

THE NATIONAL ACADEMIES

Advisers to the Nation on Science, Engineering, and Medicine

Space Studies Board
Board on Physics and Astronomy

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9/23/2004

Dr. Anne L. Kinney
Director, The Universe Division
Science Mission Directorate
NASA Headquarters
Washington, DC 20546-0001

Dear Dr. Kinney:

This letter report reviews the science goals of the current Terrestrial Planet Finder (TPF) project as well as NASA's plan for acquiring the necessary precursor knowledge to successfully meet those goals. This review by the Panel to Review the Science Requirements for the Terrestrial Planet Finder complements recommendations made in the National Research Council report *Astronomy and Astrophysics in the New Millennium* (referred to here as the 2000 decadal survey)¹ and was conducted in response to your request of January 29, 2004, asking for a science assessment of the TPF project. Your original letter of request was followed by one dated April 15, 2004, announcing NASA's intention to proceed with both coronagraphic and interferometric planet finder missions on an accelerated schedule. Both are included in this letter report's attachment. The Space Studies Board and the Board on Physics and Astronomy, in response to your requests, developed the following charge:

This panel will review NASA's current scientific objectives for the Terrestrial Planet Finder (TPF) mission and prepare a brief letter report conducting an independent scientific assessment as to whether these objectives remain consistent with the priority given to the mission by the Astronomy and Astrophysics Survey Committee.

In carrying out this charge, the panel will consider (1) the scientific goals of the mission as developed by the NASA TPF-Science Working Group; (2) plans for acquiring the necessary precursor scientific knowledge; and (3) the rationale for the mission that formed the basis of the priority assigned by the NRC's decadal survey report *Astronomy and Astrophysics in the New Millennium*.

This charge, to which NASA raised no objection, governed the scope of the current letter report; the panel emphasizes here that it was not constituted to carry out a technical assessment of the current TPF project plans and did not attempt to do so.

The panel met at the Keck Center of the National Academies in Washington, D.C., on May 18, 2004, to conduct the review (see the attachment for a panel roster and the meeting agenda). Drawing extensively on the current membership of the NRC's Committee on Astronomy and Astrophysics, the panel's membership covered a broad range of astronomical expertise. The panel also included members with specific expertise in coronagraphy and extrasolar planets.

The panel received presentations from Zlatan Tsvetanov (NASA) on the programmatic plans for TPF and from Charles Beichman (JPL) on the scientific and engineering plans for the project. Also participating

¹ National Research Council, 2001, *Astronomy and Astrophysics in the New Millennium*, National Academy Press, Washington, D.C.

in the discussion were Marc Kuchner (Princeton University), Alan Boss (Carnegie Institution of Washington), Dan Coulter (NASA), and Garth Illingworth (University of California, Santa Cruz).

The 2000 decadal survey report ranked the Terrestrial Planet Finder third in its list of major NASA missions behind the James Webb Space Telescope (then called the Next Generation Space Telescope) and the Constellation-X Observatory and sixth overall:

The main goal of TPF is nothing less than to search for evidence of life on terrestrial planets around nearby stars. The present concept calls for a space-based infrared interferometer of enormous sensitivity, capable of nulling out the light from the host star. TPF's angular resolution will also enable it to peer into the innermost regions of protoplanetary disks, galactic nuclei, starburst galaxies, and galaxies at high redshift. By a large margin, TPF is the most costly and the most technically challenging mission discussed in this report. Both SIM and NGST involve key technologies that must be demonstrated if TPF as currently envisioned is to go forward. The committee's recommendation of this mission is predicated on the assumptions that TPF will revolutionize major areas of both planetary and nonplanetary science, and that, prior to the start of TPF, ground- and space-based searches will confirm the expectation that terrestrial planets are common around solar-type stars. NASA should pursue a vigorous program of technology development to enable the construction of TPF to begin in this decade. (p. 39)

The original mission that was considered by the 2000 decadal survey ranked highly based on its potential science impact on terrestrial planet finding² and on the astrophysics reach afforded by the high angular resolution at infrared wavelengths. However, the widely recognized technical challenges of the interferometer prohibited the decadal survey committee from prioritizing it as a flight mission. Rather, that committee gave TPF its high ranking as a technology development activity with the aim of pushing the technology forward in this decade, and enabling the mission to be flown in the following decade. Specifically, "The committee attributes \$200 million [in FY2000 dollars] of the \$1,700 million total estimated cost of TPF to the current decade" (p. 37).

