Science Opportunities Enabled by NASA's Constellation System: Interim Report

Committee on Science Opportunities Enabled by NASA's Constellation System, National Research Council

ISBN: 0-309-12011-X, 70 pages, 8 1/2 x 11, (2008)

Science Opportunities Enabled by NASA's Constellation System—Interim Report

Committee on Science Opportunities Enabled by NASA's Constellation System

Space Studies Board
Aeronautics and Space Engineering Board

Division on Engineering and Physical Sciences

NATIONAL RESEARCH COUNCIL
OF THE NATIONAL ACADEMIES

THE NATIONAL ACADEMIES PRESS
Washington, D.C.
www.nap.edu

THE NATIONAL ACADEMIES PRESS 500 Fifth Street, N.W. Washington, DC 20001

NOTICE: The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This study is based on work supported by the Contract NNH06CE15B between the National Academy of Sciences and the National Aeronautics and Space Administration. Any opinions, findings, conclusions, or recommendations expressed in this publication are those of the author(s) and do not necessarily reflect the views of the agency that provided support for the project.

International Standard Book Number 13: 978-0-309-12010-4
International Standard Book Number 10: 0-309-12010-1

Copies of this report are available free of charge from:

Space Studies Board
National Research Council
500 Fifth Street, N.W.
Washington, DC 20001

Additional copies of this report are available from the National Academies Press, 500 Fifth Street, N.W., Lockbox 285, Washington, DC 20055; (800) 624-6242 or (202) 334-3313 (in the Washington metropolitan area); Internet, http://www.nap.edu.

Printed in the United States of America

THE NATIONAL ACADEMIES

Advisers to the Nation on Science, Engineering, and Medicine

The **National Academy of Sciences** is a private, nonprofit, self-perpetuating society of distinguished scholars engaged in scientific and engineering research, dedicated to the furtherance of science and technology and to their use for the general welfare. Upon the authority of the charter granted to it by the Congress in 1863, the Academy has a mandate that requires it to advise the federal government on scientific and technical matters. Dr. Ralph J. Cicerone is president of the National Academy of Sciences.

The **National Academy of Engineering** was established in 1964, under the charter of the National Academy of Sciences, as a parallel organization of outstanding engineers. It is autonomous in its administration and in the selection of its members, sharing with the National Academy of Sciences the responsibility for advising the federal government. The National Academy of Engineering also sponsors engineering programs aimed at meeting national needs, encourages education and research, and recognizes the superior achievements of engineers. Dr. Charles M. Vest is president of the National Academy of Engineering.

The **Institute of Medicine** was established in 1970 by the National Academy of Sciences to secure the services of eminent members of appropriate professions in the examination of policy matters pertaining to the health of the public. The Institute acts under the responsibility given to the National Academy of Sciences by its congressional charter to be an adviser to the federal government and, upon its own initiative, to identify issues of medical care, research, and education. Dr. Harvey V. Fineberg is president of the Institute of Medicine.

The **National Research Council** was organized by the National Academy of Sciences in 1916 to associate the broad community of science and technology with the Academy's purposes of furthering knowledge and advising the federal government. Functioning in accordance with general policies determined by the Academy, the Council has become the principal operating agency of both the National Academy of Sciences and the National Academy of Engineering in providing services to the government, the public, and the scientific and engineering communities. The Council is administered jointly by both Academies and the Institute of Medicine. Dr. Ralph J. Cicerone and Dr. Charles M. Vest are chair and vice chair, respectively, of the National Research Council.

www.national-academies.org

Other Reports of the Space Studies Board and the Aeronautics and Space Engineering Board

Opening New Frontiers in Space: Choices for the Next New Frontiers Announcement of Opportunity (SSB, 2008)
Space Science and the International Traffic in Arms Regulations: Summary of a Workshop (SSB, 2008)
Workshop Series on Issues in Space Science and Technology: Summary of Space and Earth Science Issues from the Workshop on U.S. Civil Space Policy (SSB, 2008)

Assessment of the NASA Astrobiology Institute (SSB, 2007)
An Astrobiology Strategy for the Exploration of Mars (SSB with the Board on Life Sciences [BLS], 2007)
Building a Better NASA Workforce: Meeting the Workforce Needs for the National Vision for Space Exploration (SSB with the Aeronautics and Space Engineering Board [ASEB], 2007)
Decadal Science Strategy Surveys: Report of a Workshop (SSB, 2007)
Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond (SSB, 2007)
Exploring Organic Environments in the Solar System (SSB with the Board on Chemical Sciences and Technology, 2007)
Grading NASA's Solar System Exploration Program: A Midterm Review (SSB, 2007)
The Limits of Organic Life in Planetary Systems (SSB with BLS, 2007)
NASA's Beyond Einstein Program: An Architecture for Implementation (SSB with the Board on Physics and Astronomy [BPA], 2007)
Options to Ensure the Climate Record from the NPOESS and GOES-R Spacecraft: A Workshop Report (SSB, 2007)
A Performance Assessment of NASA's Astrophysics Program (SSB with BPA, 2007)
Portals to the Universe: The NASA Astronomy Science Centers (SSB, 2007)
The Scientific Context for Exploration of the Moon (SSB, 2007)

An Assessment of Balance in NASA's Science Programs (SSB, 2006)
Assessment of NASA's Mars Architecture 2007-2016 (SSB, 2006)
Assessment of Planetary Protection Requirements for Venus Missions: Letter Report (SSB, 2006)
Decadal Survey of Civil Aeronautics: Foundation for the Future (ASEB, 2006)
Distributed Arrays of Small Instruments for Solar-Terrestrial Research: Report of a Workshop (SSB, 2006)
Issues Affecting the Future of the U.S. Space Science and Engineering Workforce: Interim Report (SSB with ASEB, 2006)
Review of NASA's 2006 Draft Science Plan: Letter Report (SSB, 2006)
Review of the Space Communications Program of NASA's Space Operations Mission Directorate (ASEB, 2006)
The Scientific Context for Exploration of the Moon—Interim Report (SSB, 2006)
Space Radiation Hazards and the Vision for Space Exploration (SSB, 2006)

Limited copies of these reports are available free of charge from

Space Studies Board
National Research Council
The Keck Center of the National Academies
500 Fifth Street, N.W., Washington, DC 20001
(202) 334-3477/ssb@nas.edu
www.nationalacademies.org/ssb/ssb.html

NOTE: These reports are listed according to the year of approval for release, which in some cases precedes the year of publication.

COMMITTEE ON SCIENCE OPPORTUNITIES ENABLED BY NASA'S CONSTELLATION SYSTEM

GEORGE A. PAULIKAS, The Aerospace Corporation (retired), *Chair*
KATHRYN C. THORNTON, University of Virginia, *Vice Chair*
CLAUDIA ALEXANDER, Jet Propulsion Laboratory
STEVEN V.W. BECKWITH, University of California System
MARK A. BROSMER, The Aerospace Corporation
JOSEPH BURNS, Cornell University
CYNTHIA CATTELL, University of Minnesota
ALAN DELAMERE, Ball Aerospace (retired)
MARGARET FINARELLI, George Mason University
TODD GARY, Tennessee State University
STEVEN HOWELL, National Optical Astronomy Observatories
ARLO LANDOLT, Louisiana State University
FRANK MARTIN, Martin Consulting
SPENCER R. TITLEY, University of Arizona
CARL WUNSCH, Massachusetts Institute of Technology

Staff

DWAYNE A. DAY, Study Director
VICTORIA SWISHER, Research Associate
CATHERINE A. GRUBER, Assistant Editor
RODNEY N. HOWARD, Senior Program Assistant

SPACE STUDIES BOARD

AERONAUTICS AND SPACE ENGINEERING BOARD

RAYMOND S. COLLADAY, Lockheed Martin Astronautics (retired), *Chair*
CHARLES F. BOLDEN, JR., Jack and Panther, LLC
ANTHONY J. BRODERICK, Aviation Safety Consultant
AMY L. BUHRIG, Boeing Commercial Airplanes
PIERRE CHAO, Center for Strategic and International Studies
INDERJIT CHOPRA, University of Maryland
ROBERT L. CRIPPEN, Thiokol Propulsion (retired)
DAVID GOLDSTON, Princeton University
JOHN HANSMAN, Massachusetts Institute of Technology
PRESTON HENNE, Gulfstream Aerospace Corporation
JOHN M. KLINEBERG, Space Systems/Loral (retired)
RICHARD KOHRS, Independent Consultant
IVETT LEYVA, Air Force Research Laboratory, Edwards Air Force Base
EDMOND SOLIDAY, United Airlines (retired)

MARCIA SMITH, Director

Preface

In January 2004 NASA was given a new policy direction known as the Vision for Space Exploration. That plan, now renamed the United States Space Exploration Policy, called for sending human and robotic missions to the Moon, Mars, and beyond. In 2005 NASA outlined how to conduct the first steps in implementing this policy and began development of a new human-carrying spacecraft, known as Orion, and the launch vehicles Ares I and Ares V. Collectively, these are called the Constellation System. In November 2007 NASA asked the National Research Council (NRC) to evaluate the potential for new science opportunities enabled by the Constellation System of rockets and spacecraft:

> The Space Studies Board, in conjunction with the Aeronautics and Space Engineering Board, will establish an ad hoc committee to assess potential space and Earth science mission concepts that could take advantage of the capabilities of the Constellation System of launch vehicles and spacecraft that is being developed by NASA. The ad hoc committee will first analyze mission concepts provided by NASA, and later mission concepts submitted in response to a Request for Information from the committee to the space and earth science communities. The committee will analyze the following information for each mission concept considered:
>
> 1. Scientific objectives of the mission concept;
> 2. A characterization of the mission concept insofar as the maturity of studies to date have developed it;
> 3. The relative technical feasibility of the mission concepts compared to each other;
> 4. The general cost category into which each mission concept is likely to fall;
> 5. Benefits of using the Constellation System's unique capabilities relative to alternative implementation approaches; and
> 6. Identification of the mission concept(s) most deserving of future study.
>
> The time horizon for the survey of possible missions should extend from 2020 to approximately 2035.
>
> For the interim report the committee will assess the mission concepts provided by NASA, and group them into two categories: more-deserving and less-deserving of future study. For the final report the committee will assess the set of mission concepts submitted in response to an RFI and group them into similar categories. The final report should then compare the mission concepts in the more-deserving categories for the interim and final reports and recommend a consolidated list of the mission concept(s) it deems most-deserving of future study for launch in the 2020-2035 time frame.

The Committee on Science Opportunities Enabled by NASA's Constellation System was formed to address this task. For this interim report, the committee evaluated 11 pre-existing "Vision Mission" studies that were conducted for NASA from 2005 to 2006. In early March 2008, the committee issued a request for information (see Appendix A) seeking proposals from the scientific community for mission concepts to evaluate for its final report. Appendix B provides committee member and staff biographical information.

Because this is an interim report and additional mission concepts will be submitted to the committee in response to its request for information, the committee has deferred discussion of a number of important issues raised in this initial phase of its study—such as technological issues and their impact on these mission concepts—and will address those more fully in its final report.

The committee acknowledges the assistance it received from NASA, particularly from Marc Allen, Marc Timm, and Phil Sumrall, in providing materials for this study.

The committee's first meeting was held in Washington, D.C., on February 20-22, 2008, and its second meeting—devoted entirely to writing this interim report—was held in Irvine, California, on March 17-19, 2008. The third meeting is scheduled for early June in Boulder, Colorado, and the final meeting will be held in August 2008. The committee's final report is expected to be delivered to NASA in November 2008.

Acknowledgment of Reviewers

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

Peter Banks, Astrolabe Ventures,
Otis B. Brown, Jr., University of Miami,
G. Scott Hubbard, Stanford University,
Richard H. Kohrs, NASA (retired),
Ruben van Mitchell, Independent Consultant,
Gordon H. Pettengill, Massachusetts Institute of Technology,
William H. Press, Los Alamos National Laboratory, and
Charles E. Woodward, University of Minnesota.

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

Contents

Summary

In 2004 NASA initiated studies of advanced science mission concepts known as the Vision Missions and inspired by a series of NASA roadmap activities conducted in 2003. Also in 2004 NASA began implementation of the first phases of a new space exploration policy, the Vision for Space Exploration. This implementation effort included development of a new human-carrying spacecraft, known as Orion, and two new launch vehicles, the Ares I and Ares V rockets—collectively called the Constellation System. NASA asked the National Research Council (NRC) to evaluate the science opportunities enabled by the Constellation System (see Preface) and to produce an interim report on a short time schedule and a final report by November 2008. The committee notes, however, that the Constellation System and its Orion and Ares vehicles have been justified by NASA and selected in order to enable human exploration beyond low Earth orbit, and not to enable science missions.

This interim report of the Committee on Science Opportunities Enabled by NASA's Constellation System evaluates the 11 Vision Mission studies presented to it and groups them into two categories: those more deserving of future study, and those less deserving of future study. Although its statement of task also refers to Earth science missions, the committee points out that the Vision Missions effort was focused on future astronomy, heliophysics, and planetary exploration and did not include any Earth science studies because, at the time, the NRC was conducting the first Earth science decadal survey, and funding Earth science studies as part of the Vision Missions effort would have interfered with that process. Consequently, no Earth science missions are evaluated in this interim report. However, the committee will evaluate any Earth science mission proposal submitted in response to its request for information issued in March 2008 (see Appendix A).

The committee based its evaluation of the preexisting Vision Missions studies on two criteria: whether the concepts offered the potential for a significant scientific advance, and whether or not the concepts would benefit from the Constellation System. The committee determined that all of the concepts offered the possibility of a significant scientific advance, but it cautions that such an evaluation ultimately must be made by the decadal survey process, and it emphasizes that this interim report's evaluation should not be considered to be an endorsement of the scientific merit of these proposals, which must of course be evaluated relative to other proposals.

The committee determined that seven of these concepts would benefit from the Constellation System, whereas four would not, but it stresses that this conclusion does not reflect an evaluation of the scientific merit of the projects, but rather an assessment of whether or not new capabilities provided by the Constellation System could significantly affect them. Some of the mission concepts, such as the Advanced Compton Telescope, already offer a significant scientific advance and fit easily within the mass and volume constraints of existing launch vehicles. Other mission concepts, such as the Palmer Quest proposal to drill through the Mars polar cap, are not constrained by the launch vehicle, but rather by other technology limitations. The committee evaluated the mission concepts as presented to it, aware nevertheless that proposing a far larger and more ambitious mission with the same science goals might be possible given the capabilities of the Ares V launch vehicle. (Such proposals can be submitted in response to the committee's request for information to be evaluated in its final report.) See Table S.1 for a summary of the Vision Missions, including their cost estimates, technical maturity, and reasons that they might benefit from the Constellation System.

The committee developed several findings and recommendations.

TABLE S.1 Summary of Vision Missions (in Alphabetical Order) Evaluated by the Committee

Vision Mission	Cost Estimate[a] (billions)	Technical Maturity[b]	Worthy of Further Study as a Constellation Mission?	Notes
Advanced Compton Telescope (ACT)	$1	Medium	No	This mission does not benefit from Constellation.
Generation-X (Gen-X)	>$5	Low	Yes	One Ares V launch of one 16-meter telescope is significantly simpler than the early proposed configurations. Cost estimates are weak. The additional mass capability could significantly reduce mirror development costs.
Interstellar Probe	$1-$5	High—concept, instruments Low—propulsion	Yes	Further study is needed of the benefits of additional launch mass enabled by Ares V, in particular alternative propulsion options.
Kilometer-Baseline Far-Infrared/ Submillimeter Interferometer	>$5	Low	No	The need for Constellation is questionable, except for human servicing.
Modern Universe Space Telescope (MUST)	>$5	High—mission concept, instruments Low—assembly	Yes	Large one-piece, central mirror is possible with Ares V rather than a robotically assembled mirror.
Neptune Orbiter with Probes	>$5	High—mission concept, instruments Low—propulsion and possibly lander	Yes	Ares V could possibly obviate the need for aerocapture and/or nuclear-electric propulsion.
Palmer Quest	>$5	Low	No	This mission does not benefit from Constellation.
Single Aperture Far Infrared Mission (SAFIR)	>$5	Medium—mission concept Low—cooling, detectors	No	This mission does not benefit from Constellation.
Solar Polar Imager	$1-$5	High—mission concept, instruments Low—propulsion	Yes	Consider propulsion options enabled by Ares V.
Stellar Imager	$5	Low	Yes	Could launch larger mirrors (2 meters vs. 1 meter) and a second hub on a single Ares V launch.
Titan Explorer	>$5	Low—requires aerocapture	Yes	Launch on Ares V may enable propulsive capture rather than aerocapture and shorten transit time.

[a] Cost estimates based on data provided to the committee.
[b] Technical maturity based on data provided to the committee.

Finding 1. The greatly increased payload capability promised by Ares V could lead to much more costly science payloads.

Finding 2. The committee determined that the Ares I capabilities are not sufficiently distinct from those of Atlas V and Delta IV to enable different types or a higher quality of space science missions.

Finding 3. The following Vision Mission studies might benefit from the opportunities enabled by the Constellation System and are therefore considered more deserving of future study: Generation-X, Modern Universe Space Telescope, Stellar Imager, Interstellar Probe, Solar Polar Imager, Neptune Orbiter with Probes, and Titan Explorer. The committee did not assess the relative scientific priority of the missions within this group. In the final report, these mission concepts will be compared to additional mission concepts (collected in response to the committee's request for information) that the committee determines to be more deserving of future study, and the committee will produce a consolidated list.

According to the committee's evaluation criteria, the four mission concepts that it deemed less deserving of future study simply do not appear to benefit highly from use of the Constellation System. The committee concluded that the seven more-deserving mission concepts require greater study of their scientific benefits and the technical benefits enabled by the Constellation System.

Recommendation 1. NASA should conduct further studies of the scientific benefits as well as the technical benefits to mission execution, such as reduction of mission complexity and risk, enabled by the Constellation System for the following missions: Generation-X, Modern Universe Space Telescope, Stellar Imager, Interstellar Probe, Solar Polar Imager, Neptune Orbiter with Probes, and Titan Explorer.

The committee accepted the cost estimates provided by the Vision Mission studies themselves or by the study representatives who presented them to the committee. Nevertheless, the committee concluded that these cost estimates are preliminary. The committee is concerned that the costs of these missions will be high, at least for the flagship-class missions, if not substantially higher. Given the fact that NASA has insufficient funding to support more than one flagship-class mission per decade in two science areas (essentially one for astronomy and astrophysics and one for solar system exploration, with the situation for Earth science and heliophysics being slightly more complicated), each of these missions would place substantial strain on the science budget, and the committee therefore emphasizes that close attention to cost issues is required. Since the committee was asked to consider missions that could be flown during the period 2020-2035, very few such large missions could possibly be funded during that period.

Finding 4. There are uncertainties in the cost estimates associated with the Vision Missions listed above when flown on the Ares V vehicle.

Although NASA has not yet produced cost estimates for many of the elements of the Constellation System, such as the Ares V launch vehicle, the committee recognized that utilization of the Constellation System, particularly the Ares V, could have a potentially dramatic effect on the costs of these missions. Incorporating the use of an expensive launch vehicle could increase costs. But it could also possibly balance increased costs by simplifying mission design (for instance, by eliminating the requirement for on-orbit assembly or complex deployment mechanisms).

Recommendation 2. NASA should perform cost analysis for the missions that the committee determined could benefit from the Ares V capability (Generation-X, Modern Universe Space Telescope, Stellar Imager, Interstellar Probe, Solar Polar Imager, Neptune Orbiter with Probes, and Titan Explorer). This analysis should use the Ares V technical capabilities together with appropriate upper stages as a baseline.

Virtually all of the mission concepts evaluated by the committee are large, complex, and costly. Several are similar to studies currently being undertaken by traditional international partners in space exploration.

Finding 5. International cooperation could provide access to international scientific expertise and technology useful for these missions, and could reduce costs through provision of foreign instruments and infrastructure.

The committee was charged with identifying the "benefits of using the Constellation System's unique capabilities relative to alternative implementation approaches." Alternative implementation approaches include technologies that allow the use of smaller launch vehicles (such as in the Evolved Expendable Launch Vehicle class that served as the baseline for the Vision Mission studies). The committee notes that several technology issues are shared by two or more missions. There are benefits to having multiple technology solutions available to achieve objectives, and the committee is concerned that it is risky to rely on only one solution that may never emerge. NASA currently lacks a technology development strategy, a gap identified by the NRC as a shortcoming.[1] The impact of technology on these missions and how it may require, or alleviate the need for, the use of the Constellation System requires further study and will be evaluated by the committee in its final report.

Finding 6. The committee identified the following technology issues as meriting further attention. Some of these technologies are of a basic, mission-enabling nature; others provide options that can be traded for alternative mission architectures.

