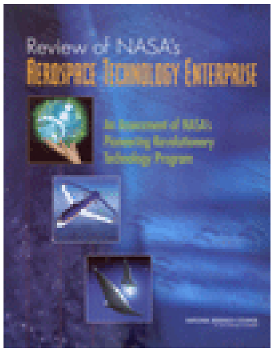

Review of NASA's Aerospace Technology Enterprise: An Assessment of NASAÂ's Pioneering Revolutionary Technology Program

Committee for the Review of NASA's Pioneering Revolutionary Technology(PRT)Program, National Research Council.

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Review of NASA's Aerospace Technology Enterprise

An Assessment of NASA's Pioneering Revolutionary Technology Program

Committee for the Review of NASA's
Pioneering Revolutionary Technology (PRT) Program

Aeronautics and Space Engineering Board

Division on Engineering and Physical Sciences

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Preface

The Committee for the Review of NASA's Pioneering Revolutionary Technology (PRT) Program of the National Research Council (NRC) and its three supporting panels have completed an approximately 20-month-long study evaluating the technical quality of the National Aeronautics and Space Administration's (NASA's) PRT program. The statement of task for this study is given in Appendix A. The study was sponsored by NASA and conducted by a committee and three supporting panels appointed by the NRC (see Appendix B for biographies of committee and panel members). The Office of Management and Budget (OMB) requested the review and assisted in the formulation of the statement of task.

This report provides a technical assessment of the quality of the PRT program and its components and offers recommendations for improving the program. The committee and panels note that they refrained from drawing any conclusions on matters of budget or recommending increases in budget levels. While some areas may suffer from a lack of critical mass, recommendations for increased resources to address the problem are of little value to management and have been avoided. The committee and panels also refrained, as much as possible, from commenting on matters related to programmatics and program organization unless a link could be established between these concerns and technical quality, portfolio management, or interaction within NASA and with the external technical community. NASA's Aerospace Technology Advisory Committee (ATAC) and its PRT subcommittee hold an annual relevance and programmatic review for the PRT program.

The committee and panels did not assess other programs within NASA on which the PRT program and its portfolio depend or other programs within NASA that research similar technology areas. The committee and panels did recommend when these programs should be integrally connected and the PRT portfolios managed with the global NASA investment in mind.

I wish to take this opportunity to thank the chairs and members of the three supporting panels for their leadership, detailed assessments, and commitment of time to the review. Their input has been vital to the quality of the entire review. On behalf of the committee and panels, I would also like to thank the various NASA program managers and technical staff for their cooperation in providing the information necessary to complete the review and in hosting our panel members at various site visits and for their open discussion during these opportunities. We also thank those who took the time to participate in committee and panel meetings and provide background materials. Finally, this study and the final report would not have been possible without the expert support of the NRC staff. Their dedication to keeping the review on track deserves special recognition and thanks. Thanks go especially to Karen E. Harwell, study director, for her professional steering of the overall committee effort as well as her support to the ECT panel, and to Douglas H. Bennett for his support to the CICT and ECS panels.

Raymond S. Colladay, *Chair*
Committee for the Review of NASA's Pioneering Revolutionary Technology (PRT) Program

Acknowledgment of Reviewers

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

Dwight Abbott, Aerospace Corporation (retired),
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Norman Sleep, Stanford University,
Patrick Stadter, Johns Hopkins University Applied Physics Laboratory,
David Waltz, NEC Research Institute, Inc. (retired), and
Mary Young, HRL Laboratories.

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

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APPROACH TO ASSESSMENT

The Committee for the Review of NASA's Pioneering Revolutionary Technology (PRT) Program and its three supporting panels were charged by the National Aeronautics and Space Administration (NASA) with assessing the overall scientific and technical quality of the PRT program and its component programs, along with their associated elements and individual research tasks (see Figure ES-1). Major issues addressed in the review include (1) research portfolios, (2) research plans, (3) technical community connections, (4) methodologies, and (5) overall capabilities. As reflected in the organization of the report, a two-pronged assessment was developed. Each panel provided a detailed assessment of the program under its purview, which was refined and updated over the course of the review. The committee, composed mainly of representatives from each panel, integrated and evaluated the panel results and provided top-level advice on issues cutting across the entire PRT program.

The committee's overall assessment of the research within PRT was based on the individual (and essentially independent) assessments of three supporting panels—the Panel on Computing, Information, and Communications Technology (CICT), the Panel on Engineering for Complex Systems (ECS), and the Panel on Enabling Concepts and Technologies (ECT). Individual research tasks judged by the committee and panels to be world-class met the following criteria: (1) they gave evidence of productivity (i.e., mission-accepted technology, publications, industry-accepted software, presentations, patents); (2) they exhibited strong linkage at the task level to actual flight projects, flight engineers, or science customers; (3) they possessed connectivity with external research communities; and (4) they were recognized by external peers as an authority in the subject matter. In some cases, excellence was also observed when basic research, facilities, systems analysis, flight integration, and testing and evaluation were vertically integrated or when programs had achieved success over a period of 10 to 15 years and continue to do so.

Key issues, findings, and recommendations relating to both the overall PRT program and its three component programs are presented below. The main text offers discussion, findings, and recommendations in addition to those highlighted here.

OVERALL ASSESSMENT

While there are important concerns about some management practices within the PRT portfolio, the committee found that the majority of PRT research consisted of good work that is important to the future of NASA and the nation. Ten percent of the individual research tasks were judged to be work of the highest quality, representing truly world-class endeavors. The

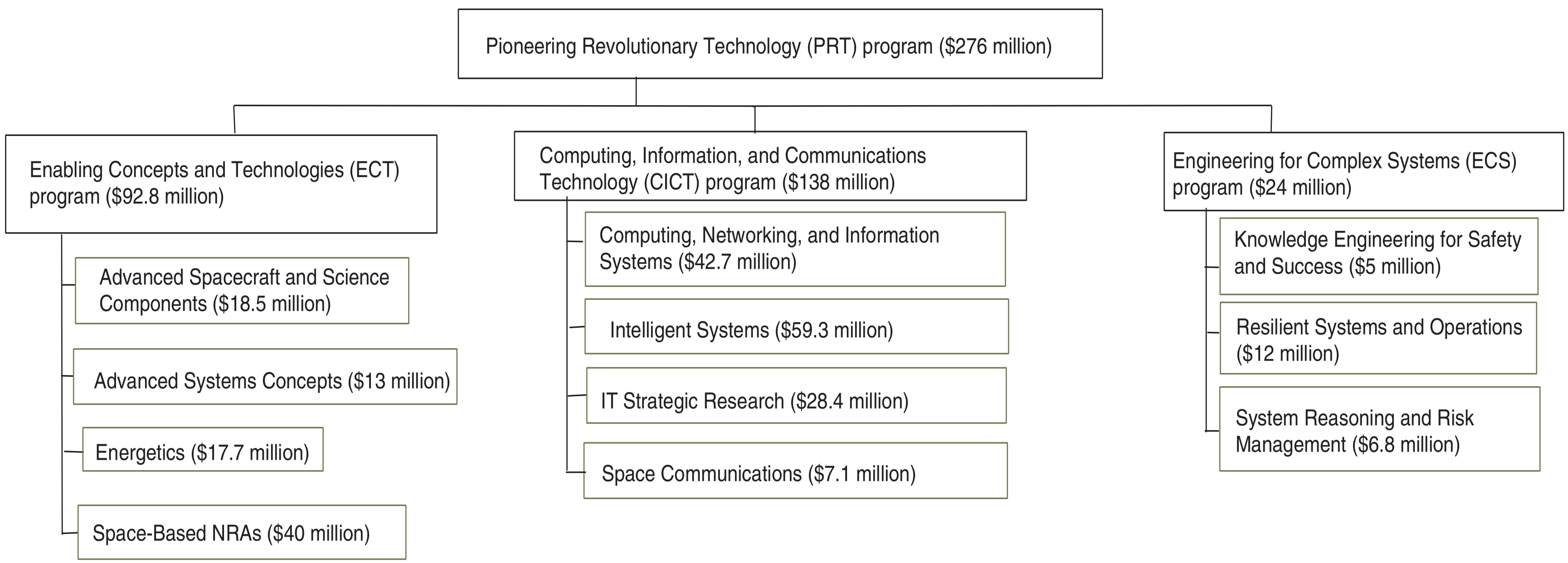

FIGURE ES-1 Pioneering Revolutionary Technology (PRT) program organization and FY2002 budget. Information based on June 10-13, 2002, meeting presentations and subsequent discussions with program managers. Program totals are not always correct because some project management budgets are not shown on this chart. Projects shown for ECT do not total to $92.6 million owing to a set of $3.6 million congressional earmarks not shown on the chart. SOURCE: Andrucyk (2003), Moore (2002, 2003), Tu (2002), and Gawdiak (2002).

committee and panels recommended that another 10 percent of the program's research tasks be discontinued or transitioned to mission applications. Tasks marked for transition are typically of excellent quality and involve successful work ready to be funded by a NASA mission or external partners. Tasks marked for discontinuation were identified primarily based on a judgment about the relative quality of the work or its value to NASA and alignment (or lack thereof) with PRT program goals. With 80 percent of the program being of good quality, but not world-class, the opportunity exists to maximize contributions from PRT program research by focusing more attention on several issues, including the need for research to be more results-oriented, more pervasive use of systems analysis, further encouragement of external peer review, and increasing collaboration between outside experts and the program.

PROGRAMWIDE COMMON THEMES

The committee noted six themes recurring across the entire PRT program that, if addressed, would strengthen the program: systems analysis, benchmarking and metrics, external peer review and competition, stability and continuity, research portfolio balance, and technology transition.

Systems Analysis

A crucial part of portfolio management, systems analysis underlies competitive task selection and ongoing refinement and redirection as technical progress is made in a program. Systems analysis also leads to an awareness of the system-level impacts of individual technologies under development. The committee observed gaps in system-level awareness and systems analysis capability throughout the PRT program, from top to bottom. Methods for risk assessment were neither widely used nor well understood. Yet, pockets of systems analysis were found within the program, typically in the areas of excellence.

Systems analysis capability that covers a range of fidelity—from back-of-the-envelope to refined parametric excursions of specific point designs—should be employed throughout the PRT program. Awareness of system-level impacts should be encouraged down to the level of individual tasks and researchers as a mechanism for ensuring that research goals retain their relevance. Such analyses should vary in complexity: In some cases, a simple, first-order calculation suffices, but in others a more rigorous state-of-the-art analysis is needed.

During the course of the review and in response to the committee's interim report (NRC, 2003), the PRT program made several changes in the area of systems analysis. The ECT program's Technology Assessment Analysis (TAA), although its planned funding was cut by approximately one-half, is focusing its work on four mission-based pilot studies chosen by the various enterprises within NASA. However, much additional work is necessary to develop a pervasive tool set with which to analyze technology portfolios and systems issues. The CICT program has filled a position responsible for program-level coordination of CICT system analysis activities and specific impact assessments (Tu and VanDalsem, 2003). However, because these efforts are so new, the committee cannot comment on their quality or predict their eventual success.

Finding: Gaps in the awareness of potential system-level impacts of individual technologies and in the use of systems analysis for research and portfolio management were observed throughout the PRT program. Further emphasis and strengthening are necessary in this area.

Recommendation: Systems analysis should be strengthened as a crucial part of the portfolio management and project selection process to support investment decisions in the technology areas needing development. This process should recognize the priorities NASA has set for its missions and the potential impact the research projects have on enabling and enhancing those missions. The process should also be applied to individual tasks and used by individual researchers as a mechanism for ensuring that research goals retain their original desired relevance. However, it should not be so rigid as to disallow serendipity and ideas of opportunity.

Benchmarking and Metrics

Benchmarking establishes quantitative goals or expectations that will serve as technical measures of success. These objective goals are expressed at the discipline, component, subsystem, and system levels, tied together by systems analysis. Excellent projects and tasks within the PRT program have always developed methodologies and goals from meaningful technical benchmarks and subjected their research progress to external assessment with appropriate metrics. The benchmarks were supported by analyses, where appropriate, and developed from basic scientific principles.

Each program element and task lacking them should establish technical benchmarks that are supported by analyses from basic principles. These metrics should be tempered with realistic engineering considerations and used to devise consistent, science-based research methodologies. Used correctly, these metrics can enable a useful assessment of long-term progress and results in the tasks, element, and projects where they are applied.

Finding: Tasks within the PRT program that developed methodologies and goals from specific technical benchmarks produced excellent work.

Recommendation: Each project, element, and task within the PRT program should establish technical benchmarks to enable assessment of progress and results. These benchmarks should include measurable, objective targets for research and should be developed in the context of the research's application.

External Peer Review and Competition

Interaction with external peers comes in a number of different forms, all of which should be encouraged throughout the research life cycle. Before research is initiated, external peer reviews are used fairly effectively in the competitively selected external portion of the PRT program but only sparingly in competitively selecting in-house research projects. Furthermore, as in-house research proceeds, there is limited involvement of external peers in evaluating its technical quality, which has implications for which tasks should continue and which should be redirected or terminated. The encouragement of peer-reviewed publication is inconsistent across the PRT program. As observed by the panels, there is a clear correlation between excellence and (1) tangible results presented in peer-reviewed publications or (2) manifested flight hardware and software.