At the time of NASA's initial request in January 2004 for the current vision, the TPF project was considering both a free-flying infrared interferometer and an optical coronagraph, with the goal of downselecting to a single architecture in the near future. The course of the TPF project has since changed in order to take advantage of the new opportunities presented by NASA's new space exploration goals³ and to maximize the scientific potential for terrestrial planet finding. Specifically, the TPF project team is now proposing to fly TPF-C (an optical telescope with a coronagraph) followed by TPF-I (a free-flying infrared interferometer) within its planet-finding portfolio. The level-1 requirement for TPF-C's wavelength coverage is proposed to be 0.5 to 0.8 μm , with "stretch" goals of 0.5 to 1.05 μm . The level-1 requirement for TPF-I's wavelength coverage is proposed to be 6.5 to 13 μm , with "stretch" goals of 6.5 to 17 μm .

The primary scientific goal of the TPF mission (direct detection and spectroscopic analysis of Earth-like planets in orbit about some of the nearest main-sequence stars) arguably requires both TPF-C and TPF-I. This requirement was not well understood at the time the TPF mission was presented to the decadal survey committee, because understanding was imperfect then concerning the spectrum that our own Earth would present to a nearby solar system. Furthermore, the identification of biomarkers (i.e., spectroscopic features indicative of chemical balances attributable to biogenic activity) requires observations in spectra that span not only the optical but also the mid-infrared (IR) bands. Assuming there are planets to be

² Terrestrial planets are planets similar in size and composition to Earth. In our solar system Mercury, Venus, and Mars (as well as Earth) are considered to be terrestrial planets.

³ National Aeronautics and Space Administration, *The Vision for Space Exploration*, NP-2004-01-334-HQ, NASA, Washington, D.C., 2004.

found within the range of these telescopes, the combination of the two could provide evidence suggesting the presence of living organisms outside our solar system.

The TPF-C coronagraph is being designed to be able to identify planets that are Earth-sized or slightly smaller within the habitable zones of about 35 single, solar-type (F, G, K) stars within about 10 parsecs of the Sun. Liquid water is required for Earth-like life, and O₂ is under most circumstances a good indicator of photosynthetic life. Equipped with a modest-resolution spectrometer, the coronagraph should be able to identify several near-IR absorption bands of H₂O, along with the 0.76-μm “A” band of O₂ in light passing through the atmosphere of such planets. Hence, this mission has the capability by itself of at least suggesting whether life is present on planets around other stars.

Flying an IR interferometer, TPF-I, several years after the coronagraph is launched (potentially in a joint mission with the European Space Agency, as is under current discussion with NASA) could help to advance the science goals of the field in several ways. The currently envisioned free-flier concept for the interferometer would make long baselines possible and could enable this mission to extend the search for planets to more than 150 single, solar-type stars—a fourfold increase over the TPF-C sample. This larger search space would, of course, be critical if the frequency of Earth-like planets is low. Even if Earth-like planets are abundant, and the coronagraph is able to see many of them, the additional information obtained by the interferometer will likely prove crucial in characterizing these planets and determining whether any of them could harbor life. Thus the spectroscopic information provided by the interferometer is complementary to that provided by the coronagraph. For example, with sufficient cooling, the interferometer is expected to be able to observe the strong 15-μm band of CO₂. The presence of CO₂ is perhaps the best indicator that a planet being observed is terrestrial (i.e., rocky) and that it has an atmosphere, as opposed to being an airless body similar to Mercury or the Moon. CO₂ is also required for photosynthesis, both aerobic and anaerobic, and hence it is a requirement for many Earth-like forms of life. Even more importantly, the IR interferometer will have the capability to detect the 9.6-μm ozone band. Ozone, formed photochemically from oxygen, may be a more sensitive indicator of photosynthetic life than is oxygen itself. The TPF-C and TPF-I data together will provide simultaneous information for the same molecules in different states (e.g., the presence of oxygen from the A-band, as well as ozone at 9.6 μm or CO₂ at both the near and the thermal infrared), thereby removing some of the degeneracies in the interpretation of TPF-C or TPF-I data alone. The simultaneous detection of ozone (TPF-I) and methane (TPF-C)—oxidizing and reducing gases—implies that life may be present. What can be learned from the combination of TPF-C and TPF-I data is therefore far greater than what either mission alone would yield.

