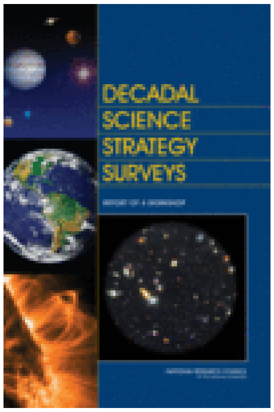

Decadal Science Strategy Surveys: Report of a Workshop

Jack D. Fellows, Rapporteur, Joseph K. Alexander, Editor, National Research Council

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DECADAL SCIENCE STRATEGY SURVEYS

REPORT OF A WORKSHOP

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

Space Studies Board

Division on Engineering and Physical Sciences

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Preface

National Research Council (NRC) decadal science strategy surveys provide decade-long retrospective and forward-looking assessments of the status of and outlook for a research field, and they provide broadly based recommendations for explicit scientific and programmatic priorities for future investments in the field. While these surveys have been widely successful, implementation problems have been encountered as the relevant government agencies and the scientific community try to follow the advice contained in them. These problems, which have been due to a combination of fiscal, technical, programmatic, and policy factors, suggest that the approach to future decadal surveys needs to be carefully examined and possibly modified or improved.

Consequently, the Space Studies Board concluded that it would be timely and important to bring members of the research community and representatives of relevant government offices together to discuss the use of NRC decadal surveys for developing and implementing scientific priorities, to review and discuss lessons learned from the most recent surveys, and to identify approaches that could enhance the realism, utility, and endurance of future surveys.

A public workshop was conducted on November 14-16, 2006, at the National Academies' Beckman Center in Irvine, California, to address these issues. A workshop planning committee, whose members were drawn from the membership of the Space Studies Board, developed an agenda for the workshop and selected and invited workshop participants. The workshop featured presentations, panel discussions, and general discussions on the use of NRC decadal surveys for developing and implementing scientific priorities in astronomy and astrophysics, planetary science, solar and space physics, and Earth science. The workshop agenda is presented in Appendix A, and short biographies of the approximately 60 participants from academia, industry, government, and the NRC are presented in Appendix B.

This workshop report, prepared by the workshop rapporteur, Jack D. Fellows, with the assistance of NRC staff member Joseph Alexander, is intended to be a summary of what occurred at the workshop. The Space Studies Board intends for the report to stimulate discussion about future decadal surveys all across the scientific community, the relevant federal agencies, and Congress and to be useful to the NRC as the next rounds of decadal surveys are planned and organized.

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 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:

Steven Battel, Battel Engineering,
Wesley Huntress, Carnegie Institute of Washington,
Joseph Taylor, Princeton University, and
Megan Urry, Yale University.

Although the reviewers listed above have provided many constructive comments and suggestions, they were not asked to endorse the statements presented in the report nor did they see the final draft of the report before its release. The review of this report was overseen by William W. Hoover, independent consultant (U.S. Air Force, retired). 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 authors and the institution.

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Summary

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

WORKSHOP BACKGROUND

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

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

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

LEVERAGING PAST SUCCESSES AND IMPROVING FUTURE SURVEYS

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

Cost and Technical Risk

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

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

Resiliency and Execution

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

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

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

Planning, Management, and Collaboration

Discussions about the timing of decadal surveys touched on both the time required to complete a survey and the time span over which a survey should look.

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

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

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

The workshop also stimulated discussion about several aspects of internal and external coordination. First, some participants acknowledged that while interagency and international cooperative programs have a chance of promoting cooperation across organizational boundaries, they tend to be a great challenge and rarely result in substantial cost savings. Therefore, agencies need to give extra attention to integrating such cooperative efforts as effectively as possible.

Second, participants noted that as long as human exploration of space is a major national space goal, future surveys should not altogether ignore such exploration. However, science surveys should stick with the principle of "science first" while integrating research that can be enabled by human spaceflight into overall science priorities to be recommended in a survey report.

1

Introduction

In the 1960s, the National Research Council (NRC) began carrying out broad disciplinary surveys of the status and long-term scientific outlook of two fields, physics and astronomy. These surveys, which were updated roughly every 10 years, became known as "decadal surveys."[1] Over the years, the astronomy surveys were particularly useful for two reasons. First, they developed clear priorities for major undertakings in space and on the ground. Second, they arrived at those priorities through a process that involved consultations with a significant cross section of the research community and that led to a broad consensus. They have come to serve as a model for other NRC disciplinary assessments.

In recent years, other disciplines, recognizing the value of the survey process, have undertaken assessments of their own fields, among them space science and Earth science (from the point of view of space). In 2002 the Space Studies Board oversaw the completion of two new reports that expanded the creation of decadal-scale consensus strategies into the research fields of solar system exploration and of solar and space physics.[2] The Space Studies Board initiated a new decadal survey study for Earth science and applications from space in 2003, and the report from that study was expected to be complete in January 2007.[3] Currently in prog-

[1]The most recent NRC astronomy and astrophysics survey report, *Astronomy and Astrophysics in the New Millennium* (National Academy Press, Washington, D.C., 2001), was completed in 2000.

[2]NRC, *The Sun to the Earth—and Beyond: A Decadal Research Strategy in Solar and Space Physics*, The National Academies Press, Washington, D.C., 2003, and NRC, *Solar and Space Physics and Its Role in Space Exploration*, The National Academies Press, Washington, D.C., 2004.

[3]See NRC, *Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond*, uncorrected prepublication copy, The National Academies Press, Washington, D.C., 2007.

ress under the Board on Physics and Astronomy, *Physics 2010*[4] has introduced some new elements, including the involvement of scientists from other disciplines and policy makers with broad experience communicating science to the public and the Washington, D.C., federal government audience. The first volume of the series, covering elementary particle physics and relevant aspects of astrophysics, was recently released. The series will be completed in 2010.[5]

The most effective surveys succeeded in attaining several goals: (1) providing an authoritative assessment of the accomplishments of the field; (2) motivating a compelling scientific research program for the future and identifying areas that show the most promise for further progress; (3) presenting explicit priorities based on a consensus of the research community on the most important, potentially revolutionary science that should be undertaken within the span of a decade; (4) developing priorities for future investments in research facilities, space missions, and/or supporting programs; (5) ranking competing opportunities and ideas and clearly indicating which ones are of higher priority in terms of timing, risk, and cost of implementation; and (6) making the difficult adverse decisions about other meritorious ideas that cannot be accommodated with realistically available resources.

All decadal surveys to date have been well received and influential with government decision makers, both in the executive branch—e.g., at the Office of Management and Budget, Office of Science and Technology Policy, National Aeronautics and Space Administration, National Science Foundation, Department of Commerce National Oceanic and Atmospheric Administration, Department of Defense, and U.S. Geological Survey—and with Congress (in the relevant authorization and appropriation committees).[6] As the NRC organizes new decadal surveys, beginning with an astronomy and astrophysics survey in 2007, it would be valuable to pause and consolidate the lessons learned from the most recent round of surveys. Government expectations remain high that the new surveys will be as good as or better than the older surveys for setting future priorities for federal investments. However, implementation problems have been encountered as the agencies and the scientific community try to follow the advice from the survey reports. These problems, which have been due to a combination of fiscal, technical, programmatic, and policy factors, suggest that it will be worthwhile to look for opportunities to continue the decades-long improvements to the NRC's model for the survey process.

One problem is related to cost and cost realism. Each survey report develops priorities for new space missions or ground-based facilities, which the survey

[4]See http://www7.nationalacademies.org/bpa/projects_physics_2010.html.

[5]NRC recently completed a decadal survey for aeronautics research (NRC, *Decadal Survey of Civil Aeronautics: Foundation for the Future*, The National Academies Press, Washington, D.C., 2006), but that study was not discussed at the workshop.

[6]On more than one occasion during the workshop, government representatives referred to the decadal surveys as the gold standard for scientific advice to the government.

committees consider in terms of their expected development costs. However, many of the highly recommended missions or facilities subsequently experienced substantial cost growth that has threatened the viability of the overall science program strategy being recommended in the survey report. Ways must be found for future decadal survey committees to consider cost and assess cost realism, technological readiness, and technology risk as they recommend new missions, facilities, and program strategies.

The second problem relates to how future decadal surveys will make priority lists most useful for decision makers and program officials. Some argue for a survey report that presents a single integrated priority list; others assert that a set of parallel priorities for different elements of a program is both more realistic to expect from an NRC committee and also more supportive of the programmatic flexibility that agency officials need. Furthermore, questions have been raised about how the agencies handle prioritized lists of recommended mission or facility investments compared with how they handle other core mission-enabling elements of an agency program (e.g., research grants and infrastructure). Thus, there is good reason to weigh the approaches that recent surveys have used so that future surveys treat these aspects as effectively as possible.

A third problem is how to keep surveys useful over time, as the budgetary, political, and programmatic landscape changes. Unforeseen changes can, and often do, occur as a consequence of new scientific discoveries and technological advances, problems with large development projects, new budgetary opportunities or constraints, and/or changes in agency missions or priorities in response to national policy direction. To provide useful, long-range advice about scientific and programmatic priorities, surveys need to be as resilient as possible in the face of such change.

The chapters that follow are organized according to the flow of the workshop agenda (see Appendix A). Chapter 2 summarizes the opening session, where five survey reports were discussed. Taking part as panelists were chairs of the NRC study committees and representatives from key federal agencies and Congress. Chapter 3 focuses on workshop discussions of a particularly challenging aspect of the surveys—namely, the treatment of cost estimates and the determination of a technology's readiness to be applied in a space mission or a research facility recommended by the survey report. Chapter 4 summarizes the discussions in the final two sessions about the kinds of assumptions that underlie decadal surveys and lessons to be considered as future surveys are organized. Chapters 2 and 3 conclude with a set of key themes or perspectives that appeared to emerge as highlights of the first two sessions and that were further developed in the last two sessions. These highlights are not intended to represent a consensus of the workshop participants, but they do reflect viewpoints that were mentioned repeatedly and that appeared to draw broad agreement among the participants in the discussions.