- **Basic enabling technologies**
  —Free-flying constellations
  —Tethered flight
  —Next-generation Deep Space Network
  —Space nuclear reactors
- **Technologies enabling alternatives to Ares V**
  —Aerocapture
  —Solar sails
  —Solar-electric propulsion
  —Nuclear-electric propulsion
  —Robotic assembly and servicing
- **Technologies enhancing Constellation capabilities**
  —Human assembly and servicing

[1] See, for example, National Research Council, *Grading NASA's Solar System Exploration Program: A Midterm Review,* The National Academies Press, Washington, D.C., 2008, pp. 11 and 59-61.

1
The Constellation System and Opportunities for Science

NASA asked the National Research Council (NRC), through the Space Studies Board and the Aeronautics and Space Engineering Board, to establish an ad hoc committee to assess potential space and Earth science research concepts that could take advantage of the capabilities of the Constellation System of launch vehicles and spacecraft and could launch in the period 2020-2035. The Constellation System is being developed by NASA to implement the initial phases of the Vision for Space Exploration and consists of the Orion spacecraft and the Ares I and Ares V launch vehicles.

The Committee on Opportunities Enabled by NASA's Constellation System is conducting its assessment in two phases; this interim report represents the conclusion of the first phase. *This interim report is not intended as a review or endorsement of the Constellation System as a whole or of any of its elements, nor is this interim report intended to make any suggestions regarding potential changes to the Constellation System.* Rather, this interim report accepts the Constellation System as it was defined for the committee in February 2008 and seeks to assess a set of proposed science missions and identify those that could benefit from Constellation's capabilities and would potentially fly sometime after the next decade.[1] *In addition, this report does not prioritize science goals for NASA. That responsibility properly lies with the decadal survey process.*[2]

The kinds of science missions that the committee evaluated for this interim report—and the kind that it envisions will be proposed in response to its request for information—are relatively large and ambitious. They all fall into a category that is generally referred to as flagship-class missions. The committee grouped the missions into three "cost bins" for the purpose of this study: less than $1 billion, $1 billion to $5 billion, and more than $5 billion. Of the mission concepts evaluated, only one was considered to be marginally in the less than $1 billion category, and seven were considered to be in the greater than $5 billion category, which would make them larger than any other space science mission developed by NASA to date (with the possible exception of the James Webb Space Telescope, currently estimated to cost $4.5 billion).

The committee notes that adding a heavy-lift launch vehicle option could lead to larger science missions and even higher costs. *There is a direct relationship between the size of a spacecraft and its cost. Expensive space science programs will place a great strain on the space science budget, which has been essentially flat for several years and is already under strain from an ambitious slate of 85 flight missions.*[3] To estimate the costs of potential large space science missions, the committee used NASA's Advanced Missions Cost Model and estimated the costs of three new-design planetary science missions

---

[1] Marc Timm, "Constellation Overview" presented to National Research Council Committee on Science Opportunities Enabled by NASA's Constellation System, February 2008. The NASA presentation on the Constellation System is available at http://www7.nationalacademies.org/ssb/constellation2008.html.

[2] The decadal survey process is the method by which NASA and its relevant scientific communities establish the scientific goals in each particular discipline at roughly one-decade intervals. The astronomy and astrophysics decadal survey has been conducted since the 1960s; decadal surveys in solar system exploration, heliophysics, and Earth science have been started only within the last decade.

[3] NASA's current science program consists of 94 flight missions—53 in operation plus 41 in development (Alan Stern, NASA Science Mission Directorate, presentation to Space Studies Board, March 10, 2008).

representing the three levels of technological difficulty used in NASA's model.[4] Illustrated in Figure 1.1, the results indicate the correlation between rising payload mass and higher cost.

The committee was not provided cost estimates of either the Ares I or the Ares V launch vehicles, but the latter can be expected to be substantially more expensive than even the largest Evolved Expendable Launch Vehicle (EELV) currently in the U.S. inventory—the Delta IV heavy with a launch cost of about $250 million.[5] The combined effect of expensive payloads and expensive launchers would distort the balance of the space science program.

Finding 1. The greatly increased payload capability promised by Ares V could lead to much more costly science payloads.

Finding 2. The committee determined that the Ares I capabilities are not sufficiently distinct from those of Atlas V and Delta IV to enable different types or a higher quality of space science missions.

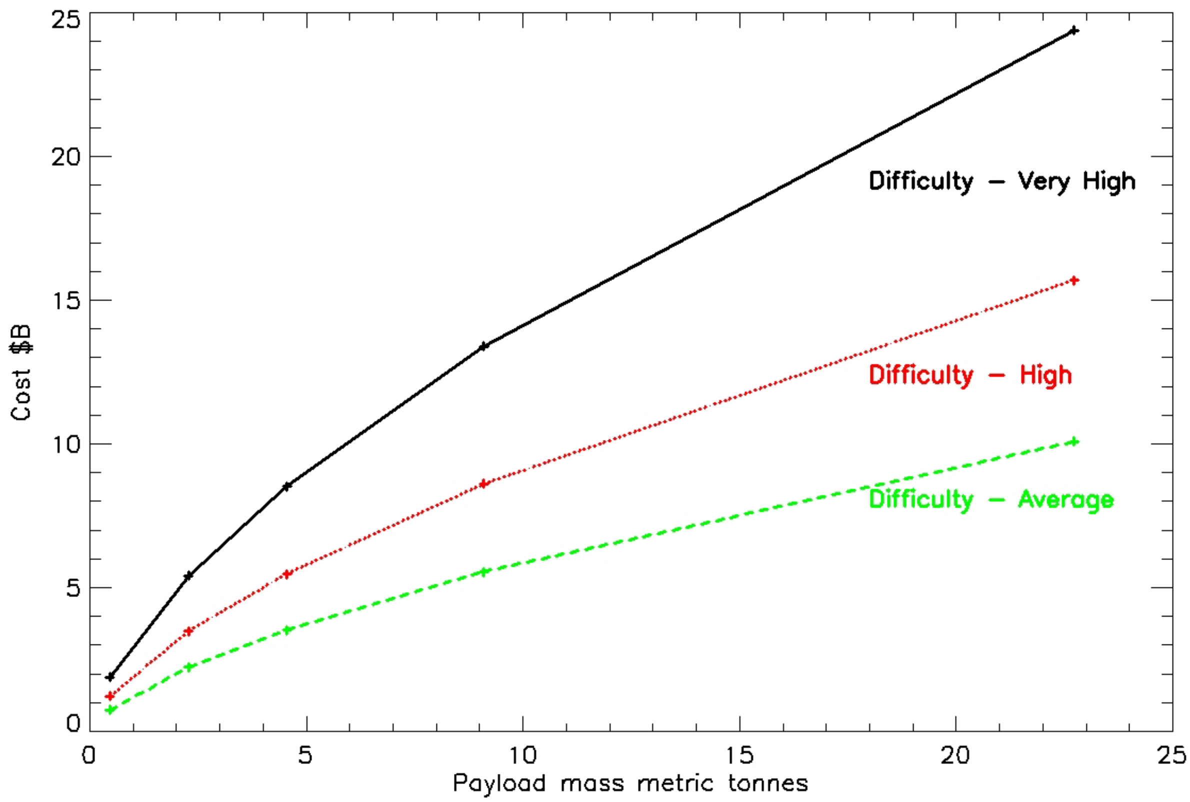

FIGURE 1.1 Estimated costs as a function of payload mass for three classes of difficulty for solar system exploration missions utilizing NASA's Advanced Mission Cost Model. NOTE: For comparison, the Cassini-Huygens payload mass was approximately 4.6 metric tons. Note that the Ares V capabilities would actually span a number of payload masses in this figure. For example, Ares V could launch 10 metric tons to Uranus or Neptune, and significantly more to Jupiter and Mars.

---

[4] The Advanced Mission Cost Model is available at http://cost.jsc.nasa.gov/AMCM.html.

[5] The committee bases the conclusion that the Ares V will be significantly more expensive than the Delta IV heavy on several factors. But one of the most straightforward is counting the number of engines: the Delta IV heavy has three RS-68 main engines, whereas the Ares V is expected to have five to six RS-68s, plus two five-segment solid rocket boosters.

BACKGROUND

In 2004, to extend analyses of potential future space science missions and to identify precursor technology requirements, NASA funded studies for a variety of advanced missions referred to as the space science "Vision Missions." These missions were inspired by a series of NASA roadmap activities conducted in 2003 and were not connected to the Vision for Space Exploration announced by President George W. Bush in January 2004. The Vision Mission concepts were predicated on the launch vehicles that were available at the time: the EELVs such as the Atlas V and the Delta IV in their various configurations. Studies of 14 potential missions were funded, and final reports on 11 of these studies were prepared and delivered to NASA in 2006-2007. These studies fell into three broad categories: astronomy and astrophysics, heliophysics, and solar system exploration (i.e., planetary exploration), with some overlap between them. The mission concepts studied were:

- Advanced Compton Telescope (ACT),
- Generation-X (Gen-X),
- Single Aperture Far Infrared Telescope (SAFIR),
- Kilometer-Baseline Far-Infrared/Submillimeter Interferometer,
- Modern Universe Space Telescope (MUST),
- The Big Bang Observer,
- Stellar Imager (SI),
- Interstellar Probe,
- Innovative Interstellar Explorer,
- Solar Polar Imager,
- Neptune Orbiter with Probes (2 studies),
- Titan Explorer,
- Titan Organics Exploration Study, and
- Palmer Quest.

Of the studies listed above, three did not result in final reports: the Big Bang Observer, the Innovative Interstellar Explorer, and the Titan Organics Exploration Study. However, for the uncompleted Innovative Interstellar Explorer mission study, the committee received a briefing that focused primarily on the changes to the mission profile that could result from use of the Constellation System. The committee addressed the two Neptune mission studies in a single assessment.

Starting in 2005 NASA began development of the Constellation System to enable the implementation of the United States Space Exploration Policy. Constellation currently consists of three main elements: an Ares I launch vehicle capable of launching 25.6 metric tons into low Earth orbit; a heavy-lift launch vehicle called the Ares V, reminiscent of the Saturn V, with a 10-meter-diameter payload fairing and the capacity to launch about 143.4 metric tons into low Earth orbit (64 metric tons to translunar injection); and the Orion human-carrying spacecraft, capable of carrying up to six crew members to low Earth orbit or four to lunar orbit.[6]

The committee was briefed on the Ares I capabilities, which are generally similar to those of the EELV family of launch vehicles. Although the committee was informed of the possible availability of a "science shroud" for the Ares I (i.e., a launch vehicle shroud adapted for the Ares I to enable it to carry payloads other than the Orion spacecraft), the committee determined that the Ares I capabilities are not sufficiently distinct from EELV capabilities that they will enable different types or quality of space science missions. The lift capability and payload volume of the Ares I, with a science shroud, are roughly equivalent to the Delta IV heavy launch vehicle configuration for missions to a low Earth orbit (LEO) at

[6] For comparison, the space shuttle is capable of launching 25 metric tons to low Earth orbit, and the Saturn V was capable of launching 119 metric tons into low Earth orbit and 47 metric tons into translunar injection.

200 km. Compared to the performance of the Delta IV, the performance of the Ares I drops dramatically at higher altitudes because the Ares I design is optimized to deliver its payload, the Orion spacecraft, to LEO, whereas the Delta IV has been designed for a broad range of mission profiles.[7]

Orion was not a significant factor in the committee's deliberations because of the types of science missions the committee was asked to evaluate; therefore the committee did not receive much information on the Orion capabilities for this first phase of its study. The committee does note, however, that Orion or its future derivative might be needed as an option for assembly, servicing, repair, and modernization for some of the mission concepts the committee analyzed. The committee plans to discuss Orion in greater detail in its final report.

The committee received a substantial briefing on the Ares V launch vehicle and believes that Ares V offers the greatest potential for an impact on science. The availability of a launcher capable of placing large-diameter, large-volume, heavy spacecraft into orbit seemingly removes the physical, although not the financial, constraints on missions that would benefit from being able to fly large, heavy payloads to their destinations. The committee was informed that the current baseline Ares V shroud has a usable volume of ~860 cubic meters, with an 8.8-meter diameter and 17.2-meter length (the maximum external diameter is 10 meters). The committee was also informed of a notional Ares V shroud with a volume of ~1,410 cubic meters, an 8.8-meter diameter, and a 26.2-meter length. During its deliberations, the committee heard statements from several presenters that for their missions, volume is more important than mass to orbit. They stated that Ares V already has the capability to lift significantly more mass than they could realistically require, but that added volume might dramatically simplify some design requirements. (For example, see the discussion of Generation-X in Chapter 2.) The committee concluded that a larger shroud for the Ares V may have important potential science utility. However, shortly after the committee's meeting, NASA publicly acknowledged that the 143.4-metric ton payload capability of the Ares V is insufficient for the planned lunar mission and that the baseline vehicle will have to be improved to launch approximately 10 metric tons more mass to low Earth orbit. These enhancements may increase the length of the vehicle and therefore make it impossible to increase the length of the Ares V payload shroud due to height limitations in the Vehicle Assembly Building.[8]

According to NASA, the first flight of Ares V is not expected until 2018 at the earliest. Lunar missions would begin in 2019 or 2020, and for at least the first several years of operations the Ares V would be firmly committed to supporting a buildup of the human lunar outpost. Ares V could therefore not be available to support science missions until the early or mid-2020s at the earliest.

APPROACH AND EVALUATION CRITERIA

The committee chose a two-phase approach to assessing potential space and Earth science mission concepts in view of the new Constellation architecture. First, the committee analyzed the 11 Vision Mission concepts already in hand from NASA's 2004 solicitation; this analysis is the subject of this interim report. Second, the committee will review additional concepts received from the scientific and technical community in response to its request for information (see Appendix A).

---

[7] The Constellation System has identified a notional Ares I configuration that includes an additional upper stage, the dual engine Centaur, for interplanetary missions. The preliminary performance estimates indicate that it provides slightly less payload capability at escape velocity (C3 = 0 km^2/s^2) than does the Delta IV heavy-lift vehicle, but slightly greater capability for missions with high C3 requirements. Since this is only a notional configuration, there is significant uncertainty about the performance estimates driven by the uncertainty in the design of the interface between the Ares I and the Centaur. Although the performance benefit of the Ares I is not significant, there may be value in utilizing the Ares I if the launch costs can be shown to be lower than those for the existing fleet of EELV vehicles.

[8] F. Morring, "Heavier Still: NASA Needs Bigger Ares V to Meet Lunar Requirements," *Aviation Week & Space Technology*, March 3, 2008, pp. 34-35.

The committee was impressed by the quality of the studies of the 11 Vision Mission concepts that it reviewed. Even a cursory reading of the studies leaves the reader with an overwhelming impression of great scientific challenges and opportunities in astronomy, astrophysics, heliophysics, solar system exploration, and astrobiology. The Vision Mission studies, some of them more than 250 pages in length and replete with appendixes, contained penetrating and extensive scientific and technical analyses and represent an enormous investment of intellectual energy by large teams of highly experienced scientists, engineers, and program managers. These missions were developed using the relatively limited capabilities of EELVs, although in several instances the missions envisioned required new and innovative techniques such as solar sails, solar-electric propulsion, or aerocapture to meet their objectives.[9] In their briefings to the committee, representatives for many of these missions indicated that their studies could have benefitted from a larger vehicle with greater mass, volume, and propulsion capabilities. In the committee's opinion, the representatives had not had enough time to fully develop and document the full scope of scientific opportunities provided by the capability of the Ares V vehicle in their presentations and supplementary material. In addition, in the opinion of the committee, there are missions for which risk or possibly even spacecraft cost might be decreased by using a launch vehicle more capable than a member of the EELV family.

The committee evaluated the 11 Vision Mission concepts using two criteria:

1. Does the concept offer a significant advance in a scientific field? ("Significant" is defined as providing an order-of-magnitude or greater improvement over existing or planned missions, and enabling a qualitative new approach to the important scientific questions in the field.)
2. Does the concept have a unique requirement for Constellation System capabilities, e.g.,
   —Does use of the Constellation System's elements make a previously impossible mission technically feasible?
   —Does use of the Constellation System's elements reduce mission risk or enhance mission success for a previously complicated mission?
   —Does use of the Constellation System's capabilities offer a significant cost reduction (i.e., 50 percent or more) in the cost of accomplishing the mission?

The committee was tasked with assessing the relative technical feasibility of mission concepts compared to each other. Relative technical feasibility was judged on the basis of material submitted to the committee without any detailed independent analysis. The committee was not asked to assess the relative scientific merit of the Vision Missions. Such prioritization of missions is more appropriately the work of decadal surveys looking at proposed missions in the context of others in each scientific field. However, the committee did take note of Vision Mission proposals that had been mentioned in previous NRC reports.

The committee had only a limited set of proposals/subjects to work with, and the evaluations presented in this interim report do not imply any restriction on future areas of interest that may be addressed in the final report. In particular, it is possible that Earth science missions could take advantage of Constellation's capabilities, although none of the Vision Mission studies addressed in this interim report proposed Earth science missions. The committee also notes that whereas the Vision Mission studies did not assume the presence of astronauts, astronauts may be included in the concepts described in responses to the committee's request for information. The request for information states that broad areas of space science could potentially utilize the capabilities being developed for Constellation.

Of the 11 mission concepts reviewed for this interim report, the committee determined that all could potentially offer a significant advance in their scientific fields. However, of the 11 mission

[9] Several space missions have employed the technique of aerobraking, which involves using a planetary atmosphere to slowly lower a spacecraft's orbit. In contrast, aerocapture is the use of a planetary atmosphere to enable the spacecraft to be captured into an initial orbit.

concepts, the committee determined that four (Advanced Compton Telescope, SAFIR, Kilometer-Baseline Far-Infrared/Submillimeter Interferometer, and Palmer Quest) did not directly benefit from Constellation. Nevertheless, the committee was impressed by the scientific objectives and ambition of all of these missions, including the ones that would not benefit from Constellation. The missions will undoubtedly be evaluated in the decadal surveys to which they are relevant and may perform well in those. That the committee did not identify several proposed missions for further study as Constellation-enabled missions should not imply anything about their future viability.

The reasons for the committee's assessment that some of the Vision Missions would or would not benefit from the Constellation System generally fall into two categories. The first concerns the mass, volume, and complexity of the proposed missions and whether they fit into the capabilities of the existing family of EELV rockets. The second category concerns the propulsion requirements of the missions and whether they could benefit from a powerful launch vehicle that could place them in their required mission orbit (propulsion required to either accelerate them or decelerate them into their proper orbits).

The committee did not conduct detailed cost assessments of any of the Vision Missions. Instead, it used the cost estimates provided by their proposers (not adjusted for inflation) and relied on the experience of the committee members and comparisons with similar missions. In most cases, the proposers were basing their cost estimates on an EELV launch vehicle and not on the Constellation launch vehicles or architecture, particularly Ares V.

Transitioning of payloads to a new, more capable vehicle would require reconsideration of several items: complexity of instrument packaging and deployment, need for auxiliary propulsion, and so on, all of which would affect mission costs. The committee was informed by NASA that no reliable cost estimates of the Ares V are available.

2
Analysis of Vision Mission Studies

In 2004 NASA commissioned a series of 14 studies of possible robotic missions in the time period 2020-2035. Based on a series of roadmaps conducted by NASA in 2003, the studies were told to assume using the Atlas V or Delta IV families of launch vehicles (collectively known as the Evolved Expendable Launch Vehicle, or EELV, system). The studies were labeled the "Vision Mission" studies. Of the 14 studies, 12 resulted in final reports covering 11 mission concepts (2 concerning Neptune missions), most produced by 2006.

The National Research Council's Committee on Science Opportunities Enabled by NASA's Constellation System asked the studies' principal investigators, or other representatives, to present them to the committee at its first meeting. The principal investigators were asked to speculate about how their existing studies might benefit from the Constellation capabilities—primarily the larger payload and shroud dimensions of the Ares V rocket, and the possibility of human servicing of spacecraft. The presenters had only limited time to assess how the Constellation capabilities might affect their proposals. Some indicated that the Constellation System would have no appreciable effect on their concepts, and others indicated that it would serve as a substitute for required technology development. Some of the proposers also indicated that their science objectives might be enlarged and expanded if they had additional mass and volume available.

The committee analyzed each of the missions according to a standard format based on the committee's statement of task, which specified analysis of the following information for each mission concept considered:

1. Scientific objectives of the mission concept;
2. A characterization of the mission concept insofar as the maturity of studies to date have developed it;
3. The relative technical feasibility of the mission concepts compared to each other;
4. The general cost category into which each mission concept is likely to fall;
5. Benefits of using the Constellation System's unique capabilities relative to alternative implementation approaches; and
6. Identification of the mission concept(s) most deserving of future study.

For this interim report, the committee chose to consider both the Vision Mission final reports and the presentations to the committee during its February 2008 meeting. In some cases there were no or only minor differences between the reports and the presentations. In others the differences primarily concerned speculation about how the mission would benefit from the Constellation capabilities. For the Interstellar Probe concept, the presentation was made by the team that did not produce a final report. However, the science objectives of both Interstellar Probe studies were identical, and the presenter focused primarily on how the mission concept would benefit from the Constellation System.

