustain a healthy space science research program in each of the disciplines?

A5. For Small-class Explorers (SMEX), the recent launch frequency is one every four years (AIM was launched in 2007 and GALEX was launched in 2003). The next SMEX, IBEX, is planned for launch in 2008 and then there is a four-year gap until the next SMEX is launched in 2012.

For Medium-class Explorers (MIDEX), the frequency is one every 2.5 years (SWIFT launched in 2004, THEMIS in 2007, and WISE is planned for 2009). With the current funding profile for Explorer program, there is expected to be a very large gap until the next MIDEX is launched in 2017!

For Discovery missions, the recent launch frequency is about one every year (Genesis in 2001, CONTOUR in 2002, MESSENGER in 2004, Deep Impact in 2005, DAWN planned in 2007, and Kepler planned in 2008).

For Earth System Science Pathfinders (ESSP), the launch frequency had been about one every two years (GRACE in 2002, CALIPSO in 2006, CloudSat in 2006, OCO planned in 2008, and Aquarius planned in 2009. However, the HYDROS mission scheduled for 2011 was not selected for formulation and there have been no solicitations for new ESSP missions since the round that selected OCO and Aquarius in 2002.

There are 24 key measurement parameters defined for the NASA Earth Observing System (EOS). Assuming that a small satellite (e.g., ESSP) could measure two key parameters and that each mission could last for six years, then the Earth Science division needs to have two launches each year.

One of the original goals for these programs is to have a launch once a year to keep the space science research program healthy, so the present mission lines are under-funded by about a factor of two to accomplish this goal. At this present pace of launch opportunities, the community is badly impacted in numerous ways. The chance of being selected in any competition—even for highly experienced teams with capable management skills—is in the neighborhood of 10–20 percent. Having both infrequent opportunities to propose and small chances of being selected means that fewer and fewer groups will survive to propose (or will be able to afford to propose). This could be a prescription for disaster in the science community. It is crucial to get the launch rate for smaller missions to a much higher level.

Questions submitted by Representative Ken Calvert

Q1. What metric should NASA use to establish an appropriate level of technology development investment across the programs? Should it be a percentage of the overall program funding, or should it be a fixed amount?

A1. The present NASA science program has very little specific funding for instrument development or for advanced technology development. Many small-end missions have been living on "off-the-shelf" instruments.[2] This is unsustainable. It is crucially important that resources be identified and made available to raise instrument and spacecraft technologies to a high readiness level in order to avoid development delays when missions are in full implementation phases.

A percentage of program total cost could be a useful guideline for gauging technology development. Some missions will require higher technology development than others, and also trades between risk and cutting-edge science is highly dependent on the mission objectives. So like most guidelines, the technology development metric should not be a rigid requirement.

Q2. What mission assurance and management requirements imposed by NASA do you believe are counter-productive or impose costs that are disproportionate to the size of the mission, or that offer little added value?

A2. There once was a management belief at NASA that the comparatively lower-cost and more frequent small satellite programs should require lower quality parts, fewer documents and processes, and fewer reviews than those for large satellite programs. The current NASA management approach is largely driven by risk aversion. Consequently, requirements for small satellite programs have evolved to have the same high level of mission assurance and management as that of the large satellite programs. However, the allocated resources for small missions have remained relatively low over the past decade. Thus, new small missions are forced to have much reduced science goals in order to afford the higher cost mission assurance expectations. The space science community generally supports having more small satellite missions that have more science per dollar that can be accomplished by having less management oversight and accepting higher risks.

The recent AIM spacecraft launched by NASA (and managed by LASP) had 54 additional reviews during the course of its development compared to what was budgeted for the program originally. This horrendous additional load on engineers, scientists, and managers for the program was nearly unsustainable. Such a huge review load detracted from real design and development work, distracted engineers and managers from their real jobs, and for the most part did not add substantial value. Such out-of-control risk aversion must be reigned in or else NASA will not be able to afford any meaningful science flight program.