Since the 2000 decadal survey the TPF project has made progress in technology development and scientific definition of the mission. NASA reported that shortly after the survey, a number of detailed studies of TPF system architectures showed that an extremely precise coronagraphic imaging telescope could achieve some—though not all—of the science goals outlined originally for the interferometer approach. The TPF team believes that it will be ready to move TPF-C into Phase-A development by 2006, pursuant to an ambitious schedule to launch the coronagraphic imaging telescope by 2014. Because of its greater complexity, TPF-I is currently planned to follow about 6 years later. The technology development plans for both TPF-C and TPF-I are aggressive. However, a promising development is that the High Contrast Imaging Test Bed at the Jet Propulsion Laboratory has successfully imaged a region next to a simulated star within which an average contrast of 1.5×10^{-9} has been achieved. If this result proves applicable to the broader TPF-C mission, the mission’s development may meet the 2006 goal for entering Phase A.

Nevertheless, TPF-C would satisfy only part of the science requirements previously ascribed to the interferometric version of TPF that was ranked in the 2000 decadal survey. TPF had two goals of equal importance: planet finding and astrophysics. As emphasized in the 2000 survey: “To ensure a broad

science return from TPF, the committee recommends that, in planning the mission, comparable weight be given to the two broad science goals: studying planetary systems and studying the structure of astronomical sources at infrared wavelengths” (p. 12). The 2000 decadal survey’s companion volume of panel reports specified that TPF cover a range from 3 to 30 μm for general imaging, and 7 to 20 μm for planet finding, with an angular resolution of 7.5×10^{-4} arcsec at 3 μm .⁴ TPF-I is necessary not only to enhance the project’s planet-finding ability but also to complete the astrophysical goals laid out in the 2000 decadal survey.

A conceptual ancillary science case for TPF-C has been developed with the addition of a 5-arcminute wide-field camera. The science achieved with this camera would be synergistic with that made possible by the James Webb Space Telescope and a 30-m ground-based telescope. An example involves extremely deep observations, significantly more sensitive than the Hubble Space Telescope ultradeep field, of the annular region around the stars targeted for the planet search. TPF-C might have additional astrophysics reach, but the TPF project has not allowed broader astrophysics goals to drive the design or the cost of the TPF-C optical telescope assembly. Ancillary science for the TPF-I mission is not as clearly developed at this point, although ideas include extended spectral coverage for exoplanetary science or fine-resolution studies of high-red-shift galaxies and protostellar disks.

The 2000 decadal survey report was also very explicit about the importance of studies to be carried out prior to designing TPF. NASA’s plans for acquiring the necessary precursor science include (1) an assessment of the extent of exozodiacal dust in other planetary systems and the effects of this dust on the detectability of terrestrial planets, (2) a determination of the biomarkers that would be optimal indicators that life exists on such planets, and (3) an estimation of the minimum number of stars in the sample necessary to detect terrestrial planets with confidence.

Both the Spitzer Space Telescope, which was launched in 2003, and new ground-based IR interferometry (using the Keck interferometer or Very Large Telescope Interferometer telescopes) should address the extent of exozodiacal dust. The NASA TPF Working Group presented a reasonable case that the combination of visible (obtainable with a coronagraphic mission) and infrared (obtainable with an interferometric mission) biomarkers would be a far stronger discriminant of life than either set of wavelength-dependent biomarkers separately. Launching both TPF-C and TPF-I would provide this combination of evidence.

The greatest unknown remains the number of stars TPF-C needs to be able to observe, in order to assure that it will detect terrestrial planets.⁵ Achieving the primary scientific goal for the TPF mission is still hampered, perhaps crucially, by a lack of information about how common Earth-mass, let alone Earth-like, planets are around the sorts of F-, G-, and K-type stars identified for the TPF sample. Estimates of the probability that Earth-like planets exist vary widely. The fraction of these planets that will have even remotely Earth-like atmospheres is also unknown, but must strictly be less than 1 (i.e., over most of its lifespan Earth’s atmosphere has been chemically quite different from its composition today). The fraction of stars with planets bearing Earth-like atmospheres is a matter of conjecture at this point. This is precisely why the 2000 decadal survey report recommended that strong constraints on these fractions be established before the TPF conceptual design is finalized. As envisioned by the survey, the mission could be designed to accommodate whatever fraction nature provides, thus maximizing the chances of success.