ASTRONOMY AND ASTROPHYSICS VISION MISSIONS

Advanced Compton Telescope[1]

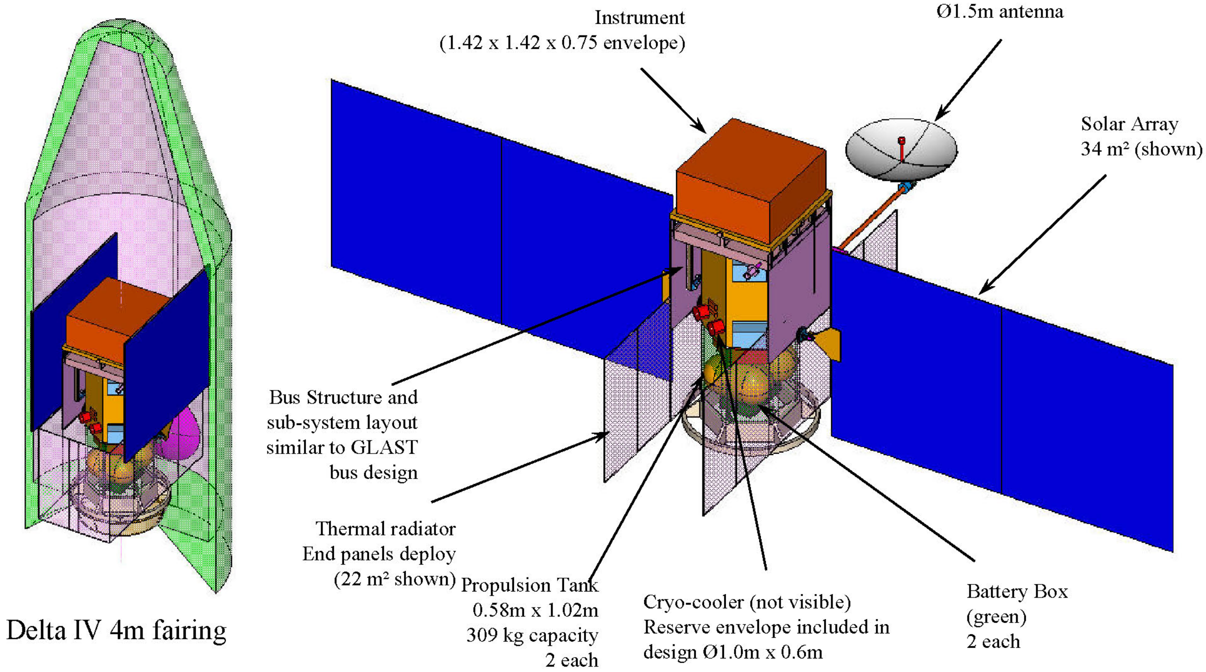

FIGURE 2.1 Artist's illustration of the Advanced Compton Telescope in both mission configuration and stowed within a Delta IV payload shroud. SOURCE: Courtesy of S. Boggs, NASA.

Scientific Objectives of the Mission Concept

The Advanced Compton Telescope (ACT) is a concept for a powerful new survey instrument for studying supernovae, galactic nucleosynthesis, gamma-ray bursts (GRBs), compact objects, and the laws of physics (Figure 2.1).[2] Since the gravitational collapse of matter into stars and galaxies a few hundred thousand years after the Big Bang, much of the visible matter in the universe has been processed through the slow but spectacular life cycle of matter: stellar formation and evolution ending in novae or supernovae, with the ejection of heavy nuclei back into the galaxy to seed a new generation of stars. Nuclear gamma-ray astrophysics is the study of emission from radioactive nuclei as tracers of this cycle of creation. In particular, the ACT maps our galaxy in a broad range of nuclear line emission from radioactive decays, nuclear de-excitations and matter-antimatter annihilations. It measures the radioactive gamma-ray and positron emitters among the particles propagating from supernovae, novae, and stellar winds populating our galaxy. Additionally, gamma rays from accretion of matter onto galactic compact

---

[1] *ACT, Advanced Compton Telescope: Witness to the Fires of Creation*, NASA Vision Mission Concept Study Report, December 2005, available at ; Jim Kurfess, "The Advanced Compton Telescope Mission," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 2008.

[2] The original Compton Gamma Ray Observatory (CGRO) was the second of NASA's "great observatory" telescopes (after the Hubble Space Telescope) and operated in low Earth orbit from 1991 to 2000. At the time of its launch, it was the heaviest scientific instrument placed in orbit.

objects and massive black holes in active galactic nuclei (AGN) are used to test accretion disk and jet models and to probe relativistic plasmas. Gamma-ray polarization can be used to study the emission processes in GRBs, pulsars, AGN, and solar flares. The origins of the diffuse cosmic MeV background can also be identified.

The ACT telescope increases detection efficiency by up to two orders of magnitude over the COMPTEL instrument (the Compton Telescope on the Compton Gamma Ray Observatory [CGRO]). The ACT instrument design is driven by its primary science goal: spectroscopy of the ^{56}Co (0.847 MeV) line from type Ia supernovae (SNe Ia), which is expected to be Doppler-broadened to ~3 percent. ACT allows hundreds of SNe Ia detections over its primary 5-year survey lifetime. In the process, ACT becomes an all-sky observatory for all classes of gamma-ray observations, Table 2.1 provides estimates of potential ACT observations compared to those of the COMPTEL.

The ACT science is complementary to the Gamma-ray Large Area Space Telescope (GLAST) science.[3] GLAST addresses the high-energy gamma rays from ~20 MeV to 300 GeV, and ACT covers the region from 0.2 MeV to 10 MeV. GLAST also includes a 10-keV to 30-MeV gamma-ray burst detector.

TABLE 2.1 Estimates of Potential ACT Observations/Detections of Various High-Energy Events Compared to Those with the COMPTEL

Sources (5 year)	COMPTEL	ACT
Supernovae	1	100-200
Active galactic nuclei and blazars	15	200-500
Galactic	23	300-500
Gamma-ray bursts	31	1000-1500
Novae	0	25-50

NOTE: ACT, Advanced Compton Telescope; COMPTEL, Compton Telescope on CGRO. SOURCE: *ACT, Advanced Compton Telescope: Witness to the Fires of Creation,* NASA Vision Mission Concept Study Report, December 2005, available at http://arxiv.org/ftp/astro-ph/papers/0608/0608532.pdf , p. 3.

The ACT mission involves a single instrument composed of a large array of multi-channel gamma-ray detectors, surrounded by anti-coincidence (ACD) shields on all sides, mounted on a zenith-pointing spacecraft. The ACT can be launched from the Kennedy Space Center on a Delta IV vehicle into a 550-km circular orbit with 8 degrees inclination for a 5-year minimum (10-year desired) lifetime. The ACT total mass is ~4,000 kg, power is 3,340 W, and the data rate is 69 Mbps. The ACT concept fits within the Delta IVs 4-meter faring.

The ACT mission is not specifically identified in any decadal survey. However, advanced gamma-ray mission concepts have been under consideration by NASA over the past decade, and such missions were considered as part of NASA's roadmap activities, including the *Universe Strategic Roadmap.* NASA's Gamma Ray Astrophysics Program Working Group named ACT as its highest-priority major mission in reports published in 1997 and 1999. The committee believes that this mission offers a significant advance in its scientific field. However, this mission would require priority endorsement in the next NRC astronomy and astrophysics decadal survey before initiation of development.

[3] GLAST is scheduled for launch aboard a Delta II rocket from Cape Canaveral in May 2008.

Characteristics of the Mission Concept as Developed to Date

The ACT concept has matured to the point that flight detector technology development and selection appears to be the major open item. Several detector alternatives are available, all entailing considerable and challenging development efforts: (1) Si-Ge baseline D2 germanium detectors, (2) thick Si detectors, (3) liquid Xe detectors, or (4) silicon controlled-drift detectors. These detector alternatives, combined with the significant improvement in collecting area over prior instruments, appear to provide reasonable risk mitigation paths and alternatives to satisfy the ACT requirements. There are also major challenges to be met for these technologies related to power and cryogenics system needs. Consequently, the highest priority is to focus on detector development and final selection.

Relative Technical Feasibility of the Mission Concept

New technology development required for ACT is relatively modest, with the most important technology development needed for the detectors. There are no significant mission design issues. The ACT mission is comparable in size, mass, and complexity to GLAST and has similar spacecraft and mission operations requirements (GLAST's mass is 4,627 kg).

The mission mass (~4,000 kg) and orbit (550 km LEO, <10 degrees) are within the capabilities of the Delta IV and Ares I. The European Space Agency's Ariane V could provide a low-altitude, near-equatorial-orbit minimizing background, therefore making this a potentially attractive mission for international cooperation.

General Cost Category in Which This Mission Concept Is Likely to Fall

The ACT study team estimates an ACT mission cost of ~$760 million in FY 2004 dollars. Comparing ACT with the comparable mission, GLAST, and considering the later time frame for development, ACT is likely a ~$1 billion class mission.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The Ares V capabilities are not required for this mission concept, since ACT can easily be packaged to fit on a Delta IV. Although the Ares V would allow for significantly greater collecting area of detectors, the science provided by the Delta IV ACT concept is a significant order-of-magnitude improvement in gamma-ray astrophysics. A gamma-ray mission of the Ares V class would logically follow only after an ACT Delta-IV-class mission has been implemented.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The ACT mission concept does not deserve further study as a Constellation-enabled science mission. The primary reason is that the spacecraft size does not exceed current EELV capabilities. The committee determined that it is worthy of further study as a Delta-IV-class mission. ACT provides significant science that deserves to be addressed by the next astronomy and astrophysics decadal survey.

Generation-X[4]

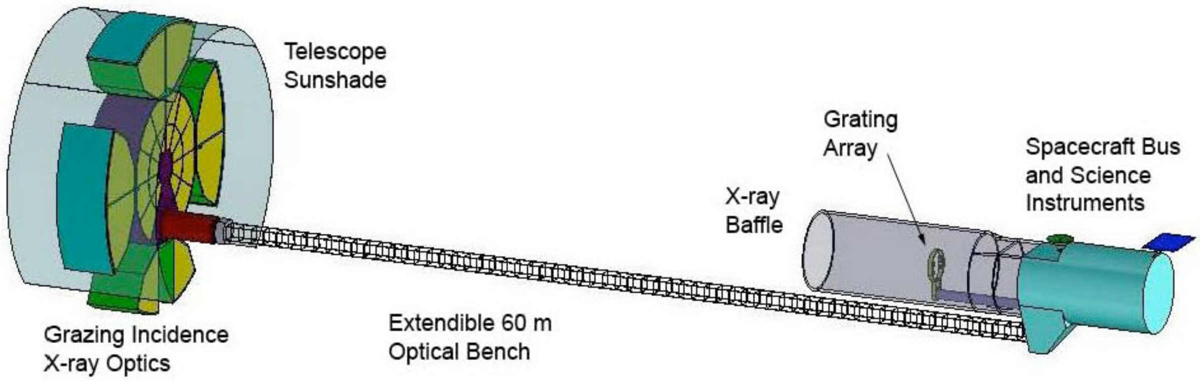

FIGURE 2.2 Artist's conception of one possible version of the Generation-X x-ray telescope. SOURCE: Courtesy of Roger Brissenden, Smithsonian Astrophysical Observatory.

Scientific Objectives of the Mission Concept

The proposed Generation-X (Gen-X) space-based x-ray telescope (Figure 2.2) complements all the other wavelength future-generation telescopes, such as the Atacama Large Millimeter Array (ALMA), James Webb Space Telescope (JWST), Square Kilometre Array (SKA), and TMT (Thirty Meter Telescope). This telescope is designed to study the new frontier of astrophysics: the birth and evolution of the first stars, galaxies, and black holes in the early universe. X-ray astronomy offers an opportunity to detect these via the activity of the black holes, and the supernova explosions and gamma-ray burst afterglows of massive stars.

The Generation-X Vision Mission is based on an x-ray observatory with 50 m^2 collecting area at 1 keV (1,000 times larger than the Chandra x-ray telescope) and 0.1-arc-second angular resolution (several times better than Chandra and 50 times better than the Constellation-X resolution goal). Such a high-energy observatory will be capable of detecting the earliest black holes and galaxies in the universe and will study the chemical evolution of the universe and extremes of density, gravity, magnetic fields, and kinetic energy which cannot be created in laboratories. The goal of Gen-X is to observe the first black holes and stars at redshift $z \sim 10$-20. The idea is to search for the effects of the early black holes on the formation of galaxies and to trace through time the evolution of galaxies, black holes, and the chemical elements.

This mission is intended to address the science goals outlined in the 2001 NRC astronomy and astrophysics decadal survey[5] and is the logical follow-on to the Constellation-X mission, an x-ray mission that is proposed for the next decade. The committee believes that this mission does offer a significant advance in its scientific field.

[4] R. Brissenden et al., *Generation-X Vision Mission Study Report*, March 2006; R. Brissenden, "The Generation-X Vision Mission," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 21, 2008.

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

Characteristics of Mission Concept as Developed to Date

Various configurations have been studied for Generation-X: a single 20-meter mirror, six 8-meter mirrors, and a single 16-meter mirror. The committee considers the 20-meter concept and the six 8-meter mirror concepts to be exceedingly complex and decided to evaluate only the 16-meter mirror configuration utilizing the Ares V launch vehicle.[6]

The Generation-X mission operates at Earth-Sun L2. A 16-meter telescope will require either robotic or human-assisted in-flight assembly. The required effective area, 50 square meters, implies that extremely lightweight grazing incidence x-ray optics must be developed. To achieve the required areal density, at least 100 times lower than in Chandra, technology development is needed to produce 0.2-mm-thick mirrors (approximately the width of two human hairs) that have active on-orbit figure control. The suite of detectors includes a large-field-of-view high-angular-resolution imager (wide field imager), a cryogenic imaging spectrometer (micro-calorimeter), and a reflection grating spectrometer. The technology relating to the mirrors would clearly require early development and investment.

The study report proposes the delivery of a detailed technology development plan for the next decade that will get key optics and detector technologies evolved to technology readiness level (TRL) 6. The report suggests the need for a decade of development effort in order that the mission could be realized in the following decade.

The use of "two-hair-thick" mirrors with active control is a technological challenge. The success of the mission depends on this development effort. Initial adjustment and alignment, and then the maintaining of the alignment, will be challenging.

The space assembly of the 16-meter mirror has not been defined in detail. The proposers have suggested using robotic assembly, but human assembly remains another possibility.

Relative Technical Feasibility of Mission Concept

The Chandra X-ray Observatory has a collecting area of 0.04 m^2 and uses glass shell technology. Gen-X has a 50 m^2 collecting area and uses thin foils with active control of shape to reduce the mass.

The Ares V launch vehicle would simplify the mission concept because of the large payload fairing and mass to L2 capability. It has the potential of further simplifying the mirror development by allowing thicker mirror shells than were studied for the earlier configurations. The technical challenges are the mirror development and the in-space assembly.

General Cost Category in Which This Mission Concept Is Likely to Fall

A November 2007 Gen-X proposal that would utilize the Ares V estimated a $4 billion cost (excluding launch costs).[7] The committee believes that this is a conservative estimate. The technology development of the mirrors is the large unknown in the cost estimates.

---

[6] There are numerous problems for the other configurations. For instance, the six 8-meter mirror concept would require six launches of Delta IV heavy rockets. The Delta IV heavy requires 3 months of ground preparation prior to launch, and therefore merely the launches alone would require 18 months of preparation during which no other Delta IV rockets—necessary for national security payloads—could be launched.

[7] "A Concept Study of the Technology Required for Generation-X: A Large Area and High Angular Resolution X-ray Observatory to Study the Early Universe," submitted in response to NNH07ZDA001N-ASMCS, November 20, 2007.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The Ares V capability results in a simplified and more cost-effective baseline mission concept. It would be able to deliver Gen-X directly to Earth-Sun L2 point. A single launch would allow a 16-meter-diameter mirror to be placed into position, would permit use of piezo-electric control of the mirror's optical surface for final figure on-orbit to achieve ~0.1-arc-second angular resolution, and would enable placing into position a 60-meter extendable optical bench between the optics and the science instrumentation package.

The estimated mass for the Gen-X Ares V configuration is 22,000 kg. The Ares V is capable of delivering 55,900 kg to translunar injection trajectory (essentially that required for Gen-X). This mass margin possibly could be used to reduce the risk in the mirror development by enabling a thicker mirror.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The Gen-X mission concept is worthy of further study as a Constellation-enabled science mission. The major issue is the mirror development. The extra mass capability of the Ares V may enable the use of thicker mirrors and therefore significantly reduce risk.

Single Aperture Far Infrared Telescope [8]

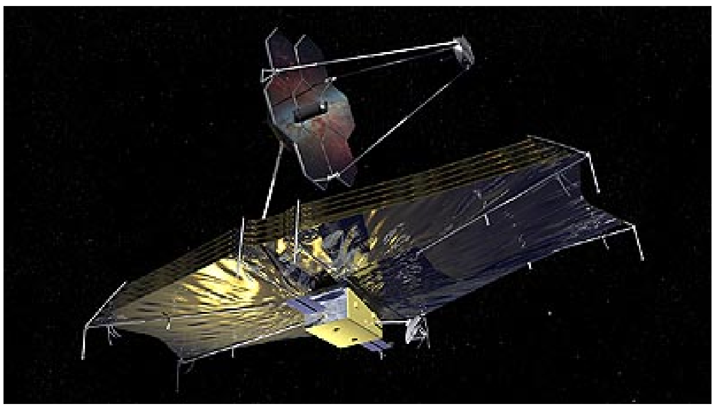

FIGURE 2.3 Artist's illustration of the SAFIR telescope.
SOURCE: Image courtesy of John Frassanito and Associates and Northrop Grumman Space Technology.

Scientific Objectives of the Mission Concept

The Single Aperture Far Infrared (SAFIR) mission is a 10-meter telescope cooled to a few degrees above absolute zero (goal of 4 K) for observations between the thermal infrared (~20 μm) and the submillimeter (Figure 2.3). It would operate at the Earth-Sun L2 point. As designed, it will improve the sensitivity of far infrared (FIR) observations by two to four orders of magnitude and the angular

[8] D. Lester et al., *Science Promise and Conceptual Mission Design Study for SAFIR—The Single Aperture Far Infrared Observatory*, June 2005; D. Lester, "Constellation Architecture and the Single Aperture Far Infrared Telescope (SAFIR)," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 20, 2008.

resolution by more than a factor of 10 compared to previous missions. More importantly, the combination of sensitivity and resolution will allow SAFIR to have a major impact on almost every major subfield of astronomy at distances stretching from the closest stars to the most distant observable galaxies. It achieves the threshold angular resolution of 0.5 arcsec in the thermal infrared needed to isolate galaxies at very high redshifts ($z > 2$), making it uniquely capable of addressing problems involving observations of the early universe at wavelengths longer than 20 μm. SAFIR is also a steppingstone to the long-range goal of very high angular resolution for far infrared astronomy. The instrument suite will also include low-resolution spectrographs.

The thermal and far infrared wavelength range is most sensitive to cool regions in the interstellar and intergalactic medium at temperatures from a few to a few hundred kelvin. In galaxies, interstellar dust and gas typically absorb and reradiate much of the light from stars and active galactic nuclei, assumed to be black holes, meaning that the far infrared radiation often contains the majority of the energy budget. Far infrared lines provide the best way to study the gas free from the obscuring effects of dust at ultraviolet and optical wavelengths. These observations provide a unique complement to optical, x-ray, and radio observations, and it is safe to say that researchers cannot understand the early evolution of stars, the creation of metals, and the creation of dust without far infrared observations at angular resolutions at least as high as those proposed for SAFIR. The extragalactic case for a facility like SAFIR is unique and is unlikely to be eroded by other observatories in the next two decades.

Far infrared observations of galactic clouds of gas and dust are also essential to understand the physics of star formation. Circumstellar disks around young and old stars alike, indicative of planet formation in the former case, emit the bulk of their luminosity at thermal and far infrared wavelengths, making SAFIR an important facility to study planet formation and making it an essential complement to the Atacama Large Millimeter Array (ALMA), as well as JWST and SOFIA. There are a variety of novel uses of far infrared observations to study the interstellar medium and other circumstellar regions that highlight the importance of this wavelength region to understand how the interstellar medium behaves and takes part in the life cycle of new stars and planets.

The key science objectives as outlined in the proposal are:

- Probe the earliest epochs of metal enrichment and see the galaxy-forming universe before metals are created. Understand the origin of dust grains in the universe.
- Resolve the far infrared cosmic background—trace the formation and evolution of star-forming and active galaxies since the dawn of the universe, and measure the history of star formation.
- Explore the connection between embedded nuclear black holes and their host galaxies. Understand the relationship of active nuclei to galaxy formation.
- Track the chemistry of life. Follow prebiotic molecules, ices, and minerals from clouds to nascent solar systems.
- Identify young solar systems from debris disk structure and map the birth of planetary systems from deep within obscuring envelopes. Assess the degree of bombardment they face, and the degree of habitability.