2

Review of Recent Decadal Surveys

The opening afternoon, Session 1, of the workshop featured three panels moderated by Space Studies Board (SSB) chair Lennard Fisk. Panelists and others discussed five recent National Research Council (NRC) studies from three perspectives: those of the science community, federal agencies, and the Congress. In the Survey Committee Panel, the chairs of the committees that wrote *Astronomy and Astrophysics in the New Millennium; Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century; Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond; New Frontiers in the Solar System: An Integrated Exploration Strategy;* and *The Sun to the Earth—and Beyond: A Decadal Research Strategy in Solar and Space Physics* summarized their studies from the perspective of approaches employed, impacts, problems, and recommendations for future surveys. An agency panel and a congressional panel provided opportunities for speakers from the National Aeronautics and Space Administration (NASA), the National Oceanic and Atmospheric Administration (NOAA), the National Science Foundation (NSF), the Office of Science and Technology Policy (OSTP), and Congress to address the utility, impacts, limitations, and problems of past surveys from the perspectives of the key government audiences they represented.

VIEWS OF PAST COMMITTEE CHAIRS

The first member of the Survey Committee Panel, Christopher McKee, of the University of California at Berkeley, provided an overview of the most recent decadal survey for astronomy and astrophysics.[1] As co-chair of the survey committee that wrote the report, he said that the 15 members had been chosen to maximize disciplinary expertise and to reflect institutional perspectives and noted that the survey had drawn on the work of nine expert panels and four cross-panel working groups that involved an additional 96 participants. To obtain community-wide input, the survey committee held two town-hall sessions at American Astronomical Society (AAS) meetings plus more than 20 meetings at individual institutions, hosted a Web site soliciting input, and devoted a significant portion of one committee meeting to gathering views from international astronomers.

Dr. McKee said that one notable aspect of the study, which was identified as an important aspect in later workshop discussions, was that although it had had interest and funding support from two of the three major federal sponsors for astronomy in the United States—NASA and NSF—the Department of Energy (DOE) did not participate. He said that the impact of the survey report on federal actions was mixed. At NSF, three major recommended telescope facilities were all making good design progress, and NSF had acted positively on some of the survey's key policy recommendations. On the other hand, the prospects for funding construction of recommended facilities at the NSF remained uncertain. Similarly, some highly recommended NASA space missions were making excellent progress, but a larger number of other highly ranked initiatives were on hold. For a complete assessment of the impacts of the deferrals of recommended missions, see *A Performance Assessment of NASA's Astrophysics Program*.[2]

Dr. McKee indicated that unforeseen developments, including postsurvey budget constraints, cost growth in recommended missions, and the Columbia disaster, which delayed the refurbishment of the Hubble Space Telescope by several years and incurred many hundreds of millions of dollars in additional costs, impaired NASA's ability to act on the survey report recommendations.

In looking back at the astronomy and astrophysics survey, Dr. McKee saw three important lessons. First, he suggested that future surveys should be broadened even more than in the past. For example, the interfaces between fundamental physics and cosmology (see discussion of *Connecting Quarks with the Cosmos*, below) should be integrated into the survey more fully. He also recommended expanded outreach efforts to include interaction with the American Physical Society as well as the AAS and even more town meetings. Second, Dr. McKee called for improved cost estimates for high-priority initiatives if at all possible, along with a uniform approach to estimating costs for all mission and facility candidates. He noted that current estimates for some major projects had reached 1.5 to

[1]NRC, *Astronomy and Astrophysics in the New Millennium*, National Academy Press, Washington, D.C., 2001.

[2]NRC, *A Performance Assessment of NASA's Astrophysics Program*, The National Academies Press, Washington, D.C., 2007.

3 times the survey estimates.[3] Third, he emphasized that survey committees need to consider full life-cycle costs, including costs for both facility development and operations, so that useful comparisons could be made between candidates for new initiatives and initiatives recommended in prior surveys.

Dr. McKee concluded by offering the following recommendations for future assessments of astronomy and astrophysics projects:

- Get broad expertise on the survey committee.
- Continue to make the hard decisions in setting priorities.
- Do better on highlighting the science—*Astronomy and Astrophysics for the New Millennium* was science-based.
- Secure DOE support and add panels related to DOE astrophysics.
- Reprioritize previously recommended projects that have not started.
- Study operating costs of ground-based facilities, which are very long-lived. Operating costs far exceed construction costs over the operational lifetime of a facility.
- Consider prioritizing the Explorer program, provided that launch vehicles are available.
- Be prepared to follow up for a decade. Rearrange the membership of the NRC Committee on Astronomy and Astrophysics to include a significant fraction of the survey committee. Doing so will allow changing priorities if costs increase greatly, but only as a last resort since this could undermine the process.

Michael Belton, of Belton Space Exploration Initiatives, LLC, astronomer emeritus at the National Optical Astronomy Observatories and chair of the steering committee for the solar system exploration survey, discussed experiences for that study. The survey report[4] was the product of five expert panels, which prepared the scientific material, and a 15-person steering committee, which drew on the panel inputs to create a set of integrated priorities. In addition to addressing NASA explicitly, the survey indirectly had recommendations for NSF and DOE. Community input and involvement had been ensured by means of multiple town-hall-style meetings that involved committee and panel members, contacts with relevant professional societies, and a collection of white papers solicited at the initiative of the AAS Division of Planetary Sciences.

In discussing the impacts of the survey on solar system exploration, Dr. Belton noted that members of the community seemed to be very satisfied with the outcome and more unified as a community. He added that both NASA and the Office

[3]Dr. McKee indicated that the cost of the ground-based Giant Segmented Mirror Telescope exceeded the survey committee's estimate by a factor of 1.6, and the cost of the James Webb Space Telescope was roughly three times as much as the figure provided by NASA and used by the survey committee.

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

of Management and Budget (OMB) seemed satisfied with the results, which provided a scientific basis for a number of programmatic and budgetary decisions. One exception to NASA's positive reaction was the report's recommendation that NASA contribute to NSF's support of a major ground-based telescope, the Large Synoptic Survey Telescope.

Dr. Belton indicated that the survey had faced a number of problems, including the short time available to complete the study; uncertainty surrounding the costs of the candidate missions and their readiness to be implemented; rapid, ongoing changes in NASA's senior staff, organization, and goals; and the difficulty of coping with new political initiatives that had uncertain lifetimes (e.g., the introduction of lunar exploration objectives into the 2004 Vision for Space Exploration). However, he noted that the most damaging problem for the survey had been the tendency for the community, and especially NASA's follow-up advisory committees, to diverge from the recommended survey goals and to recreate strategies in response to new scientific discoveries and inevitable new start problems, budgetary crises, and new political initiatives.

Dr. Belton offered the following recommendations for future surveys:

- Start survey preparations early, including early identification of the steering committee and preliminary mission cost assessment work at NASA laboratories. At least 2 years should be allocated to conducting a survey.
- Assure adequate funds for the survey itself and also provide support for peripheral technical and mission studies thought necessary by the steering committee for public outreach and for advocacy.
- Because the political climate does matter, finalize the strategy of a survey in the first year of a new or incumbent administration. This suggests that decadal surveys should take place every 8 or 12 years to match the political cycles.
- Initiate a one-solar-system exploration survey that includes the science of Mars and the Moon with the rest of solar system science. This will ensure that resources applied to solar system exploration are used most effectively.
- Establish a formal follow-up connection between NASA (and possibly other agencies) and the NRC to ensure that proper focus is maintained on contemporary survey goals in light of new discoveries and personnel, budgetary, and political changes. Perhaps NRC might consider utilizing the survey steering committee on an ad hoc basis throughout the decade following a survey instead of disbanding it. Given the substantial community input to surveys, agencies should not reinvent or redefine the survey.
- Create explicit interfaces to the international community, the human spaceflight program, and the extrasolar-planet community. This is not difficult in principle but will require time and money.

Louis J. Lanzerotti, chair of the Solar and Space Physics Decadal Survey Committee and research professor at the New Jersey Institute of Technology, discussed his committee's study,[5] which was organized under the auspices of the Committee on Solar and Space Physics (CSSP) of the SSB and which had sponsorship from NASA, NOAA, NSF, the Air Force Office of Scientific Research, and the Office of Naval Research. Like its counterpart surveys summarized above, Dr. Lanzerotti's survey committee held numerous town meetings and then integrated input from six expert panels to prepare overall recommendations for program goals and priorities. It differed in one important way, however, in that its charge included addressing relevant operational programs of NOAA, the Department of Defense (DOD), and DOE, as well as the basic scientific research programs of NASA and NSF. To facilitate attention to this aspect of the survey, the committee had held a special transition-to-operations workshop on space weather. In prioritizing new initiatives, the committee applied criteria that included scientific merit, contribution to national goals, technological readiness, adequacy of the theoretical foundation, temporal synergies with other missions or facilities, and cost.

Dr. Lanzerotti summarized lessons learned from *The Sun to the Earth—and Beyond* as follows:

- A strong chair of the survey's oversight committee (in this case, the CSSP) is essential.
- NRC staff involvement and knowledge are essential.
- Because the importance of applications in this field was recognized from the outset, relevant agencies and outside communities (e.g., industry) were invited to participate.
- Panel reports were subjected to NRC standard review procedures to provide extra credibility.
- Continued follow-up by the survey's oversight committee after survey delivery is critical; otherwise participants who are involved in the survey do not know how or if their recommendations are being acted on.
- Without follow-up, it will not be known for how long an individual agency will pay attention to the survey.
- Early buy-in by stakeholder agencies and their continued involvement are critical if the survey is to provide the most credible advice to the nation.
- Perhaps the greatest weakness is credible cost estimation, even when it is recognized as a weakness and addressed from the outset.
- One legacy program from *Astronomy and Astrophysics in the New Millennium* was treated, incorrectly as it turns out, by assuming it would be carried out.
- The next survey of solar and space physics will probably need to place even more emphasis on addressing applications.

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

Berrien Moore of the University of New Hampshire discussed another survey that has a strong applications element as well as fundamental science aspects—*Earth Science and Applications from Space*. The survey committee, for which Dr. Moore served as co-chair, completed its interim report in April 2005,[6] but the final report was not ready in time for the workshop.[7] The survey, which had sponsorship from NASA, NOAA, and the U.S. Geological Survey, was organized around seven thematic panels and an executive committee made up of 11 at-large members plus the chairs of the seven panels.