The PRT program should institutionalize an external peer review process in all aspects of the research and technology enterprise: task selection (including the in-house portion of the program), ongoing progress reviews, and final assessment of results. It is important for the credibility and success of such reviews that an appropriate number of nonadvocate reviews and reviewers be used.

Finding: The PRT program makes little use of external peer review to select and evaluate the internal research program.

Recommendation: The PRT program should incorporate external peer review in all aspects of the program, including selection of internal research tasks, ongoing progress reviews and working groups, and final assessment of results.

Finding: The committee observed uneven involvement of researchers in publishing in peer-reviewed publications (either in journals or in the proceedings of peer-reviewed conferences).

Recommendation: NASA management should encourage peer-reviewed publication in landmark journals and peer-reviewed conference proceedings.

It is important for NASA to ensure that competencies in areas critical to NASA's mission (O'Keefe, 2002) be maintained, whether inside NASA or out. However, this does not mean that research in these areas should be exempt from competition, even for technologies where NASA is the only customer. In many cases, NASA will be the most appropriate place for such research, because of its unique capabilities, infrastructure, or superior skills—for example, space power and propulsion sources and autonomous robots. In such cases, NASA will be competitive. In other cases, academia, research laboratories, or industry may be better placed to pursue the research. Cooperation and teaming with external partners would enhance the quality of research in the program.

A systematic use of competitive processes and external peer reviews will ensure that the research is of the highest quality. However, even where research is done outside NASA, it is critical that NASA maintain subject matter expertise so it can effectively direct and interact with external researchers and integrate their work within NASA.

Finding: Broader external participation in the PRT program can enhance productivity, cooperative teaming, and quality of research. World-class programs within PRT exhibit these qualities.

Recommendation: All PRT research projects should be subject to competition. Internal and external competition should be separate to avoid conflicts of interest and ensure fairness and cooperation. Clearly, NASA must maintain internal technical expertise to ensure that research products are effectively transitioned and integrated.

Stability and Continuity

Changes in priority, organization, and funding will always occur and should be expected in a dynamic research program. However, the PRT program has undergone frequent and sometimes disruptive restructuring and reorganization. Some of these changes appeared to be a destructive force rather than a natural reallocation of resources as a part of research progress and maturation. For example, portions of the program have been managed by five different enterprises within NASA during the past 10 years (Moore, 2002). A link can be made between the stability of a project in this regard and the project's technical performance over a long time horizon. This is especially so for the more challenging basic research tasks, where fundamental advances in science and engineering are required.

The committee recognizes that certain program time spans are imposed by the Office of Management and Budget (OMB). However, the OMB constraints apply 5-year time horizons, whereas the past incarnations of the PRT program experienced reorganization at 1- and 2-year intervals. Even during the course of this 12-month review, portions of the PRT program were renamed and other portions reorganized in significant ways. NASA should strive to redirect programs based on sound technical issues and progress. NASA management and the technical team must share responsibility for providing stability and continuity in the face of inevitable change. A well-structured process is needed for selecting and maturing technology through development and transition to application. Such a process was noted in the Advanced Measurement and Detection element in ECT.

Finding: The PRT program components have undergone frequent and sometimes disruptive restructuring and reorganization.

Recommendation: To provide stability and continuity despite inevitable program changes, NASA should further develop and utilize more structured processes for selecting and developing technology from basic research to application. Program redirection should be based primarily on technical issues and progress. Projects should be provided with stable funding and assured stable organization to the extent possible.

Research Portfolio Balance

The committee observed that the PRT program consisted of tasks apparently assembled from a bottom-up selection and lacking top-down connection to the NASA Strategic Plan (Goldin, 2000; O'Keefe, 2002). Clearly, the connection between the top-down, mission-driven technology needs of the NASA mission codes and the bottom-up technology planning must be tighter. While top-level PRT program goals and objectives (Hanks, 2002) are well connected to the NASA Strategic Plan, they are not generally well connected to the individual tasks or even, in some cases, to missions. This is due in part to the restructuring of the program and to an apparent lack of acceptance on the part of researchers of the NASA-wide strategic plan. This disconnect can be rectified by engaging individual researchers in a more collaborative planning process. Space Communications and Advanced Measurement and Detection are two areas (one a project, the other an element) where the top-down, bottom-up connection is strong.

Finding: The NASA strategic plan is not well connected top to bottom.

Recommendation: NASA should use a more collaborative process in strategic planning and the execution of goals in order to involve researchers, customers, and managers in the strategic planning process.

In an ideal collaborative planning process, technology development plans (including tasks, priorities, and investment levels) are created and accepted by all the stakeholders. Periodic reviews should be used to assess progress and make appropriate project adjustments. The design, execution, funding, and assessment of a research portfolio as substantial as that of PRT must weigh a number of factors to determine a good balance of projects and tasks to meet NASA's mission. There is no single best balance, and the definition of a tuned portfolio will change over time, but once the portfolio is defined through strategic planning and a competitive selection process that balances need and opportunity, further adjustments should be expected based on such factors as relative funding for the three programs, CICT, ECS, and ECT; the balance between fundamental scientific research and engineering, user-driven research; and the proportion of evolutionary (low-risk) versus revolutionary (disruptive, high-risk) research.

Determining an optimum balance among these factors is not possible until a well-defined method for developing a program architecture is in place. As a result, the committee felt it inappropriate to suggest such a balance. However, the committee did feel it appropriate to comment on the amount of revolutionary technology research in the program. The committee recognizes that a large portion of the PRT program appropriately contains evolutionary technology. Only a few stretch, high-risk research efforts were observed—those that, if successful, disrupt conventional thinking and open up new approaches, missions, and systems. Although the program is investing in some so-called revolutionary areas (such as nanotechnology and quantum computing), the committee notes that a research topic perceived as revolutionary does not necessarily mean that the research itself is of excellent quality or high potential relevance to NASA. Also, the committee noted that some excellent research very relevant to NASA missions is more evolutionary and supports a core technical competency that is unique to NASA capabilities and needs. For this reason, the committee urges NASA to select research projects on the basis of the quality of the research and its relevance to NASA, independent of whether it is perceived as revolutionary. That said, the committee also believes that the PRT portfolio should exhibit *more* tolerance for taking on stretch goals (properly grounded in physics) that could yield high-payoff results in areas where NASA can have a unique impact.

Finding: Few efforts within the PRT program were considered to be high-risk, high-payoff efforts. Most of the work, much of it high in quality, was evolutionary.

Recommendation: The PRT program should encourage more stretch goals in revolutionary areas that could yield high-payoff and mission-enabling results.

Technology Transition

The committee observed that some useful technology becomes caught between the end of PRT support (at a lower TRL) and the start of user support (at a mid- to high TRL). Every effort should be made to work with the user enterprises of NASA and industry to prevent such breaks in funding. As successful research efforts mature, transition funding should come jointly from PRT and the user enterprises or industry. Such cost-sharing of transitional research is a goal of the ECT program and is used quite frequently. This practice should be continued and expanded beyond ECT.

Finding: Promising technology often fails in transition, when the PRT program concludes, often with good reason, that it is mature enough for application but before a mission organization has accepted ownership.

Recommendation: Provisions for cost-sharing of transitional research between the PRT program and mission organizations at NASA and in industry should be pursued as an explicit milestone in the TRL maturation process.

PANEL ASSESSMENTS OF THE THREE PRT PROGRAMS

Computing, Information, and Communications Technology Program

The CICT panel found that the great majority of the work within CICT was good, NASA-focused research that should continue. Of 242 research tasks, 17 were highlighted by the panel as examples of world-class work. Four areas (comprising multiple tasks) were judged world-class: autonomous robots, planning and scheduling, software validation and verification, and space communications hardware. The panel also identified nine tasks that, for various reasons, were ready for transition out of the research and development funding line, were complete and should be discontinued, or should no longer be pursued.

In several instances, the CICT panel identified tasks that originally started as research and later produced very good and useful engineering or research tools. Once the tools were established, the task within CICT became one of providing a service by maintaining the tools for use by NASA as a whole. This practice should be discontinued, and the CICT program should make certain that mechanisms are in place to transition completed tasks to an end user.

The CICT panel believes that the current CICT program could benefit from a research program architecture as well as an architecture that identifies future targets. Such a program architecture would clearly identify what is included in a program and what is not, the relationships among the program components, and the principles and guidelines under which the components are to function.

The CICT panel also observed on numerous occasions a poor understanding of the requirements for the final application of the work being conducted. Also, the program should ensure that all tasks, elements, and projects have clearly defined measures of success.

CICT research in human-centered computing could be improved through better cross-center coordination and new research in distributed collaboration. Early in the review, the panel also found little evidence of the use of assessments based on cognitive human factors in the human-centered computing area. Program changes made after the committee's interim report (NRC, 2003) resulted in an improvement in this area. The emphasis on carbon nanotube basic research within the CICT nanotechnology effort should be periodically reevaluated to ensure that such research is relevant to the NASA mission.

The panel noted two gaps in the CICT computing research portfolio. NASA scientists and missions generate terabytes of data that must be globally distributed and analyzed. Initially, the CICT panel saw little or no research on the management of massively distributed data and found no work on the new software architectures needed for highly distributed processing (in both real-time and information systems applications). In response to the PRT committee's interim report, the CICT program has taken positive steps to address both issues (Tu and VanDalsem, 2003).

The qualifications of CICT's technical staff are very good. NASA should continue to ensure that it has expertise in all areas of research deemed critical, whether the work is performed internally or externally, and should strive to maintain a lead relative to industry and academia in areas critical to NASA's mission, such as autonomous robots; space communications hardware; planning and scheduling; and software validation and verification. The CICT panel was troubled by the varying levels of researcher awareness of others working outside the PRT program and outside NASA and of researcher collaboration and cooperation with them. For example, the high-performance computing research within CICT does not appear to exploit outside work. On the other hand, the software verification and validation team showed good awareness of work done outside NASA. Similarly, some outside researchers have a poor understanding of NASA's work, in part because NASA researchers do not publish their results in peer-reviewed journals often enough. NASA's robotics and software verification and validation teams are well known outside the agency; however, its efforts on parallel programming tools are not well known. CICT managers should continue to encourage close connections between its researchers and the external research community through peer-reviewed publication of research results, participation in and organization of major conferences and technical workshops, involvement as reviewers and editors for journals, and other similar efforts. As of April 2003, there were some indications that this is starting to take place. The panel encourages the CICT program to continue these efforts.

Finding: The overall CICT research portfolio is very good and supports NASA objectives. Four technology areas (comprising multiple tasks) in CICT were judged world-class: autonomous robots, planning and scheduling, software validation and verification, and space communications hardware.

Recommendation: To manage the technical quality of work more effectively so that research tasks are meaningful and on track, CICT management should ensure that each task has a clearly defined, realistic, yet challenging measure of technical success.

Recommendation: To expose the external NASA technical community to NASA-specific issues and provide maximum leverage for CICT-funded tasks, CICT management should strongly encourage task principal investigators to seek peer-reviewed publication in journals and in the proceedings of major conferences and workshops. CICT management should also organize and run technical workshops.

Engineering for Complex Systems Program

The ECS program is in a state of flux and is in the early stages of developing a critical mass—that is, becoming a large enough effort to make a difference within NASA and the external community—of research in programmatic risk management. However ECS does not have the resources to develop a comprehensive programmatic risk management program in the foreseeable future that would contribute to the comprehensive programmatic risk management approach that is under development and being applied by safety organizations within NASA. Such work is critical to NASA in light of the Mars exploration losses and the Columbia tragedy.

Over the course of the review, the ECS program worked to stabilize itself by downselecting to a core set of research tasks and pursuing those tasks consistently, as opposed to constantly reorganizing. These efforts to redirect the program have been appropriate given the importance of risk assessment and management to NASA's mission.

ECS work in individual tasks is, in general, considered good—even given the state of flux in much of the program. Of the 52 individual research tasks within the ECS program, 3 are examples of world-class work: Organizational Risk Perception and Management, Virtual Iron Birds, and Advanced Software Verification and Testing Tools. The ECS program appears to address the right problems through multidisciplinary research; however, there are also gaps that weaken the ECS portfolio.

The panel recommends that the ECS program increase its use of benchmarks—quantitative goals or expectations that serve as measures of technical success and progress—at the lowest practical organizational level. The ECS program should also carefully consider the system-level impact of the work being conducted.

The panel initially had concerns about the state of flux within the portfolio of the System Reasoning and Risk Management (SRRM) project. As presented to the panel in June 2002, the SRRM portfolio appeared to include mainly internal work and knowledge, with few signs that external work in risk management was being leveraged. As of April 2003, the SRRM project's rebaselined portfolio appeared to be appropriate given the limited amount of funding available. The ECS panel was encouraged by this significant improvement, since programmatic risk management research is critical to future NASA missions and has the *potential* to achieve cross-NASA applicability and national importance.

In the Knowledge Engineering for Safety and Success (KESS) project, developing the much-needed models of risk perception and management is challenging, and current efforts are commended by the panel. The Resilient Systems and Operations (RSO) project has top-quality researchers working on problems, but the panel has concerns about whether the right NASA-specific tasks are being pursued. The ECS program should explore the use of nonconventional software research, including dependable computing and static analysis, to help NASA reduce unproductive overlap in the current portfolios.