Q3. How should NASA and the space science disciplines best develop estimated mission costs, at a reasonable level of confidence, during the next round of decadal surveys? Who should perform these estimates? What level of confidence do you believe is appropriate?

A3. NASA has primarily used scientists for defining the mission concepts for its decadal surveys. While scientists are required to specify the science objectives critical for strategic planning, most scientists are not well trained in costing all components and phases of satellite missions. Future mission cost estimates could be improved by having at least two panels, one of primarily scientists for science planning and one of primarily satellite engineers for mission definition, with significant overlap between the panel members.

When developing the first concepts for a mission, a 50 percent margin (reserve) is usually added for resources like mass, power, and data rate, and a similar 50 percent margin should also be added to the first cost estimates. NASA missions have sometimes been defined with these early mission concepts and then later forced to reduce scope or have been canceled because of exceeding the expected cost cap. Starting with realistic cost estimate and with generous margin can mitigate this type of mission development disaster. Recently, the National Research Council published a report of a workshop on decadal surveys that addresses the issues above in greater detail.[3]

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[2] This point has been made in a number of NRC reports; e.g., National Research Council, 1997. *Scientific Assessment of NASA's SMEX–MIDEX Space Physics Mission Selections,* National Academies Press, Washington, D.C.

[3] National Research Council, 2007. DECADAL SCIENCE STRATEGY SURVEYS: REPORT OF A WORKSHOP, (2007), National Academies Press, Washington, D.C.

ANSWERS TO POST-HEARING QUESTIONS

Responses by Joseph A. Burns, Irving P. Church Professor of Engineering and Astronomy; Vice Provost, Physical Sciences and Engineering, Cornell University

Please recognize that my answers, unless noted otherwise, represent the perspective of someone who has participated primarily in the exploration of the solar system. My knowledge of other disciplines of space science is through reading and listening at committee and department meetings, not through practice.

Questions submitted by Chairman Mark Udall

Q1. NASA has imposed cost "caps" on number of its small- and medium-sized mission programs, such as Discovery. With the advent of full-cost accounting, are those cost caps still realistic or should they be adjusted? Have the cost caps proven to be an effective tool?

A1. As an academic never involved in NASA's business models and bookkeeping procedures, I do not know how full-cost accounting has influenced the agency's actual out-of-pocket expenses, nor its use of federal employees vs. contractors. Thus I cannot address whether the cost caps should be adjusted specifically to accommodate the agency's newly instated method of accounting.

I can note, however, that any effectiveness of the cost caps in limiting expenses today has been compromised by problems both inside and outside the agency's control. First, excessive program reviews carried out within NASA Headquarters (as well as those done at its request) have added expense and time to the development of missions [see the response to Representative Calvert's #2 below]. Furthermore, the poor estimation of costs when missions are proposed followed then by a lack of discipline as missions are developed has also made it difficult for the agency to carry through on the caps. These failings by proposing teams mean that NASA is then confronted by an unpleasant choice, often late in the game: either lose the funds already spent or fly a mission that accomplishes little more than previous flights. Delays associated with insufficient funds lead to a stutter-step development, further increasing costs. Finally, cost caps over the last few years have had to confront major cost breakers outside the agency's control, namely a substantial inflation in the cost of launches plus the added expenses associated with ITAR requirements.

An important subtext to this topic is specifically what cost caps should particular mission classes have and can effective missions be developed within contemporary cost caps, currently $425 M (FY06) for Discovery and $750 M in FY07$ for New Frontiers, according to the SSE 2006 Roadmap. A simple test exists: are the current missions in these classes providing good value. Most observers would answer, 'Yes!' but with some strain in their voices concerning the future, as missions necessarily become more ambitious payloads increase in sophistication, technology costs grow at above the inflation rate, and launch vehicles are less available but more costly.