A fundamental difficulty with observations at wavelengths longer than about 50 μm is that the relatively low angular resolution combined with zodiacal light and the light from many distant galaxies creates the primary noise source; that is, the sensitivity is limited by foreground emission and confusion. However, because SAFIR will still gain orders of magnitude in observing capability over all existing and planned facilities in this important wavelength range, its science case is very strong. The committee believes that this mission does offer a significant advance in its scientific field.

Characteristics of the Mission Concept as Developed to Date

SAFIR would build on the technological improvements developed for the James Webb Space Telescope and would appear to be a logical follow-on to that observatory (subject to evaluation by the decadal survey). It is 1.5 times larger than JWST but with far less stringent requirements on surface accuracy. It has much more demanding requirements for instrument cooling (4 K for SAFIR against 40 K for JWST). Because the longest wavelengths are confusion-limited by distant galaxies, even modest gains in cooling over JWST—to 20 K, for example—will satisfy many of the science goals, allowing some margin in this challenging technological development. SAFIR would need improved far infrared detectors (by about an order of magnitude) requiring unknown cost, but it seems likely that great improvements could be made with a modest investment. In all other respects, the concept is relatively mature.

Relative Technical Feasibility of the Mission Concept

The current concept arises from the heritage of JWST, and therefore appears technically feasible with the proviso that SAFIR would have to achieve lower temperatures under passive cooling that any telescope to date. Of the different design concepts considered by the team, the current one most closely derives from already-developed technology. SAFIR does not propose enormous technological advances over JWST, although the cooling to 4 K is likely to be a major challenge.

General Cost Category in Which This Mission Concept Is Likely to Fall

The mission is likely to be similar in cost to JWST ($4.5 billion). It uses similar technology, but it will have a 50 percent larger mirror with more demanding thermal requirements but less demanding surface accuracy, wavefront control, and pointing. It is not known how much investment is needed to improve far infrared detectors to take full advantage of the SAFIR telescope.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The Ares V capabilities are not required for this mission concept as proposed. The size of the telescope in the Vision Mission study was chosen as one that can fit in an EELV using at least some JWST heritage, and at this size it would enable significant scientific advances to be achieved. Assuming that the JWST is successfully developed, SAFIR should not be inherently more difficult to design and build. However, if JWST proves to pose major design challenges, then large telescopes such as SAFIR could potentially benefit from having a larger shroud on a more capable launch vehicle, easing packaging and integration of the telescope.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The SAFIR mission concept does not deserve further study as a Constellation-enabled science mission. SAFIR is likely to be very attractive as a Delta-IV-class mission and does not require the increased capabilities of the Ares V launch vehicle. SAFIR could be considered as a candidate stand-alone Ares V mission, but the heavy launch vehicle is not required. SAFIR received a high rank in the

2001 astronomy and astrophysics decadal survey,[9] and the more developed concept presented in the Vision Mission study should make it equally attractive in the next decadal survey.

Kilometer-Baseline Far-Infrared/Submillimeter Interferometer[10]

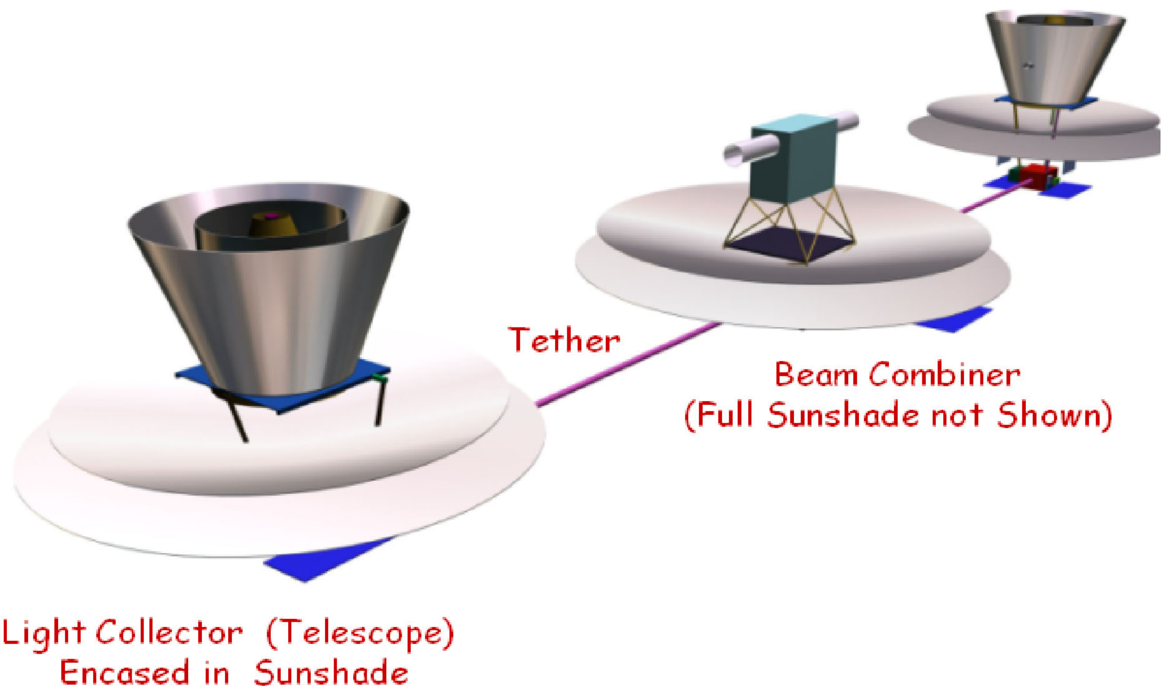

FIGURE 2.4 Artist's illustration of the Kilometer-Baseline Far-Infrared/Submillimeter Interferometer. SOURCE: Courtesy of the SPECS Consortium, prepared as part of a NASA Vision Missions study.

Scientific Objectives of the Mission Concept

The long-term goal of far infrared astronomy has been a combination of large collecting area with high angular resolution. This combination is provided by the Submillimeter Probe of the Evolution of Cosmic Structure (SPECS), a Michelson interferometer consisting of two 4-meter telescopes separated by up to 1 km (Figure 2.4) to provide angular resolutions of a few tens of milliseconds of arc, similar to Hubble, JWST, and ALMA at far infrared and submillimeter wavelengths. The spacecraft would be located at the Earth-Sun L2 point.

---

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

[10] M. Harwit et al., *Kilometer-Baseline Far-Infrared/Submillimeter Interferometer*, Vision Mission Final Report, May 2005; "A Kilometer-Baseline Far-Infrared/Submillimeter Interferometer in Space: Submillimeter Probe of the Evolution of Cosmic Structure, SPECS," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 2008.

Far infrared (>40 μm) observations of distant galaxies, the intergalactic and interstellar medium, and circumstellar winds and disks are considered necessary to understand the formation of stars and galaxies and the energy balance in the universe.

Two big problems have prevented far infrared astronomy from becoming as important as optical astronomy as an information channel for studying the universe: relatively low sensitivity and poor angular resolution owing to the small diameters of those telescopes, and the strong, variable far infrared background from the zodiacal cloud, galactic cirrus, and extragalactic light from unresolved galaxies. The poor angular resolution is especially problematic compared to the typical size of distant galaxies, ~0.1 arcsec or less at high redshifts, and it will limit the sensitivity of even large infrared telescopes, such as SAFIR.

If it works as planned, the SPECS will advance many areas of astrophysics by substantial factors. For example, typical galaxies at redshifts beyond 1 are of order 0.1 arc second or less in size. Resolving these galaxies in the far infrared will be essential to study ongoing star formation in the same manner as is done now for local galaxies by comparing optical, infrared, and radio images. Most regions of active star formation have significant obscuration from dust penetrated only in the far infrared. Very high resolution is also required to see placental stars under assembly. Circumstellar disks in the act of creating new planets will produce gaps a few tens of milliseconds of arc in size around the nearest young stars, and the temperatures of the disk material will be cool enough, ~50 K, that most of the energy will be emitted at far infrared wavelengths. Line radiation at these wavelengths will provide even more diagnostic information about galaxies, disks, and the interstellar medium than the continuum light. These are only a few of the important observations that a SPECS could do.

Sensitive far infrared observations at the resolution provided by a kilometer-baseline interferometer will be both essential and unique for solving a wide suite of astrophysical problems. It is for this reason that the 2000 astronomy and astrophysics decadal survey[11] ranked far infrared space telescopes very highly, and the scientific case for these capabilities will remain strong and uncompromised for the foreseeable future. There is little doubt that if the goals of the currently proposed SPECS can be achieved, it will represent a major advance in observational astronomy that is unlikely to be obtained any other way. The committee believes that this mission does offer a significant advance in its scientific field.

A European Space Agency study of a similar telescope is currently examining both a free-flying option and a connected spacecraft. This mission could be attractive for future international cooperation.

Characteristics of the Mission Concept as Developed to Date

This mission pushes far infrared technology to new limits. No large far infrared space telescope has been operated in space, and no interferometer of the proposed scale has ever been tested. The challenges of beam combination and aperture synthesis, particularly with the current generation of far infrared detectors, are daunting. Several levels of technology development will be needed to bring this mission to maturity, including:

1. Large, cooled telescopes operating in space (these might derive from JWST heritage);
2. Controlled tethered telescopes or formation flying to maintain baseline control;
3. Advanced far infrared array detectors and heterodyne detectors working efficiently at the quantum limit; and
4. Large, rapid, cooled delay lines operating in space.

---

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

Additionally, aperture synthesis routines for a rapidly rotating two-telescope interferometer with a constantly changing baseline will need to accommodate complex engineering data sets (metrology of the baselines) with complex data streams from the detectors to assemble useful astronomical data.

By most traditional measures, this mission is immature and will require several technical developments, probably as heritage from precursor missions, to reach flight readiness.

Relative Technical Feasibility of the Mission Concept

Every approach to far infrared space astronomy considered by the team is challenging.

The approach adopted in this Vision Mission concept is one reasonable choice among several. It appears technically feasible only after considerable investment to develop space hardware as outlined above.

General Cost Category in Which This Mission Concept Is Likely to Fall

At present, this mission appears to be considerably more expensive than SAFIR owing to the amount of technical development yet to be done, meaning more than $5 billion. Future developments in tethered flight, far infrared detectors, cooled telescopes, and fast delay lines might decrease these costs substantially, but the up-front investment at this time appears large.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The current project as proposed does not require the Ares V capabilities because it meets the mass and volume limits of the EELV. However, the technical goals are ambitious, and it appears likely that many of the packaging problems of fitting an interferometer into a small fairing and deploying it in space might be alleviated by using the Ares V. Complete analysis of the fuel budget needed for moving the array may show the advantages of using an Ares V to increase the lifetime of the mission. But the committee determined that the fundamental technological challenges of the mission are substantial and would not benefit from the capabilities of the Ares V or other aspects of the Constellation System.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The Kilometer Baseline Far-Infrared/Submillimeter Interferometer mission concept does not deserve further study as a Constellation-enabled science mission. This mission concept currently appears to be within the capabilities of the EELV family. A far infrared interferometer is a long-term goal for this important wavelength range, and that goal is unlikely to be diminished by developments in the next two decades. It is not known how difficult it will be to develop the contributing technologies necessary to make this mission viable, but each of the elements has plausible development paths with a modicum of investment over the next decade.

Modern Universe Space Telescope [12]

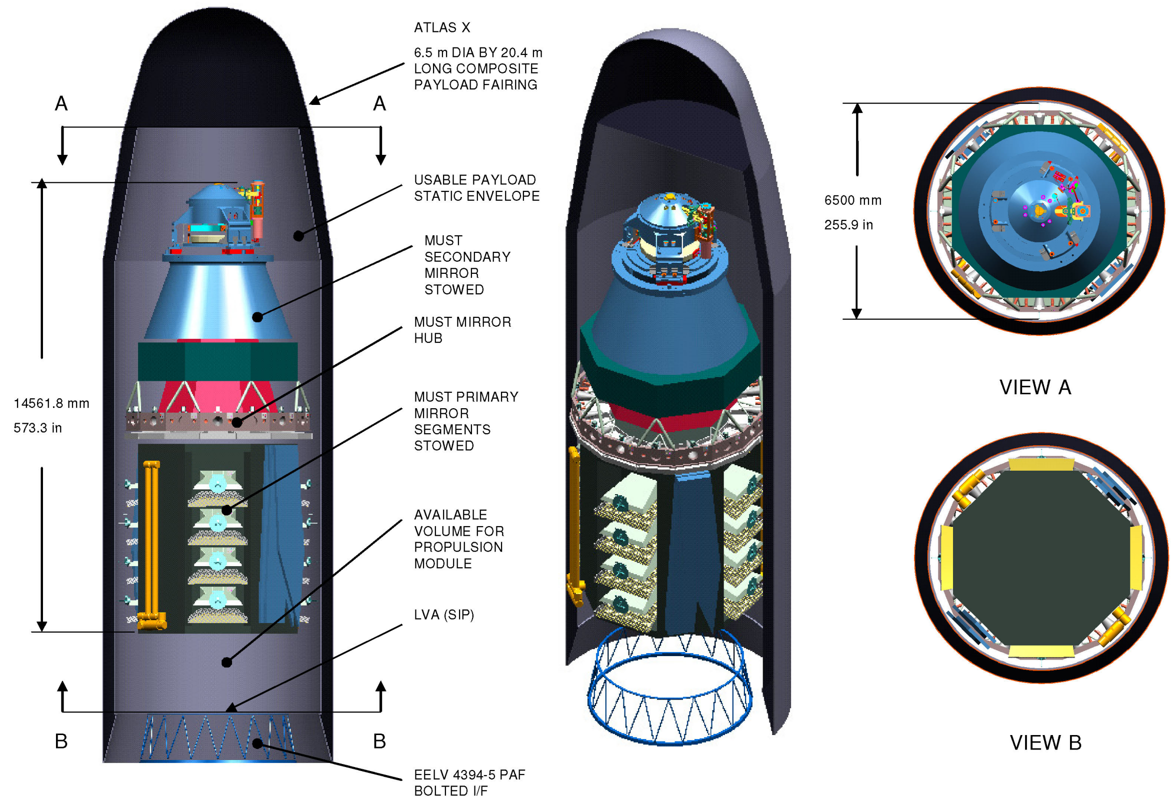

FIGURE 2.5 Artist illustration of the Modern Universe Space Telescope (MUST) inside its launch vehicle prior to assembly in space. SOURCE: Courtesy of NASA.

Scientific Objectives of the Mission Concept

The Modern Universe Space Telescope (MUST) is a 10-meter, diffraction limited optical-ultraviolet telescope (Figure 2.5) that establishes a new threshold in terms of sensitivity, imaging resolution, and scientific return.[13] MUST would consist of a single large (4- to 6-meter) central optical mirror surrounded by a number of ~2-meter petal-like segments. The segments would be robotically assembled around the primary mirror at the Earth-Sun L2, enabling a telescope architecture that will be scalable to larger mirror concepts in the future. The observatory would consist of four elements: the 10-meter telescope, four or more science instruments, a spacecraft bus, and a sunshield/baffle. The telescope will cover the wavelengths from 1150 Å in the ultraviolet to approximately 2.5 µm in the near-infrared.

[12] J. Green et al., *The Modern Universe Space Telescope: A Vision Mission Concept Study for a Large UV-Optical Space Telescope;* J. Green, "The Next Large UV-Optical Space Telescope: The Modern Universe Space Telescope," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 21, 2008.

[13] This mission concept has been referred to as the Large Ultraviolet/Optical Modern Universe Space Telescope, or LUVO-MUST. LUVO is considered to be a class of telescopes, and MUST is a specific proposal within that class.

Robotic capabilities investigated for the Hubble Space Telescope service mission and developed for the International Space Station will be used to assemble the observatory in space at Earth-Sun L2.

MUST's resolution in the optical and ultraviolet will isolate individual stars in other galaxies and extend the evaluative techniques familiar with objects in our own galaxy to distant ones. The telescope will focus on the following key scientific questions:

- How are metals created and distributed through the modern universe?
- How are galaxies assembled, and how do they evolve?
- How do stars and planetary systems form, and how does this affect their likelihood of supporting life?
- Where are the baryons in the modern universe, and how are they distributed?

The committee believes that this mission does offer a significant advance in its scientific field. However, to receive broad community support to enable such a multibillion-dollar mission, any such telescope should address one or more of the emerging new science areas that will have an impact qualitatively different from that of other contemporaneously proposed missions.

Characteristics of the Mission Concept as Developed to Date

The general science goals and observing concepts for a base mission of MUST are fairly mature at this time in most areas. This mission will be a "next-generation Hubble Space Telescope" in many ways. Some of the technologies follow on from JWST and SAFIR in terms of large mirror technology and construction and placement at L2. Coronagraphic performance at contrast levels needed for exo-planet science objectives (10^{-9} to 10^{-10}) are likely to be limited due to scattered light from the segmented mirrors. The current detectors and optics requirements require minor advances from those available today. The mission proposes to have a 10-year start-to-flight timescale.

MUST concentrates on science themes that are addressed largely by other observatories, such as JWST and ALMA, and that were the main themes of a similar mission, ST2010, proposed during the last decadal survey. The ST2010 project was not highly ranked, owing to the limited advances of ultraviolet (UV)/optical performance when compared to less mature fields like far infrared astronomy, and to the relative maturity of UV-unique observations when compared to emerging fields.

The segmented mirror design for MUST may preclude direct observation of extrasolar planets, (one of its stated goals), a scientific area that is one of the most exciting new possibilities in optical/infrared astronomy, by adopting a segmented mirror architecture. This issue may well merit further study.

Relative Technical Feasibility of the Mission Concept

The key technologies identified for MUST are large format buttable high-quantum-efficiency charge-coupled devices (CCDs) and high-resolution, large-format anodes for improved microchannel plate detectors. None of these are likely to be challenging. The current baseline design of a segmented primary mirror might have low-enough scattered light to allow progress for observations of Earth-like planets in habitable zones.

On-orbit assembly and verification for the large optics may benefit from astronaut involvement. Optic coatings and tunable filters need to be developed as well. However, these do not represent substantial technological improvements from current technology.

General Cost Category in Which This Mission Concept Is Likely to Fall

No cost estimate was provided for this mission concept. However, extrapolating from the costs of the Hubble Space Telescope and JWST, the committee estimates that MUST will cost at least $4 billion to $6 billion.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

MUST was proposed for launch on an Atlas V Phase II launch vehicle with a 7.2-meter fairing. Although this vehicle configuration has been identified as a potential growth option, it does not currently exist and there are no plans at the moment to develop this upgraded capability. Current mission plans would use robotic assembly at L2. Robotic assembly tools would be part of the launched telescope assembly payload. The 10-meter primary mirror (4- to 6-meter segment and surrounding 2-meter petals) would be deployed using shuttle and space station robot arm technology. Reduction of risk and cost may be likely if astronaut participation in assembly at Earth-Sun L2 is used.

Because the baselined launch vehicle does not exist, executing MUST will require that it be scaled back to fit within the existing capabilities of the Delta IV heavy, or reconfigured to use the Ares V launch vehicle. The benefits of using the Constellation suite of launch vehicles would be that (1) an Ares V would allow a large (single) primary mirror (or a fully deployable larger single mirror) to be placed at L2 and (2) the Orion spacecraft, launched atop an Ares I rocket and then boosted out of low Earth orbit in some manner, might provide the ability to assemble and/or service and upgrade the telescope at Earth-Moon L1 or Earth-Sun L2. Although this would enhance the capabilities of the telescope, it would also increase costs. The long history of human servicing of the Hubble Space Telescope offers lessons in the costs and advantages of this approach.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The MUST mission concept is worthy of further study as a Constellation-enabled science mission. The robotic assembly technique proposed in this mission concept could be simplified, enhanced, or eliminated with the capabilities of the Constellation System, particularly a launch vehicle with a larger shroud diameter. The remarkable success of the Hubble Space Telescope and its continued importance to many fields of astronomy indicate that there should be further study of a next-generation large ultraviolet/optical space-based telescope.