Finding: NASA has a critical need for a comprehensive risk management program that can be implemented throughout program life cycles. The ECS program should contribute to the development and application of such a program for NASA.

Recommendation: In light of the Mars exploration failures and the Columbia tragedy, the ECS program should aggressively contribute to a comprehensive programmatic risk management program that would develop the probability (with uncertainty delineated) of achieving each of the following system requirements:

- **System safety (probability of crew survival),**
- **Reliability (probability of system completing its designed mission),**
- **Performance (probability of achieving the design parameters of system performance),**
- **Cost of the program (probability of staying within the budget), and**
- **Schedule for system delivery (probability of meeting the schedule).**

Finding: The current ECS program, as formulated and funded, will not by itself develop a comprehensive programmatic risk management program in the foreseeable future, yet this ECS risk management work is important for NASA.

Enabling Concepts and Technologies Program

While the panel found that much of the FY2002 ECT program's portfolio was inherited in a piecemeal fashion from previous programs without a comprehensive strategy, it does note that NASA managers plan to develop future ECT portfolios using strategic planning tools and processes. The panel supports such a systems approach to portfolio management.

Most of the tasks within the ECT program were deemed either good or excellent on an individual basis. ECT panel members judged approximately 20 percent of the ECT program tasks as world-class. The Energetics project had seven tasks of world-class quality (27 percent of its slate of tasks). The Advanced Measurement and Detection (AMD) element had eight world-class tasks (24 percent of the AMD tasks). Revolutionary and world-class areas of research noted by the panel within the ECT program are radio-frequency/terahertz (RF/THz) and focal planes for astrophysics and planetary exploration. Other areas of world-class excellence have been successfully transitioned to missions, including the microshutter and microthermopile sensor arrays and electric propulsion. Within the Resilient Materials and Structures (RMS) element, two tasks were found to be of world-class quality, and within the Distributed and Micro-Spacecraft (D&MS) element, three tasks were considered world-class. The Space Environmental Effects (SEE) element provides a unique and much-needed service to the spacecraft design community. Conversely, the panel determined that several ECT research tasks should be considered for discontinuation or transition.

The panel did not make a specific judgment on the Technology Assessment Analysis (TAA) element within the Advanced Systems Concepts project of the ECT program because the TAA is so new. However, there is concern that although the type of research in this program element is crucial to the PRT program and possibly to all of NASA, it is not receiving the emphasis and technical direction it needs, and appropriate attention should be paid to it.

Consistently lacking across the ECT program was an expectation of peer-reviewed publication. NASA should maintain an environment that nurtures and rewards intellectual leadership and technical excellence. Expectations should be aligned with metrics of excellence and leadership in the broader technical community—for example, the acceptance of work in refereed publications and the receipt of patents. These metrics should be looked at in addition to, not in place of, metrics for progress toward technology maturation and transition to NASA flight programs. The highest-quality tasks managed to do all these things.

The facilities used by the ECT program are excellent. NASA should strive to maintain several that are world-class, including the Electron-Beam Lithography Laboratory at the Jet Propulsion Laboratory, the Polymer Rechargeable Battery Laboratory at NASA Glenn Research Center, and the electric propulsion and photovoltaic test facilities at NASA Glenn. Panel members also observed that the colocation of basic research, systems analysis, engineering, testing and evaluation, and flight qualification improves quality and keeps research focused. This was evident for both the AMD element and the Energetics project. The panel recommends that researchers, test facilities, and systems analysis capabilities be vertically integrated wherever possible, at least virtually if colocation is not possible.

Connectivity of the ECT program to other areas within NASA and to the broader technical community varied from project to project. There were specific examples of good teaming between NASA researchers and external partners in the SEE element and the Energetics project. The panel recommends that this type of teaming and collaboration be encouraged and expanded whenever possible. The panel observed, however, a lack of connectivity between the nanotechnology, microsensors, distributed and microspacecraft, and intelligent systems work in the PRT program overall. NASA should take actions to ensure value-adding communication between these programs.

About 40 percent of the ECT program is funded through Cross-Enterprise NASA Research Announcements (NRAs). While the panel views this type of competitive solicitation as a valuable incubator for technology development, the NRA solicitation rules prevented NASA researchers and NRA winners from working together. Upon formation of the ECT program, NRA management was transferred from the Space Science Enterprise to the Aerospace Technology Enterprise. This management change, coupled with the broad focus of the announcement and the absence of a clear mechanism for evaluating progress during the award's duration, has meant that Cross-Enterprise NRA research is generally not integrated with NASA programs and centers. This effect may also be due in part to the competitive environment that prevails between the awardees and NASA researchers who did not win awards.

Finding: The panel judged approximately 20 percent of the ECT program to be world-class. Specific areas of world-class quality within the ECT program include the radio frequency/terahertz thrust, the focal plane thrust, the microshutter arrays, and the microthermopile arrays in Advanced Measurement and Detection; electric propulsion, advanced photovoltaics technology, and advanced energy

storage in Energetics; modulated sideband technology and formation flying in Distributed and Micro-Spacecraft; and gossamer structure characterization in Resilient Materials and Structures.

Finding: The Technology Assessment Analysis element within the ECT program is an important area for NASA and one where it should continue investment. However, the panel feels that the area has not been given the emphasis it needs.

Finding: The ECT panel observed a general lack of integration of Cross-Enterprise NRA research with NASA programs and centers, limiting the overall return on investment.

Recommendation: The research performed under the Cross-Enterprise NRA contracts should be managed as an integral part of in-house PRT research activities, with individual program elements being responsible for the performance of the contract, including contract deliverables and milestone monitoring.

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1

Introduction

BACKGROUND

NASA's Aerospace Technology Enterprise (Code R) contracted with the National Research Council's (NRC's) Aeronautics and Space Engineering Board (ASEB) to provide biennial assessment of NASA's Aerospace Technology Enterprise programs—the Pioneering Revolutionary Technology (PRT) program, the Revolutionize Aviation program, and the Space Launch and Transfer Technology program. The first review in the series is that of the PRT program group; other reviews will follow in the coming years. Programs within the PRT group are the Computing, Information, and Communications Technology (CICT) program, the Engineering for Complex Systems (ECS) program, and the Enabling Concepts and Technologies (ECT) program.

After most of the NRC's review of the PRT program had taken place, the Aerospace Technology Enterprise underwent a slight restructuring. NASA changed the name of the PRT program to the Mission and Science Measurements (MSM) theme in FY2003. The Revolutionize Aviation program was renamed the Aeronautics Technology theme. The Space Launch and Transfer Technology program included within the broader Space Launch Initiative theme has been reorganized for FY2004 under a new name: Next Generation Launch Technology program.

In this report, the committee continues to refer to the program as the PRT program since most of the work reviewed began under the PRT program and continues under the new MSM theme. Most changes in structure and content will be reflected in the ECT program in a later fiscal year. Appendix C provides an organizational and budget chart outlining the programs and elements of the PRT program during FY2002-2003.

APPROACH TO ASSESSMENT

A committee and three panels (one for each of the three subprograms of the PRT program) were formed by the NRC in May 2002. The membership of the committee and the panels includes a cross section of senior executives, engineers, researchers, and other aerospace professionals (see Appendix B). The committee and the panels were charged with independently assessing the overall scientific and technical quality of the PRT program elements (Appendix A). These assessments include findings and recommendations related to the quality and appropriateness of NASA's internal and collaborative research, development, and analysis. While the primary objective was to conduct peer assessments that provide scientific and technical advice, the committee and panels did offer programmatic ad-

NOTE: A listing of acronyms and abbreviations can be found in Appendix F.

vice when such advice followed naturally from technical considerations.

The committee and the three panels met at NASA Ames Research Center in Mountainview, California, June 10-13, 2002, for an overview of the PRT program and its various elements. Subgroups of panel members subsequently participated in laboratory site visits, teleconferences, and other information-gathering activities throughout the summer. (A list of committee and panel activities can be found in Appendix D.) NASA researchers submitted completed questionnaires describing individual research tasks funded within the program to be assessed by the panelists. The two questionnaires can be found in Appendix E. A total of 385 internal NASA research tasks were reviewed and 13 site visits were made.

In September 2002, each panel met in Washington, D.C., to reach consensus on observations, findings, and recommendations and to engage in an interactive dialogue with NASA program managers. Panel draft reports were then submitted to the committee. The committee met in Washington, D.C., on November 6-8, 2002, to discuss the panel findings, recommendations, and overarching issues and to engage in dialogue with NASA managers representing the PRT program. During this meeting equal amounts of time were given to (1) discussing the panels' assessments of the top-tier and bottom-tier work in their respective programs, normalizing the results of these two sets of work, and choosing efforts to be highlighted to NASA, and (2) pulling together a set of common issues that cut across the PRT program.

Because the research conducted under the PRT program is so diverse and in order to provide the best possible assessment of technical quality to NASA, the committee felt it should rely on the experts on the panels to assess the individual programs and their respective projects, elements, and individual research tasks. The committee's role was to integrate the results and provide overarching advice to NASA management. Following this meeting, the committee published a short report on its preliminary observations, findings, and recommendations (NRC, 2003).

A period of reevaluation was built into the review in order to provide NASA management with opportunities to address issues of importance that surfaced during the September panel meetings and to revisit programs in the midst of critical change. Selective site visits were carried out and additional information on program changes was obtained during the spring of 2003 (see Appendix D); the panel reports were then updated to reflect the new information. A final meeting was held May 6-7, 2003, in Washington, D.C., to finalize the committee's top-level findings and recommendations based on the site revisits and to complete the committee's report taking into account new information from the spring site revisits and updated panel reports.

REPORT ORGANIZATION AND DEVELOPMENT

This report focuses on two levels of assessment: (1) an overall evaluation of the technical quality of the PRT program and (2) an evaluation of individual programs and projects within PRT. Chapter 2 examines the overall quality of the PRT program and presents a series of overarching issues and recommendations for quality improvements. Chapters 3, 4, and 5 are reports from the three independent panels to the main committee. They provide individual assessments of the three PRT programs and more specific recommendations to technical managers within those programs. During the review process, as the panels became better acquainted with the contents and organization of the programs under their purview, it was determined that the panel reports would stand as individual reports of the panels. The three programs under review are very different in size, scope, research content, and organization. For example, the CICT program, at $139 million, funded 242 individual research tasks during FY2002. ECS, at $24 million, funded 52 tasks, and ECT, at $92 million, funded 91 in-house tasks and 111 external research awards that were selected and managed separately from the rest of the program. Each panel evaluated the individual research tasks funded by the specific program under its purview, providing a level of detail similar to that provided by the other two. Review results were coordinated, and top-tier and bottom-tier criteria were normalized at both of the committee meetings listed previously. This report presents an integration, evaluation, and summary of the efforts of the three individual (and essentially independent) panels and a top-level assessment of the entire PRT program.

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Overall Assessment of the Pioneering Revolutionary Technology Program

The observations and recommendations presented here are overarching issues of concern throughout the PRT program. Detailed assessments of the individual PRT programs (CICT, ECS, and ECT) can be found in Chapters 3 through 5.

OVERALL ASSESSMENT

The committee's overall assessment of the research within PRT was made based on information from the individual assessments of the three supporting panels. Tasks judged by the committee and panels to be world-class met the following criteria: (1) they gave evidence of productivity (publications, software, presentations, patents, mission-accepted technology); (2) they exhibited strong linkage at the task level to actual flight projects, flight engineers, or science customers; (3) they possessed connectivity with other research communities external to NASA; and (4) they were recognized by external peers as an authority in the subject matter. In some cases, excellence was also observed when basic research, facilities, systems analysis, flight integration, and test and evaluation were vertically integrated or when programs had achieved success over a period of 10 to 15 years and continue to do so. Exemplifying this long-term excellence were the Energetics project and the Advanced Measurement and Detection (AMD) element.

The panels and committee were very careful to use the descriptor "world-class" only when a project or task clearly met the set of criteria listed above and was clearly a leader in the field. The word "revolutionary" was used only in very specific instances, when it was perceived the work promised to provide leaps in capability or technology over current methods.

While there are some important concerns about management practices within the PRT portfolio, the committee found that most of the Pioneering Revolutionary Technology (PRT) research consisted of good work that is important to the future of NASA and the nation. Ten percent of the individual research tasks were judged as work of the highest quality, representing truly world-class endeavors. The committee and panels recommended that another 10 percent of the program's research tasks be discontinued or transitioned to mission applications. Tasks marked for transition are typically of excellent quality and involve successful work ready to be funded by a NASA mission or external partners. Tasks marked for discontinuation were identified primarily based on a judgment about the relative quality of the work or its value to NASA and its alignment (or lack thereof) with PRT program goals. With 80 percent of the program being of good quality, but not world-class, there is an opportunity for improving PRT program research by focusing more attention on several common issues, including the need for research to be more results-oriented, pervasive use of systems analysis, peer review, and increasing collaboration between outside experts and the program.

Finding: The committee judged approximately 90 percent of the PRT program to be good work, important to NASA and the nation. Of this 90 percent, 10 percent was deemed to be world-class.

COMMON THEMES

The committee noted six themes cutting across the entire PRT program where special attention would strengthen the program: systems analysis; benchmarking and metrics; external peer review and competition; stability and continuity; research portfolio balance; and technology transition.