Dr. Moore noted that an interim report had not been part of the original study plan but was added when it was seen to be critical, because, to quote the report (p. 2), "Today, this system of environmental satellites is at risk of collapse." He added that since the interim report, the situation confronting the survey committee had deteriorated further owing to the major problems of cost, schedule, and scope affecting the DOD/NOAA/NASA National Polar Orbiting Environmental Satellite System and the termination or delay of still other satellite programs at NASA. Other problems facing the survey committee had included the overly broad and open-ended charge, which was to review the status of the field; overlapping or ambiguous agency roles; and obtaining and verifying mission cost estimates. Dr. Moore also noted that the way the expert panels had been set up posed a challenge for the survey. By using a thematic approach (e.g., there were panels on weather, climate, and human health and security) instead of a more traditional disciplinary approach (e.g., atmospheric science, oceanography, and glaciology), the study risked appearing to disenfranchise some disciplines and favor others even if that had not been the intent of the organizers.

Even though the survey on Earth science and applications from space was not yet final, Dr. Moore drew on the experience of his survey committee and panels to offer a slate of lessons learned:

- "Building trust" in the community was a nontrivial task; this may not be an issue in the future, but it taught the survey committee a painful lesson.
- "Reviewing the status of the field(s)" is too open-ended an assignment; that aspect of the charge needs to be more carefully framed for Earth science.
- Agency roles need to be handled better, but how this can be done is problematic.
- Cost estimates need to come from outside the survey committee. NASA and the Aerospace Corporation are candidates for doing this, but whether they can be really independent is uncertain.

[6] NRC, *Earth Science and Applications from Space: Urgent Needs and Opportunities to Serve the Nation*, The National Academies Press, Washington, D.C., 2005.

[7] The survey committee's final report, *Earth Science and Applications from Space: National Imperatives for the Next Decade and Beyond*, was released in prepublication form on January 15, 2007.

- Given that an interim report was added to the list of tasks, the schedule was unrealistic.

The fifth and final speaker on behalf of recent survey studies was Michael S. Turner, chair of the Committee on the Physics of the Universe, professor at the University of Chicago, and former NSF assistant director for mathematical and physical sciences. The committee prepared an interim report in January 2001 and a full report, *Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century,*[8] in April 2002. Neither was strictly a decadal survey, but they had similar impacts. Dr. Turner explained that the study dealt with science at the boundaries between physics and astronomy and therefore cut across both disciplines and across multiple agencies (DOE, NASA, and NSF). For that reason it was a study about exciting fundamental science (dark matter, solar neutrinos, etc.) that could all too easily fall through the cracks. Dr. Turner said that a key aspect of the study was that it framed the key science questions first and then formulated programmatic recommendations that were always tied to the science.

Unlike the other classical decadal surveys, *Connecting Quarks with the Cosmos* did not produce a strictly ordered list of priorities; rather, it presented a strategic package of projects to achieve the science goals. The study resulted in a federal plan of action put together by OSTP.[9] Dr. Turner indicated that many of the report's recommendations were receiving positive treatment and making progress, and that all of the recommendations were at least in the planning stages at all three agencies. Unfortunately, however, constrained science budgets have limited progress in implementing the recommendations of this study as well as all the other studies. He noted that the committee's science-first approach had helped create an atmosphere of excitement and that the effort to foster interdisciplinary and interagency cooperation was bearing fruit. While *Connecting Quarks with the Cosmos* served to reinforce the astronomy and astrophysics decadal survey, it did add a bit of confusion because it created, in effect, a separate set of priorities. Nevertheless, because the recommendations did not contradict or change the astronomy and astrophysics survey priorities and because it added a new dimension of scientific excitement, the net effect of the survey was positive.

Dr. Turner concluded with three lessons learned:

- The science-first format is well suited for discovery science.
  —It captures the excitement of the enterprise,
  —It is the metric by which OSTP and OMB are now judging the discovery science fields,

[8]NRC, *Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century*, The National Academies Press, Washington, D.C., 2003.

[9]Office of Science and Technology Policy, *The Physics of the Universe: A Strategic Plan for Federal Research at the Intersection of Physics and Cosmology*, OSTP, Washington, D.C., 2004.

—It provides a reference point for planning, measuring progress, and responding to a changing fiscal climate, and
—It is an effective mechanism for bridging disciplines and agencies.

• Interdisciplinary and interagency approaches characterize today's science, and since they are not going away, they must be accommodated.
• Texture and a small number of recommendations are as important as ordered lists in a report.

VIEWS FROM THE FEDERAL AGENCIES

The Panel on Agencies provided an opportunity for representatives from four executive branch agencies to comment on how their agencies had received recent survey reports. Mary Cleave, NASA's associate administrator for science, opened the discussion. She began by noting that the surveys had played a number of roles: They had facilitated NASA–community communications and consensus development, securing broad expertise and independence, authoritatively documenting recommendations, and maintaining unity among the scientific community and other stakeholders. She cited NASA impressions about particular strengths and weaknesses of the four recent reports that had been discussed by Drs. McKee, Belton, Lanzerotti, and Moore and also shared some NASA recommendations for the future. Among the strengths of the surveys were the inclusion of explicit, clear priorities (or, in the case of the Earth science and applications interim report, issues rather than priorities) and the comprehensive coverage provided by the studies. Weaknesses included the surveys' tendency to gloss over where to rank initiatives recommended in earlier surveys, the cost implications of (some) recommended missions, and relative priorities for small missions (e.g., Explorers) and the core research programs.

In looking toward the future, Dr. Cleave offered the following recommendations for the next decadal surveys:

• Focus on science questions and objectives, not specific missions. If missions are identified, NASA and the NRC should work together on approaches to find mission cost boundaries that fit within the budget.
• Consider how objectives fit together to answer science questions.
• Consider how to know when an objective has been met.
• Provide clearer definitions of mission classes and of which missions fit within those classes.

In discussing the NASA presentation, workshop participants called attention to the fundamental conflict between having the surveys provide prioritized sets of key scientific objectives (which participants seemed to agree was important) and being able to estimate the costs of pursuing those objectives. As one participant put it, "How does one [put a] price [on] an objective?" The challenge of translating a science-based survey into useful and actionable recommendations that are credible and realistic from the standpoint of a budget became one of the recurring themes of the workshop.

Judith Sunley, the NSF executive officer for mathematical and physical sciences, provided comments from the perspective of the NSF. She said that the decadal surveys, especially their treatment of priorities, had been very valuable for the NSF. They help the NSF articulate the scientific arguments and gain community support for the programs and projects it is considering and help make trade-off choices between small and large programs. She noted her agreement with the science-first approach to the surveys, which had been mentioned earlier by Dr. Turner and others. In looking forward to future decadal surveys Dr. Sunley offered the following recommendations:

- The costing for proposed initiatives needs to be done in a reasonably consistent way so that the costs of current and recommended future programs or facilities can be compared.
- Cost estimates need to include full life-cycle costs, including those of predevelopment definition and design, construction, operations, and decommissioning.
- In addition to recommending construction of new facilities, surveys should be prepared to consider what existing facilities might be phased out to ensure robust support for new initiatives.
- Surveys should look for opportunities for international partnerships, which can help make development, construction, and operations costs more affordable to the United States. Workshop participants noted, and Dr. Sunley agreed, that the impact of international cooperation on such costs needs to be understood.
- Explicit considerations of programmatic roles and relationships between agencies and opportunities for interagency collaboration, as emphasized earlier by Dr. Turner, are also important.
- Survey committees need to be mindful of the breadth of the scientific communities that they are charged to represent and of inadvertently disenfranchising some segments of a community.

Mary Kicza, the NOAA assistant administrator for satellite and information services, provided a NOAA perspective. She began by calling attention to important differences between NASA and NOAA, which survey committees need to recognize. The differences include the fact that NOAA's budget is only about one quarter that of NASA and is increasingly earmarked, and that NOAA's civil servant population is two thirds that of NASA. NOAA has a special need to ensure long-term continuity in its observing programs, and in a constrained budget environment there are fewer opportunities and less flexibility to introduce new directions to the program. Furthermore, NOAA's external operational user community is very broad, and interfacing with it presents a more difficult challenge than with NASA's research missions.

Ms. Kicza said it is important to understand the difference between managing cost growth during mission development and relying on premature cost estimates, as both have presented problems for NOAA and have resulted in having to delay and/or downsize programs. She said that putting realistic bounds on missions being considered for the distant future remains a difficult challenge. Finally, she noted that Earth observations have to be examined in the context of a global system of observing systems; international roles, responsibilities, and partnerships provide an important contextual overlay. One aspect of this is the need to have international standards for instrument calibration and data validation.

The final speaker from the federal executive branch panel (Panel on Agencies) was Jon Morse, senior policy analyst in the Science Division of the White House Office of Science and Technology Policy. Dr. Morse noted that his comments would draw also on views from colleagues in the Executive Office of the President, especially the Office of Management and Budget. He commented on the beneficial aspects of decadal surveys; particular issues and concerns about the most recent survey reports; approaches for facilitating decision making and other suggestions for improvements; and considerations for making surveys realistic. (Key excerpts from Dr. Morse's presentation are reproduced in Appendix C.)

Dr. Morse supported an approach that begins by framing key science questions ("science first") for a field and then explaining what measurements and capabilities are needed to answer those questions. He argued that the surveys should recommend priorities associated with the current program and the future program and should define what the survey committee looks for in terms of program balance and the mix between small, medium, and large initiatives and core research. He indicated that budget analysts and decision makers at OMB preferred to see cost estimates in terms of rough, rounded-off life-cycle costs rather than more detailed (and often underestimated) construction costs. Dr. Morse said it would be especially helpful for survey reports to discuss how unforeseen programmatic, policy, or scientific circumstances might alter the priorities and what science could be accomplished under several different budget scenarios.

VIEWS FROM CONGRESS

The third and final panel of Session 1 drew views about the decadal survey process from one current and two former congressional staffers. William Adkins, president of Adkins Strategies LLC and former staff director of the House Subcommittee on Space and Aeronautics, called the decadal surveys the gold standard for scientific advice. He said that while surveys should focus on the science and steer clear of political considerations, their timing should be sensitive to political milestones or changes in the organization of Congress. He also recommended that future surveys should characterize the level of uncertainty ("error bars") in cost estimates and technology risk, comment on whether high-priority initiatives need to go in a particular order, and propose priorities for different budget scenarios.