⁴ See National Research Council, *The Sun to the Earth—and Beyond: Panel Reports*, Chapter 5, The National Academies Press, Washington, D.C., 2003.

⁵ Precursor studies will constrain the fraction of Sun-like stars that are orbited by Earth-like planets. This information determines the minimum volume of space that needs to be searchable by TPF-C in order to reasonably ensure that it will directly detect at least one Earth-like planet, thus driving the physical design of the telescope.

Thus, the precursor science is of paramount importance to the success of TPF. The target stars must be surveyed with any and all available resources before a detailed preliminary design is finalized. These searches can be undertaken with both ground- and space-based precursor missions, such as SIM and other lower-sensitivity projects (e.g., the planned “extreme” adaptive optics coronagraph for the Gemini Observatory). The Kepler mission may place strong constraints on the frequency of Earth-mass planets around Sun-like stars. Unfortunately, on NASA’s proposed schedule these missions may not produce results before the design of TPF-C is completed. The panel concludes that accelerating the schedule for TPF-C development carries considerable risk of settling on a design that the results obtained with SIM, Kepler, and microlensing and other observations will subsequently reveal to be incapable of seeing terrestrial planets. Therefore, the panel urges NASA to plan the development of TPF-C at a pace that allows the design to take into account the results of SIM, Kepler, and other observations as outlined above. A TPF flight mission could then be well positioned for a high ranking, possibly including both TPF-C and TPF-I, in the next decadal survey.

The 2000 decadal survey took into account, among other things, the broad programmatic implications of TPF. The proposed addition of TPF-C represents a major new mission of the Great Observatory class and is proposed for launch in 2014, 3 years after the James Webb Space Telescope. The panel agrees that TPF as envisioned in the 2000 decadal survey remains an exciting mission scientifically. The combination of TPF-C and TPF-I will cover at a minimum the planet-finding goals as laid out in that report.

Although NASA gave the panel no cost estimates, presenters did suggest a few bounds that lead toward a conclusion that the mission cost of TPF-C will be at least the cost of the current James Webb Space Telescope. According to NASA, the decision to fly both TPF-C and TPF-I was triggered by NASA’s new space exploration goals, in which planet finding received a very high priority. Neither the 2000 decadal survey nor any prior NRC reports had considered the added value for terrestrial planet finding of having an optical mission such as TPF-C as a complement to TPF-I.

The panel finds that the current scientific goals of the TPF project are consistent with those envisioned in the 2000 decadal survey, *Astronomy and Astrophysics in the New Millennium*. But the panel does not consider that this finding justifies advancing at this time the priority that can be accorded TPF as combined TPF-C and TPF-I missions. A decision after the fact to initiate a major project such as TPF-C implicitly reorders without due process the prioritized list developed by the 2000 decadal survey. Any such decision about prioritization should be made with the input of a broadly constituted committee that has sufficient time to weigh all of the scientific and technical issues.

In summary, the panel reaffirms that TPF, as envisioned in the 2000 decadal survey, remains an exciting mission scientifically. The panel concludes that, with the addition of TPF-C, there is considerable potential for interesting ancillary science in addition to the science connected with the search for life-bearing planets. The panel also concurs with the 2000 decadal survey on the importance of precursor missions (e.g., SIM and Kepler) toward enhancing TPF’s overall scientific productivity. It is critical that their results continue to drive the development of the project. The panel also concurs with the 2000 decadal survey’s recommendation that the astrophysics goals of TPF be weighted comparably to the planet-finding goals.

Yet although the proposed new camera for TPF-C possesses interesting capabilities, the associated science case has been neither carefully developed nor critically reviewed. The panel recommends that NASA solicit input from the astronomical community in order to develop the strongest possible science case. A strong science case would enhance TPF’s competitiveness in any priority-setting process, whether conducted in the context of the next decadal survey of astronomy and astrophysics or in an exercise of smaller scope conducted before the next survey.

Finally, the panel is concerned about the process by which NASA's decision to propose two TPF missions and to start one of them this decade was reached. The statement of task asked for input on the likely impacts on the 2000 decadal survey priorities, and these are large. The plan for TPF-C is clearly not consistent with the 2000 decadal survey's recommendations regarding TPF.