Systems Analysis

A crucial part of portfolio management, systems analysis includes competitive task selection, ongoing refinement, and redirection as technical progress is made in a program. Systems analyses are engineering analyses that integrate the effects of specific scientific and engineering disciplines, components, and assemblies, and their interactions in order to predict the performance or otherwise explain the behavior of hardware (system) and enable trade studies to be performed on assumptions, boundary conditions, and other constraints. Systems analysis also leads to an awareness of the system-level impacts of individual technologies under development. The committee observed gaps in system-level awareness and systems analysis capability throughout the PRT program, from top to bottom. Methods for risk assessment of various types were neither widely used nor well understood. Yet, pockets of systems analysis were found within the program, typically in the areas of excellence. For example, the Energetics project within the ECT program has effectively used excellent-quality systems analysis for much of its work to guide research efforts toward the critical highest-payoff technical challenges on the system level. It is the committee's understanding that the Technology Assessment Analysis (TAA) process within the ECT program is being developed to address a portion of this need; however, there was no clear indication that the TAA, as structured for FY2003, could ever develop into a true portfolio analysis tool set.

Systems analysis capability that covers a range of fidelity—from back-of-the-envelope to refined parametric excursions of specific point designs—should be employed. Awareness of system-level impacts should be encouraged down to the level of individual projects and researchers to ensure that research goals retain their original desired relevance. Such analyses should vary in complexity; in some cases, a simple calculation suffices, but in others a more advanced state-of-the-art analysis is needed.

During the course of the review and in response to the committee's interim letter report (NRC, 2003), the PRT program has made several changes in the area of systems analysis. The ECT program's TAA, although reduced in funding by approximately one-half, is focusing its work on four mission-based pilot studies chosen by the various enterprises within NASA. However, much additional work is necessary to develop a pervasive tool set to analyze technology portfolios and systems issues. The TAA effort and other systems analysis issues are discussed in further detail in Chapter 5. The CICT program has recruited an individual who will be responsible for program-level coordination of CICT system analysis activities and specific impact assessments (Tu and VanDalsem, 2003). Other more specific impact assessments and related efforts are also under way. Because these efforts are new, the committee cannot comment on their quality or success.

Finding: Gaps in the awareness of potential system-level impacts of individual technologies and in the use of systems analysis for research and portfolio management were observed throughout the PRT program. Further emphasis and strengthening are necessary in this area.

Recommendation: Systems analysis should be strengthened as a crucial part of the portfolio management and project selection process to support investment decisions in the technology areas needing development. This process should recognize the priorities NASA has set for its missions and the potential impact the research projects have on enabling and enhancing those missions. The process should also be applied to individual tasks and used by individual researchers as a mechanism for ensuring research goals retain their original desired relevance. However, it should not be so rigid as to disallow serendipity and ideas of opportunity.

Benchmarking and Metrics

Benchmarking establishes quantitative goals or expectations that serve as technical measures of success. These objective goals are expressed at the discipline, component, subsystem, and system levels, tied together by systems analysis. Excellent projects and tasks within the PRT program have always developed methodologies and goals from meaningful technical benchmarks and subjected their research progress to external assessment using appropriate metrics. These benchmarks were supported by analyses, where appropriate, and developed from basic scientific principles. The Space Communications project within CICT is an excellent example of how setting and using proper metrics can enhance a research program. The project's tasks had clearly defined goals for even the most basic research. Both the Advanced Measurement and Detection (AMD) element and the Energetics project within ECT also exemplify this characteristic. Both have well-defined goals and objectives that derive from the program needs of the relevant mission within the associated NASA program office. For example, the various investigators leading superconducting-transition-edge, sensor-array research in AMD have taken the task's benchmarks from the Constellation X scientific measurement requirements defined by Code S.[1]

Each program element and task should, in conjunction with element and program managers, establish technical benchmarks that are supported by analyses from basic principles. These metrics should be tempered with realistic engineering considerations and should be used to devise consistent, science-based research methodologies. Used correctly, these metrics can enable a useful assessment of long-term progress and results in the tasks, elements, and projects where they are applied.

Finding: Tasks within the PRT program that developed methodologies and goals from specific technical benchmarks produced excellent work.

Recommendation: Each project, element, and task within the PRT program should establish technical benchmarks to enable assessment of progress and results. These benchmarks should include measurable, objective targets for research and should be developed in the context of the research's application.

External Peer Review and Competition

Interaction with external peers comes in a number of different forms, all of which should be encouraged throughout the research life cycle. Before research is initiated, external peer reviews are used fairly effectively in the competitively selected external portion of the PRT program but only sparingly, if at all, in competitively selecting in-house research projects. Furthermore, there is limited involvement of external peers in evaluating the technical quality of ongoing in-house work to decide what should continue, be redirected, or be terminated. Finally, as mentioned in the subsequent chapters on individual programs, the encouragement of publication in peer-reviewed technical journals is inconsistent. As observed by the panels, there is a clear correlation between excellent-quality work and tangible results presented in peer-reviewed publications and manifested in deliverable flight hardware and software. For example, in the Resilient Materials Structures (RMS) element within ECT, about 80 percent of the publications are from two of the nine tasks. Both tasks were judged by the panelists to be of excellent quality.

The PRT program should institutionalize an external peer review process in all aspects of the research and technology enterprise: task selection (including the in-house portion of the program), ongoing progress reviews, and final assessment of results. This peer review process would

- Increase the quality of program planning processes,
- Increase communication across groups within NASA,
- Provide another means of recognizing and rewarding research talent in NASA,
- Increase communication with researchers outside the agency, and
- Reduce unintentional overlaps of research with ongoing academic and commercial research.

It is important for the credibility and success of such a review that an appropriate number of nonadvo-cate reviews and reviewers be used.

[1]Further information on this example is available online at <http://constellation.gsfc.nasa.gov/docs/technology/sxt.html>, accessed August 8, 2003.

In response to the committee's interim letter report, the CICT program has placed more emphasis on having internal work peer reviewed by external experts in the field (Tu and VanDalsem, 2003). This is a positive step for the program; however, the procedure for the reviews has not been evaluated by the committee for effectiveness.

Finding: The PRT program makes little use of external peer review to select and evaluate the internal research program.

Recommendation: The PRT program should incorporate external peer review in all aspects of the program, including selection of internal research tasks, ongoing progress reviews and working groups, and final assessment of results.

Finding: The committee observed uneven involvement of researchers in publishing in peer-reviewed publications (either in journals or in the proceedings of peer-reviewed conferences).

Recommendation: NASA management should encourage peer-reviewed publication in landmark journals and peer-reviewed conference proceedings.

It is important that NASA maintain competencies in areas critical to its mission, whether inside NASA or out. This does not imply that research in these areas be exempt from competition, even for technologies where NASA is the only customer. In many cases, NASA will be the most appropriate place for such research because of its unique capabilities, infrastructure, or superior skills (e.g., radioactive power sources, autonomous robots). In such cases, NASA will be competitive. In other cases, academia, research labs, or industry may be better placed to pursue research in designated areas.

A systematic use of competitive processes and external reviews will ensure that the highest quality research is performed. However, even where research is done outside NASA, it is critical that NASA maintain a subject matter expertise in the relevant areas in order to effectively direct and interact with external researchers and integrate their work within NASA.

Finding: Broader external participation in the PRT program can enhance productivity, cooperative teaming, and quality of research. World-class programs within PRT exhibit these qualities.

Recommendation: All PRT research projects should be subject to competition. Internal and external competition should be separate to avoid conflicts of interest and ensure fairness and cooperation. Clearly, NASA must maintain internal technical expertise to ensure that research products are effectively transitioned and integrated.

Based on the premise that competition for resources improves the quality and relevance of both in-house and external research, the committee developed the following peer assessment process as an example. The intent of this example is not to add to the administrative burden but to improve the technical quality and productivity of those programs that are already considered good but could be made better. In fact, this process could even replace or augment some of the internal review currently used by the program. The committee anticipates a process by which interactions with external peer reviewers will be welcomed by the technologists, providing valuable collaborative discussions. The peer process also benefits the junior researcher by exposing her or him to means by which established research leaders set and maintain high standards and construct pathways to achieve difficult goals. In addition, the process should bring a broader perspective on transferring technology to flight programs and techniques to research groups. The process will also provide valuable input from the external scientific and technological community into how internal NASA research is prioritized and chosen.

The first step of such a process requires that NASA management set top-level goals and strategic objectives that establish expectations and specific directions for Space R&T consistent with NASA's Strategic Plan (Goldin, 2000; O'Keefe, 2002), just as they do now. That would be followed by an allocation of budget resources between in-house and externally funded research in categories that are broad enough to promote a healthy competition in ideas, concepts, and approaches. Following a broad announcement of opportunity, a separate competitive selection process would be followed for internally funded projects just as is done now for externally funded research. Nationally recognized technical experts from universities, industry, and other government laboratories and NASA personnel in other in-house organizations and enterprises would assess the proposals and report to NASA management on matters of technical quality and appropriateness of content compared with that of related work in their own institutions. Competing proposals would be rank ordered and funded by rank down to the limit of available resources. For example, if 20 percent of the available resources were competed for in this manner each year and the other 80 percent were allocated to the continuation of multiyear grants, there would be sufficient continuity from year to year to ensure stability, while promoting the infusion of new ideas and talent. The pace of the rolling competition would be guided by NASA management.

The committee does note that a one-size-fits-all process may not be appropriate for the PRT program due to the program's size and diversity of technology. The process should be adaptable over the wide range of programs and technical areas and include both basic researchers and hands-on technology developers.

Stability and Continuity

Changes in priority, organization, and funding will always occur and should be expected in a dynamic research program. However, the PRT program has undergone frequent and sometimes disruptive restructuring and reorganization, often based merely on advocacy hype. Some of these changes appeared to be a destructive force rather than a natural reallocation of resources as a part of research progress and maturation. For example, portions of the program have been managed by five different enterprises within NASA during the past 10 years. A link can be made between the stability of a project (or lack thereof) in this regard and the project's technical performance over a long time horizon. This is especially so for the more challenging basic research tasks, for which fundamental advances in science and engineering are required and long time horizons are necessary to adequately investigate and transition ideas.

The committee recognizes that certain current program time spans are imposed by the Office of Management and Budget (OMB). However, the OMB constraints apply 5-year time horizons, whereas the PRT program has experienced reorganization at 1- and 2-year intervals. Even more significant, during the course of this 12-month review process, the entire PRT portfolio was renamed the Mission Science and Measurements theme. Portions of the ECS program were in flux throughout the entire course of the review. Moreover, if current plans for the FY2005 ECT program are implemented, the program will have undergone three top-level organizational changes within the course of this review. While the committee understands that many of the research projects within these programs will continue, this is yet another example of constant churning in the program.

NASA should strive to redirect programs based on sound technical issues and progress. It should avoid organizational churning and stutter-step reprogramming motivated by advocacy or external pressure. NASA management and the technical team must together provide stability and continuity for the management of inevitable change. A well-structured process is needed for selecting and maturing technology through development and transition to application.

Projects in the PRT program show why a process for effectively managing change is needed. The Advanced Measurement and Detection (AMD) element within ECT is exemplary in its well-structured process for selecting and maturing technology through instrument development and transition to application. More detailed information on this process can be found in the Annex to Chapter 5, on the ECT program. This process has led to the successful integration of instruments in NASA missions despite the management and organizational change endured by the element. This element within ECT and other programs of excellence within NASA have on their own adopted management practices that can accommodate frequent reorganizations at the top. They have achieved progress in spite of those reorganizations, not because of them.

Finding: The PRT program components have undergone frequent and sometimes disruptive restructuring and reorganization.

Recommendation: To provide stability and continuity despite inevitable program changes, NASA should further develop and utilize more structured processes for selecting and developing technology from basic research to application. Program redirection should be based primarily on technical issues and progress. Projects should be provided with stable funding and assured stable organization to the extent possible.

Research Portfolio Balance

The committee observed that the PRT program tasks consisted of tasks apparently assembled from a bottom-up selection and lacking top-down connection to the NASA Strategic Plan (Goldin, 2000; O'Keefe, 2002). Clearly, the connection between the top-down, mission-driven technology needs of the NASA programs in the NASA mission codes (Codes M, S, U, and Y) and the bottom-up technology planning need to be better established. Top-level PRT and program goals and objectives (Hanks, 2002) are well connected to the NASA Strategic Plan; however, the program goals are not generally well connected to the actual individual tasks or even, in some cases, to missions. This is due, in part, to both the restructuring of the program and the apparent lack of acceptance of the NASA-wide plan by researchers. This disconnect can be rectified by engaging individual researchers in a more collaborative planning process. The Space Communications project in CICT and the Advanced Measurement and Detection element in ECT are two positive examples where the top-down, bottom-up connection is evident.

Finding: The NASA strategic plan is not well connected top to bottom.

Recommendation: NASA should use a more collaborative process in strategic planning and the execution of goals in order to involve researchers, customers, and managers in the strategic planning process.