Johannes Loschnigg, the current staff director of the House Subcommittee on Space and Aeronautics, noted that survey reports are read closely on Capitol Hill and their recommendations are compared carefully with agency program plans. He said that the surveys are viewed as being a firm expression of community preferences and that a well-done survey is the best antidote for having Congress yield to special pleading or making arbitrary decisions about agency programs. He noted, however, that future surveys need to address a problem with the recommendations of recent surveys—that is, their incompatibility with the resources that are likely to be available. Survey committees also need to state explicitly what they see in terms of mission size trade-offs in a balanced program.

Robert Palmer, a former staff director of the House Committee on Science, agreed with his colleagues that members of Congress think very highly of the decadal surveys because they help them do their jobs of advocating a strong U.S. science program. He sampled the views of other congressional staff and learned that they concurred on two points about decadal surveys. First, future surveys need to describe a healthy core program—that is, they need to state a level of activity and content below which an agency program should never fall. Second, new surveys should start afresh and not assume that legacy priorities automatically apply when setting priorities for new programs.

RECURRING THEMES

Discussions among workshop participants in response to the presentations of the three panels drew out several recurring themes. First, all participants seemed to share the view that the decadal surveys are important and that they have positive impacts on federal agency planning and decision making and on science community unity. The efforts by survey committees to seek broad community participation and consensus building and to set explicit, science-based priorities were cited again and again. The survey chairs and others also often agreed about problem areas that need to be understood and addressed. These include obtaining credible cost estimates; addressing and engaging all relevant government agencies rather than only one or two sponsors of major programs; reevaluating recommendations from prior surveys; not overlooking small programs such as NASA Explorers; taking into account the portfolio mix of mission sizes in an agency's program; and following up after survey reports are completed and delivered.

Various speakers also highlighted other ends that effective surveys should attempt to attain:

- Breadth in committee membership and in community input;
- Synchrony with agency planning processes, and maybe even with election cycles;

- Balance between identifying broad science objectives versus providing specific advice about new program initiatives;
- Healthy core programs and attention to how they fit into the context of recommended new projects;
- Appreciation of budget realities;
- Full life-cycle costs for projects;
- Clarity about potential biases that might be hidden in cost estimates;
- Advice on phasing of projects;
- Principles for deciding how to respond to major budgetary, programmatic, or political changes;
- Attention to the challenges intrinsic to international or interagency cooperative programs;
- Integration with complementary or overlapping surveys for other disciplines;
- Attention to applications as well as fundamental science; and
- Mechanisms to monitor government attention and responsiveness.

All of these points reinforce the concept of a decadal survey as a strategic package. That is, decadal studies need to provide the best balance of scientific priorities and prioritized missions (a strategic package), work with the agencies to understand the budgetary and political realities, and give the agencies advice and decision rules to help them respond to major budget, programmatic, or political changes, while understanding that the agencies need flexibility in implementing specific programs and missions. A report needs texture and creative recommendations centered around key scientific questions and goals as much as it needs ordered lists.

3

Assessing Cost and Technology Readiness

On the second day of the workshop the five panelists at Session 2 examined how surveys could better deal with new project costing, technology readiness, risk, and execution. The questions posed to the panel included these:

1. How does a survey committee determine the cost and risk for candidate missions that have not yet been fully defined?
2. How does it define an affordable decadal plan?
3. Is it feasible or reasonable to expect a survey to be stable for 10 years?
4. What similarities and distinctions are there between considerations for NSF ground-based facilities and space missions?

This panel was moderated by Tom Young (Lockheed Martin, retired), and the panel members were Steve Battel (Battel Engineering), John Casani (Jet Propulsion Laboratory), Noel Hinners (University of Colorado), Bruce Marcus (TRW, retired), and George Paulikas (Aerospace Corporation, retired). Following brief remarks by each panelist, the moderator invited all the workshop participants to comment on cost and technology assessment.

Mr. Young opened the session by saying it appeared from the prior day's discussion there had been a bias toward unrealistically low budget estimates, and he asked whether a survey's priorities might change if the survey committee had better cost estimates. The current process seems to support *initiation* of a large number of programs, but it fails to support the *completion* of many programs. He went on to say that if the objective is to complete projects successfully, then the survey committees aren't doing the cost estimating right—too many of them suffer later from large budget and schedule overruns. Perhaps surveys should aim to start fewer programs and complete most or all of them.

PANEL MEMBER PRESENTATIONS AND DISCUSSION

Steve Battel started by remarking on what he thought were two key problems relevant to cost and technology assessments:

- It is very difficult to match the cost and schedule assumptions of a mission with an uncertain 10-year agency budget outlook.
- The limited opportunities to start a mission in the course of a decade, the competitive nature of the planning process, and very early technical assumptions lead to overoptimism on costs, schedule, and technical readiness and drive mission designs to be quite complicated—that is, there is often a desire to maximize science returns and to do things because we can rather than because they need to be done.

Mr. Battel mentioned that cost estimates in past decadal surveys had been 1.5 to 4 times lower than the actual costs of a mission. He thinks that these low preliminary cost estimates are due to (1) pressures from agency representatives and community members to adopt top-down, cost-capped approaches on missions, (2) inadequate investment up front to reduce technology and design risk, and (3) lack of discipline and tools for system evaluation and cost estimates.

Mr. Battel also believes the sources of real cost growth in projects include the following:

- Mission creep, changes in project management during a project, and unreliable funding;
- Course corrections or management mandates in response to other mission failures;
- Underestimates of technical complexity and maturity;
- Management approaches that simply seek to avoid risk rather than to manage it (risk aversion management deoptimizes a project by cutting cost, capability, or other resources to create an artificial contingency margin);
- Unwillingness on the part of management to “push back” on projects that are in trouble (i.e., to terminate or downsize);
- Uniqueness of aerospace practices, loss of experienced aerospace workforce (failure to share expertise), transition from military to less robust commercial aerospace technologies, and too much simulation in place of actual testing;
- Fewer options for launching smaller missions; and
- Overcommunication and superfluous management processes (“too many meetings”).

Mr. Battel thought that NASA deserved credit for establishing a process for developing and rating technology readiness. He cited both the Space Interferometry Mission and the James Webb Space Telescope as missions with clear technology roadmaps for achieving appropriate technology readiness before entering development. He thinks that smaller missions (e.g., Explorer and Discovery) should be based on existing technology or a controlled extension of existing technology. Mr. Battel believes that decadal surveys need tools to properly rate mission complexity and readiness. He thinks that the most recent surveys in the 2000s have been impacted by the paradigm shift away from faster-better-cheaper at NASA and by major reductions in science planning budgets. Mr. Battel recommended that agencies should

- Continue to develop and improve parametric cost and schedule models based on real data and realistic assumptions,
- Add experts to review mission costs and technology readiness,
- Develop mechanisms to better couple mission planning and the identification of associated technology,
- Work to simplify and optimize the management of small and medium missions to reduce cost and development time, and
- Work to improve the affordability and availability of launches.

Similarly, he noted that survey committees should

- Require that programs are technically ready so they can be accomplished within a decade,
- Ensure that programs are properly scaled in size and have realistic cost numbers vetted through certified modeling processes,
- Provide specific guidance on mission-enabling technologies,
- Establish metrics and methods for creating and maintaining program balance, and
- Work with agencies to find effective advisory mechanisms for obtaining tactical advice from the research community.

The discussion after Mr. Battel's presentation focused largely on how wasteful it is to design smaller missions with the same approaches as large missions and how hard it is today for universities to undertake even very small missions. Given current constrained agency budgets, universities can no longer afford to maintain the infrastructure or staff to oversee missions.

John Casani started his brief remarks by stating that knowing the scope of a mission is the most important aspect of reliable cost estimating—one must be as certain about performance requirements as about cost before initiating a project. He said that one will never get good cost or technology readiness estimates if the input information about a mission is weak or if there is a lot of uncertainty.

Because his focus was solar system exploration, he mentioned that uncertainties about the environment in which a mission would be operating (e.g., Mars's surface) were a major factor in cost uncertainty and made costs very difficult to characterize 10 years in advance. Dr. Casani agreed with earlier speakers that industry is under tremendous pressure to come in with low bids and that we do not spend enough time defining requirements. He recalled former NASA mission readiness assessment guidelines as saying that investments in Phase A (requirements definition) should be ~7 percent of the total project cost. Dr. Casani also made the following observations:

- International cooperation helps secure a mission but rarely saves the United States money, and it introduces other risks (organizational communication problems, schedule and technology interdependencies, and politics).
- One can spend a lot of money on risk management and not improve the mission success. Fewer mission opportunities encourage even more conservative and wasteful risk management approaches, perhaps in response to fear of failure.
- Cost modeling does not take into account different management approaches and will probably be helpful only for comparing mission costs rather than giving good estimates for a specific mission.

NASA representatives agreed that cost models are currently best suited for comparing mission costs. Dr. Casani believes that NASA should develop a standard approach to cost estimating that is independent of the approaches used by advocacy groups and that NASA must take the leadership in challenging bad cost estimates. He mentioned that some of the missions he had worked on that did not have overruns used a lot of fixed-price contracting and had highly capable engineers overseeing them. They were able to do this because they invested time in defining the requirements up front, did the needed system engineering, and did not change the contract or requirements. When asked how decadal surveys are going to estimate mission costs when previous missions have been as much as four times more expensive than originally estimated, Dr. Casani said that we must do a better job of realistically assessing mission complexity and the likely cost of attendant risk mitigation. After starting a mission, we must insist on adequate systems evaluation and requirements definition prior to starting development: Spend the money needed to better define requirements, and do not go beyond those requirements.

Panel member Noel Hinners focused on how surveys produce better cost and risk estimates for missions that are not well defined. He said it is unrealistic to think we can get valid cost estimates for undefined missions unless we develop "ridiculously unambitious" missions. He suggested that survey committees need to look more at mission class, their risk, and their damage potential. Table 3.1 summarizes Dr. Hinners's ideas on how cost overruns impact the rest of a pro-

TABLE 3.1 Potential Cost Growth in Some Mission Classes

Mission Class	Characteristics	Mission Cost (billion $)	30% Overrun (billion $)	Damage Potential
Flagship	5-10 year development cycle, little heritage from prior missions. Examples are Viking, Voyager, Galileo, HST.	3-5	1-1.5	One New Frontier or two Discovery or three Explorer missions
New Frontier	~5 year development cycle, new instrument technology. Examples are SIRTF, GP-B, Juno.	1-1.5	0.3-0.5	One Discovery mission or two Explorers
Discovery	Cost-capped. PI competition drives innovation, financials, and risk. Some system heritage, but challenging instruments.	0.5-0.7	0.15-0.20	One Explorer mission
Explorer	Same as Discovery with small, colocated, experienced project teams. Most risk in instruments.	0.1-0.3	0.03-0.06	Another Explorer mission or all the research, data analysis, and technology development

gram's portfolio, which he called "the mission impact food chain." For example, paying for a 30 percent overrun on a multi-billion-dollar-class flagship mission can eliminate a New-Frontier-class mission or two Discovery-class missions or three Explorer-class missions. Similarly, covering the cost of a 30 percent overrun on a billion-dollar New Frontiers mission can eliminate a Discovery mission or two Explorers.