Q2. The National Academies report, Rising Above the Gathering Storm, *recommends "emphasis on physical sciences, engineering, mathematics and information sciences," as well as high-risk research, grants to early career researchers, and funding for advanced research instrumentation and facilities, among other actions, that can help foster innovation and sustain a strong economy. How relevant are NASA's space science research programs to those recommendations? Can you offer any specific examples? NASA's science program was not included in the President's American Competitiveness Initiative (ACI). Would you advocate NASA's science programs be made part of the ACI in future budgets? If not, why not?*

A2. The influential NAS report *Rising Above the Gathering Storm* convincingly documents and argues that America must bolster its competitiveness by strategically strengthening those disciplines that most contribute to our global business position. I firmly believe the same point: the physical sciences and engineering are crucial if the United States is to remain internationally competitive. It is primarily our technological prowess that sets the U.S. apart from its economic competitors.

NASA's Space Science Research Program covers many economically important fields through its support for fundamental research in physics, chemistry and biology. These sciences contribute directly to placing our nation in the world's forefront technologically, thus stimulating the economy. This connection is perhaps a little less apparent for the earth sciences, space physics and astronomical sciences. Nonetheless research in such scientific subjects benefits the mining and oil industries, the energy sector, telecommunications companies and those investigating plasma fusion. NASA's space science missions and their instrument payloads have obvious ap-

plications of considerable interest to a number of commercial and defense spacecraft builders. To mention just a few engineering examples relevant to these latter industries, NASA's Space Science Research Program supports instrument design, remote-sensing devices across a broad spectrum, the miniaturization of advanced detectors and image-processing algorithms.

In addition, the space program plays an important role in our country's science education at all levels. Images of Earth's planetary siblings and the wider universe appeal broadly to the public, especially young people. Such materials help to attract K–12 students to the STEM disciplines. At the other end of the educational chain, graduate students drawn to, and trained in, the space sciences often move into the commercial and defense sectors upon graduation.

Thus I feel that many of *Gathering*'s recommendations apply equally to NASA research as to NSF, DOE and DOD research. Accordingly I urge Congress to include NASA's Space Science Program in the President's American Competitiveness Initiative. Its exclusion was an oversight.

Q3. A recently released study of the National Academy of Sciences on Building a Better NASA Workforce *recommended 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."*

a. Could you please explain in concrete terms how the sub-orbital programs are used to train students and young workers?

b. What do these sub-orbital programs typically cost and do they produce peer-reviewed research?

A3. Low-altitude terrestrial flights employing rockets, aircraft and balloons are generally not effective schemes for pursuing **planetary** research: missions can achieve much better resolution and coverage with close planetary encounters. Hence these Earth-bound platforms are no longer actively employed by planetary scientists. However, I understand that the sub-orbital program benefits other space-science disciplines in NASA. This program provides effective training for students because its missions flown are usually small-scale and, of course, nearby. Hence students can complete an end-to-end project (wherein they select a topic and target, build appropriate instrumentation, launch and collect data, and finally analyze results) for their doctoral dissertations rather than being assigned to a single aspect of research (e.g., data analysis) within a large team. For students in planetary exploration, some of the benefits of rocket and balloon research (e.g., hands-on) may be gained by building instruments for Discovery missions.

My answer to this question highlights that the various disciplines of space sciences may have substantial differences in research techniques and funding. One of the questions that was asked during our May 2 hearing dealt with data analysis funds for various missions and the answer given by an astronomer on the panel (who probably was considering the success of the investigator support for astrophysics' "Great Observatories" (e.g., Hubble, Chandra and Spitzer) was that all was well. Time prevented me from mentioning that this is *not true for* the information returned by recent *planetary missions,* whose data analysis has been sorely underfunded. As a personal example, I received no funds as an associate of the Galileo mission to Jupiter beyond travel support even though I planned all the image sequences of Jupiter's rings and published the primary papers interpreting those results. After this several-billion-dollar mission ended, the total funds for the analysis of its returned data was a few million dollars. As another local illustration, I am a current member of the imaging team on the Cassini mission (the latest solar-system-exploration flagship) in orbit about Saturn since 2004, for which my support is primarily for planning and archiving data, not for modeling or interpretation. My group's analysis of the data is covered by six weeks of summer salary. We've been fairly effective only because Cornell has been partly supporting my research. As in the case of Galileo, the Cassini data analysis program for last year and again this year only receives about $2–3 M. Effectively the U.S. is annually spending about 0.1 percent of the total planetary mission cost for the analysis of the data that is the advertised reason for the mission. This seems very low to me.