In an ideal collaborative planning process, technology development plans (including tasks, priorities, and investment levels) are created and accepted by all the stakeholders. Periodic reviews should be used to assess progress and make appropriate project adjustments. Designing, executing, funding, and assessing a research portfolio as substantial as that of PRT must weigh a number of factors to determine the balance of projects and tasks that would best achieve NASA's mission. There is no single best balance, and the definition of a tuned portfolio will change over time, but once the portfolio is initially defined through a strategic planning and competitive selection process that balances need and opportunity, further adjustments should be expected after assessing such factors as the following:

- Relative funding levels for the three programs, CICT, ECS, and ECT.
- The balance between fundamental scientific research and engineering, user-driven research.
- The proportion of evolutionary (low-risk) and revolutionary (disruptive, high-risk) research.

An example of a potentially disruptive technology in the PRT program is CICT's work in neural net flight controls. The research has been demonstrated to work very well in a simulator; however, it is so novel and unusual that the regulatory processes to field it may be problematic. The AMD work in radio frequency/terahertz and focal planes for astrophysics and planetary exploration will open up new mission possibilities.

Determining an optimum balance among these factors is not possible until a well-defined method for developing a program architecture is in place. As a result, the committee felt it inappropriate to suggest a balance. However, the committee did feel it appropriate to comment on the amount of revolutionary technology research in the program. The committee recognizes that a large portion of the PRT program appropriately contains evolutionary technology. Only a few stretch, high-risk research efforts were observed—those that, if successful, disrupt conventional thinking and open up new approaches, missions, and systems. Although the program is investing in some so-called revolutionary areas (such as nanotechnology and quantum computing), the committee notes that a research topic perceived as emerging or revolutionary by the scientific and technical community does not necessarily mean that the research itself is of excellent quality or great potential relevance to NASA. Also, the committee noted that some excellent research, very relevant to NASA missions, is more evolutionary than revolutionary and supports a core technical competency that is unique to NASA capabilities and needs. For this reason, the committee urges NASA to select research projects on the basis of the quality of the research and its relevance to NASA, independent of whether it is perceived as revolutionary. That said, the committee also believes that the PRT portfolio should exhibit *more* tolerance for taking on stretch goals (properly grounded in physics) that could yield high-payoff results in areas where NASA can have a unique impact.

Finding: Few efforts within the PRT program were considered to be high-risk, high-payoff efforts. Most of the work, much of it high in quality, was evolutionary.

Recommendation: The PRT program should encourage more stretch goals in revolutionary areas that could yield high-payoff and mission-enabling results.

Technology Transition

The committee observed that some useful technology becomes caught between the end of PRT support (at a low technology readiness level [TRL]) and the start of user support (at a mid- to high TRL). Every effort should be made to work with the user enterprises of NASA and industry to prevent such breaks in funding. As successful research efforts mature, transition funding should come jointly from PRT and the user enterprises and industry. The committee notes that a few projects within PRT have an effective process for transitioning new technology to the successful production of mission hardware. The JPL autonomous robotics work in CICT and the AMD element in the ECT program both transition technology successfully. For AMD, an enduring, well-defined process exists that allows a natural transition through mid-TRL instrument development programs such as the Planetary Instrument Definition and Development Program (PIDDP) and the Instrument Incubator Program (IIP). Recent examples include the microshutter array that is now baselined for the future James Webb telescope and the microthermopile array for the Mars Climate Sounder instrument on the Mars Reconnaissance Orbiter. Cost-sharing of transitional research is a goal of the ECT program and is used quite frequently. This practice should be continued and expanded beyond ECT.

Finding: Promising technology often fails in transition, when the PRT program concludes, often with good reason, that it is mature enough for application but before a mission organization has accepted ownership.

Recommendation: Provisions for cost-sharing of transitional research between the PRT program and mission organizations at NASA and in industry should be pursued as an explicit milestone in the TRL maturation process.

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3

Report of the Panel on Computing, Information, and Communications Technology

INTRODUCTION

The Computing, Information, and Communications Technology (CICT) program is one of three programs under NASA's Pioneering Revolutionary Technology (PRT) program. The CICT program in turn comprises four broad, level 2 projects (see Table 3-1):

- Space Communications (SC) project
- Intelligent Systems (IS) project
- Information Technology Strategic Research (ITSR) project
- Computing, Networking, and Information Systems (CNIS) project

Each project is divided into level 3 elements, and those elements into tasks. The CICT program, funded at $138 million for FY2002, comprises 242 individual research tasks.

The goal of the CICT program is to "enable NASA's scientific research, space exploration, and aerospace technology missions with greater mission assurance, for less cost, with increased science return through the development and use of advanced computing, information and communications technologies" (Tu, 2002). The CICT program plans to accomplish this goal by

- Creating goal-directed, human-centered computer systems where the tools are more adaptive and computers can work collaboratively with humans,
- Enabling seamless access to NASA information technology in all locations, including space,
- Enabling high-rate data delivery that provides continuous presence in all locations that NASA operates, and

TABLE 3-1 Computing, Information, and Communications Technology (CICT) Program Organization and Budget, FY2002-2003

	Budget (million $)	
	FY2002	FY2003
CICT program, total	137.5	153.3
Projects		
Computing, Networking, and Information Systems (CNIS)	42.7	40.9
Intelligent Systems (IS)	59.3	75.9
Information Technology Strategic Research (ITSR)	28.4	29.0
Space Communications (SC)	7.1	7.5

SOURCE: Tu (2002) and Andrucyk (2003).

- Developing a broad portfolio of information technologies and bio- and nanotechnologies that have the potential to revolutionize future NASA missions (Tu, 2002).

REVIEW PROCESS

The National Research Council's Panel on Computing, Information, and Communications Technology (referred to as the CICT panel in this report) conducted its review in two phases. (Biographies of the panelists may be found in Appendix B.) The first phase was to gain an understanding of the top-level objectives of NASA's Computing Information and Communications Technology (CICT) program as the program relates to overall NASA needs. This phase was completed at the first meeting of the CICT panel, June 10-13, 2002, at NASA Ames Research Center at Mountainview, California. The second phase of the review was aimed at understanding the quality and technical merits of individual tasks being conducted under the auspices of the CICT program. To accomplish this task-level evaluation, the panel gave CICT management a one-page questionnaire which the management distributed to some 242 task managers and principal investigators (PIs). A copy of the questionnaire can be found in Appendix E. The CICT panel then evaluated the individual tasks by referring to the questionnaires, conducting follow-up site visits, reviewing technical publications, and talking directly to PIs as needed. Subpanels of the CICT review panel visited three sites:

- Ames Research Center in California (June 13, 2002, and April 14, 2003),
- Jet Propulsion Lab (JPL) in California (July 2, 2002), and
- Glenn Research Center in Ohio (July 24, 2002).

This report discusses top-level issues that are relevant to the entire CICT program in the next section, "Overall Observations." Other sections discuss the research portfolio of the CICT program, the quality of CICT research plans and overall methodology, how well the CICT program has connected with the community outside NASA, and the quality of the technical staff and facilities at the NASA CICT facilities visited by the CICT panel. Specific tasks are highlighted throughout the report as illustrative examples.

OVERALL OBSERVATIONS ON THE CICT PROGRAM

During the review process, the CICT panel placed each task into one of three broad categories:

- World-class,
- Good work focused on the NASA mission, and
- Work that is complete or that should be discontinued.

The great majority of the work reviewed by the CICT panel was good, NASA-focused research. Research categorized as excellent by the CICT panel was work that was typically state of the art and at the same time directly focused on the NASA mission. Such research showed high productivity in terms of published papers, delivered hardware and software, and public presentation. World-class work appeared to address a specific customer or set of customers, regardless of the task's technological maturity.

If a task is not mentioned at all in this report, the CICT panel has deemed that the effort was good work focused on the NASA mission. Such work should continue in the current CICT program plan. This work demonstrated that the researchers had generally well-defined hypotheses, directions, and products to build. While not state of the art, the work was good and focused enough for its undisturbed continuation.

There were two general criteria for work that was complete or should be discontinued. First, work being conducted by CICT that was primarily service-oriented was called into question by the CICT panel. There are several instances discussed in this report where tasks produced useful products and should be transitioned out of the research budget and into NASA operations and their separate funding lines.

Second, research tasks that the CICT panel recommended for discontinuation are efforts that the panel believes do not contribute to the NASA mission and therefore are not appropriate for NASA to continue. This type of research typically showed little in the way of productivity, few or no papers published, little or no software developed, generally few or no public presentations, and little or no direct applicability to a NASA mission. Quite often such low productivity efforts had high full-time-equivalent (FTE) values. The CICT panel was concerned that this situation indicated a significant amount of effort was being put into the task with little return. In general, the support of work that did not appear to map well to NASA missions, or was duplicative of efforts being carried out external to NASA, appeared to be unwise and unnecessary. The CICT panel looked at these tasks carefully to help NASA assess whether a critical mass of research was being carried out.

The CICT panel highlighted the 17 out of 242 tasks that are examples of world-class work:

- Intelligent Systems (IS) project
  —Spacecraft Micro Robot
  —Automated Science Investigation Using Multiple Rovers
  —An Onboard Scientist for Multi-Rover Scientific Exploration
  —A Hybrid Discrete/Continuous System for Health Management and Control
- Information Technology Strategic Research (ITSR) project
  —Quantum Dot Infrared Photodetector (QDIP) Focal Plane Arrays for NASA Applications
  —Nanoscale Acoustic Sensors Using Biomimetic Detection Principle
  —High-Throughput Metabolic Profiling by Multidimensional Nuclear Magnetic Resonance and Mathematical Modeling of Metabolic Networks
  — Advanced Semiconductor Lasers and Photonic Integrated Circuits
  —Intelligent Flight Control
- Space Communications (SC) project
  —Reconfigurable Antennas for High Rate Communications
  —Liquid Crystal Based Beam Steering[1]
  —Internet Protocol (IP) Infrastructure for Space Systems
  —Micro-Surface Wireless Instrumentation Systems
  —Radio Frequency (RF) Microphotonics
  —Efficient Deep-Space Laser Communications
  —High Efficiency Ka-Band Metamorphic High Electron Mobility Transistor Monolithic Microwave Integrated Circuit
  —High Efficiency Miniature Traveling Wave Tube Amplifier

[1]While the Liquid Crystal Based Beam Steering task could use a better understanding of space qualification requirements, the task is still considered by the panel to be world-class for its potential impact on space architecture.

In summary, ITSR had five world-class tasks. SC had eight world-class tasks, and IS had four world-class tasks. One project, Computing, Networking, and Information Systems (CNIS), had no world-class tasks. The CICT panel also identified nine tasks that were complete and should be moved out from under CICT, or that were of questionable value to NASA's core mission and should be discontinued:

- CNIS project
  —Grid Infrastructure Support and Development
  — User Services
- IS project
  —Model-Based Programming Skunk Works
  —Mind's Eye: Knowledge Discovery Process Capture
  —Automated Discovery Procedures for Gene Expression and Regulation for Microarray and Serial Analysis of Gene Expression Data
  —Robust Intelligent Systems Based on Information Fusion
- ITSR project
  —Low Dimension Nanostructures and Systems for Devices and Sensors
- SC project
  —Backbone Network Communication Architectures
  —Distributed Space Communications Systems—Large-Scale Emulations

During the course of this review, the CICT program demonstrated that it had taken appropriate action and either terminated or redirected these nine tasks (Tu and VanDalsem, 2003).

Finding: Most of the work being conducted under the CICT program is good, NASA-focused research.

GENERAL OBSERVATIONS

The CICT panel made some observations on matters of concern that showed up in the CICT program. These observations are general, and there are numerous exceptions to them within the CICT program.

Research Program Architecture

The CICT program would be more uniformly effective if the communication lines between program-level management and task-level PIs were clearer and better established. Problems with communication were evident to the panel during its information-gathering phase. Also, the panel sensed that, to some extent, CICT management was required to "force fit" a top-level research vision onto disparate research tasks that it had inherited from other programs (Tu, 2002). The CICT panel believes that NASA could address this concern by using a research program architecture for the current CICT program as well as an architecture that identifies future targets. Such a program architecture is a framework that would clearly define the program's scope (what is included in the program and what is not), the relationships among the components within the framework, and the principles and guidelines by which the components are to function.

This framework should be applied cautiously, however. NASA should ensure that there are organizational mechanisms in place that allow for research, inspiration, and radical advances to shine through in a bottom-up manner. The framework would help CICT management to (1) organize interrelationships and dependencies among related research investments, (2) distinguish redundancies from complementary efforts, (3) understand where program gaps exist, and (4) describe the key technologies addressed by research projects. The architecture would also help CICT management alter the course of research based on tasks that generate solid results. Gaps between the actual and the desired state of task completion would identify deficiencies as well as high-payoff areas for future research investments. The CICT panel derived a set of key technologies, which it listed in the first column of Table 3-2 for NASA's consideration. In addition, it appeared to the CICT panel that some of the tasks should have been described as a product development effort rather than a research effort. In a research program architecture, CICT management should clearly and correctly identify what is research and what is development and speed the movement of research activities into development as appropriate.

Recommendation: CICT management should establish clear research program architectures to improve communication between top-level management and the task PIs, as well as to improve the overall effectiveness of the program.