ASTRONOMY AND ASTROPHYSICS/HELIOSPHYSICS VISION MISSION

Stellar Imager[14]

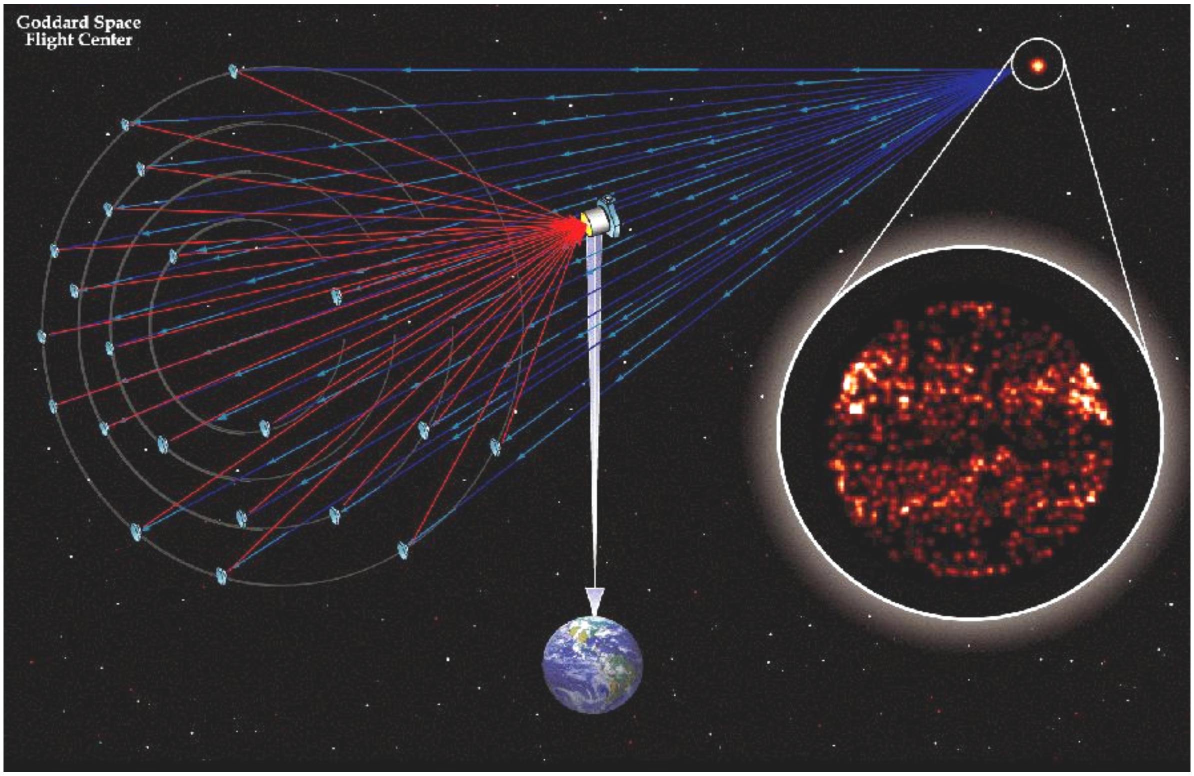

FIGURE 2.6 Illustration of the Stellar Imager, which uses a large array of formation-flying mirrors (left) to focus light on a common beam-combining hub (center) to produce ultrahigh-resolution images of stars and other celestial targets (right). SOURCE: Courtesy of K. Carpenter, NASA Goddard Space Flight Center.

Scientific Objectives of the Mission Concept

The magnetic fields of the Sun cause coronal mass ejections, which in turn can affect satellites, Earth-based technology like navigation systems, communications systems, and power grids. The associated accelerated relativistic particles can also be harmful to astronauts and airline passengers. The goals of the Stellar Imager are to understand solar and stellar magnetic activity and its impact on space weather, planetary climates, and life, and to understand magnetic processes and their roles in the origin and evolution of structure and the transport of matter throughout the universe.

The stellar dynamo is a complex process that involves both nonlinear and nonlocal events. It remains a mystery to scientists, who are still unable to accurately model and predict the Sun's behavior on monthly or decade-long timescales. A possible way to improve current understanding of the dynamo on

---

[14] K.G. Carpenter et al., *SI—The Stellar Imager: A UV/Optical Deep-Space Telescope to Image Stars and Observe the Universe with 0.1 Milli-arcsec Angular Resolution*, September 2005; K.G. Carpenter, "Stellar Imager (SI): Revealing Our Universe at High Resolution," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 20, 2008.

the Sun is to conduct a population study of stars similar to the Sun and compare these data to those of older stars, binary stars, and others. Such an understanding might enable predictive capabilities for space weather by examining stellar dynamics on "stellar-cycle" timescales.

The Stellar Imager (Figure 2.6) will enable general astrophysics research about the effect of magnetic fields on the transport of matter throughout the universe. Its long-baseline interferometer will benefit astrophysics in many ways, allowing unprecedented images of active galactic nuclei, quasi-stellar objects, supernovae, interacting binary stars, supergiant stars, hot main-sequence stars, and protoplanetary disks. The Stellar Imager will provide information on the interiors of stars using astroseismology to constrain internal structure, differential rotation, and large-scale circulations.

The Stellar Imager plans to use formation flying 1-meter mirrors to form a Fizeau interferometer providing 0.1-milliarcsecond resolution or, in "light bucket mode," providing an equivalent 11-meter-mirror ultraviolet/optical telescope. The Stellar Imager (SI) would cover the ultraviolet and optical wavelengths (1200-6600 Å). It would provide images of dozens of stars over a period of up to a decade each. SI would be composed of 20 one-meter primary mirrors on a virtual surface (formation flying) that can have their baseline varied from 100 meters up to 1,000 meters depending on the target requirements. SI would orbit at Earth-Sun L2 and would provide significant advances in observation of stellar interiors and magnetic variability. The committee believes that this mission does offer a significant advance in its scientific field.

Characteristics of the Mission Concept as Developed to Date

The general idea of interferometry from many ground-based instruments is quite mature. Space-based interferometric platforms are still immature, and SI would benefit greatly from early-demonstration interferometric missions such as the Fourier–Kelvin Stellar Interferometer (FKSI) or Pegase (a proposed space mission to build a double-aperture interferometer composed of three free-flying satellites). The major technology requirements related to formation flying for SI (see the next section below) are all immature at the present time. Some small efforts in development have been undertaken via NASA and Air Force funding, but these efforts would need to be further developed in a timely manner to realize SI operation in the 2020 time frame.

Relative Technical Feasibility of the Mission Concept

The Stellar Imager is a technically challenging mission but one that is a natural follow-on to ground-based (e.g., Center for High Angular Resolution Astronomy [CHARA], ESO Very Large Telescope Interferometer [VLTI]), and space-based (e.g., FKSI, Pegase) interferometers. The Fizeau interferometer mode will reposition the same mirrors to form the interferometer. Baselines from the central hub (containing the secondary mirror and focal plane instrument) would range from 100 to 1,000 meters. Each Stellar Imager operating mode requires all-formation flying of the many mirrors, and so there is a significant technology challenge for all the options.

The Stellar Imager is a somewhat easier concept than the Space Interferometer Mission (SIM) or the Terrestrial Planet Finder Interferometer (TPF-I) (these two being astrometric or nulling missions) because of its less stringent tolerances for the interferometry. Both missions may offer technology development useful for the Stellar Imager.

The main technology challenge/development areas for the Stellar Imager are as follows:

1. Formation flying of 20 spacecraft
    a. Launch to and deployment at Earth-Sun L2
    b. Positioning and control of formation elements to 3 nm
    c. Aspect control to tens of microseconds of arc

   d. Need for variable, non-condensing, continuous-use micro-Newton thrusters
   e. Large dynamic range of motion control required
   f. Autonomous real-time analysis and/or real-time wavefront sensing control needed
2. On-board and ground-based methods for dynamic control testing and validation.

There are additional, although less severe, challenges in technologies for mirror fabrication, spacecraft construction, and detector development. Key specific issues such as uncertainties about propellant (what propellant, how much propellant, and contamination issues involved with many free-flying mirrors constantly firing thrusters) need to be addressed as well.

The SI proposal team has a well-thought-out technology plan, at this early stage, for approaches to the needed technology development. However, this plan requires technology development by other programs that have not yet been flown.

General Cost Category in Which This Mission Concept Is Likely to Fall

The Stellar Imager is a large, flagship-class mission estimated by the proposers to cost approximately $3 billion if flown with ~20 one-meter mirrors and a single hub. Larger formation-flying mirrors and an additional hub (to increase scientific and operational efficiency as well as provide redundancy for risk mitigation) would be possible with a single Ares V launch vehicle, but it is currently not possible to estimate cost.

As discussed above, SI is technically challenging and as such, the initial cost estimate is likely to be too low. The mission depends critically on earlier test-bed missions for technology development in certain areas. Thus, it is likely that the mission falls in the greater than $5 billion category.

It is hard to determine any cost savings that may occur if a single Ares V launch is accomplished compared to a pair of Delta IV heavy launches. The trade-offs between larger fairing and weight limits versus the complexity of initiating formation flying once the two missions reach Earth-Sun L2 are difficult to assess at present. However, it seems highly likely, based on past experience, that mission complexity and risk (and thus cost) are far reduced by use of a single launch because it would allow the integration of as much of the hardware as possible prior to launch.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The scientific benefit and reliability of this mission would be enhanced if an Ares V was available as the launch vehicle. Larger formation-flying mirrors could be easily accommodated in the fairing at only a modest mission cost increment. Larger mirrors would increase the sensitivity and science productivity of the observatory by increasing the amount of light gathered and enabling much faster asteroseismic observations (and therefore more stars to be studied in less time). Flying two central hubs would also be possible, thereby providing risk mitigation and easing complexity of deployment. Cost-savings may occur as well using an Ares V because the Stellar Imager could be flown in a single launch placing the entire mission payload in orbit and to Earth-Sun L2. The Stellar Imager could be launched using the current fleet of EELVs. However, two launches using the Delta IV launch system would be required for the full mission concept. This includes the complete free-flying mirror complement as well as two hubs to allow increased on-target efficiency as well as redundancy.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The Stellar Imager mission concept is worthy of further study as a Constellation-enabled science mission. A large launch vehicle such as the Ares V could allow for the full concept to be placed in orbit with a single launch, thereby reducing launch risks. This mission would allow a dramatic improvement in scientific understanding of our Sun and similar stars.

HELIOPHYSICS VISION MISSIONS

Interstellar Probe[15]

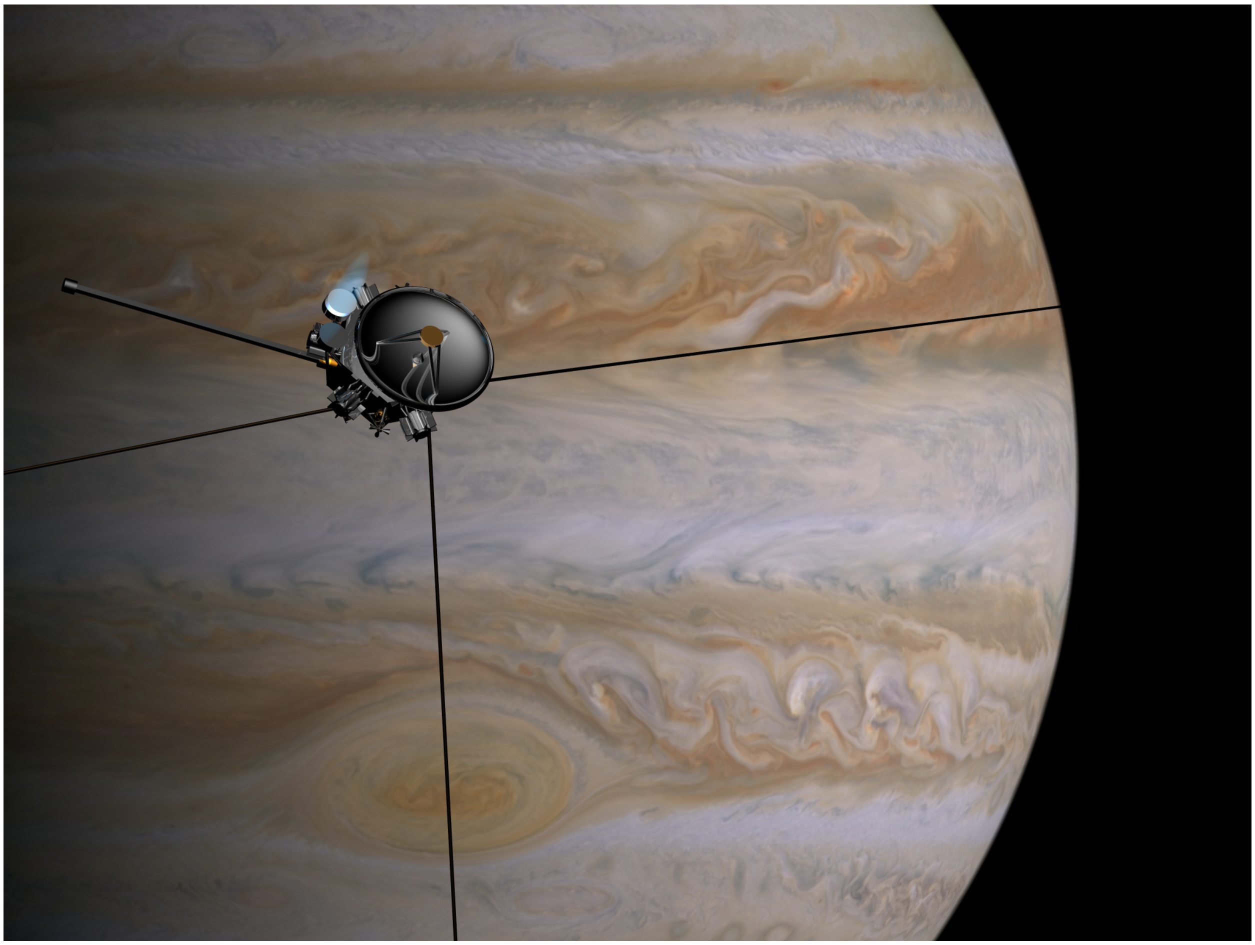

FIGURE 2.7 Illustration of one possible Interstellar Probe configuration during its flyby of Jupiter. SOURCE: Reprinted with permission of the Johns Hopkins University Applied Physics Laboratory.

[15] T.H. Zurbuchen et al., *Nuclear-Powered Interstellar Probe*, 2006; R.L. McNutt, Jr., "Enabling a Faster, Better Innovative Interstellar Explorer with NASA's Constellation System," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 21, 2008. Note that the presentation to the committee was not made by a member of the team that delivered the final report. However, the presentation was made at the recommendation of the principal investigator for the report *Nuclear-Powered Interstellar Probe,* and both studies had nearly identical scientific recommendations.

Scientific Objectives of the Mission Concept

The heliosphere, the bullet-shaped bubble created by the interaction of the solar wind and solar magnetic field with the interstellar medium, shields our solar system from most interstellar plasma, cosmic rays, and dust. The Interstellar Probe (ISP; Figure 2.7) will travel beyond the heliopause (the boundary of the heliosphere) to study cosmic rays, plasma, neutral particles, and dust that constitute the galactic environment close by our solar system. It is designed to answer the following fundamental questions:

1. What is the nature of the nearby interstellar medium?
2. How do the Sun and galaxy affect the dynamics of the heliosphere?
3. What is the structure of the heliosphere?
4. How did matter in the solar system and interstellar medium originate and evolve?

The Sun moves through the interstellar medium at a supersonic speed and thus creates a shock bounding the heliosphere at about 180-200 AU. ISP proposes to explore interstellar space, located beyond this boundary.

To reach interstellar space in a reasonable amount of time will require a spacecraft with unprecedented propulsion capability. The initial NASA-funded Vision Mission concept studies included two implementations of the Interstellar Probe, one utilizing nuclear reactor technology (Zurbuchen et al.) and one utilizing solar sails (McNutt et al.). Although a final report was written only for the nuclear option, the committee received a presentation on the solar sail implementation and two others, one using radioisotope electric propulsion (REP) and one using conventional propulsion. The two Vision Mission approaches had almost identical science goals and very similar instrument payloads. The main difference is that, because of its large size, the nuclear reactor implementation included remote sensing instruments in the visible, ultraviolet, and infrared wavelengths, allowing it to observe the universe from a unique vantage point beyond the zodiacal light and far from Earth. Because of the similarity of the primary instruments and science goals, the concept maturity, technology readiness, and advantages gained via use of the Constellation architecture for both, they are discussed together below.

The ISP (previously called Interstellar Explorer; IEX) has been endorsed by the 2003 NRC decadal survey *The Sun to the Earth—and Beyond*.[16] The mission concept is included in the 2005 Heliophysics Roadmap as a "Vision Mission." It has also been discussed in earlier roadmaps and decadal surveys. In addition, an ESA Technology Reference Studies (TRS) report, funded through the Science Payload and Advanced Concepts Office[17] and published in April 2007, describes a solar sail mission with two close flybys of the Sun. This mission may therefore provide an opportunity for international cooperation.

The ISP mission logically follows on the limited in situ observations by the Voyager spacecraft and the upcoming remote sensing observations to be obtained by the Interstellar Boundary Explorer (IBEX), which will image the large-scale shape of the boundaries separating the heliosphere from the interstellar medium. Because of its complete instrument payload and data rate, ISP will provide a very significant improvement over existing missions, potentially enabling revolutionary discoveries about our local galaxy. The committee believes that this mission does offer a significant advance in its scientific field.

[16] National Research Council, *The Sun to the Earth—and Beyond: A Decadal Research Strategy in Solar and Space Physics,* The National Academies Press, Washington, D.C., 2003.
[17] See http://sci.esa.int/sciencee/www/object/index.cfm?fobjectid=36022.

Characteristics of the Mission Concept as Developed to Date

The scientific concept, instrument package, and mission plan of ISP are quite mature. The goal is to leave the solar system within ~20° of the incoming interstellar wind and to reach 200 AU in as short a time as possible. The primary technology issues are (1) need to have radioactive power sources; (2) low-power, low-mass Xe-ion thrusters for radioisotope electric propulsion and nuclear-electric propulsion or solar sail development for that option; and (3) Deep Space Network upgrade to a phased array of many antennas. In addition there is the problem of qualifying parts for a 30- to 50-year lifetime.

Most of the scientific instruments on ISP have substantial heritage. These include a fast plasma instrument to measure the solar wind, pick-up ions, and interstellar plasma; a magnetometer to measure the quasi-static magnetic field; and a plasma/radio wave receiver. There are also instruments to measure energetic particles, an instrument to measure the composition of the dust in the interstellar medium, and a neutral imager.

The ISP requires either a nuclear reactor or radioisotope thermoelectric power (RTG/RPS). The ISP requires high-capability propulsion. Four options were considered: conventional, nuclear reactor electric, radioisotope electric, and solar sail. Given the current lack of technology development programs for nuclear propulsion and for solar sails, the conventional propulsion or the REP provide the most mature mission concepts. The major disadvantages are (1) longer flight time to 200 AU, ~30 years with Jupiter gravity assist versus 20 years for nuclear, or 15 years for solar sail. There are also trade-offs in the use of a Jupiter assist, which requires use of radiation-hardened parts and shielding.

Relative Technical Feasibility of the Mission Concept

The scientific concept, instrument package, and mission plan of the ISP are quite mature. For all four implementations, the primary technology issues are (1) need to have radioactive power sources; (2) qualifying parts and ensuring instrument operations for a 30- to 50-year lifetime; and (3) Deep Space Network upgrade to phased array of many antennas. All four implementations have propulsion technology issues.

The nuclear reactor option described in the Zurbuchen et al. Vision Mission report is the more complex of the two missions because of the requirements levied by the use of a nuclear reactor. The mission plan incorporated a mother craft with the nuclear reactor and remote sensing instruments and two identically instrumented daughter spacecraft with in situ particles and fields instruments that would be released after most of the reactor burnout. This approach provides additional science capability but at a higher cost. Because there is no nuclear reactor development program at this time, this option is not considered in the next sections.

General Cost Category in Which This Mission Concept Is Likely to Fall

The conventional propulsion (using the Ares V with an additional Centaur upper stage) and the REP missions fall into the $1 billion to $5 billion category.[18]

[18] This estimate is based on the presentation to the committee by Ralph McNutt.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

Although a number of different options were considered, two were described in detail. One was a Delta IV heavy with two solid rocket motors as upper stages; the other was an Ares V with Centaur upper stage (a capability that NASA has no plans for at the moment). For the same spacecraft dry weight, the Ares option reached 200 AU in ~23 years, compared to 30 years for the Delta IV option, and was traveling 1.9 AU/year faster at that time. The benefits of using Constellation are avoiding non-conventional propulsion (solar sail or REP). The Delta IV option is a unique configuration that has never been flown. It would require further study to determine if it is feasible and, if so, the cost relative to the Ares V option.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The Interstellar Probe mission concept is worthy of further study as a Constellation-enabled science mission. Utilizing an Ares V, the mission may provide a very significant improvement over earlier mission concepts. The use of the Ares V may enable the mission to be flown using only conventional propulsion, thus removing the need for solar sail technology or radioisotope electric propulsion, and it could shorten the time to reach 200 AU. Shortening the mission lifetime would reduce costs and risk. Additional capability may also enable instrument package redundancy, further reducing risk. The science motivation for the mission has continued to receive the endorsement of decadal surveys and other studies for two decades.