The less than optimal funding for doing science on missions extends to the Discovery line.

Q4. NASA's Research & Analysis (R&A) programs are mentioned as being critical for developing new mission concepts and advanced technology. What impacts will the cutbacks in R&A have on the opportunities for future missions and programs? If R&A remains at current levels, what are we likely to see, or not see, in the next five years?

A4. The researchers supported by R&A funds accomplish more than simply publish papers that interpret the data returned by missions. They deepen our understanding of our solar-system surroundings, thereby introducing new paradigms for how the planets work and how the solar system originated. As a result of these findings, the program as a whole evolves: new mission concepts and perhaps new targets are chosen in order to address the latest "big questions."

A large fraction of the R&A monies that are awarded to university staff are used to hire graduate students. So these funds also produce the next generation of space explorers, the individuals who will be designing, building and operating future missions and programs. In addition, R&A dollars are used to develop improved instruments to permit more sensitive and broader observations.

Because of the crucial role that R&A programs play in shaping the future missions, as laid out in the paragraphs above, the answer to the last question is straightforward. If NASA's planetary R&A budget continues at its present level, which is 25 percent below FY05 support and even further beneath that recommended by the 2003 decadal report, the program will be addressing last year's questions with an older, less numerous work force using outdated equipment.

Q5. What is the current frequency of Explorer and Discovery missions, and what do you believe should be the frequency of launch opportunities if we want to sustain a healthy space science research program in each of the disciplines?

A5. The Discovery program was initiated to provide a continuing stream of low-cost, focused, innovative missions, chosen competitively, that would complement the much-less-frequent but much more competent and broader flagship missions that are usually designed conservatively. The Scout program serves the same function within the Mars program. The Discovery line is now fifteen years old, during which time seven missions have been launched, eight if the Dawn spacecraft flies this summer as planned. That's one flight every 22 months, whereas the original plan was to launch one every 18 months. That is a good record. However, the selection rate has slowed dramatically: after choosing a pair in each of alternate years (FY 95, FY97,. . .) no selections have been made since FY01, although two (responding to last year's AO) may be yet chosen in FY07. There is a reasonable concern that this slow rate will continue due to potential future budget reductions with the higher cost cap and increased launch costs with the end of the Delta 2 line.

The New Frontiers line for medium-scale missions was endorsed by the 2003 decadal panel and then placed in the NASA budget. This line was jump-started with the already-in-process New Horizons (based on an FY01 AO). The rate of New Frontiers too may have slowed; Juno was chosen from an FY05 solicitation and the next AO is yet to be released. This slower pace for both Discovery and New Frontiers should be placed against the backdrop of the indefinite deferral of both flagship missions (Europa Geophysical Orbiter and Mars Sample Return) recommended by the decadal panel. These delays likely result from the draining of $4B from the science program, the vast majority coming from solar system exploration. Analysts suggest that a minimum of $200 M more annually would be needed in the Planetary Sciences Division in order to meet the decadal survey's strategic goals.

Discovery and New Frontiers require frequent launches to fulfill their roles as recommended by the decadal report. Based on the historical record, 5–6 Discovery missions per decade should be sustainable along with 2–3 New Frontiers per decade.

Questions submitted by Representative Ken Calvert

Q1. What metric should NASA use to establish an appropriate level of technology development investment across the programs? Should it be a percentage of the overall program funding, or should it be a fixed amount?

A1. While I can't give you an informed number, technology development should be a fixed percentage or, better, within a fixed range of percentages, flexible on an annual basis, of the total mission development budget. The metric should be developed depending on the anticipated needs for future missions in the decadal plan.