Service-Oriented Tasks

The pathway from research to development to service is generally not well defined at the task level within the CICT program. On several occasions, the panel identified tasks that originally started as research and produced very good and useful engineering or research tools. Once the tools were established, the task within CICT became one of maintaining the tools for use by NASA as a whole (Alfano, 2002). Two examples of such activities are the tasks (1) Grid Infrastructure Support and (2) Development and User Services, both under the CNIS project. These two tasks are of questionable value to NASA's core research and development mission, since the basic research portion of the project is complete.

The CICT review panel strongly believes that CICT management needs to establish a mechanism to quickly transition final products, such as grid tools, to a service unit or entity outside the CICT program. This service unit can then maintain the infrastructure of the tools so that the rest of NASA, and even researchers from CICT, can then use them. Of course, the service unit may naturally consult and seek guidance from the original tool developers from CICT when engineering changes to the tool are required.

Recommendation: To establish a more effective research program, CICT management should periodically review all CICT tasks to ensure that they are centered on productive research and development efforts. Any tasks that are providing a service, or those for which the research component is complete, should be quickly transferred out of the CICT program.

In response to the interim letter report of the PRT committee (NRC, 2003), CICT implemented a new management practice—namely, that most tasks under the CICT program will be reviewed by external peer review panels in the same manner that NASA NRA proposals are selected (Tu and VanDalsem, 2003). The CICT panel commends NASA for taking this strong action but cannot yet assess the effectiveness of the peer review since it had not been conducted at the time of this report. The panel does, however, encourage NASA to reinforce the message to the advisory panels being formed that there should be a clear delineation between service-oriented tasks and research and development tasks, as discussed here.

TABLE 3-2 Relationship of Technology Expertise Areas to NASA Abilities and Goals

Technology Area (Project)	CICT Status	Relevance to NASA Mission	Positives	Selected Areas for Improvement
High-performance computing (CNIS)	Led by industry (hardware, especially Japanese) and consortia (software), not CICT.	High-performance computing directly applies to many NASA issues.	CICT has considered some unique and difficult problems (e.g., large-scale shared memory).	Can be connected more closely to broader high-performance computing community. Work closely with appropriate standards organizations to influence emerging standards.
Networking (CNIS)	Following industry leadership.	NASA is a network-dependent organization.	CICT is paying some attention to monitoring and improving network utilization.	Can work with network industry partners to transfer technology. Work closely with appropriate standards organizations to influence emerging standards.
Algorithms for scientific computing (CNIS)	NASA (not industry) is problem focused.	NASA has extensive developments in scientific and engineering applications.	Continuing to improve the algorithms that are core NASA scientific applications.	Can shift from current focus, which is incremental improvements to existing algorithms, toward inventing new fundamental approaches to additional problems.
Distributed computing (CNIS)	Cooperating with others to establish the state of the art but has matured to the point of more general deployment.	Improves usefulness of current NASA computation resources.	Working to extend services, use the capabilities, and transfer technology.	Since the technology is rapidly maturing to the point of relatively few new research opportunities, can be expected to transition to general deployment.
Autonomous robots (IS)	NASA is the international research leader on mission-specific applications.	Essential for unmanned missions.	Integration of multiple disciplines into a coherent whole. Excellent experimental processes and demonstrations.	Possible to improve collaboration between NASA and university researchers.
Planning and scheduling (IS)	NASA is the international research leader.	Essential for robotics, on-board activities, and mission planning.	Multiple approaches being investigated. Being implemented in late 2000s missions (both mission planning and robot task planning). Supporting excellent university research.	Can improve collaboration between internal NASA researchers on preferred techniques for planning and scheduling. Can also develop detailed understanding of the most appropriate application of different approaches.

TABLE 3-2 (continued)

Technology Area (Project)	CICT Status	Relevance to NASA Mission	Positives	Selected Areas for Improvement
Data mining (IS)	At one time NASA was an international leader, but the agency has lost several key personnel.	NASA has significant internal needs to analyze complex engineering data and imagery.	Applying different data mining approaches to specific projects to determine preferred usage patterns. Providing tools for end users (technology transfer). Using mining techniques for scientific/engineering data.	Can regain leadership status to address NASA-specific problems (massive amounts of complex data gathered rapidly and/or remotely). Pay additional attention to the full end-to-end data mining process (initial gathering to analysis to interpretation to archiving). Can increase work in visualization (just a couple of CNIS tasks) to complement the data mining activity. Improve tie between current application areas to NASA projects and NASA scientists (e.g., work with biology data).
Human-computer interface (IS)	CICT follows rather than leads research directions and trends.	Area is highly relevant to astronauts, operations, and design/engineering. Stronger in the astronaut and operations areas. Little evidence of progress with design/engineering.	Having such a program is essential to NASA. Looking at alternative input modalities (albeit at a low level of effort).	Can concentrate on longer-term tasks since current tasks are quite short term. Can address fundamental issues in collaboration and visualization.
Software validation and verification (ITSR)	An international leader in applying formal methods and techniques.	Highly reliable software is essential to NASA.	Application to real problems with some success. Understand problems of scale. Cadre of skilled practitioners developed.	Positive results to date indicate that NASA-wide interest will expand rapidly. Need to consider and plan for the daunting task of making validation and verification a NASA-wide effort.
New computing paradigms (ITSR)	Neophyte in an emerging field. Unlikely to impact NASA missions in next 10 years.	Supports long-term need to find faster ways to compute.	Learning about field. Low level of expenditure.	Can attempt to understand the nature of NASA missions at least 10 years in the future to determine applicability of the new computing paradigms.
Nanotechnology (ITSR)	Beginning to develop skills in a specific area.	Stronger relevance will emerge with clearer definition of the relationship between ongoing research in CICT and research being conducted in other NASA areas (e.g., sensing materials).	Experimental efforts on carbon nanotube research to validate theory. Impressive nanostructure etching and cryogenic camera technology.	The very strong emphasis on carbon nanotube research should be continuously scrutinized for its ultimate practicality. Other microsystem technologies should be considered and weighed against the carbon nanotube work.

Continues

TABLE 3-2 (continued)

Technology Area (Project)	CICT Status	Relevance to NASA Mission	Positives	Selected Areas for Improvement
Space communications hardware (SC)	Leader in developing and using NASA unique technology.	Essential to continued missions.	Understanding of real problems with broadly applicable techniques. Developing new techniques in collaboration with industry.	Can plan for an increase in activities to accommodate high-bit-rate transmission for long-distance missions in the future.
Space communications protocols (SC)	Limited part of the CICT portfolio.	Essential to continued missions.	Accept standard protocols to accommodate long-term nature of NASA missions.	Can develop experiments to improve current standard practice. Can support improved protocols in space-to-earth transmissions that will accommodate large datasets and extended delays.

Final Research Applications

The CICT panel observed on numerous occasions what seemed to be a lack of understanding of the requirements for final application of the work being conducted, be it aeronautics or space. In particular, there was often little understanding of the requirements for space qualification of certain hardware and software (Tu, 2002). Indeed, it sometimes appeared as if space deployment was not a measure of success for some of the tasks even though the clearly stated long-term goal of such research was for hardware or software to be placed on space vehicles.

In addition, some task plans said little about how to transition a task from research to deployment, even when these tasks were being conducted in support of a specific mission. The tasks Liquid Crystal Based Beam Steering and Multibeam Antennas, both under the Space Communications project, and the Flexible Access Networks element are examples of undertakings where a greater understanding of the space qualifications requirements for hardware and software would benefit the work being conducted. It was not clear if this deficiency was caused by insufficient interaction with mission program managers or was simply an oversight on the part of the researchers.

Understanding the demands of the environment in which a research product may operate can easily change the research approach. For example, knowing that the Federal Aviation Administration (FAA) has to ultimately certify onboard pilot advisory systems might lead researchers to discover techniques that are more amenable to certification processes. Or, if a researcher knows that a data set must be analyzed within certain time and memory constraints, he or she could adopt techniques that would be more amenable to satisfying these constraints.

Recommendation: To ensure that task goals are properly oriented, CICT management should ensure that principal investigators and managers clearly understand the requirements of the environment in which the research products will be used. This is especially important for tasks whose stated goal is ultimately to place a hardware or software product in space.

Final Products and Research Benchmarks

Task deliverables are important long-term benchmarks of success. Without them, it is difficult for managers to judge the effectiveness of a research program. While the majority of tasks under the CICT program were good, a subset of tasks often did not have clearly defined products or system deliverables or clearly identified customers. Even under a pure research agenda, benchmarks for success should be established early in the process by task PIs in coordination with the eventual customer for the research. Put another way, if the PI has a specific application with a potential internal or external customer, that customer should be involved in setting the benchmarks for the task. If each task has a clearly defined deliverable or measure of success, CICT management will be able to manage the program more effectively.

Recommendation: To manage the technical quality of work more effectively so that research tasks are meaningful and on track, CICT management should ensure that each task has a clearly defined, realistic yet challenging measure of technical success.

RESEARCH PORTFOLIO

All four projects under the CICT program—SC, IS, ITSR, and CNIS—are working to develop revolutionary technologies and technology solutions to enable fundamentally new aerospace capabilities and missions (Venneri, 2001). The CICT panel verified its expectation that the four project areas would cover very different kinds of tasks and fundamental technologies. Specifically,

- SC covered the hardware and protocols for communicating and transmitting data in space.
- IS covered autonomous robots, planning and scheduling, data mining, and the human-computer interface.
- ITSR covered software validation and verification, new computing paradigms (e.g., quantum, evolutionary, and bio computing), and nanotechnology.
- CNIS generally covered research in high-performance computing, networking, algorithms for scientific computing, and distributed computing.

The portfolios of the four projects contain research tasks that range from concept development to application development. The CICT program has a reasonable balance between fundamental research and applied research. The portfolios are also characterized by different expertise levels when contrasted with the outside technical community. For example, NASA has led the country in work on autonomous robots and the methods by which they operate. It has maintained its position as an international research leader in mission-specific robotic applications for over a decade. On the other hand, universities, industry, and national laboratories have performed and currently lead the field in fundamental research in microelectromechanical systems (MEMS) and nanotechnology, so that in this case NASA is not leading the research charge. Rather, NASA is investing, justifiably, in nanotechnology to assess possible applications and determine methods that will infuse this new technology into NASA products and missions.

Such differences are natural. NASA will lead in some research that is mission-critical either by working on it in-house or by outsourcing and will follow in other research that may become mission-critical in the future. The panel believes that it is essential to maintain this perspective when attempting to assess the value of the entire CICT research portfolio.

This chapter looks at each technology area from the standpoint of how NASA is or is not positioned to lead or exploit that area; strengths and weaknesses in the general tasks within each area; and those areas that require additional NASA attention in order to improve.

Detailed Assessment of Research Portfolio

The panel has determined that the overall CICT research portfolio contains good research projects that support NASA objectives. Four technology areas (comprising multiple tasks) are world-class (criteria listed in Chapter 2):

- Autonomous robots (IS)
- Planning and scheduling (IS)
- Application of software validation and verification (ITSR)
- Space communications hardware (SC)

These technology areas are generally driven by a need unique to NASA that is not being fulfilled by industry, academia, or other government agencies. The panel urges CICT management to examine these areas in detail so that other segments of the CICT program may emulate their success.

The status of these and other technology areas within CICT and their relevance to NASA missions are presented in Table 3-2, under “selected areas for improvement.” The panel suggests possible future direction within each technology area. However, these suggestions are not intended to imply that there are deficiencies throughout the CICT program.

Finding: The overall CICT research portfolio is very good and supports NASA objectives. Four technology areas (comprising multiple tasks) in CICT were judged world-class: autonomous robots,

planning and scheduling, software validation and verification, and space communications hardware.

Overlap with Other PRT Programs

As the CICT panel assessed the overall PRT portfolio and the CICT program's role within PRT, it faced the challenge of understanding the manner in which the portfolio was organized and evaluated. The panel observed that some projects in the ECT and CICT programs appeared not to be clearly bounded. This was especially true for nanotechnology. The CICT panel examined the overall CICT research portfolio and also had high-level exposure to the ECT and ECS programs, the other two programs that make up PRT. CICT panel members did not, however, receive information on the full scope of research in the PRT program or across NASA. There may well be research in other parts of the PRT program and NASA, such as research in MEMS for microsensors, distributed and microspacecraft, and intelligent systems, that might alter some of the recommendations of the CICT panel.

Recommendation: The CICT panel recommends that CICT and PRT management act to ensure (1) that there is adequate communication between related groups in ECT and ECS, (2) that the overall research portfolio is well balanced in areas of potential overlap, and (3) that all task PIs working in the areas of potential overlap are aware of the high-level goals for their research.

Expanding Existing Research Areas

In analyzing the CICT portfolio, the panel occasionally struggled with the definitions and scope of specific CICT expertise areas. To clarify the analysis, the CICT panel discussed specific aspects of "working in the small" in the case of nanotechnology, and "working with people" for human-centered computing. The following two sections provide some ideas for NASA to consider when looking to expand the scope of research areas.

Nanotechnology: Working in the Small

In the CICT program, research to bring about smaller, better-performing, cost-effective systems is plainly consistent with the NASA mission and the general field of nanotechnology. Most of the funding bearing the nanotechnology label under the CICT program is directed toward basic material science studies of carbon and carbon compound nanotube materials (Alfano, 2002). Giant steps need to be taken, however, before this research area can produce hardware of use to NASA. Nanotechnology is far less certain to be incorporated into NASA missions than, for example, microsystems research based on more established technologies and materials. The panel believes that the nanotechnology work in the CICT program is very narrow in its scope and that, by itself, the work seriously overlooks important, promising research areas such as those focused on lightweight, high-strength materials that are of obvious relevance to NASA for launch into space.