Solar Polar Imager[19]

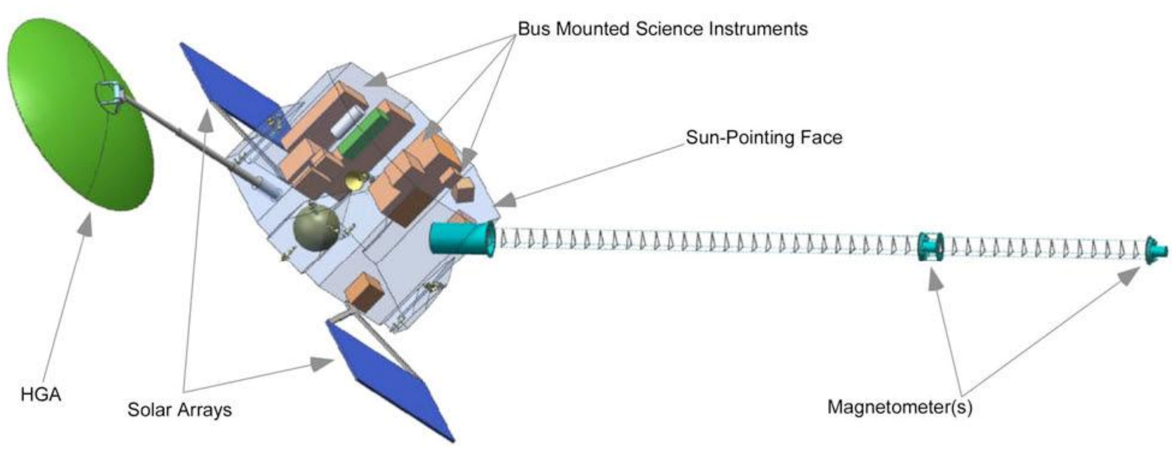

FIGURE 2.8 Illustration of the Solar Polar Imager spacecraft. SOURCE: Courtesy of NASA, Jet Propulsion Laboratory, California Institute of Technology.

[19] P.C. Liewer et al., *Solar Polar Imager: Observing Solar Activity from a New Perspective*, Vision Mission Study Final Report, December 2005; "Solar Polar Imager: Observing Solar Activity from a New Perspective," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 2008.

Scientific Objectives of the Mission Concept

Current understanding of the Sun and its atmosphere is severely limited by the lack of good observations of the polar regions. The Solar Polar Imager (SPI) is a spacecraft (Figure 2.8) in a 0.48-AU circular orbit around the Sun with an inclination of 75° to the ecliptic plane, intended to provide remote sensing observations of the Sun and in situ observations (measurements of the local properties of the plasma and electromagnetic fields) of this critical region. Observations of the Sun's poles, not possible from the usual ecliptic viewpoint, may revolutionize scientific understanding of the internal structure and dynamics of the Sun and its atmosphere. Through the use of remote sensing and in situ instrumentation, SPI will investigate the physical connection between the Sun, the solar wind, and solar energetic particles. The primary science questions motivating the SPI mission are the following:

1. What is the relationship between the magnetism and dynamics of the Sun's polar regions and the solar cycle?
2. What is the three-dimensional global structure of the solar corona, and how is this influenced by solar activity and coronal mass ejections?
3. How are variations in the solar wind linked to the Sun at all solar latitudes?
4. How are solar energetic particles accelerated and transported in radius and latitude?
5. How does the total solar irradiance vary with solar latitude?
6. What advantages does the polar perspective provide for space weather prediction?

Because SPI will make the first close-up remote sensing measurements of the polar regions of the Sun, there is the possibility of surprising discoveries and greatly improved understanding. SPI would serve as a pathfinder for permanent high-ecliptic-inclination solar sentinels, if it is deemed that the polar perspective is important for space weather prediction.

The SPI will have a Doppler-magnetograph imager, a white light coronagraph, an extreme ultraviolet (EUV) imager, a total solar irradiance imager, an ultraviolet (UV) spectrograph, a magnetometer, a solar wind ion composition and electron spectrometer, and an energetic-particle instrument. In the original Vision Mission study, the spacecraft used a Delta IV launch vehicle and required 6.7 years of flight time to reach its science orbit. Once it reached its intended orbital distance from the Sun, it would use a solar sail to achieve the highly inclined orbit of 75 degrees. The use of the solar sail determines the mission duration, the choice of launch vehicle, the spacecraft configuration and control subsystems, and the operations cost during the long cruise. It also complicates the thermal, power, and telecommunications designs. Use of the Ares V might permit use of solar-electric propulsion or perhaps direct injection with consequent simplification and shortening of the time to arrive at the science orbit. The science goals for the mission require simultaneous observations in the ecliptic plane for the helioseismology and magnetographic studies, and the cost for these is not included in the SPI mission description.

The only measurements obtained to date of the Sun's polar regions are the observations of the solar wind made by the Ulysses spacecraft (in a polar orbit around the Sun) at 2-3 AU. The structure of the solar wind at the poles was very different at solar maximum compared with solar minimum, but Ulysses' long (8-year) orbital period permitted only snapshots of the solar wind speed and magnetic field at the poles. Ulysses did not have any remote sensing instruments to probe solar structure and magnetic fields. In comparison, the SPI orbit would provide measurements of the poles every 4 months, closer to the Sun and with more complete diagnostics.

Although SPI is not mentioned by name in the 2003 solar and space physics decadal survey,[20] a multi-spacecraft mission to provide imaging of the poles of the Sun is described as an important next step in providing understanding of the solar structure, magnetic field, and dynamics, and a multi-spacecraft mission is a possible enhanced version of the SPI mission. The committee believes that this mission does offer a significant advance in its scientific field.

The European Space Agency (ESA) Solar Orbiter mission is planned to reach latitudes of ~32 degrees. ESA has performed a preliminary study of Solar Polar Orbiter, making this mission is a candidate for international cooperation.[21]

Characteristics of the Mission Concept as Developed to Date

The scientific concept, instrument package, and mission plan of SPI are mature. The primary technology issues are (1) need to use solar sails to reach the desired polar orbit; (2) weight/power for total irradiance monitor; and (3) flight test for the Doppler magnetograph and the UV spectrograph.

The mission concept as described depends on the use of solar sails, which are an untested technology that currently has no development path. If use of the Ares V would allow the high-inclination orbit to be attained with solar-electric propulsion or conventional propulsion, it would greatly simplify the mission.

Relative Technical Feasibility of the Mission Concept

The instrument payload is relatively mature, with essentially all instruments, except the Doppler magnetograph imager, having heritage from many missions. The heritage for the white light coronagraph is STEREO; for the EUV is SOHO and STEREO; for the helium vector magnetometer, Ulysses and Cassini; the ion/electron instruments are similar to those for FAST/THEMIS and MRO; the ion time-of-flight (TOF) is similar to that for MESSENGER; and the energetic-particle instruments are similar to those flown on the STEREO mission. The Doppler magnetograph imager design is based on a ground instrument that has received study as a flight instrument. There is no existing total irradiance monitor that can meet the low weight and power requirements, but there is an ongoing instrument development program.

The telemetry assumes X-band and a new Deep Space Network using 180 twelve-meter-diameter antennas, although SPI could use Ka-band and 30-meter antennas with an increase in power and mass.

The mission concept for the Solar Polar Imager is mature. Propulsion to enable the spacecraft to reach the desired high-inclination orbit is the only element that is not mature.

General Cost Category in Which This Mission Concept Is Likely to Fall

The cost estimated for the initial Delta IV version was less than $1 billion. Adding two extra spacecraft would increase this cost, although there are cost benefits to building multiple spacecraft.

---

[20] National Research Council, *The Sun to the Earth—and Beyond: A Decadal Research Strategy in Solar and Space Physics,* National Academies Press, Washington, D.C., 2003.

[21] See http://sci.esa.int/science-e/www/object/index.cfm?fobjectid=36025.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The Solar Polar Imager would require the development of solar sail technology to reach the desired orbit utilizing a Delta IV launch vehicle. However, with a launch on Ares V, the mission may be achievable without solar sails. Based on performance data provided to the committee by NASA concerning an Ares V equipped with an upper stage, it would be possible to launch SPI with solar-electric propulsion using advanced ion thrusters. The result would be a shorter time to mission orbit (5 years compared to 8 years). Eliminating the solar sails would also simplify the mission. Because power and mass were drivers in the initial design of instruments, there may also be some simplification in instruments.

An interesting possibility would be launching multiple satellites to monitor solar activity simultaneously. The baseline SPI concept observes a particular point on the Sun every 4 months; however, activity on the Sun occurs on much shorter timescales. The optimum would be to insert three identical satellites into equally spaced polar orbits, thereby ensuring continuous coverage of the Sun. Given the projected launch capability of the Ares V with a dual-engine Centaur, it appears that two identical spacecraft could be inserted into complementary orbital inclinations, providing enhanced coverage of the Sun. (Note that NASA has no current plans to adapt the Centaur to the Ares V.) Additional study is required to determine if a third spacecraft could be launched on a single mission to provide more continuous coverage of the polar region. The cost of the project would increase primarily by the cost of the additional spacecraft. Note that some of the SPI science goals require simultaneous observations from the ecliptic plane, which could possibly be provided by these additional spacecraft.

The Vision Mission report suggests that astronauts could assemble the satellite for the solar sail implementation, an activity that would not be necessary if this mission is launched on an Ares V.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The Solar Polar Imager mission concept is worthy of further study as a Constellation-enabled science mission. The Ares V launch vehicle could allow the elimination of the unproven solar sail technology. Solar Polar Imager would make unique observations of the Sun's polar regions, dramatically improving and possibly changing current understanding of the behavior of the star closest to Earth. This better understanding could be applied to the study and understanding of stars elsewhere in the universe. The enhanced understanding may enable predictive capabilities about the behavior of the Sun, needed for space weather and climate prediction.

SOLAR SYSTEM EXPLORATION VISION MISSIONS

Neptune Orbiter with Probes[22]

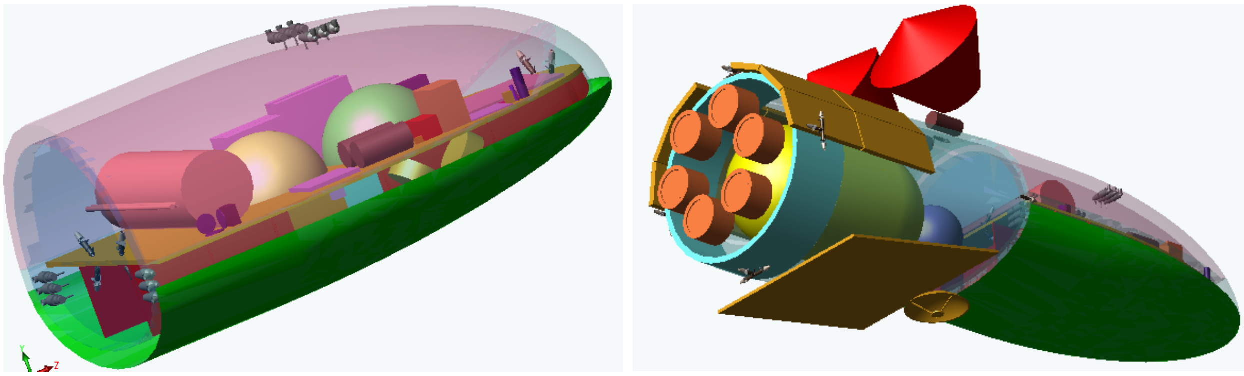

FIGURE 2.9 Illustration of a possible Neptune Orbiter mission utilizing aerocapture. The aerocapture stage is on the left; the coast stage, including two probes, is on the right. SOURCE: T.R. Spilker, Jet Propulsion Laboratory.

Scientific Objectives of the Mission Concept

The planets in our solar system fall into three classes: terrestrial planets, gas giants, and ice giants. Only the last category has not been studied comprehensively, meaning that the Neptune Orbiter (Figure 2.9) would bring the first detailed exploration of such a body. The Voyager 2 reconnaissance of the Neptune system nearly two decades ago produced most of our knowledge of this distant planet. It revealed a surprisingly dynamic atmosphere, surrounded by a displaced and highly distorted magnetosphere. Many of the extrasolar planets detected to date fall into this category in terms of mass and size. A Neptune mission would also survey the almost-planet-sized geologically active satellite Triton, which—owing to its retrograde orbit—is suspected to be a captured Kuiper Belt object. Within the context of comparative planetary studies, these topics deserve further study.

The Neptune system was ranked as one of the three “other important objects,” after the top four choices, in the NRC’s 1994 report *An Integrated Strategy for the Planetary Sciences 1995-2010*, and was the focus for two of the nine “deferred high-priority flight missions” listed in the 2003 NRC solar system exploration decadal survey.[23] A Neptune mission was also recommended for further science definition in the next solar system decadal survey by the 2007 NRC report *Grading NASA’s Solar System Exploration*

---

[22] *NASA Vision Mission Neptune Orbiter with Probes*, Contract No. NNH04CC41C, Final Report, Volumes 1 and 2, Revision 1, September 2005; Andrew P. Ingersoll and Thomas R. Spilker, *Study of a Neptune Orbiter with Probes Mission*, Final Report, May 2006; Thomas R. Spilker and Andrew P. Ingersoll, “Aerocapture Implementation of NASA’s ‘Neptune Orbiter with Probes’ Vision Mission,” presentation to the Committee on Science Opportunities Enabled by NASA’s Constellation System, February 20, 2008; Bernie Bienstock and David Atkinson, “NEPtune Orbiter with Probes,” July 21, 2005, Outer Planets Assessment Group (presented by Thomas Spilker to the Committee on Science Opportunities Enabled by NASA’s Constellation System, February 2008). Note that both Neptune reports were presented by Thomas Spilker, a lead author of one of the studies.

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

Program: A Midterm Review.[24] A comprehensive study of an ice giant would be a logical mission to follow the detailed explorations of Jupiter by Galileo (mid-1990s) and of Saturn by Cassini-Huygens (ongoing). The committee believes that this mission does offer a significant advance in its scientific field.

Characteristics of the Mission Concept as Developed to Date

Neptune is 30 times as far from the Sun as Earth, and as such any mission will take many years to reach its target; for example, the most energetically efficient orbit takes 30 years. An EELV-based mission using chemical propulsion both to reach Neptune and to decelerate into orbit provides either too little payload mass or too long a flight time. In order to reduce flight times to an acceptable duration, technology is called upon to slow down the spacecraft on approach to its target. In the proposed non-nuclear version of this mission, solar-electric propulsion is employed to speed the interplanetary transit, and then aerocapture reduces the orbital energy, allowing an elliptical orbit about Neptune. In the alternative version, nuclear-electric propulsion is used to accelerate and then decelerate the spacecraft to allow a conventional powered capture. Both of the proposed Neptune missions, with launch in 2016, perform a Jupiter flyby to arrive at Neptune in 2029. Early on, just before and just after capture of the mother spacecraft into a Neptune orbit, a probe is released for ultimate insertion into Neptune's atmosphere (one at the equator, and one at high latitude). Four years later the spacecraft will rendezvous with Triton on its retrograde orbit in order to allow deployment of an orbiter or a lander on the satellite. The mission ends in 2033. Since the gravity assist by Jupiter is critical, similar missions can be accomplished only every dozen years, meaning that another opportunity will occur late in the 2020s with the mission's end happening about 2045.

Any Neptune orbiter mission requires strategic investments in power sources, transportation, communication, and sensor technology. In order to have reasonable trip times while carrying a comprehensive payload, some new technology is required, either a significantly powered flight (via nuclear-electric propulsion) or aerocapture. Maneuvering within the Neptune system is accomplished in one case by solely using gravity assists by Triton; a mission carrying nuclear-electric propulsion would be able to use that capability to supplement gravity assists when switching orbits.

Of the two missions reviewed by the committee, only the non-nuclear version can fly on a Delta IV heavy launch vehicle, according to the current Delta IV Payload Guide (PPG). Using the performance characteristics given in the 2002 PPG, the proposers concluded that, if a lander were to be deployed, a Delta IV heavy would be inadequate. However, the more recent PPG claims an increased payload capability, and this should be sufficient for all stated mission options. This proposed mission utilizes both solar-electric propulsion and aerocapture, the latter representing a significant development challenge. Currently NASA has no plans to develop or demonstrate aerocapture and has recently determined that this technology will not be developed further.

The Neptune mission would carry at least two entry probes. In order to constrain formation models of Neptune, the probes would measure the elemental abundances of He, Ne, Ar, Kr, Xe, C, and S, and the isotopic ratios 15N/14N and D/H. By combining these data with similar available information for Jupiter and possibly Saturn, it should be possible to determine whether the gas giants and the ice giants originated out of the same material in the same general locale. One of the probes would carry a gas chromatograph mass spectrometer (GCMS) that is critical to the mission goals. The probes would study temperature and abundance data from Neptune's stratosphere to ~100-200 bar. Complementary measurements of winds, structure, composition, cloud-particle size, and lightning would also be obtained.

The orbiter would carry the usual broad array of instruments to observe the planet, its satellites, rings, and magnetosphere. These include imaging instruments at ultraviolet, optical, infrared, and radio

[24] National Research Council, *Grading NASA's Solar System Exploration Program: A Midterm Review,* The National Academies Press, Washington, D.C., 2007.

wavelengths, as well as measurements of the magnetosphere both in situ and remotely. Dust and plasma wave detectors might be carried.

Any Neptune orbiter would also have the ability to provide global mapping of Triton's surface and atmosphere, in order to establish the composition and age of the surface, the inventory of volatiles, the satellite's current geologic activity, and the atmosphere's character. A Triton surface lander will enable direct measurement and sampling of the atmosphere and surface, to study surface-atmosphere interactions, and may allow direct seismic probing of Triton's interior.

Relative Technical Feasibility of the Mission Concept

The technical maturity of the instruments is high, but the maturity of propulsion options is low. Each version of the basic Neptune mission with probes concept (i.e., aerocapture or nuclear-electric propulsion) has an aspect that would require significant further development. At present, neither is expected to be available in the foreseeable future.

General Cost Category into Which This Mission Concept Is Likely to Fall

The Cassini-Huygens mission, an ongoing comprehensive exploration of the Saturn system that dropped a single probe into Titan's atmosphere in early 2005, is an acceptably close analog to this proposed flight. That mission cost $3 billion to $4 billion. Given inflation and their somewhat more complex mission profiles, the proposed Neptune missions likely would cost more than $5 billion in contemporary dollars.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The nuclear-electric propulsion mission concept significantly exceeds the capabilities of the largest rocket currently in any nation's inventory. The Ares V would be able to lift the 36 metric tons identified for this concept. However, the nuclear-electric propulsion required for this mission represents significant technical challenges.

In the case of the second mission profile, an Ares V rocket would likely allow such a mission to be flown without the requirement for aerocapture at Neptune. Because there is currently no development program for either aerocapture or nuclear reactors, the chemical propulsion option my provide the most mature option.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The Neptune Orbiter with Probes mission concept is worthy of further study as a Constellation-enabled science mission. The Ares V launch vehicle could eliminate the need for aerocapture in Neptune's atmosphere. The study of an ice giant planet is pivotal to understanding the origins of the solar system. Studying Neptune and Triton is also important for comparative planetary studies, in particular for the study of what drives the activity of both bodies in the frigid outer solar system. Finally, understanding an ice giant would be useful for the study of extrasolar planets.

Titan Explorer[25]

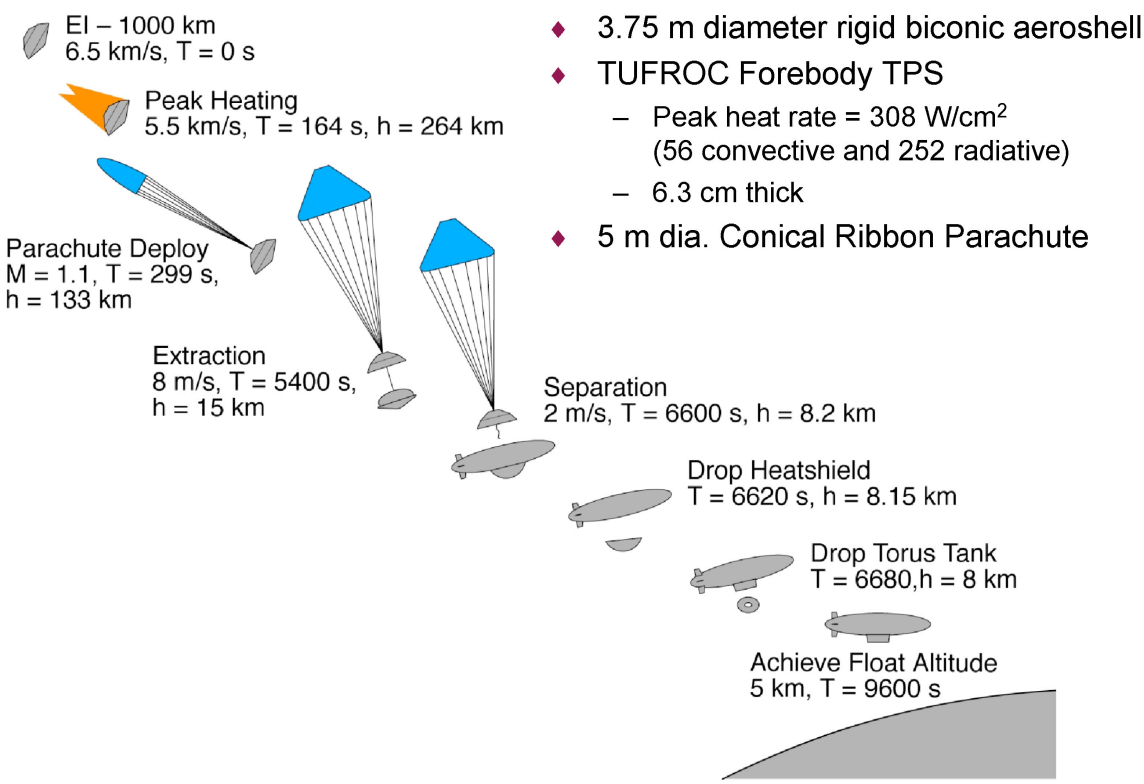

FIGURE 2.10 Illustration of the deployment sequence for the Titan Explorer airship. SOURCE: Courtesy of Joel S. Levine, NASA.