Even though NASA has rearranged the order of missions occasionally in the past when funding or technology concerns warranted such changes, TPF-C is so expensive and challenging that the panel believes that, from the perspective of astronomy and astrophysics,⁶ it must be placed in the broader context of the other highly ranked space missions identified in the 2000 decadal survey. The panel is very concerned about breaking with a process for developing a strategy that has served astronomy and astrophysics very well—the broadly debated, carefully balanced, and widely endorsed portfolio that the 2000 decadal survey presented. If implementation of TPF-C were to delay, or even preclude, other highly ranked astronomy and astrophysics missions, such an outcome would represent a substantial tipping of the portfolio's scientific balance. The panel urges NASA to consider the addition of TPF-C within the broader context of the entire astronomy and astrophysics program.

Sincerely,

/s/

Wendy L. Freedman, *Chair*
Panel to Review the Science Requirements for the Terrestrial Planet Finder

Attachment:

Letter of Request, January 29, 2004
Follow-up Letter, April 15, 2004
Panel Roster
Panel Meeting Agenda, May 18, 2004
Acknowledgment of Reviewers

cc: Alphonso V. Diaz, Associate Administrator, Science Mission Directorate
Lennard A. Fisk, Chair, Space Studies Board
Burton Richter, Chair, Board on Physics and Astronomy
Joseph K. Alexander, Director, Space Studies Board
Donald C. Shapero, Director, Board on Physics and Astronomy
Lia S. LaPiana, TPF Program Executive, NASA/SMD
Zlatan Tsvetanov, Project Scientist, TPF, NASA/SMD
Charles A. Beichman, Jet Propulsion Laboratory, NASA.

⁶ The panel acknowledges that the TPF mission is of interest to disciplines throughout the space sciences, and that the mission could conceivably be of higher priority to other disciplines.

Attachment

LETTER OF REQUEST, JANUARY 29, 2004

National Aeronautics and
Space Administration
Headquarters
Washington, DC 20546-0001



January 29, 2004

Reply to Attn of:

SZ

Dr. Wendy L. Freedman
Co-Chair, Committee of Astronomy and Astrophysics
Space Studies Board
National Research Council
National Academy of Sciences
2101 Constitution Avenue, NW
Washington, DC 20418

Wendy
Dear Dr. Freedman:

With this letter I am requesting that the Committee on Astronomy and Astrophysics (CAA) provide an updated assessment on the proposed scientific objectives and scope of the Terrestrial Planet Finder (TPF) mission.

The 2000 Decadal Review identified the TPF mission as one of the Major Initiatives recommended for development and recognized TPF as the most ambitious mission ever attempted by NASA. The Decadal Review also noted that significant advances in the state of our scientific knowledge and technological capabilities were necessary to accomplish TPF.

Since the 2000 Decadal Review, there has been a truly remarkable progress in many relevant areas. Today there are approximately 120 known extra-solar planets covering a wide range of masses and orbital periods. In the past 5 years, NASA has provided significant funding for the technological and scientific development of TPF, which has led to significant advances in key technologies of starlight rejection, as well as to a greater understanding of the challenges and promises of TPF.

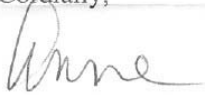
Most recently, the President's vision for NASA explicitly identified "advanced telescope searches for Earth-like planets and habitable environments around other stars" as one of the most important goals for the agency and space science. Currently, TPF is in pre-Phase A with a goal of entering Phase A stage later in this decade. It is in this context that we ask the CAA to assess the scientific goals of the mission as developed by the TPF-Science Working Group (SWG) and the TPF project. Stated simply, these goals include finding any terrestrial planets that might exist in the "habitable zones" of the closest 30~50 stars, characterizing the atmospheres and looking for life in any planets found in these planetary systems, conducting a program of comparative study of all constituents of planetary systems, and carrying out a program of ancillary astrophysics that would be achievable at little or no additional capital cost. In addition, we would like the CAA to

assess the project's proposed plans to acquire the precursor scientific knowledge needed for the successful design and operation of TPF.

Our office would require the results of this review by August 2004, as input to the Office of Space Science (OSS) strategic plan and the OSS decision of how to proceed with the TPF program.