The panel believes that there is significant work being done within NASA, but outside the CICT program, on a variety of MEMS and in areas sometimes classified as nanotechnology. Even given the limited purview of the CICT review panel, it appears that the nanotechnogy work under the CICT program is too narrow in scope.

Recommendation: CICT nanotechnology research efforts should be assessed in terms of their potential contributions to NASA missions. More direct focus on potential applications is needed as well as coordination between programs that could interact to provide advances in microsystems.

Human-Centered Computing: Working with People

The CICT panel defined "human-centered computing" to include the assessment of the impact of computing technology on people as well as the development of tools and techniques that facilitate interaction between humans and computers. The ways people interact with computing systems are expanding rapidly. Figure 3-1 illustrates the expansion of the technology and user base but shows that research funding levels do not yet extend to technology areas where growth is anticipated.

It is crucial to the NASA mission for NASA to have cutting-edge expertise in human-centered computing. Outside NASA, the considerable development in the human-computer interface area focuses quite naturally on the most frequent circumstance—namely, that in which a single user deals with a midsized display. For NASA, however, communication travels over a number of routes with disparate interface environ-

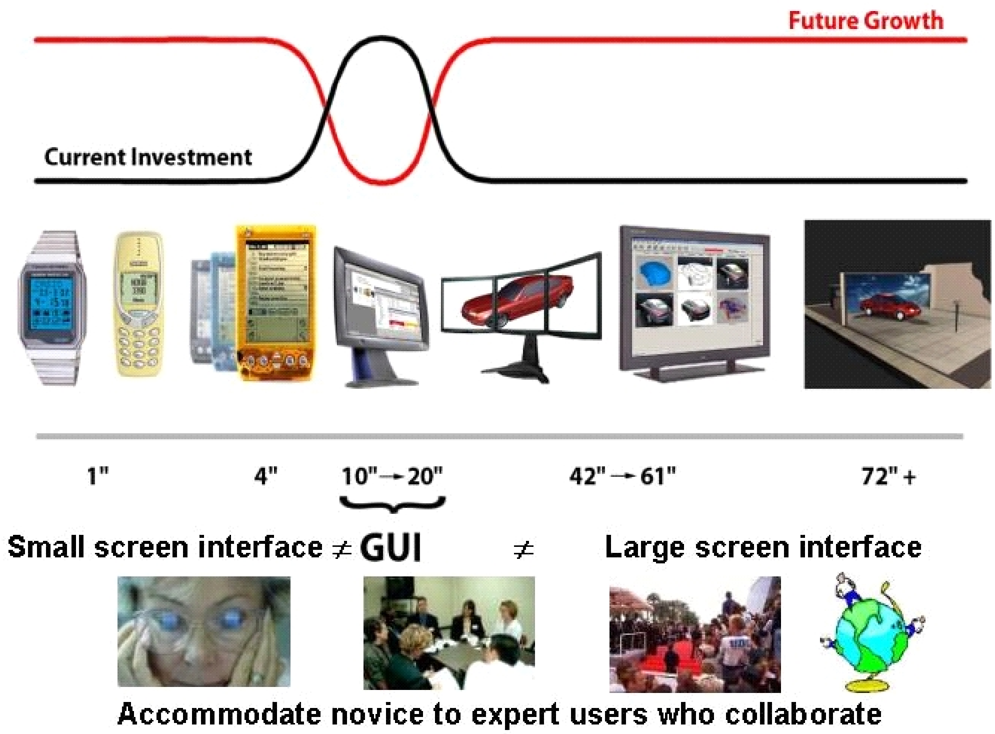

FIGURE 3-1 Future expansion of the technology for human-centered computing.

ments. For example, mission control’s display mechanisms differ greatly from those in the cockpit of a human-occupied spacecraft.

There is little work in the CICT portfolio focused on how scientists and engineers can improve their productivity through collaboration and collaborative environments (Yan, 2002). It is essential to continue the efforts in human-computer interaction to evaluate and understand how NASA’s people will work more effectively with computing systems. NASA must also consider a rapidly expanding and challenging environment for its people that goes far beyond the “single user with a midsized display” paradigm. Small display screens, which will be used throughout NASA both on earth and in space, still pose exceedingly difficult problems when used to display complex instructions or graphics. Such screens, as well as distributed human-computer interaction, are challenges that require additional work.

In terms of the overall impact on people (both earth-bound and space-bound), research in how to work collaboratively is essential for increasing staff productivity before, during, and after NASA missions. In addition, the skill base for the highly technical work that NASA performs and contracts is located at laboratories across the country and must often be brought together at a central location, virtual or physical. Much of the work the CICT panel described as world-class involved teams that are physically colocated, a characteristic that is becoming increasingly rare. The implication here is that if NASA enables virtual colocation by using new collaborative technologies, more teams may reach world-class status.

NASA has substantial skill in cognitive human factors assessment. In fact, the team that has emerged over the course of the review is particularly strong, especially in terms of its links to universities. There was little evidence, however, that the team’s skills were being used to improve collaboration or to improve the usefulness and usability of new devices or the user interface paradigms. The review panel acknowledges that there has been progress improving the user interface for individual users—for example, the Mars Exploration Rover (MER) or the International Space Station (ISS). However, the CICT program has not yet addressed the significant fundamental research on distance collaboration and alternative device evaluation that is essential to the entire NASA community.

Finding: Collaborative work environments engaging geographically distributed users are becoming increasingly important to NASA's mission. These users will employ a wide variety of interactive devices.

Recommendation: CICT should increase the involvement of NASA human factors experts in the cognitive evaluation of collaborative environments. To ensure that the new technology is used in the most appropriate manner for NASA missions and research, CICT should work on new graphics and interactive device technology.

Critical Computing Expertise That May Be Missing

Based on the CICT panel's understanding of the NASA mission and the impact of computing on the goals of NASA, there are some areas of computing that are critical to NASA's excellence as a globally and spatially distributed enterprise. The panel did not find these in the CICT portfolio (Hine, 2002; Yan, 2002). This does not mean that such computing expertise is not covered in other areas of NASA. To be prudent, the panel points out these critical areas for NASA to review and act on appropriately.

Distributed Data Management

NASA scientists and missions generate terabytes of information that must be distributed and analyzed throughout the country. The CICT panel has observed that a significant amount of work is being done in this area at NASA Goddard Space Flight Center. Such work is fundamental research for projects in distributed computing in the CICT portfolio.

In response to the PRT committee's interim report released in January 2003 (NRC, 2003), the CICT program has planned for a large effort in distributed data management titled Knowledge Access and Discovery Systems (KADS), to start in FY2005. While a delayed start, the CICT panel commends the CICT program for planning this effort.[2]

[2]The panel understands distributed data management to include location, replication, access, and configuration management.

Information Systems Architecture

The organization of interrelationships between information system components is essential for more than planning and technology roadmaps. The development of information system architectures is an emerging discipline.[3] One very important goal here that NASA should carefully plan for is to ensure that all computing and data management software components developed under this architecture will work together. The architecture should also ensure that when a system is placed into use, individual components can be installed or implemented with little to no disruption. In addition, new strategies are needed to make highly distributed, parallel processing work efficiently in both real-time applications and conventional applications.

Recommendation: In order to make sure distributed NASA computing systems work together, NASA should establish a carefully developed information systems architecture.

RESEARCH PLANS AND METHODOLOGY

This section is intended to evaluate the plans or methodologies by which the tasks within the CICT program are carried out. In general, the CICT review panel found the high-level goals of PRT to be well defined and relevant to NASA's mission (Tu, 2002). PRT and its constituent programs, such as CICT, should also have clearly defined metrics. It was not clear to the CICT panel what the measurements for success are at the top level of PRT and its constituent programs. For instance, is CICT assessed against metrics such as technologies transferred to missions, publications, and commercialization? As stated earlier, the CICT panel encourages all managers within the CICT program to establish clear metrics as a means of evaluating the tasks under their purview.

Task Deliverables and Their Fit to NASA Goals

The CICT panel found task deliverables other than those of the SC project to be poorly defined. The SC project was exemplary in that it generally had clear objectives, measurable outcomes, and milestones

[3]Information systems architectures will establish the implementation framework, interrelationship, principles, and guidelines.

(Bhasin, 2002a). In the other projects, however, the task PIs did not seem to have a clear view of how their task fit into a program mission. Many task statements that the CICT panel received, for instance, did not list any customers. As examples, the following tasks within the IS project did not mention customers on their task description questionnaires. This list is not meant to be all-inclusive for the entire CICT program.

- Onboard Fault Identification for Planetary Rovers
- Domain-Specific Self-Adaptive Software
- Multi-Resolution Planning in Large Uncertain Environments
- Team-Oriented Robotic Exploration Tasks on Scorpion and K9 Platforms
- Probabilistic Reasoning for Complex Dynamic Systems
- Causal Reasoning
- Automated Data Management
- Distributed Data Mining for Large NASA Databases
- Robust Intelligent Systems Based on Information Fusion

This apparent disconnect between the task and NASA missions or even CICT program goals may be due, in part, to a lack of communication from top management to the PIs. NASA managers should clearly articulate and communicate to PIs the mission and the potential customers for various programs, as discussed earlier in the report. The following recommendation appears earlier in this chapter, but it also applies here.

Recommendation: To manage the technical quality of work more effectively so that research tasks are meaningful and on track, CICT management should ensure that each task has a clearly defined, realistic, yet challenging measure of technical success.

Maturing a Technology

It is vitally important that the excellent quality research CICT conducts eventually be transferred to a main mission, be it internal or external to NASA. Thus, the maturation process for a technology is very important. The CICT panel found that the process for maturing research was clearly articulated for research directly related to a well-defined NASA mission. It was, for obvious reasons, more vague for research that is long term and not directly applicable to current NASA missions. These long-time-horizon tasks with potentially high payoff (such as in CICT tasks on revolutionary computing and, in general, CICT's bio-nanotechnology efforts) are often at high risk of failure—that is, they may fail to reach the stated project goals.

It was also not clear to the CICT panel what process CICT has in place for allocating or deallocating resources to such long-term efforts. For instance, the quantum computing field will most likely not yield any technology directly usable by NASA in the next 20 years. While this is currently a good effort that is properly being funded by NASA, the CICT panel had general questions about such long-term projects. Will NASA continuously fund quantum computing over the next 20 years? Has NASA the expertise to invest in the best research approaches in such an area?

The process NASA uses for transferring a technology to an application was also not well defined for some of the technologies with broad applicability outside NASA. Success for such technologies should be measured not only in terms of their deployment within NASA but also in terms of their broad deployment and use outside NASA. If broad deployment outside NASA does not take place, then future NASA missions will be burdened with providing continued support for NASA-unique technology, thereby missing the opportunity to leverage a broader external base of support.

Another way to think of the problem is that NASA must choose carefully between developing the best technology for NASA and developing technology good enough for NASA but that will have a broader applicability and will not require a continuous investment stream from NASA. In such cases, success outside NASA that drives standards and pushes commercialization should be the main goal.

This need to leverage outside investment seems to be recognized by most tasks within grid computing that are contributing to a broad community effort. The recognition of this need is less apparent in the tasks in high-performance computing (a.k.a. advanced computing), which seem to pursue many technologies that are similar to or perhaps the same as technologies being pursued outside NASA.

The high-performance computing area is an excellent case study in the type of problem NASA faces when maturing a technology. The work in this area is currently embodied by two tasks at NASA Ames Research Center: (1) High-End Computing Architecture

Research and (2) Research in Programming Paradigms. A significant amount of effort has gone into developing a shared-memory programming model to support high-performance computing. NASA has an opportunity to take the lead on such development, but at this time it does not have the critical mass to successfully engage the broader community. It is essential that NASA make clear decisions on how to proceed with the transition.

The CICT program has demonstrated that there is value in the approach it has taken with shared-memory models from both a hardware perspective and a software programming tools perspective. However, NASA cannot afford, in the long run, to follow a NASA-unique approach in this area. In the near term, it should focus on a broader effort that encourages others outside NASA to adopt common shared-memory models and to develop a shared infrastructure of libraries and tools. There is strong interest in similar programming models in much of the high-performance computing community.

Some examples of actions that NASA can take include (1) normalization and extension of NASA benchmarks, (2) participation in standardization activities external to NASA, and (3) making a complete set of CICT's MLPlib library and associated programming support tools available to the broader community at no cost. Within 1 year of the onset of this activity, CICT should, at a minimum, be able to formulate an acceptable benchmark set and programming environment for the parallel libraries that NASA chooses to support. These benchmarks should be appropriate for use not only by NASA, but also by the general hardware and aerospace technical community.

Finding: NASA has an opportunity to take the lead on shared-memory programming model development, but at this time it does not have the critical mass to successfully engage the broader community.