From this analysis, Dr. Hinners proposed to draw the following generalized conclusions:

- The most expensive missions pose the greatest risk of major cost overruns and cause the most pain—"Beware the Big Vision."
- The main factor in cost risk for all mission classes is instrument development, which tends to be inadequately assessed in the approval process.
- Competitively selected missions have built-in mechanisms to help restrain cost increases (i.e., caps).
- NASA flight programs are self-insured in that they have no reserve other than delay, cancellation, or postponement of future missions.
- Cost models are not that bad, but one must build only that which is actually costed at the preliminary design review (PDR).

Dr. Hinners's suggestions for survey cost management included these:

- Decadal survey plans should devote perhaps no more than 30 percent of their 10-year budget to a flagship mission(s).
- Compete the flagship missions and apply cost caps that are established after well-funded Phase A/B studies.
- Develop better capabilities for assessing the costs of new instruments and technology.
- Maintain a program reserve (~20 percent of mission cost) separate from the project manager's own mission contingency budget.

In the ensuing discussion participants asked if the ability to assess cost and technology readiness was better or worse today than in the past. The industry is smaller and the infrastructure is older, but the tools are better. However, today's flight mission systems are much more heavily reliant on software than in the past, when a spacecraft was more likely to be dominated by hardware challenges. For this reason, cost estimates based on traditional spacecraft system parameters such as mass are not as accurate today. Some participants expressed doubts about the ability of survey committees to produce a good estimate of flagship mission costs so early in their conceptual design. Furthermore, principal investigators (PIs) can impose some control on a small mission but not on a flagship mission. The latter is just too challenging, and the technology is well beyond a group of university PIs. Participants added that the missions driven by NASA centers seem to be allowed to have cost growth, but PI missions are not. Finally, participants noted that developing a cost-capped mission with an inexperienced project manager is a very risky business.

Bruce Marcus made a number of points about cost estimating and cost risk. He noted that a fundamental weakness with cost estimating is the subjectivity of judgments about complexity, technology maturity, and the like. Optimistic cost modeling is ever present when a program is being sold, and everyone is a player in this (scientists, vendors, agencies, Congress). Cost growth is closely related to development risk, increasing nonlinearly with the number of new technologies, and is very sensitive to schedule slips (especially the so-called "marching army" cost of keeping a mission team in place during a delay). Cost risk increases more rapidly than nominal cost with increasing technology development requirements (see Figure 3.1). He also suggested that using multiple-spacecraft platforms (versus larger multisensor platforms) can reduce overall mission cost risk, but generally at an increase in nominal costs (particularly due to launch costs).

Dr. Marcus then applied these observations to lessons for survey committees. He suggested that surveys should employ the same cost model and estimators for all missions so one can evaluate their "relative" costs, and then select a collection of small (<$300 million), medium ($300-$600 million), and large (>$900 million) missions from a single priority list that fit into the overall survey decadal budget.

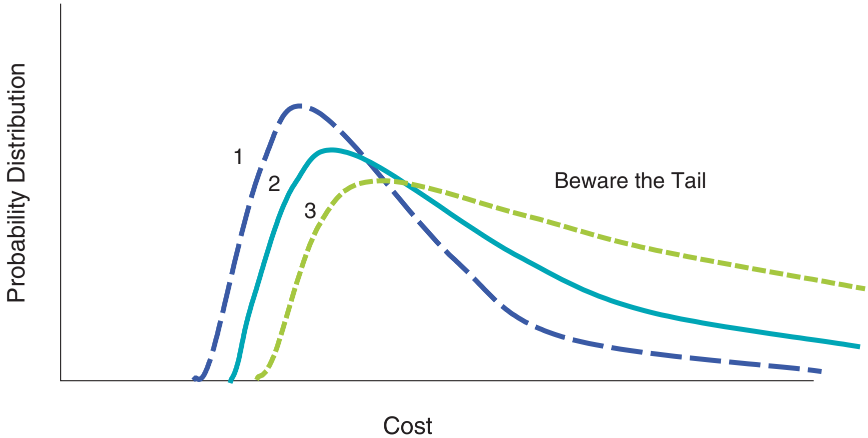

FIGURE 3.1 Notional illustrations of the cost probability distributions for projects having different levels of requirements for technology development. A project that depends on few new technology developments (Curve 1) will have a lower most probable cost than projects having more technology development requirements (Curves 2 and 3), which will have both higher probable costs and a significantly higher likelihood of costing much more than the most probable cost value. The risk of dramatic cost growth is represented by the area under the tail of the probability distribution. Increasing technology development requirements significantly increases the area under the tail even though the estimated most probable cost increments may be modest. SOURCE: Workshop presentation by Bruce Marcus.

Decadal surveys should prioritize missions based on science value, technology readiness, and benefits to society. He noted that technology readiness is very subjective and what is deemed to be acceptable development risk is dependent on mission architecture (how much is at stake). Therefore, better coupling between advanced mission planners and programs like NASA's New Millennium Program is needed to lower technology risk for missions. Finally, Dr. Marcus noted that because survey committee members are mostly academicians they are mostly qualified to identify key science questions and scientific mission priorities. This means that survey committees need to also include expertise on cost modeling, technology development, and societal benefit.

Workshop participants noted that the Explorer and Discovery programs have had to solve their own cost growth problems, while flagship programs appear to have "a license to steal" from other programs to fix their problems. Surveys need to have guidelines or rules on how to deal with this. There were also questions about how to optimize investments based on bins of small, medium, and large missions.

George Paulikas was the final panel member for Session 2. He said that one never really knows which cost estimates are realistic until the time of a PDR and that it requires a lot of history and experience to get reasonable cost estimates (or to recognize if estimates are bad). He suggested that survey cost bins should be set at a very coarse level. He also suggested that the biggest problems are the following:

- Mission requirements creep,
- Excessive optimism about even modest technology development and underestimation of mission complexity and program management demands,
- Increased risk due to complexity of interagency or international collaboration,
- Software complexity in today's modern missions,
- Ignoring past mistakes instead of learning from them, and
- Switching launch vehicles during a project.

There was much discussion about how cost models could be used in surveys. Most participants believe they might have limited utility or accuracy since most missions are far short of being ready for PDR at the time of a survey. That said, survey committees need to be educated consumers of cost models and need to have members who have cost modeling expertise. It was also suggested that survey committees should contract out at least two independent cost estimates. It was clear from the discussion that missions (particularly large ones) need to have the technology development well understood or characterized before a cost estimate can be reasonable. It was suggested that a flagship mission should not go forward if its technology is not fully understood and should be recommended only as a candidate for technology development until it is better understood. Participants also noted that cost estimates usually do not take into account other key factors (such as the cost of the "marching army") if a problem introduces a significant delay in the development schedule. That situation demands that a large reserve be maintained for such problems, and that reserve should be off-limits to other program needs (e.g., technology development). True reserves should be a critical component of any mission budget and should probably be ~20 percent of the total mission cost estimate.

The discussion turned to whether there were also lessons to be learned with respect to how NSF plans and manages large, ground-based projects. Some participants thought that there was some commonality with space missions, but others saw the terrestrial construction industry as quite different from the aerospace industry. Furthermore, the NSF major research equipment and facility construction budget represents only about 5 percent of the total NSF portfolio, and final cost estimates for projects take a long time. The National Science Board signs off on these large projects after an extended definition phase, and the board approves projects with fixed budgets. NSF facility construction projects are also separate from NSF division research budgets. Therefore, while development costs do not affect the research division, the long-term operational costs must be accounted for within the research budget.

There was considerable discussion of the cost risks associated with international collaboration. NASA representatives pointed out that NASA has explored these risks recently and is moving toward not getting too dependent on international contributions. Participants cited the contrasting examples of the European Space Agency's Huygens Probe and the German propulsion system on the Galileo mission. The Huygens atmospheric entry probe would not have happened without international cooperation, but the probe was separate from the rest of the Galileo mission and the core mission was not dependent on it. On the other hand, because the German propulsion system was highly integrated into the Galileo spacecraft, it had an impact on the overall mission cost. Others mentioned that when technology development is outsourced to other countries it diminishes the robustness of the technology coming from our nation's aerospace community. Some participants, however, reminded their colleagues that one cannot ignore the scientific benefits that can come from international cooperation—they are not solely a matter of cost.

RECURRING THEMES

Many participants argued that the cost estimates put forward by the most recent series of surveys have been surpassed by revised costs that are as much as four times greater. Some observers noted that these increases have extremely disruptive impacts on the overall survey recommendations and must be addressed. Participants recognized that obtaining an accurate cost estimate is not an easy task because most initiatives recommended by a survey report are in very preliminary stages of design as the survey report is being prepared.

Participants generally viewed the following as important for future survey committees to consider:

- Survey committee members having expertise in cost and technology readiness.
- Use of uniform, independent costing approaches so that costs can be cross-compared.
- Recognition that early cost estimates are better for comparing mission costs than for providing accuracy (since most missions are far short of being ready for a PDR).
- Use of cost bins or ranges rather than specific costs.
- Survey recommendations that provide guidance on risk mitigation via investment in mission-enabling technologies and by moving forward on less complex missions.
- More time invested in defining mission requirements.