Scientific Objectives of the Mission Concept

Knowledge and understanding of Titan, Saturn's largest moon, have increased significantly as a result of measurements obtained from the Cassini spacecraft following its orbital insertion around Saturn on June 30, 2004, and more recently with measurements from the descent of the Huygens probe through the atmosphere and onto the surface of Titan on January 14, 2005. More than 3 years of analysis of the Huygens data and nearly 4 years of analysis of the Cassini data have dramatically improved understanding of Saturn and its moons, particularly Titan and Enceladus. In the search for life in the solar system, Titan is in a unique position. Its density suggests that it is composed of a mixture of rock and ice in almost equal amounts, and Titan's atmosphere may reveal answers to questions about chemical evolution on the early Earth. The Titan Explorer mission focuses on the following scientific questions:

1. What is the chemical composition of the atmosphere, including the trace gases?
2. What is the isotopic ratio of the gases in the atmosphere?

[25] J.S. Levine, *Titan Explorer: The Next Step in the Exploration of a Mysterious World*, June 2005; J.S. Levine, "Titan Explorer: The Next Step in the Exploration of a Mysterious World," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 2008. Note: This study should not be confused with the Titan Explorer study conducted for NASA during 2007. However, the two studies had similar science goals. That study can be found at http://www.lpi.usra.edu/opag/Titan_Explorer_Public_Report.pdf.

3. What prebiological chemistry is occurring in the atmosphere/surface of Titan today, and what is its relevance to the origin of life on Earth?
4. What is the nature, origin, and composition of the clouds and haze layers?
5. What is the nature and composition of the surface?
6. Are there oceans of liquid hydrocarbons on the surface of Titan?
7. What is the nature of the meteorology and dynamics of the atmosphere?
8. What processes control the meteorology and circulation of the atmosphere?
9. What is the nature of the hydrocarbon "hydrological cycle" on Titan?
10. What are the rates of escape of atomic and molecular hydrogen from the upper atmosphere of Titan, and what impact does this escape have on atmospheric chemistry?
11. How does the atmosphere of Titan interact with the solar wind and Saturn itself?
12. How have the atmosphere and surface of Titan evolved over its history?

The committee believes that this mission does offer a significant advance in its scientific field.

Characteristics of the Mission Concept as Developed to Date

The Titan Explorer mission (Figure 2.10), with a launch mass of 5,961 kg, would launch on a Delta IV-heavy rocket. The spacecraft consists of a Titan orbiter and a Titan airship that will traverse the atmosphere of Titan and can land on its surface. The airship is designed to have an operational lifetime of 4 months after entry, and the orbiter is designed to have a lifetime of 40 months after arriving in orbit around Titan.

To answer the questions posed above, instruments on the Titan orbiter include:

a. Solar occultation spectrometer to measure atmospheric composition and isotopic ratios,
b. Radar mapper to measure the nature of Titan's surface,
c. Magnetometer to search both for a planetary dipole field and surface magnetism,
d. Ultraviolet spectrometer to measure the escape of gases from Titan's upper atmosphere, and
e. Visual and infrared mapping spectrometer to measure cloud and haze layers and the nature of the satellite's surface.

Instruments on the Titan Explorer airship include an imager, mass spectrometer, surface composition spectrometer, haze and cloud particle detector, Sun-seeking spectrometer, and a surface science package similar to that flown on the Huygens mission.

Titan's surface will be explored from an airship, the preferred platform. Success with this technique requires the development of cryogenically capable envelope materials for the airship. This requires technology development.

Relative Technical Feasibility of the Mission Concept

Approximately 1,057 kg of supercritical xenon is used as the solar-electric propulsion module propellant. There is a question whether sufficient xenon is available.

The airship instruments (the imager, the mass spectrometer, and the surface composition spectrometer) and the orbiter instruments (the magnetometer and the radar mapper) all need additional development to result in improved resolution.

Electrical power via radioisotope thermoelectric generators (RTGs), an enabling technology, for the orbiter would be a second-generation multi-mission RTG, and for the airship a second-generation Sterling radioisotope generator. Development efforts still are needed.

The spacecraft will be captured into orbit about Titan via aerocapture technology. Aerocapture is one of the key enabling technologies for the Titan Explorer mission.

Some of the major features of this mission including solar-electric propulsion, an enhancing technology, and next-generation ion engines, need major attention.

General Cost Category in Which This Mission Concept Is Likely to Fall

The project as described in the final Vision Mission report, would have a total cost in FY 2008 dollars in the range of $2.8 billion to $3.5 billion. The mission was planned around the use of an existing expendable launch vehicle (Delta IV heavy).

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The Ares V with a dual-engine Centaur has significant excess capability to enable various mission design options. It has more than enough capability to deliver the Titan Explorer, without the use of Earth gravitational assist or the solar-electric propulsion module, within the proposed 6-year mission timeline outlined in the baseline mission architecture. The excess capacity (nearly 10 metric tons) could be used to carry a conventional propulsion system to reduce or eliminate reliance on aerocapture. Further, the elimination of the solar-electric propulsion module is projected to save ~$500 million, offsetting much of the cost differential between the Delta IV and Ares V launch systems. Alternatively, the basic Ares V (without the Centaur) could potentially eliminate the need for Earth gravity assist. It may also be possible to eliminate or dramatically reduce the reliance on the solar-electric propulsion module. Further study of the various mission design options is warranted.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The Titan Explorer mission concept is worthy of further study as a Constellation-enabled science mission. The use of an Ares V launch vehicle could eliminate the requirement for aerocapture. The Titan Explorer will provide a significant improvement in knowledge of the evolution of prebiological chemistry. The committee notes that NASA is already conducting ongoing studies of possible Titan missions, although the most recent one studied utilizes a balloon instead of an airship.

SOLAR SYSTEM EXPLORATION/ASTROBIOLOGY VISION MISSION

Palmer Quest[26]

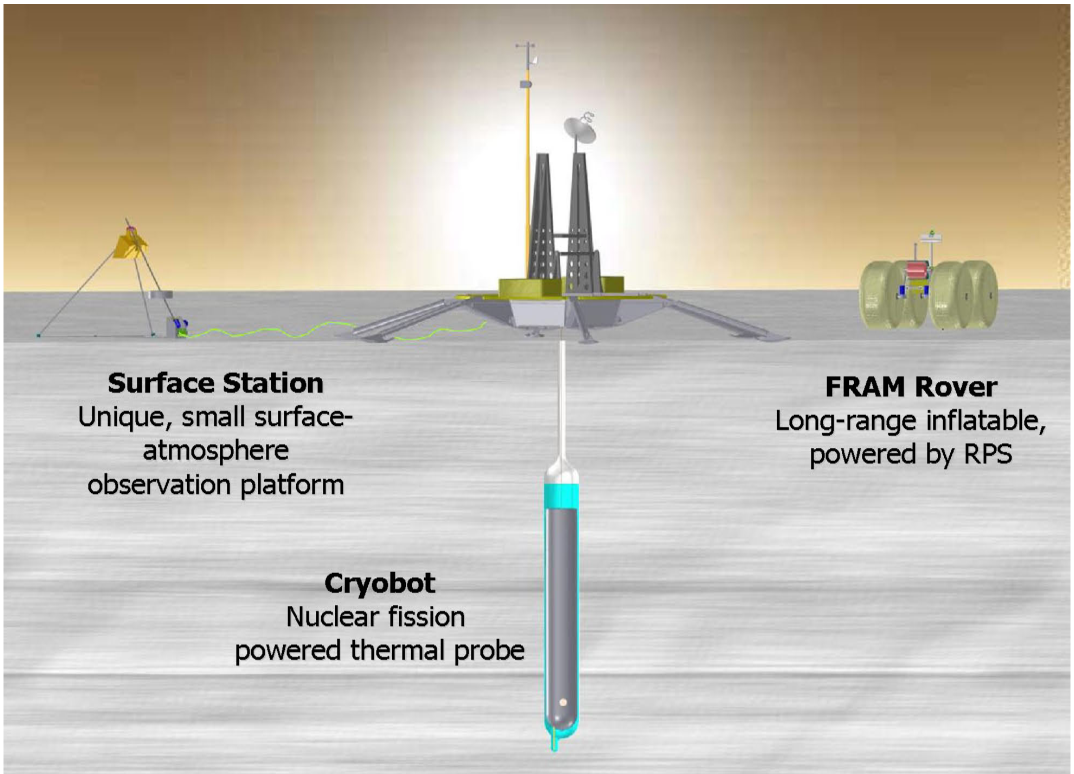

FIGURE 2.11 Illustration of the Palmer Quest spacecraft elements, including a cryobot powered by a nuclear reactor, and the surface station and rover. SOURCE: Courtesy of NASA, Jet Propulsion Laboratory, California Institute of Technology.

Scientific Objectives of the Mission Concept

Palmer Quest, a proposal for a mission to the North Polar Cap of Mars, would include a nuclear reactor-powered "cryobot" to drill down through the polar ice to its bottom and determine if life once existed on Mars (Figure 2.11). The main science goals of Palmer Quest are to assess the presence of life on Mars and to evaluate the habitability of the basal domain of the martian polar caps. Science questions for Palmer Quest are as follows:

- Does life currently exist on Mars?
- Did life ever exist there?
- How hospitable was and is Mars to life?

[26] F.D. Carsey et al., *Palmer Quest, the Search for Life at the Bed of the Mars Polar Cap*, July 2005; L. Beegle, "Palmer Quest: A Mission to the Martian Polar Cap," presentation to the Committee on Science Opportunities Enabled by NASA's Constellation System, February 2008.

The Palmer Quest objectives include looking for the presence of microbial life, amino acids, nutrients, and geochemical heterogeneity in the ice sheet; quantifying and characterizing the provenance of the amino acids in Mars's ice; assessing the stratification of outcropped units for indications of habitable zones; and determining the accumulation of ice, mineralogical material, and amino acids in Mars ice caps over the present epoch. The Palmer Quest mission will also address several objectives from decadal reports of the planetary community, including understanding the current state and evolution of the atmosphere, surface, and interior of Mars; determining if life exists or has ever existed on Mars; and developing an understanding of Mars in support of possible future human exploration. The science to be addressed by Palmer Quest has the potential for paradigm-altering discoveries related to life on Mars.

The mission would launch in August 2022 on a Delta 4050 heavy launch vehicle and a probe would descend through the ice sheet of the Mars North Polar Cap to search for life at its bed. It could be extended to quantify surface fluxes of biochemicals, nutrients, and water ice over the annual cycle and to examine outcropped ice cap strata and basal units from a surface rover. The mission would land a surface package that includes a thermal drill (the cryobot, powered by a nuclear reactor), a mobile vehicle (the FRAM [Far Ranging Arctic Mission], powered by a nuclear thermal source), and a surface observation system (the surface station, powered by the nuclear reactor). The drill would pass through approximately 2 km of dusty ice while simultaneously acquiring data. There would also be data recorded of the outcropping at the contact of the ice cap and the bed in addition to the sedimentary record stored in the polar layered deposits. The probe would also observe the seasonal cycle of the accumulation of water and CO_2 ice, dust, and organics on the polar cap surface. The development of the thermal drill may be useful in future missions involving drilling through the ice surface layer of Jupiter's moon Europa and Saturn's moon Enceladus.

The committee believes that this mission does offer a significant advance in its scientific field. However, the mission has not been endorsed by any recent NRC reports and does not appear in the 2003 solar system exploration decadal survey.[27]

Characteristics of the Mission Concept as Developed to Date

The maturity of this mission is low. Some of the scientific instruments follow in the footsteps of current and planned Mars missions such as Phoenix Lander and the Mars Science Laboratory. However, the use of a thermal drill, FRAM rovers, and a substation all powered by nuclear fission are unique to this mission. The challenges and technical feasibility associated with these components, illustrating the low maturity of this mission, are described below.

Relative Technical Feasibility of the Mission Concept

This mission is based on past successful Mars missions while incorporating novel technologies yet to be developed and that are not yet launch-ready. The technological complexity of the mission concepts is high. Three areas of technology development required by this mission and described in the presentation to the committee are thermal issues, radiation protection, and instrumentation capabilities.

Data provided as part of this presentation include a listing of the high-level risks associated with each of the components.

1. *The Cryobot,* a tool designed to melt its way through the "ice" and provide a variety of measurements related to water and biological-related chemical components. The experimenters have

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

listed the risks associated with the work of the cryobot as (a) breaking of the tether in the ice, (b) clogging of the filter system, (c) failure of pumps and valves that supply samples to the instruments, and (d) low technology readiness level (TRL) of instruments for life detection.

2. *The Rover,* the workhorse of the experiments that moves equipment and carries out exploration traverses of some 300 km, has listed as possible risks (a) failure of the inflatable wheels, (b) failure to successfully deploy the Rover, (c) difficulties during towing of the surface station, and (d) survivability over the martian northern winter.

3. *The surface station,* whose role is in situ determination of rates of accumulation of gases and dust, the estimation of annual net gases and dust accumulation, and the determination of some properties of fine-scale morphology and structure related to current polar climate, has listed as risks (a) getting stuck on the main lander body during deployment, (b) the assumption of a solid, flat surface for the deployment, and (c) physically altering the environment especially during and after winter.

4. *Mass is limited by the aeroshell.* There is little room for mass growth in this mission without a major new aeroshell development; therefore, Ares V offers no improvement.

The biological assay involved in the mission is not described in enough detail to determine its technical feasibility. The use of nuclear fission and the complexity of this mission place it in a high-risk category.

General Cost Category in Which This Mission Concept Is Likely to Fall

Although the mission costs were not directly calculated in the written report presented to the committee, a rough cost in the $3 billion to $5 billion range was mentioned in the oral presentation to the committee. This cost did not include the technology development of the more expensive mission elements, including the nuclear reactor, Mach 3 parachute, and biology-focused instrumentation. The development, required safety elements, and use of a nuclear-powered device are major cost contributors in this mission. Additional costs will be required for the new technology developments needed by this mission. Therefore, the committee places the cost estimate for the Palmer Quest mission at greater than $5 billion dollars.

Benefits of Using the Constellation System's Unique Capabilities Relative to Alternative Implementation Approaches

The Constellation System is not required for this mission because the entry mass at Mars limits the mission size. The payload volume and weight requirements are met by the Delta IV heavy launch vehicle. Although the Ares V could launch far more payload to Mars than any previous launch vehicle, there remains a severe limitation on how much mass can be delivered to the Mars surface. Increasing that mass delivery capability would require an entirely new aeroshell design and associated technology development and testing. However, the Constellation System could possibly provide increased safety for the nuclear components during launch.

Should This Concept Be Studied Further as a Constellation-Enabled Science Mission?

The Palmer Quest mission concept does not deserve further study as a Constellation-enabled science mission. This is an expensive, complex, and high-risk mission that does not require the Constellation System and could utilize an existing launch vehicle such as the Delta IV. The Palmer Quest mission has not appeared in any previous NRC reports, including the solar system exploration decadal survey[28] or the recent report *An Astrobiology Strategy for the Exploration of Mars*.[29] In addition, the technical maturity of this mission is low.

OVERALL VISION MISSION ASSESSMENT CONCLUSIONS AND RECOMMENDATIONS

The mission concepts reviewed by the committee represent a substantial amount of time and effort invested by their study teams. If implemented, they would likely result in significant advances in several fields of space science. The committee hopes that NASA will build on these studies.

Finding 3. The following Vision Mission studies might benefit from the opportunities enabled by the Constellation System and are therefore considered more deserving of future study: Generation-X, Modern Universe Space Telescope, Stellar Imager, Interstellar Probe, Solar Polar Imager, Neptune Orbiter with Probes, and Titan Explorer. The committee did not assess the relative scientific priority of the missions within this group. In the final report, these mission concepts will be compared to additional mission concepts (collected in response to the committee's request for information) that the committee determines to be more deserving of future study, and the committee will produce a consolidated list.

Recommendation 1. NASA should conduct further studies of the scientific benefits as well as the technical benefits to mission execution, such as reduction of mission complexity and risk, enabled by the Constellation System for the following missions: Generation-X, Modern Universe Space Telescope, Stellar Imager, Interstellar Probe, Solar Polar Imager, Neptune Orbiter with Probes, and Titan Explorer.

The committee did not conduct detailed cost assessments of any of the Vision Missions. Instead, the committee used the cost estimates provided by the mission proposers (not adjusted for inflation), and supplemented by the experience of the committee members and comparisons with similar missions. In most cases, the proposers were basing their cost estimates on an EELV launch vehicle family and not on the Constellation launch vehicles or architecture, particularly Ares V.

Finding 4. There are uncertainties in the cost estimates associated with the Vision Missions listed above when flown on the Ares V vehicle.

Transitioning of payloads to a new, more capable vehicle would require reconsideration of several items: complexity of instrument packaging and deployment, need for auxiliary propulsion, and so on, all of which would affect mission costs. The committee was informed by NASA that no reliable cost estimates of the Ares V are available.

Recommendation 2. NASA should perform cost analysis for the missions that the committee determined could benefit from the Ares V capability (Generation-X, Modern Universe Space Telescope, Stellar Imager, Interstellar Probe, Solar Polar Imager, Neptune Orbiter with Probes, and Titan Explorer). This analysis should use the Ares V technical capabilities together with appropriate upper stages as a baseline.

---

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

[29] National Research Council, *An Astrobiology Strategy for the Exploration of Mars,* The National Academies Press, Washington, D.C., 2007.

The original NASA solicitation for the Vision Missions did not require respondents to address opportunities for international cooperation. In reviewing many of the individual Vision Missions, however, the committee noted parallel or complementary activities on the part of potential international partners. For example, the Advanced Compton Telescope, which does not require an Ares-V-class launch vehicle, does require a low-inclination orbit such as could be provided by the European Space Agency's (ESA's) Ariane V launch vehicle. Similarly, both the Interstellar Probe and the Solar Polar Imager correspond to missions that are under study by the ESA. It is beyond the scope of this interim report to make recommendations regarding international cooperation, but the committee nevertheless considers it important to point out that due to cost, many of these extremely large missions would probably require some degree of international cooperation or collaboration in order to be feasible. The cost, schedule, and performance implications of such international cooperation are not considered in this report.

Finding 5. International cooperation could provide access to international scientific expertise and technology useful for these missions, and could reduce costs through provision of foreign instruments and infrastructure.

During its review, the committee determined that a major constraint affecting the viability of nearly all of these missions as originally conceived was the lack of availability of certain technologies that currently exist only as concepts. In some cases the technologies, such as advanced sensors, are required for the spacecraft to accomplish its mission. However, the committee noted that several technologies were required for several mission concepts and were essentially "mission enabling." These include propulsion technologies that might allow an alternative to the use of a heavy-lift launch vehicle like the Ares V and are applicable to multiple missions (for example, aerocapture, which can be used at Venus, Mars, Titan, and Neptune, and solar sails, which can be used for the Solar Polar Imager and Interstellar Probe missions).[30]

The committee learned during its review that a substantial fraction of the mission concepts would benefit from human assembly and servicing, assuming that this capability is built into the Constellation System. Because of the limited nature of the information presented to the committee during its first meeting in preparation for this report, the committee chose to defer consideration of these technology issues and the potential impact of human servicing and to discuss them in its final report.

The committee also learned that some of the missions anticipate upgrades to the Deep Space Network (DSN). The committee expects to gather further data from NASA on the agency's plans for upgrading the DSN and will address these missions' interactions with and expectations for the DSN more fully in the final report. The overall technology requirements are summarized in Table 2.2.

Finding 6. The committee identified the following technology issues as meriting further attention. Some of these technologies are of a basic, mission-enabling nature; others provide options that can be traded for alternative mission architectures.

- **Basic enabling technologies**
  —Free-flying constellations
  —Tethered flight
  —Next-generation Deep Space Network
  —Space nuclear reactors

[30] The committee did note that the principal investigators of the outer planet missions and the Interstellar Probe mission assume the availability of nuclear (Pu-238) power supplies to be able to execute these missions. No new technology other than that already in development by NASA is considered necessary for these missions, although there is insufficient plutonium currently available to fly these missions. The committee understands that a separate NRC study will address the issue of further plutonium sources for future deep-space missions.