One will want to have a range within which to operate, so that the amount is somewhat variable as demand changes. For example, preparations to return a sample from the Moon are quite different than if we wish to take a piece of a comet back to Earth. Similarly, it's much easier, and already has been demonstrated, to drop a long-lived mobile laboratory onto the plains of Mars, than it will be to parachute a capable probe through Venus's lethal environment onto its scorching surface. Any sophisticated in-situ laboratory will demand much advanced development and the associated funding stream as will many generic. outer solar system vehicles. Because of this variability in the sorts of technology development that is required

at any particular time, it is only rational if the level of funding is a percentage of overall program funding. However, what the correct percentage is for the next decade is a question that should be studied by the NRC. For a well-managed, focused program, I would guess that 10 percent is appropriate, but that's only a guess.

To some degree, however, this query overlooks another important issue. The fact is that NASA technology development has not generally been very effective in correctly choosing and then maturing the technologies necessary for future missions. In some areas, for example electronics and communications, the record is a "solid B." In others, for example, sampling devices, it is much less acceptable. What is required is a prioritized list of critical technology for the highest priority future missions and then the willingness and financing necessary to promptly produce these technologies. At present, it's as much the lack of focus as the lack of funds that has been a problem.

Q2. What mission assurance and management requirements imposed by NASA do you believe are counter-productive or impose costs that are disproportionate to the size of the mission, or that offer little added value?

A2. The mission assurance and management requirements imposed by NASA are well defined in its mission development directive 7120. Any additional reviews are superfluous and of no value. NASA HQ should re-adopt the 'trust but verify' attitude it used to take toward its mission implementers. Currently, NASA HQ's attitude is fearful and distrustful, imposing costly and even damaging additional reviews with every flight project hiccup or milestone. Today's world is increasingly risk-averse. To mitigate this, NASA should insist that decisions are made at the lowest possible levels, by individuals who are close to the details and specifics. Money is wasted when inappropriate requirements are imposed by someone far removed from the actual projects who does not understand what is appropriate.

Furthermore, the level of management burden imposed should be commensurate with the funds expended. Since New Frontiers missions cost two or three times less than Flagship missions, whereas Discovery missions typically cost one-half New Frontiers missions, the Discovery should not be expected to go through all the bureaucracy appropriate for a Cassini-class endeavor. The agency should be willing to accept more risk for Discovery than for any other mission class. In the original manifestation of Discovery, a significant fraction of these missions were expected to be high-risk since in part they demonstrated new technologies. One might ask whether today's missions in this line have been aggressive enough since only one has failed, and that was due to a well-known but occasional flaw in a conventional rocket.

Finally, any group of principal investigators will always, and probably justifiably, ask that the review processes be streamlined.

Q3. How should NASA and the space science disciplines best develop estimated mission costs, at a reasonable level of confidence, during the next round of decadal surveys? Who should perform these estimates? What level of confidence do you believe is appropriate?

A3. Forecasting estimated mission expenditures has been a perennial problem in the space sciences and it will never be entirely solved. Not every difficulty can be foreseen and some expenses lie outside the NASA's control, such as launch charges as well as the cost of radioisotope thermal generators and fuel. The prices for the development and ultimate construction of new technology items are notoriously hard to predict in all the scientific disciplines. Nonetheless, the recent scorecard within the agency and in the space industry has been poor. Forecasts of mission costs could be improved significantly by spending more in deriving these costs. Enough effort needs to be devoted to this early in order to develop a cost estimate that has a reasonable chance to be correct. History indicates that 1–2 percent of the eventual total mission price tag should be invested to get a reliable preliminary estimates of mission costs. By this metric, the $1M being spent on each of the current studies for outer planet missions is low by a factor of ten.

This indicates that much more investment should be made in order to get reliable cost estimates for the prospective missions in the beginning stages of any decadal report. Otherwise the estimates lack credibility, implying that the full report is likely to be unrealistic advice and hence flawed. Most likely NASA must take this responsibility on itself to use its implementers plus independent cost estimators to provide the technical studies and cost estimates. I believe that the NRC has a study underway about how to properly devise this process. A probable answer is that two or three independent estimates are needed from separate autonomous institutions. In order for these estimates to be meaningful, any mission's concept has to be clearly defined to make sure that each institution is costing the same mission.