- Appreciation of the need to have the technology well understood or characterized before cost estimates for the initiatives (particularly large ones) can be reasonable. It was suggested that a flagship mission should not go forward if its technology development is not fully understood and should be recommended only as a candidate for technology development until it is better understood.
- Establishment of metrics for creating and maintaining a balanced set of large, medium, small, and core research and technology programs in a survey. A flagship program with cost increases can have a substantial impact on a survey, so agencies should seek advice on how to maintain a survey's programmatic balance if the flagship project's cost exceeds a certain threshold.
- Consideration of incorporating competitively selected projects as much as possible.
- Recognition that instrument development can trigger cost risk and that a program to reduce risk on instruments is important.
- Prioritization criteria for new initiatives that look at science value, technology readiness, and benefits to society.
- Recognition that interagency and international collaboration can substantially increase risk.

Participants also said that funding agencies rather than survey committees should consider the following things:

- Continued development of parametric cost and schedule models, including models taking into account that, today, software has an impact on cost and schedule that is greater than that of spacecraft mass.
- Simplifying and optimizing management processes on small and medium missions to cut cost and development time. Designing smaller missions under the same management constraints as large missions is viewed as inappropriate and wasteful.
- Maintaining a reserve (equivalent to ~20 percent of the mission cost) separate from the mission budget.
- Holding NASA center missions to the same cost growth criteria as PI-led missions.

4

Lessons Learned and Implications for the Next Decadal Surveys

The afternoon session on November 15 (Session 3) was devoted to the assumptions on which decadal surveys have been or should be based, and the final session on the morning of November 16 (Session 4) featured lessons learned from the preceding workshop sessions and how they might be incorporated into future surveys. (See Appendix A for a detailed agenda and lists of panelists.) Speakers said their observations reflected not only their own opinions but also the views of colleagues in their respective research communities. For example, Megan Urry noted that the NRC Committee on Astronomy and Astrophysics, which she formerly co-chaired, had devoted more than a year to gathering input from other relevant committees, experts, and agency representatives and from other sources and had used those perspectives to draft a white paper concerning the next astronomy and astrophysics survey. The last two sessions reinforced and elaborated on points that had been introduced in the first two workshop sessions, and their highlights are presented below.

SUSTAINING THE OVERALL VALUE OF SURVEYS

All workshop participants appeared to share the view that these surveys continue to be important to their respective communities and to those who use them to make programmatic, policy, and budget decisions. Not doing decadal surveys was not viewed as an option, for without these surveys there would be no way to maintain community coherence on priorities. In short, the alternative would be lots of parochial lobbying for individual programs. Several agency representatives mentioned that they get plenty of conflicting advice, so having a stable, long-term perspective in the form of a decadal survey is very important for their planning.

The discussions indicated that future surveys should perform functions that have been important in the past—namely, they should (1) reflect broad community consensus; (2) present ranked priorities; (3) integrate all aspects of a research program, including measurements, theory, data analysis, technology development, training, and so on; (4) demonstrate the field's benefits to society; and (5) include plans to market the survey to users.

The discussions identified four important audiences for survey reports:

- Practitioners in the research community, who expect fairness, wisdom, and advocacy;
- Students, who expect to see a big picture of the field and how the pieces fit;
- Federal agencies, which expect direction and judicious advocacy; and
- OMB and Congress, which expect unambiguous rankings.

Speakers asserted that it is at least as important for policy makers in senior offices of the executive branch (i.e., OSTP and OMB) to understand the consensus within the community (as embodied in a survey report) as it is for the federal agencies themselves to understand. Furthermore, participants argued that is very important that members of Congress and their staff be fully aware of survey results and have a chance to question survey committee members freely and directly, because the best communicators and advocates for the surveys are really the community members themselves.

Participants noted that strong community ownership of a survey is a key to success. Such ownership is possible only if community members, including emerging leaders who will be important players in succeeding decades, have had ample chance to provide input and give guidance to the survey panels. In his comments, workshop panel member Joseph Burns applied his experience as chair of the 1994 Committee on Planetary and Lunar Exploration to the challenge of building broad community support and acceptance. Insisting that solidarity is essential, he suggested that the way to build (and communicate) that solidarity is (1) to utilize large survey panels, which engage more people who then become agents for the survey; (2) to invite white papers, which engages the entire community; (3) to organize community forums where people can offer views in person; (4) to use Internet tools effectively; and (5) to have an active outreach program after the study is complete. By the same token, issuing decrees from on high, publicizing the outcome, and then ignoring feedback would result in failure to secure community buy-in.

In response to questions about ensuring independence and objectivity in the survey process, participants tended to accept that absolute independence and objectivity are generally not possible on survey panels. They argued that the goal should be to have people on panels who are highly engaged and passionately interested in the programs, and that the best that one can do is to balance vested interests. Surveys probably always will involve some trade-offs and even some obvious advocacy, so the NRC and the survey committees need mechanisms to keep aspects of advocacy in check. An effective way to do this, some noted, would be to have a portion of the survey committee's membership come from outside the discipline community—they would be experts who could look at the committee's conclusions dispassionately.

SURVEY DESIGN, STRUCTURE, AND TIMING

Surveys require a lot of work, and there seemed to be general agreement among the workshop participants that roughly 2 years are needed to complete a survey and to ensure community coherence on recommendations. Participants commented on the need for the survey charge to be clear and focused and to avoid open-ended tasks. Surveys also need to be comprehensive (e.g., astronomy surveys should include both ground- and space-based efforts and should span the interests of NASA, NSF, and DOE). The survey charge needs to be fully vetted at the outset to ensure community and agencies buy-in and to avoid later delays. The surveys need to reflect the possibility that the recommendations may impact the programs of agencies other than the survey sponsors, and the survey committees will have to think carefully about that.

Participants noted that the survey committee membership must adequately represent the scientific field, but it also should include other experts in areas beyond the science (e.g., in hardware development, program and project management, systems engineering and operations, cost estimation, and policy). Most surveys have used panels to explore subfields, and that approach was widely accepted by workshop participants, who also noted that panel structure is what enables cross-prioritization, something that some fields have had trouble doing. Finally, some participants emphasized that different fields all have unique aspects that a survey's design needs to accommodate; a one-size-fits-all approach would not be sensible.

Many participants seemed to feel 10 years is probably the right time interval between surveys. Some suggested that there is value in synchronizing surveys with other key planning activities. For example, agency roadmaps and strategic plans tend to be revised every 3 years and to look out over about 10 years. Hence, decadal surveys can be most useful if they also look out over a decade and if they are available when agencies begin a cycle of roadmapping and strategic planning. Agency roadmapping and implementation planning are likely to be done more frequently than the surveys, but there needs to be a way to keep the agency plans consonant with the survey's strategic priorities.

Providing clear and convincing advice to multiple agencies was noted as being especially challenging for a survey. As one workshop speaker put it with tongue in cheek: "Asymmetries in agency sizes and budgets can promote fear, loathing, and jealousy." A survey needs, therefore, to identify the right role for each agency and to tailor advice to each agency's culture. Many appeared to agree that if surveys can rise to that challenge then they have great potential to promote efficiency in science programs and even better interagency cooperation.

PRIORITIES, QUEUING, BALANCE, AND PORTFOLIO MIX

Workshop panelists noted that surveys should lead with the science. Every survey report needs a compelling exposition of the science: where progress in the field stands, where the field hopes to go, and how and why the science is exciting. They recognized how hard it is to market science and how more emphasis needs to be placed on science advocacy. Scientific communities clearly need to make a better public argument for their science, and the surveys are a good way to do this.

The deliverables from a research program cannot be just another set of interesting questions for scientists to ponder. Thus, while surveys must stay focused on the best science, the recommended programs or missions can still contribute substantially to economic and societal applications. Participants argued that good science underpins sound applications, and surveys have a duty to spell out clearly how important applications can be drawn from basic science missions and programs. They also seemed to accept that for disciplines such as the Earth sciences, applications are the central motivating goals of the field. Applications tend to be interdisciplinary by nature, they said, pushing the envelope of science and forcing a look at the entire system being studied, and they do produce something useful (beyond journal articles). Thus, speakers proposed that a decadal survey's broad science questions be articulated in a way that is comprehensible and appealing to the public and that, among other things, explains why taxpayer money should be invested in an effort.

Participants fully agreed with the need to establish priorities in the surveys, but there was a range of opinions about exactly how to do that. Most of the discussion was focused on what would be most useful for the agencies—that is, it asked which of the following a survey should aim to present:

- A single integrated priority list *or* parallel lists for projects of different size or scope.
- Lists of specific initiatives *or* lists of priority science objectives.
- Recommendations for specific project queues *or* delegation of queuing to the agencies.

Some participants noted that agencies are in a better position to decide on the queuing of projects than are the survey committees. Several agency representatives mentioned that in today's funding-strapped world, something will probably have to be given up before something new is started, and they encouraged the surveys to look at this issue of triage. That is particularly true because operational costs are now commensurate with development costs for many projects.

Most participants appeared to share the view that surveys should be as broadly inclusive as possible. That is, all elements of the discipline should be evaluated and ranked in the survey, and all aspects of the discipline's program, from smallest to largest and from major missions and facilities to core research infrastructure, should be placed into scientific and programmatic context and their roles should be made clear to government decision makers and the public. The failure of some recent decadal surveys to explicitly endorse the Explorer program and to include it in priority rankings was cited as an example of how devastating the consequences could be when such program elements are assumed to be sacrosanct and are not treated explicitly. In the Earth sciences, the recent dramatic de-scoping of the National Polar-orbiting Operational Environmental Satellite System (NPOESS), which had once been the principal platform for obtaining climate data from space, was cited as another case where unquestioned assumptions led to major headaches and reassessments for a survey committee.

Similarly, most participants appeared to agree that a priority recommended by an earlier survey that had not been initiated by the time of a new survey should be considered explicitly and reevaluated. If, however, a legacy project has started up and continues to have high merit, it should be fully carried out. But priorities can change over a 5- to 10-year period, so that if a legacy project has lost relevance or been superseded it should not be allowed to block progress on more important efforts.

Participants also noted that there is value in discussing the synergies between different missions or initiatives that are recommended; queuing and the relative phasing of recommended missions; and the benefits, if any, of conducting particular missions simultaneously, both within a given field and between related fields.

It was clear from the discussion that participants believed the surveys should consider the balance between small, medium, and large projects and the portfolio mix of missions and mission-enabling elements of a comprehensive program—that is, what it takes to do a complete project: a mission, its data analysis, technology development, training, and theory. Participants emphasized that all recommended priorities and portfolio mixes should be driven by, and traceable to, science priorities. A number of speakers noted that larger projects cannot be accomplished without key supporting research and technology efforts.