- **Technologies enabling alternatives to Ares V**
  —Aerocapture
  —Solar sails
  —Solar-electric propulsion
  —Nuclear-electric propulsion
  —Robotic assembly and servicing
- **Technologies enhancing Constellation capabilities**
  —Human assembly and servicing

TABLE 2.2 Correlation of Technology Needs with the Vision Missions Analyzed by the Committee

	Basic Enabling Technologies				Technologies Enabling Alternatives to Ares V				Technologies Enhancing Constellation Capabilities
Mission	Free-flying Constellations	Tethered Flight	Next Gen DSN	Space Nuclear Reactors, NEP	Aerocapture	Solar Sails	Solar/ Nuclear Electric Propulsion	Robotic Assembly and Servicing	Human Assembly and Servicing
Advanced Compton Telescope									
Generation-X								√	√
Interstellar Probe			√	√		√	√		
Kilometer-Baseline Far-Infrared/ Submillimeter Interferometer		√	√[a]						√
LUVO-MUST								√	√
Neptune Orbiter with Probes			√	√	√		√		
Palmer Quest Mars Mission			√[a]	√					
SAFIR									√
Solar Polar Imager			√			√	√		√[b]
Stellar Imager	√								
Titan Explorer			√[a]		√		√		

NOTE: Basic enabling technologies are those technologies that exist in concept at present but whose development is needed for some of the missions. Technologies enabling alternatives to Ares are those whose availability would provide options for the missions currently conceived as being executed without requiring an Ares V. Human assembly and servicing would enhance the capabilities of these missions. Some of the missions, like the Advanced Compton Telescope, would not directly benefit from these technology developments. DSN, Deep Space Network; LUVO, Large UV/Optical Telescope; MUST, Modern Universe Space Telescope; NEP, nuclear-electric propulsion; SAFIR, Single Aperture Far Infrared Observatory.
[a]Significant mission enhancement.
[b]Only if solar sail is implemented.

3
Plans for the Final Report

In March 2008 the committee issued a request for information to the scientific community seeking concepts for additional science missions that might utilize the Constellation System (see Appendix A). The due-date for these proposals is early May, and the committee intends to review the concepts and ask a select number of the proposers to present them at the committee's third meeting in June 2008.

The committee will analyze these concepts using the criteria discussed above in Chapter 1 and meld the results of the committee's analysis with the analyses presented in this report, arriving at a consolidated list of concepts recommended for future studies. The mission concepts expected to be submitted in response to the request for information will likely add to the number of technological issues the committee has already noted in Chapter 1. The committee expects to expand its discussion of technological issues and trade-off options in its final report.

The committee learned from the Vision Mission proposers that many of these missions assume upgrades to the Deep Space Network (DSN). The committee had insufficient time to address this issue in the interim report and intends to collect more data from NASA on the agency's plans for the DSN. The committee will address this issue in its final report.

Furthermore, because the issue of human servicing was mentioned by several presenters of the Vision Mission studies, the committee expects that more of the concepts it is asked to evaluate for the second part of its study will require human servicing or human activities. The committee will therefore devote more of its final report to the subject of human involvement in Constellation-enabled science missions.

The committee intends to hold two more meetings, one in June and the other in August, and to produce a final report by November 2008.

Appendixes

A
Request for Information

To: Members of the Space Science Community

From: George Paulikas, Chair and Kathryn Thornton, Vice Chair
NRC Committee on Science Opportunities Enabled By NASA's Constellation System

Date: March 6, 2008

The Space Studies Board and the Aeronautics and Space Engineering Board of the National Research Council (NRC) have begun a study of science opportunities enabled by NASA's Constellation System of launch vehicles and spacecraft. The Committee on Science Opportunities Enabled by NASA's Constellation System will first analyze a set of "Vision Mission" concepts provided by NASA. The results of this analysis will be included in an interim report to be completed by the end of April 2008. The mission concepts that the committee is analyzing for its interim report are listed on the committee's website:

http://www7.nationalacademies.org/ssb/constellation2008.html

In order to obtain the greatest possible input of ideas from the community about potential mission concepts addressing space science research, we are soliciting input from the broad community concerning ideas for missions or programs that are *uniquely enabled* by NASA's Constellation System. The capabilities of the Constellation System, some or all of which should be used in this input, are also available at the committee's website. These missions or programs can include (but are not limited to): Earth sciences, solar system exploration, heliophysics, astronomy and astrophysics.

<u>We invite you to write a concept paper for a new space-based mission or program, from existing or new vantage points, that promises to advance an existing or new scientific objective. Proposals that are selected by the NRC's Committee on Science Enabled by NASA's Constellation System will be asked to make a formal presentation at the committee's third meeting June 9-11 in Boulder, Colorado.</u>

The committee will analyze the following information for each mission concept:

1-Scientific objectives of the mission concept;
2-A description of the mission concept;
3-The relative technical feasibility of the mission concepts compared to each other;
4-The general cost category into which each mission concept is likely to fall;
5-Benefits of using the Constellation System's unique capabilities relative to alternative implementation approaches.

The committee will identify the mission concepts most deserving of future study. Identification of promising mission concepts by the committee does not imply future study funding by NASA.

The time horizon for the *launch* of possible missions should extend from 2020 to approximately 2035. These may include science missions benefitting from the unique capabilities of the

Constellation System, or from human spaceflight enabled by Constellation missions in lunar orbit, other orbits, or missions to planetary objects. In addition, constellations of spacecraft or spacecraft that fly in formation with existing, planned, or future spacecraft may also be considered.

The committee will use two criteria for evaluating the concepts:

1-Does the concept offer a significant advance in a scientific field ("significant" is defined as providing an order of magnitude or more improvement over existing or planned missions)?
2-Does the concept have a unique requirement for Constellation System capabilities, e.g.,
-Does use of the Constellation System's elements make a previously impossible mission technically feasible?
-Does use of the Constellation System's elements reduce mission risk or enhance mission success for a previously complicated mission?
-Does use of the Constellation System capabilities offer a significant cost reduction (i.e., 50% or more) in the cost of accomplishing the mission?

All responses will be considered non-proprietary public information for distribution with attribution. Those submitting responses must also fill out the relevant (i.e., government or non-government) NRC copyright form provided on the committee's website.

The concept papers should be no longer than ten pages in length and provide the following items (by numbered sections), if possible:[1]

1. A summary of the mission concept, including how it is uniquely enabled by the Constellation System.
2. A summary of the science goals, including a description of how the proposed mission will help advance science.
3. In addition to the two criteria listed above, other factors pertaining to the mission concepts may be used to evaluate and prioritize the candidate proposals:

a. Whether the mission has been identified as a high priority or requirement in previous studies, for example NRC reports;
b. How the mission contributes to important scientific questions facing space sciences today (scientific merit, discovery, exploration);
c. How the mission complements other space science systems;
d. NASA has asked the committee to analyze "the general cost category into which mission concept is likely to fall." We recognize the lack of accuracy of cost estimates for space missions in the early conceptual stages of development. You may consider using the NASA Advanced Missions Cost Model located at http://cost.jsc.nasa.gov/AMCM.html to determine approximate costs.
e. Technology development required by the proposed mission;
f. Risk mitigation provided by use of the Constellation System.

Please submit the concept papers to the NRC by May 5, 2008. Papers should be submitted to constellationrfi@nas.edu.

Questions about the RFI may be directed to the study director, Dwayne A. Day (dday@nas.edu), or to us: (George.A.Paulikas@aero.org); (kt4n@virginia.edu). You can also contact Dr. Day by telephone at 202-334-3477, or by fax at 202-334-3701.

[1] 10-page limit is a rough guideline, not an absolute limit, and refers to single-space text excluding references and front matter.

B
Committee and Staff Biographical Information

GEORGE A. PAULIKAS, *Chair,* has been at the forefront of advances in space science and space systems, and he has made many technical contributions to the development of national security space systems. He retired after 37 years at Aerospace Corporation, having joined Aerospace in 1961 as a member of the technical staff and later becoming department head, laboratory director, vice president, and senior vice president. He became executive vice president in 1992. His contributions to space science and the development of national security space systems have been recognized by Aerospace Corporation, U.S. Air Force, and the National Reconnaissance Office. Dr. Paulikas is a past vice-chair of the National Research Council (NRC) Space Studies Board. He has also served on a number of NRC study committees, including the Committee on the Scientific Context for Exploration of the Moon (chair), the Committee on an Assessment of Balance in NASA's Science Programs (vice-chair), the Committee on the Scientific Context for Space Exploration, the Committee on Systems Integration for Project Constellation, the Workshop Committee on Issues and Opportunities Regarding the Future of the U.S. Space Program, and the Committee to Review the NASA Earth Science Enterprise Strategic Plan.

KATHRYN C. THORNTON, *Vice Chair,* is a professor in the Department of Science, Technology and Society, and in Mechanical and Aerospace Engineering at the University of Virginia. She is also associate dean for graduate programs in the School of Engineering and Applied Science. Dr. Thornton has extensive human spaceflight experience and served for 12 years as a NASA astronaut, flying on four shuttle missions and performing extravehicular activities (i.e., spacewalks) on two of them. Dr. Thornton served on the NRC Aeronautics and Space Engineering Board, the Committee for Technological Literacy, and the Committee on Meeting the Workforce Needs for the National Vision for Space Exploration, and served as co-chair of the Stanford University/Planetary Society Workshop on Examining the Vision: Balancing Science and Exploration.

CLAUDIA ALEXANDER is the project manager and project scientist for NASA in the European Space Agency's Rosetta mission to study comet 67P/Churyumov-Gerasimenko. Her research focuses on the evolution and interior physics of comets, Jupiter and its moons, magnetospheres, plate tectonics, space plasma, the discontinuities and expansion of solar wind, and the planet Venus. Previously, she was a science representative on the Galileo mission to Jupiter. She is a member of the American Geophysical Union (AGU) and the Association for Women Geoscientists, and she was awarded the 2003 Emerald Honor for Women of Color in Research and Engineering by the Career Communications Group. Dr. Alexander has served on several NRC committees, including the Committee on Solar and Space Physics and the Committee on Distributed Small Arrays of Small Instruments for Research and Monitoring in Solar-Terrestrial Physics: A Workshop.

STEVEN V.W. BECKWITH is the vice president for research and graduate studies for the University of California System. He is a former professor of physics and astronomy at Johns Hopkins University and the former director of the Space Telescope Science Institute. Previously, he was managing director of the Max-Planck Institut fur Astronomie. His principal research interests are the formation and early evolution of planets, including those outside the solar system, and the birth of galaxies in the early universe. Dr. Beckwith served as chair of the NRC Panel on Ultraviolet, Optical, and Infrared Astronomy from Space.

MARK A. BROSMER is general manager of the Launch and Satellite Control Division at Aerospace Corporation, where he is responsible for Aerospace's support to the Air Force Satellite Control Network and Spacelift Range. He is responsible for launch operations at Cape Canaveral Air Force Station and the Vandenberg Air Force Base. He joined Aerospace Corporation in 1985 as a member of the technical staff in the Thermal Control Department of the Engineering and Technology Group. He transferred to the Fluid Mechanics Department in 1987. He has since held several positions, including manager of the Launch Vehicle Thermal Department, Engineering and Technology Group and project engineer for the system integration and launch readiness of the Titan IV Solid Rocket Motor Upgrade. He joined the Evolved Expendable Launch Vehicle (EELV) Program in 1996 as a senior project engineer, serving as Aerospace's IPT lead for systems engineering and integration for the Delta IV launch system. In 1998 he was promoted to systems director for Delta IV development, and in 2001 he was promoted to principal director of Delta IV. While supporting the EELV Program, he provided technical leadership from the early development phase and source selection process through the eight inaugural launches of the Delta IV, including the first operational launches of the medium-, intermediate-, and heavy-lift configurations.

JOSEPH BURNS is the Irving Porter Church Professor of Engineering, professor of astronomy, and vice provost for physical sciences and engineering at Cornell University. He is heavily involved with the imaging team on the Cassini mission around Saturn. Dr. Burns's current research concerns planetary rings and the small bodies of the solar system (dust, satellites, comets, and asteroids). He is the president of the International Astronomical Union's (IAU's) commission on celestial mechanics and dynamical astronomy. Dr. Burns is a fellow of the AGU and the AAAS, a member of the International Academy of Astronautics, and a foreign member of the Russian Academy of Sciences. In 1994 he received the DPS's Masursky Prize. Dr. Burns previously served as a member of the NRC Committee on a New Science Strategy for Solar System Exploration.

CYNTHIA CATTELL is a professor of physics in the School of Physics and Astronomy at the University of Minnesota. She is a fellow of the AGU. She is an author on more than 130 refereed journal articles and a co-author of the Auroral Plasma Physics book (ISSI). She is a co-investigator on Polar, Cluster, FAST, STEREO, and RBSP and a principal investigator on the AMPS mission study. She has been a member of various advisory committees, including the NRC Committee on Solar Terrestrial Research, the NRC Plasma Sciences Committee, the NASA Sun-Earth-Connection Advisory Subcommittee, and the SSSC Roadmap Committee. She was the chair of the 2003 NASA Plasma Sails Working Group and a member of the Advisory Committee to the UCLA Basic Plasma Science Facility. She has also served on the science definition teams for a number of missions, including the Mercury Dual Orbiter and the Grand Tour Cluster. She is a member of the Physics Force, a team performing large-scale "physics circus" shows for K-12 schools and the general public throughout Minnesota and the upper Midwest.

ALAN DELAMERE is a retired senior engineer and program manager at Ball Aerospace and Technology Corporation. He is currently involved as co-investigator on the Mars Reconnaissance Orbiter High Resolution Imaging Science Instrument and on the Deep Impact mission to Comet Tempel 1. Mr. Delamere has been involved in the Mars program since the 1980s. His expertise focuses on instrument building and mission design. He was a member of the NRC Committee on Preventing the Forward Contamination of Mars and the Committee on New Opportunities in Solar System Exploration.

MARGARET FINARELLI is a senior fellow in the Center for Aerospace Policy Research at George Mason University (GMU). Ms. Finarelli's earlier career with NASA and other U.S. government agencies focused on strategy development and negotiations in the fields of domestic space policy and international relations in science and technology. At NASA, she served as associate administrator for policy coordination and international relations. She was responsible for developing the international partnerships in the International Space Station program, and she led the U.S. team conducting the international negotiations that resulted in the agreements governing NASA's cooperation with Europe, Japan, and Canada. As the International Space University's vice president for North American Operations, she was responsible for strategic partnerships and business development in the United States for the Strasbourg, France-based international university.

TODD GARY is the director of the Institute for Understanding Biological Systems (IUBS) at Tennessee State University, where he leads research efforts in astrobiology. He is also the co-director of the Minority Institute Astrobiology Collaborative, the principal investigator on the NASA Astrobiology Institute Minority Institution Research Support (MIRS) program, a member of the NASA Astrobiology Institute Astrovirology focus group, and part of the NASA Goddard Space Flight Center education and public outreach team for the Mars Science Laboratory. He was the first candidate chosen for a faculty fellowship in astrobiology by the NASA Astrobiology Institute MIRS program and completed his fellowship in astrobiology at UCLA. Part of his work centers on the integration of research on viruses in extreme environments and extrasolar planet detection into educational settings. He is the principal investigator on several NSF and NASA programs developing national astrobiology research and education opportunities within Native American, African American, and Hispanic communities. He received his Ph.D. in molecular biology from Vanderbilt University, where he published one of the first studies on how viruses evolve at the DNA level, and he completed a 2-year research fellowship within the Center for Space Medicine at Vanderbilt University Medical Center.

STEVEN HOWELL is an associate astronomer and assistant director of the WIYN observatory at the National Optical Astronomy Observatory in Tucson. He has held previous jobs as a faculty member, NASA center scientist, and physics researcher. Dr. Howell has more than 20 years of teaching and research experience with research interests in interacting binaries, extrasolar planets, wide-field photometric surveys, and two-dimensional digital detector instrumentation. Dr. Howell's space mission experience includes work building and using astrophysics experiments flown on the space shuttle, service as a member of the Galileo spacecraft solid-state imager team, and participation as a guest observer on essentially every NASA astrophysics mission flown in the past 25 years. He was an advisor on the design and operation of the imagers flown on the Cassini and Deep Impact missions, and he chaired the science advisory committee for the NASA EUVE spacecraft. Dr. Howell regularly serves on NASA and NSF oversight and review panels, referees journal articles for numerous publications, and is currently a science team member on the NASA Kepler Discovery mission slated for launch in February 2009.

ARLO LANDOLT is the Ball Family Professor Emeritus of Physics and Astronomy in the Department of Physics and Astronomy at Louisiana State University and Agricultural and Mechanical College, where he has taught since 1962. He served as the director of the Louisiana State University Observatory from 1970 to 1988, and also served as a program director in the astronomy section of the National Science Foundation. He served as the secretary of the American Astronomical Society. Dr. Landolt has served on several NRC committees, including the U.S. National Committee for the International Astronomical Union; the U.S. Delegation to the 25th General Assembly of the International Astronomical Union; and as a delegate to the 23rd General Assembly of the International Astronomical Union in Kyoto, Japan.

FRANK MARTIN has worked for 4-D Systems since October 2002, with a major focus of his work on improving the performance of NASA teams. He is also the president of Martin Consulting, Inc. providing services to aerospace projects. His long career with NASA and with Lockheed Martin includes Apollo Science Operations; director, Astrophysics; Goddard's director for space and Earth science; deputy associate administrator, Space Station; associate administrator, Human Exploration, and director, Civil Space, for Lockheed Martin with responsibility for the Hubble Servicing Missions, Space Infrared Telescope Facility, Lunar Prospector, and Gravity Probe-B. He resigned from NASA in 1990 and retired from Lockheed Martin in 2001. He received the NASA Outstanding Leadership Medal; the Exceptional Service Medal; and the presidential ranks of Distinguished Executive and Meritorious Executive. He also served on the NRC Committee on Advanced Concepts.

SPENCER R. TITLEY is a professor in the Department of Geosciences at the University of Arizona. He previously worked on NASA's Lunar Orbiter program and was also a member of the Apollo Field Geology Investigation Team, serving on Apollo missions 16 and 17. His current research involves the study of the origin of mineral deposits and the distribution and location of mineral and mineral fuel resources. His research has also included the study of chemical baselines of trace elements in rocks and ores for environmental purposes. Dr. Titley is a member of the National Academy of Engineering. He previously served on the NRC Committee on the Assessment of Solar System Exploration.

CARL WUNSCH is the Cecil and Ida Green Professor of Physical Oceanography at the Massachusetts Institute of Technology. His research focuses on ocean observing technologies and on the general circulation of the ocean and its implications for climate change. Dr. Wunsch has chaired a number of ocean science advisory groups, such as the NRC Ocean Studies Board and the International Steering Group for the World Ocean Circulation Experiment. He is a member of the National Academy of Sciences, a foreign member of the Royal Society, and a recipient of the AGU's Macelwane Award and Bowie Medal, and the American Meteorological Society's Henry Stommel Medal.

Staff

DWAYNE A. DAY, *Study Director*, has a Ph.D. in political science from the George Washington University and has previously served as an investigator for the Columbia Accident Investigation Board. He was on the staff of the Congressional Budget Office and also worked for the Space Policy Institute at the George Washington University. He has held Guggenheim and Verville fellowships and is an associate editor of the German spaceflight magazine *Raumfahrt Concrete,* in addition to writing for such publications as *Novosti Kosmonavtiki* (Russia), *Spaceflight*, and *Space Chronicle* (United Kingdom). He has served as study director for several NRC reports, including *Space Radiation Hazards and the Vision for Space Exploration* (2006), *Grading NASA's Solar System Exploration Program: A Midterm Review* (2008), and *Opening New Frontiers in Space: Choices for the Next New Frontiers Announcement of Opportunity* (2008).

VICTORIA SWISHER joined the Space Studies Board in December 2006 as a research associate. She recently received a B.A. in astronomy from Swarthmore College. She has presented the results of her research at the 2005 and 2006 AAS meetings and at various Keck Northeast Astronomy Consortium (KNAC) undergraduate research conferences. Her most recent research focused on laboratory astrophysics and involved studying the x rays of plasma, culminating in a senior thesis entitled "Modeling UV and X-ray Spectra from the Swarthmore Spheromak Experiment."

CATHERINE A. GRUBER is an assistant editor with the Space Studies Board. She joined SSB as a senior program assistant in 1995. Ms. Gruber first came to the NRC in 1988. She was a research assistant (chemist) in the National Institute of Mental Health's Laboratory of Cell Biology for 2 years. She has a B.A. in natural science from St. Mary's College of Maryland.

RODNEY N. HOWARD joined the Space Studies Board as a senior project assistant in 2002. Before joining SSB, most of his vocational life was spent in the health profession—as a pharmacy technologist at Doctor's Hospital in Lanham, Maryland, and as an interim center administrator at the Concentra Medical Center in Jessup, Maryland. During that time, he participated in a number of Quality Circle Initiatives that were designed to improve relations between management and staff. Mr. Howard obtained his B.A. in communications from the University of Baltimore County in 1983. He plans to begin coursework next year for his master's degree in business administration.