If you have any programmatic questions regarding TPF you may contact Lia LaPiana at 202-358-0346 (Lia.S.LaPiana@nasa.gov), the TPF Program Executive. The Headquarters TPF Project Scientist is Dr. Zlatan Tsvetanov (202-358-0810, Zlatan.Tsvetanov@nasa.gov).

Cordially,



Anne L. Kinney, Director
Astronomy and Physics Division
Office of Space Science

cc:
SZ/L. Lapiana
SZ/Z. Tsvetanov

FOLLOW-UP LETTER, APRIL 15, 2004

National Aeronautics and
Space Administration
Headquarters
Washington, DC 20546-0001



April 15, 2004

Reply to Attn of: SZ

Dr. Wendy Freedman
Director of the Observatories
813 Santa Barbara Street
Pasadena, CA 91109


Dear Dr. Freedman:

As part of the President's new vision for NASA, the Agency has been directed by the President to "*conduct advanced telescope searches for Earth-like planets and habitable environments around other stars.*" This is leading to some exciting new developments for TPF. NASA Headquarters has been assessing alternatives for TPF, with the project, since the release of the new vision. The results are the following.

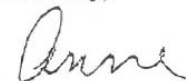
After substantial trade studies on four different architectures, two are deemed to be scientifically robust and technically feasible and so will be continued. These two architectures, an optical coronagraph and an infrared formation flying interferometer, offer complementary science return and are both worthy of pursuit. The coronagraph appears at this time to be more technologically mature and so will be pursued first. The formation flying interferometer will be pursued next, with an ongoing program of technology development that is needed to bring this capability into maturity. This ordering of the missions is subject to the readiness of critical technologies and the availability of funding.

We are now starting the process of discussing these changes with the community especially within the forums of the Origins Subcommittee, the Structure and Evolution of the Universe Committee, and the Committee on Astronomy and Astrophysics. Note also that, after some discussion among OS and SEUS, there was a strong recommendation that any Science Centers formed to support TPF be competed, and that is our intent.

The opportunity to move TPF forward as part of the new NASA vision called for rapid and dramatic actions. What has made these steps possible has been the hard work by the entire TPF team, including the TPF-SWG, the two "TPF architecture teams", and all the technologists at JPL, GSFC, and around the country, that has demonstrated that NASA is ready to proceed with both a coronagraphic TPF and an interferometric TPF.

We will be making more information available as soon as more details become available.

Cordially,



Dr. Anne Kinney
Director, Astronomy and Physics Division

PANEL ROSTER

Panel to Review the Science Requirements for the Terrestrial Planet Finder

WENDY L. FREEDMAN, Observatories of the Carnegie Institution, *Chair*

CHARLES ALCOCK, University of Pennsylvania

LARS BILDSTEN, University of California, Santa Barbara

ROGER D. BLANDFORD, Stanford University

JOHN E. CARLSTROM, University of Chicago

JAMES KASTING, Pennsylvania State University

BEN OPPENHEIMER, American Museum of Natural History

EVE OSTRIKER, University of Maryland

FRAZER N. OWEN, National Radio Astronomy Observatory

MARK J. REID, Harvard Smithsonian Center for Astrophysics

CHARLES E. WOODWARD, University of Minnesota

Staff

BRIAN DEWHURST, Study Director

CELESTE NAYLOR, Senior Program Assistant

PANEL MEETING AGENDA

MAY 18, 2004

Closed Session

8:15 am Convene
Discussion

Open Session

9:00 am TPF project Charles Beichman, JPL; Zlatan Tsvetanov, NASA;
Dan Coulter, NASA

10:15 am TPF science: Science Working Group and previous reports

11:00 am Break

11:15 am TPF discussion

Closed Session

1:15 pm Discussion and letter report writing

4:00 pm General discussion

5:00 pm Adjourn

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 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 review of this report:

Arthur D. Code, University of Arizona,
Andrea M. Ghez, University of California, Los Angeles,
Andrew Gould, Ohio State University,
John P. Huchra, Harvard-Smithsonian Center for Astrophysics,
Geoffrey W. Marcy, University of California, Berkeley,
Christopher F. McKee, University of California, Berkeley, and
Marcia J. Rieke, University of Arizona.

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 Richard M. Goody, Harvard University. Appointed by the National Research Council, he was responsible for making certain that an independent examination of this report was carried out in accordance with institutional procedures and that all review comments were carefully considered. Responsibility for the final content of this report rests entirely with the authoring committee and the institution.