There were differing views about how surveys should characterize project balance. For example, should a survey recommend a specific mix, such as 60 percent large, 30 percent medium, and 10 percent small projects? To some, such levels would be arbitrary and difficult to defend. To others, they establish metrics for needed balance and can be used to review whether project cost growth is significantly impacting portfolio balance. There appeared to be considerable interest in having surveys set forth decision rules (principles by which departures from survey recommendations could be decided or implementation decisions could be guided) that allow agencies to cope with exigencies that might threaten program balance. Participants did note that the ability to have a balanced portfolio varies by agency, with some suggesting that NASA is now more focused on human spaceflight than on science.

There was an extended discussion about the fact that the human exploration of space is now a major national goal. Some participants argued that human spaceflight should not be ignored in future decadal surveys. Human spaceflight is relevant in some fields—for example, planetary science and solar and space physics—specifically from the standpoint of how research in those fields will enable future human exploration missions. There are cases, most notably the Hubble Space Telescope, where human spaceflight has served to directly expand the scientific capability of a science mission. Human space exploration missions might even enable entirely new research.

Two views were expressed. Some participants were concerned about the field-of-dreams approach, whereby if human exploration goes forward people will feel compelled to gravitate toward those programs to use them as research opportunities. These concerns recall the rueful experience of the scientific community when NASA promised abundant research opportunities from the space shuttle and the International Space Station. Other participants argued that while the scientific opportunities that might accompany human spaceflight should not be ignored, they should be considered from a science-first perspective. That is, science that is enabled by human spaceflight should be prioritized along with other science and against the same criteria, not given higher priority simply because it is aligned with current NASA policy.

There was also a sense that science to enable human space exploration is an appropriate topic for future decadal surveys but that in those cases the science and the applications should be treated separately and priorities for the human spaceflight exploration applications and for fundamental science should not be intermixed.

RESILIENCE, EXECUTION, TECHNOLOGY READINESS, AND RISK

The panelists and other participants discussed at length (1) how to make surveys more resilient to future (scientific, technical, programmatic, or political) changes, (2) how much survey recommendations should push the likely programmatic, technological, and budget limits, and (3) how to assess the feasibility of a project and its difficulty of execution. Recent dramatic changes in agency planning environments bring a basic instability to the system that cannot be eliminated but that must be managed. One thing is predictable: Changes will occur. Participants cited changing political agendas (the announcement of the Vision for Space Exploration), apparent changes in agency priorities (removal of "protect the home planet" from the NASA mission statement), budget pressures, technical challenges (restructuring of NPOESS), unanticipated disasters (Columbia accident and Hurricane Katrina), and unanticipated scientific results (e.g., discovery of the ozone hole, compelling evidence for dark energy). Past surveys have had to deal with issues such as these but lacked robust ways to address them. Therefore, many participants agreed that a balanced portfolio of missions provides resilience so long as there are some decision rules included in the survey on what to do if missions get in trouble or if there are radical shifts in budgets or politics. It may be difficult to predict what these decision rules should include so far in advance, but it was clear from the discussions that participants believed having them would help keep a survey flexible in the face of change.

Many participants argued that including competitively selected, smaller missions (e.g., Explorer, Discovery, and Earth Probe missions) also makes the surveys resilient. Small missions are of strategic value because they can respond more quickly to new discoveries or changes in available resources, contribute to a higher mission launch rate, accept more technological risk, provide an effective vehicle for training students and new engineers and scientists about space project design and management, and facilitate testing and demonstration of new technologies. Participants also agreed that insisting on sufficient technology development and mission planning can substantially improve the resiliency of future surveys and ensure that immature mission concepts are avoided. They also seemed to agree that it is quite unlikely that a new large mission would arise ab initio and be ready for initiation in the 2 years it takes to carry out a survey.

Speakers also noted that it is worth working with survey sponsors and affected agencies to get the best sense of realistic budget and policy environments (i.e., the likely funding wedge) during the period when the survey recommendations are to be implemented. That said, many thought that better cost estimating was more important than a better planning wedge, since projected budget wedges come and go so quickly in the annual federal budgeting process. This will require effective communication during the survey period, which is important for ensuring strong partnership between the survey committee and various stakeholders (e.g., agencies and Congress).

There were different views about providing multiple budget scenarios as a means of building resiliency into the surveys. Some participants thought that including multiple scenarios was a good idea, but others suspected that the lowest cost alternative would tend to be selected, thereby undermining the broad scientific reach of a survey.

Most participants seemed to feel that because decadal surveys should be held in high regard, they should not ("like the Constitution") be revised arbitrarily, largely because of the arduous and balanced process of getting broad community buy-in on their recommendations. However, some participants said that surely there can come a point in time when cost growth or policy changes warrant revisiting the survey. Such a trigger point should be built into a survey if possible. Speakers cited examples such as a flagship mission's cost growth or a major event like a space shuttle accident or the deletion of climate observation from NPOESS, any of which could be viewed as destroying the balance of the overall program recommended by a survey. It was pointed out that seldom is there a specific trigger—it is more likely to be a slippery slope that leads to big change (e.g., the JWST project had spent more than $1.2 billion before a good PDR cost estimate could be obtained).

Some past survey chairs said that NASA's internal roadmapping activities or advisory committees had deviated from survey priorities and intent and that there should be a mechanism to follow up on an agency's execution of a survey. Many participants voiced strong support for keeping survey committees together for the 10-year life of a survey so they can monitor implementation of the survey's recommendations and participate in any needed revisions. They noted that standing oversight committees (e.g., the Committee on Astronomy and Astrophysics and the Committee on Earth Studies) should be able to address specific issues that arise between surveys. Most of the agency representatives strongly agreed with the need for decision rules and trigger points in surveys.

COST ESTIMATES

The session panelists and other participants discussed how surveys should handle cost estimates and who should prepare them. A clear message was that cost-estimating problems are plaguing every agency (e.g., NASA, DOD, NOAA, and NSF) and that the quality of these estimates depends on project design maturity and size. One speaker noted that there is a "conspiracy of optimism" to get new initiatives into the queue but that priorities need to be matched with technological readiness and mature cost estimates. The impacts of poor cost estimates included loss of balance in project sizes and support for the core research program, limited capacity to accomplish survey recommendations, and loss of scientifically important overlap of mission phases.

While growth in the JWST cost estimates was seen as having crowded out other important investments, the mission is still seen as holding great scientific promise and supporting a healthy research and data analysis program. Some participants asked whether the community would have supported JWST at the time of the 2000 decadal survey if it had suspected the potential for cost growth and the likely current budget environment. How can a survey committee foresee such an outcome? A NASA representative said that the 2000 survey had not helped NASA evaluate the opportunity cost of the JWST when it started to overrun its budget significantly, nor did it provide guidance on whether there should be a point in JWST costs beyond which its priority should change vis-à-vis the other astronomy program elements.

Many participants noted that it is unlikely that one can get credible cost estimates until a PDR is completed. This poses a dilemma, because most projects that are considered in a decadal survey are far from the PDR stage. Indeed, they are more likely to be mission concepts than defined missions at that time. Some speakers suggested that the surveys should either insist on better cost estimates or set objectives and define cost caps tied to a level of effort determined by science priorities. Getting better estimates requires that a mission be well defined, which can be very time consuming to achieve. Setting objectives and cost caps is more feasible and provides agencies with mission design flexibility.

Independent cost estimates, it was agreed, would add value to surveys, and having a cost uncertainty index would help define risk of cost growth. Cost estimates tend to be treated as gospel truth and surveys need to be very careful with them. Indeed, cost ranges are preferred if there are doubts. NASA and NSF representatives said that life-cycle costs are clearly becoming more important these days and must be considered by a survey if possible. Several agency representatives added that while the survey committees do not need to be project managers, they do need to help establish the scope of missions and provide rough guidance on what to do if there are problems. Uniform cost-estimating tools should be used within a survey so as to facilitate cost comparisons between initiatives, and uncertainties should be identified wherever possible (i.e., estimate confidence levels and ranges rather than specific costs).

INTERAGENCY COLLABORATION

Panel members were asked to consider how interagency plans, missions, and mission opportunities should be treated in decadal surveys. There were many examples of interagency projects that became very problematic in surveys. Most notably, while both NASA and NOAA support climate observations on NPOESS, NPOESS is strongly driven by DOD, which has a compelling interest in short-term meteorology but relatively little interest in climate change research. Sometimes important agencies have not even been part of the survey sponsorship—for example, DOE was not a sponsor of the astronomy and astrophysics survey in 2000. There was also much discussion about the disconnect between NASA and NOAA: The former focuses on research, the latter on observational monitoring. Yet, speakers noted that NOAA cannot really perform its observational mission without NASA research, and NASA either has failed to see the value of long-term monitoring for science or has double standards, in that HST, which is a pure science mission, has monitored the skies for more than 16 years. There are no clear lines between research and operations for either NASA or NOAA.

INTERNATIONAL COLLABORATION

Participants suggested that international collaboration is good and that many missions have been enabled or enhanced by international participation. However, they also suggested that the management complications that occur in such collaborations might limit any cost savings, as might the extra demands arising from International Traffic in Arms Regulations restrictions, visa restrictions, international coordination, and differences in the way that different foreign agencies make plans and budget commitments. Speakers cited the need to avoid duplicating or competing with foreign missions, and they noted that international participation might help stabilize a program (because, for example, an agency would be reluctant to drop a project with international partners).

Appendixes

A

Workshop Agenda

TUESDAY, NOVEMBER 14, 2006

Workshop Session 1 (Moderator: Lennard Fisk)

1:15 pm Opening remarks—Lennard Fisk

- Welcome, background, and objectives and structure of the workshop

1:30 pm Retrospective summaries of the five recent surveys by representatives of the survey committees—survey approaches, notable successes and difficulties

Survey Committee Panel

Astronomy and Astrophysics—Christopher McKee
Solar System Exploration—Michael Belton
Solar and Space Physics—Louis Lanzerotti
Earth Science and Applications from Space—Berrien Moore
Connecting Quarks and the Cosmos—Michael Turner

Panelists summarize

- How the survey was organized,
- Means and extent of community involvement,
- Sources of input,

- Agencies addressed by the survey,
- Impacts,
- Problems, and
- Approaches recommended for future surveys.

3:50 pm Client perspectives on past surveys—views from agencies and Congress

Panel on Agencies

NASA—Mary Cleave
NSF—Judith Sunley (via video conferencing)
NOAA—Mary Kicza
OSTP—Jon Morse

Panel on Congress

William Adkins
Johannes Loschnigg
Robert Palmer

Panelists discuss views about past surveys with respect to the following attributes:

- Utility,
- Impacts,
- Limitations, and
- Problems.

5:30 pm Adjourn

WEDNESDAY, NOVEMBER 15, 2006

Workshop Session 2 (Moderator: Tom Young)

8:30 am How can surveys deal with costing, technology readiness and risk, and executability? Panelists give perspectives from NASA, industry, and the science community.

Cost and Technology Assessment Panel

Steven Battel
John Casani

Noel Hinners
Bruce Marcus
George Paulikas

Panelists comment on issues and potential future approaches in dealing with the following:

- How does a survey committee determine the level of cost and risk for candidate missions that have not yet been fully defined?
- How does a committee define an affordable decadal plan?
- Can a decadal survey be stable for 10 years?
- What similarities and distinctions are there between planning for NSF ground-based facilities and space missions?

General discussion

Noon Lunch

Workshop Session 3 (Moderator: Jacqueline Hewitt)

1:15 pm Basic assumptions underlying surveys

Panel

Daniel Baker
Joseph Burns
Megan Urry
Warren Washington

Panelists comment on issues and potential future approaches in dealing with the following:

- *Sacrosanct programs.* Should certain programs be taken off the table for discussion in a survey? What kinds and why or why not?
- *Legacy projects.* How should surveys deal with priorities from prior surveys?
- *Time horizon(s).* Is a decade too short or too long? What are appropriate timescales for consideration?
- *Community buy-in.* How much and what kind of community buy-in should be expected? How should it be obtained? How should it be communicated?
- *Stakeholder expectations.* Who are the audiences for the surveys, and what do they (or should they) expect of a survey?

- *Science vs. applications.* Are the considerations for science different from those for applications?
- *Advice to multiple agencies.* Are there special considerations for dealing with multiple agencies?
- *The meaning of independence, objectivity, and ownership.* How should surveys deal with independence and objectivity in the face of vested interests, advocacy, and horse trading?

General discussion

THURSDAY, NOVEMBER 16, 2006

Workshop Session 4 (Moderator: Ed Stone)

8:30 am Lessons learned and implications for next surveys. Consideration of the following:

- *Resilience.* How can surveys be resilient in the face of likely but unpredictable (scientific, technical, programmatic, or political) developments?
- *Queuing.* Should surveys recommend specific mission queues or should queuing be left to agency decision makers? Under what conditions is one approach preferred over another?
- *Balance.* How should surveys treat priorities for missions vs. priorities for a mix of mission sizes?
- *Costing.* How, and how explicitly, should surveys handle costs? What factors should be considered? Where should cost data come from?
- *Technology readiness and risk.* How, and how explicitly, should surveys handle technology readiness and risk?
- *Executability and realism.* How can surveys be as realistic as possible about feasibility and executability? How much should recommendations push the programmatic, technological, and budget envelopes? Should there be multiple budget scenarios?
- *Priorities.* Should or can surveys produce single integrated priority lists, or are parallel lists more reasonable and useful? Should surveys prioritize only science or both science and missions?
- *Portfolio mix.* How, and how explicitly, should surveys handle portfolio balance? Should all elements of a program and their relative sizes be scrutinized by surveys?
- *Timing.* How often should surveys be conducted? How long should they take, and can they be accelerated? When do we know the time has come to start a new survey?

- *International perspectives.* How should international plans, missions, and mission opportunities be treated in surveys?
- *Committees and staffing.* What are key considerations for constituting a survey committee and staff? What are provisions for ensuring expertise, independence, community and agency buy-in, and efficiency?

Synthesis Panel

Spiro Antiochos
Charles Bennett
Judith Curry
Laurie Leshin
Government representatives

11:00 am Summary synthesis presentation (Stone)

Noon Workshop adjourns

B

Participant Biographies

LENNARD A. FISK, *Chair*, is the Thomas M. Donahue Distinguished University Professor of Space Science in the Department of Atmospheric, Oceanic, and Space Sciences at the University of Michigan. He is an active researcher in both theoretical and experimental studies of the solar atmosphere and its expansion into space to form the heliosphere. From 1987 to 1993, Dr. Fisk was the associate administrator for space science and applications and chief scientist of NASA. From 1977 to 1987, Dr. Fisk served as professor of physics and vice president for research and financial affairs at the University of New Hampshire. He is a member of the board of directors of the Orbital Sciences Corporation and co-founder of the Michigan Aerospace Corporation. He is currently chair of the Space Studies Board.

A. THOMAS YOUNG, *Vice Chair*, is retired executive vice president of Lockheed Martin. Mr. Young previously was president and chief operating officer of Martin Marietta Corp. Prior to joining industry, Mr. Young worked for 21 years at NASA, where he directed the Goddard Space Flight Center, was deputy director of the Ames Research Center, and directed the Planetary Program in the Office of Space Science at NASA headquarters. Mr. Young received high acclaim for his technical leadership in organizing and directing national space and defense programs, especially the Viking program. He is currently vice chair of the Space Studies Board, and he served on the recent decadal survey steering committee for solar system exploration.

JACK D. FELLOWS, workshop rapporteur, is vice president for corporate affairs at the University Corporation for Atmospheric Research (UCAR) and the director of UCAR's Office of Programs. Before joining UCAR in 1997 he spent 13 years at the Office of Management and Budget overseeing budget and policy issues related to NASA, the National Science Foundation, and federal-government-wide R&D programs. In 1984, he spent a year in the U.S. Congress as the American Geophysical Union's (AGU's) Congressional Science Fellow. He is a member of the Space Studies Board.

JOSEPH K. ALEXANDER, workshop study director, is a senior staff officer with the Space Studies Board, where he served as director from 1998 to 2005. He earlier was deputy assistant administrator for science in the Environmental Protection Agency Office of Research and Development, associate director of space sciences at NASA Goddard Space Flight Center, and assistant associate administrator for space sciences and applications in the NASA Office of Space Science and Applications. Other positions included deputy NASA chief scientist and senior policy analyst at the White House Office of Science and Technology Policy. Mr. Alexander's own research work has been in radioastronomy and space physics.

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JOSEPH F. VEVERKA is a professor of astronomy and chair of the Astronomy Department at Cornell University. His research focuses on the use of spacecraft imaging data to identify the important processes that have affected the evolution of small bodies in the solar system. Recent subjects of his research include the asteroids Gaspra and Ida, Ida's satellite Dactyl, the asteroid 433 Eros, Mars, Neptune's moon Triton, and the polar caps of Jupiter's Galilean satellites. He is a member of the Space Studies Board.

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Excerpts from Presentation by Jon Morse, Office of Science and Technology Policy

The Role of NRC Decadal Surveys in
Prioritizing Federal Funding for Science & Technology

Jon Morse
Physical Sciences and Engineering
Office of Science and Technology Policy
Executive Office of the President

Beneficial Aspects of NRC Decadal Surveys

- Community-based documents that provide consensus views of frontier science opportunities for maintaining the nation's scientific leadership
- Provides for each field a single, well-respected source for community priorities and the scientific motivations to the agencies, OMB, OSTP, and the Congress
- Limits the range of activities to consider for funding
  —Cost estimates, technical risk assessments, and technology roadmaps aid in budget planning

Issues and Concerns with NRC Decadal Surveys

- Prioritizing specific projects can become static and inflexible, making it nearly impossible to account for project setbacks, new discoveries, changing budgetary circumstances, etc.
- Technical risks are often not well known or stated clearly
- Cost estimates have often been inaccurate
  —Project cost estimates are too low and do not reflect total life-cycle costs
- Recommended project portfolios cannot fit in any realistic budget scenario (unrealistic expectations)
  —Small, medium, and large projects are not compared
  —Surveys often do not address how projects should be phased, individually or relative to one another
- Surveys usually assume only growth in the number and scale of facilities and missions and do not identify offsets in the existing portfolios to enable new initiatives

What Is Most Useful for Making Decisions?

- Frame the discussion by identifying the key science questions
  —Focus on what you want to do, not on what you want to build
  —Discuss the breadth and depth of the science (e.g., impact on our understanding of fundamental processes, impact on related fields and interdisciplinary research, etc.)
- Explain what measurements and capabilities are needed to answer each question
- Discuss the complementary nature of initiatives, relative phasing (domestic and international context)
  —How do various past, present, and future measurements and facilities work together to answer the questions?
  —What roles do/could private, interagency, and international partnerships play?

- Reporting by capabilities (e.g., wavelength range and in situ vs. remote sensing) is not useful for policy and budget planning

Suggested Improvements

- Establish science and project priorities in the broad context of past, present, and future projects and changing conditions
- New initiatives, upgrades and/or recapitalizations
- Establish relative priority amongst new initiatives, projects currently under development (e.g., from previous surveys), operating projects, R&A, PI-led projects, and technology/R&D investment needs
- Prioritize across all initiatives vs. grouping into small, medium, large. That is, remove ambiguities about what is meant by "a balanced program"
- Explain the associated risks (technical, dependencies on other projects)
  —Assume that large projects (>$1 billion) will need international support
- Provide tables that summarize key information about science and projects
- Provide timeline/phasing charts and diagrams for project portfolios under various budget scenarios
- Consider recruiting nonspecialists or even nonscientists for committees to aid in communicating societal benefits (e.g., interdisciplinary aspects, education, workforce training, public outreach)

Managing Expectations

- Acknowledge stewardship role in taxpayer investment
- Identify highest priority activities but within a framework that allows flexibility to react to new scientific opportunities
- Use order-of-magnitude life-cycle cost estimates instead of specific (often underestimated) construction costs or costs by decade
- Explain how circumstances (e.g., project overruns, changing budget forecasts, phasing with other projects, new discoveries) would change priorities
- Consider multiple realistic budget profiles and what science various profiles would buy
  —Work with agencies, OMB, Congress to define constraints
  —Macrobudgetary pressures are expected to increase during the next decade, so flat budget projections may actually be optimistic
  —Also need to consider project terminations that allow new initiatives to move forward (part of Decadal Survey or subsequent Senior Review process)