Reviewing and Selecting Proposals

CICT appears to have a good methodology for reviewing and selecting proposals, although at the start of this review, it was not apparent to the review panel how labor is divided between internal and external reviews in the CICT program. There is also an inherent conflict of interest in having a NASA manager choose between keeping a task in-house—that is, having NASA employees perform the work on the task—and outsourcing it (where an external company performs the work) since that manager will be managing any in-house effort selected. Individual task owners have thought about their future plans in a reasonable manner. However, these future plans must be balanced with other suggested research, including that suggested by the CICT panel.

The CICT panel commends the use of external reviews and of a competitive process for proposal selection, as done by the IS project. Such a process leads to the selection of technically good proposals in defined areas. Some tasks, such as the tasks NSF Collaboration (under the ITSR project) and IP Infrastructure for Space Systems (under the SC project) seem to successfully take advantage of external reviews for assessing progress during task execution. CICT management should encourage this type of activity.

Based on the interim letter report issued by the PRT committee in January 2003 (NRC, 2003), CICT management decided that all tasks for the majority of CICT projects will be reviewed by peer committees, similar to the NASA NRA process. The panel believes that this is a step in the right direction and encourages CICT to keep an active peer review process in place for the entire program; however, the process by which reviews will take place has not been evaluated by the panel for effectiveness. The drawback to this type of review process is that it may not lead to a good mix of low risk and high risk of project failure and of short-term and long-term tasks, and the process may also not provide a rational allocation of resources for entering into new technology areas. It may be useful for the CICT management to explicitly manage the allocation of resources between low risk and high risk of project failure and short-term and long-term tasks for each technology area.

There are three basic types of risk associated with tasks and elements: the risk of failure for a given task, the risk of a successful task not fitting into a larger system, and the risk associated with not starting a task or an element at all. In general, few tasks were rated as having a low likelihood of success in the written questionnaire responses. This may indicate a bias in the reporting, little investment in high-risk-of-failure tasks, or inadequate analysis of system-level risks. The CICT panel believes it is important to have a balance of risks, and it appeared that the CICT program could stand to pursue a greater proportion of tasks with a higher risk of failure. Risk of research failure can be managed using well-defined milestones as decision points for the continuation, revision, or cancellation of tasks. Such a risk management process (e.g., systems level analysis and customer knowledge) seems to be applied in certain projects, in particular the SC project, but not in others.

A possible solution is for CICT to have a clean division between categories of risk and then clearly define the research tasks and the criteria for assessing success of the tasks within these risk categories. External review panels, which CICT already plans to use, could then select proposals for tasks within these categories, similar to the National Science Foundation (NSF) model, so that program managers can still exercise some judgment in the process. External advisory panels that mix people having a long association with NASA with people who are domain experts and have no significant interaction with NASA may be a good mechanism for identifying and initiating new technology areas in which to invest. This task selection process could allow CICT management to use external review as a positive tool in its program, while maintaining an appropriate risk balance in the research portfolio and providing a mechanism by which CICT can branch into new technology areas.

Technology Readiness Level

The CICT panel found that, in general, the PIs did not assess the technology readiness level (TRL) of their tasks in a consistent manner. The panel's impression is that many tasks were ranked too low on the TRL scale by the PI. The following is a short, random handful of examples:

- *Visualization (CNIS):* This task was ranked as a TRL 1 to 6, which is not very precise.
- *Robust Intelligent Systems Based on Information Fusion (IS):* This task is ranked TRL 1 by the PI. The work may well be fundamental and novel, but if it is successful, the path to actual deployment could be quite rapid.
- *Evolutionary Algorithms for Scheduling (ITSR):* This is an application of genetic algorithms to satellite scheduling and was ranked TRL 2 by the PI. The use of genetic algorithms for this type of scheduling problem is not new and appears to the CICT panel to be an application of a known technology to a new kind of problem.

Reorganization of Projects and Management Structure

The CICT review panel has only addressed programmatic issues as they arise from technical issues. The CICT panel has found that some projects within CICT, such as the SC project, seem to have a more coherent vision, better plans, and better project management than other projects in the CICT program (Bhasin, 2002a, 2002b). The panel feels strongly that SC's positive performance reflects, in part, the relative stability of the SC project compared with projects that experienced frequent reorganizations. The reader may remember that the SC project had the highest number of world-class tasks, as reported earlier in this chapter. Stability is important. It is important to let plans mature, allow management to track progress, and develop a coherent vision.

Finding: The CICT program appears to be suffering from too frequent reorganization. There is a direct link between the stability of a project and the project's technical performance. It is important that tasks be given time to mature under a consistent leadership.

TECHNICAL COMMUNITY CONNECTIONS

The CICT panel was charged with looking at how well the CICT program is linked to the technical community at large. These are some of the questions asked in the statement of task:

- Is there evidence that the research plan for the area under review reflects a broad understanding of the underlying science and technology and of comparable work within other NASA units as well as industry, academia, and other federal laboratories?
- Is there evidence that the research builds appropriately on work already done elsewhere? Does it leverage the work of leaders in the field? Are partnerships, if any, well chosen and managed?
- Is the research being accomplished with a proper mix of personnel from NASA, academia, industry, and other government agencies?

A large number of tasks within the CICT program seem to be quite small in size and effort, with only one or two FTEs per task (based on the questionnaire responses). This conclusion might be an artifact of the funding and reporting mechanisms used by NASA. It might also be indicative of too many efforts spread too thin over too many areas. If the latter is true, the panel encourages CICT management to have fewer but larger efforts. This strategy might improve the chances for having an impact, improve interactions and collaborations, and expand the involvement of management with external research projects. In particular, CICT should seek to establish more collaborations involving multiple research institutes, as well as collaborations with other researchers inside NASA. The planned use of external review panels to select proposals, as discussed previously, may well reduce the conflict of interest problems that such collaborations often face. An example of a successful collaboration is Microfabricated Force-Detection Spectroscopy, where the PI is apparently able to leverage a modest 0.5 FTE to work on a task that promises to yield significant results.

CICT management should strongly encourage task PIs to seek peer-reviewed publication in the proceedings of major conferences and workshops. This process provides an objective measure of research quality, gives NASA visibility in the research community, and provides useful peer feedback, especially for new, low-TRL areas such as bio-nanotechnologies, which seem to have relatively few publications considering the considerable effort being devoted to them.

An example is the task Molecular Electronics under the ITSR project, which provides funds for three investigators, addresses a very-high-risk-of-failure research area, and still has no publications. By way of contrast, the task Computational Nanotechnology-Chemistry, being carried out by six investigators, has contributed several publications. As stated earlier, the central role given to carbon nanotube and related materials research in the bio-nanotechnologies element should be carefully evaluated from time to time for progress toward NASA-mission-related applications.

In a related example of peer review, the CICT panel commends a small advisory board formed at JPL to guide the work being conducted under the task Biological Computing–BioInspired Information Processing and Exploration with Active Sensor Arrays. This research group has made significant improvements since its initial review by the panel (Tu and VanDalsem, 2003). The research group now needs to expand its expertise by involving researchers in computer science, materials, and engineering design on the advisory board, to work on the possible deployment of new technologies. The CICT panel believes that such a formal advisory board will help the group establish clear technical standards for whether or not a research activity will yield products of interest and use to NASA.

The panel also encourages CICT management to organize and fund workshops to enhance the program's interaction with the external world and expose outside researchers to NASA's problems. NASA did this extensively in the past, and the panel encourages the CICT program to continue to do so. For example, NASA Langley hosted the Satellite Networks and Architectures Conference in Cleveland in June 1998. At the conference, NASA presented its work on TCP/IP communications over satellites. Commercial and university representatives gave talks on various aspects of data communications, with an emphasis on communication protocols. This conference is an example of how NASA can engage the external community by having NASA researchers interact with their peers in industry and academia. The exemplary work conducted under the SC program within CICT testifies to the value of these types of exercises.

The panel notes that the CICT program has taken steps in the right direction. For example, the chair of the IEEE Nanotechnology meeting held in August 2003 was a NASA Ames researcher working under the nanotechnology portion of the program.

Finally, export controls are a reality with which NASA researchers must contend. Researchers should not use export controls as an excuse for the absence of publications, software distribution, and peer review. Researchers and management should anticipate such constraints and plan so that they do not impede the publication of early CICT work or the distribution of software.

Recommendation: To expose the external NASA technical community to NASA-specific issues and provide maximum leverage for CICT-funded tasks, CICT management should strongly encourage task PIs to seek peer-reviewed publication in journals and in the proceedings of major conferences and workshops. CICT management should also organize and run technical workshops.

Recommendation: PIs and CICT management should anticipate and plan so that export controls or other restrictions do not impede the publication of early CICT work.

Awareness of Relevant Research

The scientific work done in various tasks was generally sound and frequently of good quality. Most PIs appeared to be aware of relevant research work inside and outside NASA. CNIS researchers, in particular, had a good awareness of relevant research.

Awareness of Tasks Within NASA

The CICT panel found that some areas within the CICT program could have better communication and collaboration between various tasks and between researchers and developers in other parts of NASA. Some tasks overlap, and the overlap should be recognized and managed by the CICT program. The PIs working on such tasks did not always seem aware of the overlap.

For example, the tasks MER Rover Sequence Generation and MER Collaborative Information Portal, both under the Intelligent Systems (IS) project and both targeted at the MER mission to Mars, should be better coordinated than appears to be the case. Also, the tasks in the following lists seem to be closely related and appear to comprise different approaches to solving the same problem. If this is the case, there is nothing wrong with it, but the PIs for the tasks should be in close contact with each other and should be managed and coordinated by upper management. The reader should not infer that the following lists include all such overlaps across the CICT program. Examples of possible overlap from the IS project are these:

- Artificial Collective Intelligence (Automated Reasoning element) and Adapting Coordination and Cooperation Strategies in Teams (Human-Centered Computing element).
- Robust Speech Recognition Using Dynamic Synapse Neural Networks (Human-Centered Computing element) and Advanced Spoken Dialogue Interface Systems (Human-Centered Computing element).

There may be overlap among all of the following tasks within CNIS:

- Grid Science Portals (Grid Common Services element).
- Storage Research Broker Development and Support, and Grid Testbed Support (Grid Common Services element).
- Development (Grid Common Services element).
- Visualization (Information Environments element).
- Grid User: Project Portal Development Environment (Information Environments element).

One specific example not listed above, the Data Fusion IS-IDU-SHT task under IS and the Intelligent Data Understanding element, should be placed under the direct control of similar work being conducted at JPL and the University of Minnesota in the task Discovery of Changes from the Global Carbon Cycle and Climate System Using Data Mining.

It should be noted that the CICT panel has deemed that these tasks centered on grid computing are worthy efforts and appropriate for NASA to pursue. However, the panel did not get a good understanding of how the various organizations within NASA will collaborate. There are many possible reasons why the top-level vision is not clear and why the overlap discussed above is taking place. One might be that a lack of communication between PIs keeps Ps from knowing the big picture. Regardless of the reason, it should be up to the CICT management to identify causes and address issues.

Awareness of Tasks Outside NASA

A separate issue is the overlap of CICT efforts with work done outside NASA. In some of the tasks, the work is very specific to NASA missions, so little work being conducted outside NASA will apply to those missions. In almost all other tasks within the CICT program, it appears that PIs are sufficiently familiar with current research being conducted in their fields outside NASA.

Better communication could improve the work on various tasks in the antenna lab at NASA Glenn Research Center. In some cases, researchers seemed unaware of key relevant research performed elsewhere, as evidenced in the write-ups provided by each PI. Some tasks from the IS project that could benefit from a survey of the relevant technical literature are these:

- Machine Learning and Data Mining for Improved Intelligent Data Understanding of High Dimensional Earth Sensed Data.
- Robust Intelligent Systems Based on Information Fusion.
- Machine Learning for Earth Science Modeling.
- Knowledge Discovery and Data Mining Based on Hierarchical Segmentation of Image Data.

Use of Talent Inside and Outside NASA

The CICT program does a good job of selecting external talent, from both academia and industry, to work or collaborate on tasks. As an example, the procedure followed by the IS project for soliciting and choosing among proposals for work at low TRLs (Hine, 2002) seems to be very effective. Many of the external PIs chosen by this project are clearly active and well-respected researchers in their technical fields. The IS program should also be commended for having allocated a larger portion of its resources to establishing external community connections over the past several years.

Many projects appear to involve a mix of internal NASA researchers and external researchers. It is essential to the long-term success to retain internal expertise not only in technologies unique to NASA mission success (e.g., autonomous robots, planning and scheduling, application of software validation and verification, space communication hardware) but also in other critical technologies. This expertise is necessary to set the appropriate research agenda, to ensure the quality of results acquired from outside resources, and to integrate and assemble technology acquired externally into NASA systems. The panel believes that all CICT projects should be open to competition and should consider external researchers. Internal and external competition should be conducted separately and in a manner that encourages collaboration. There are also a number of research areas that are funded in part by NASA and in part by other agencies, such as DOD and NIH. The mix of personnel on these projects seems, in general, to be appropriate.

There are many instances within the CICT program where the program would be benefited by expending some effort to maintain a direct connection between NASA and external researchers. In this way, CICT management can guide the external work so that, as much as possible, it is relevant to NASA missions. One way of doing this would be to make sure that NASA personnel familiar with mission goals and needs have a chance to work closely with external researchers to keep their research focused and relevant.

Internal NASA research uses both civil service staff and contractors to perform the work. The relationship between the two groups in CICT was seamless, a good situation. Overall technical and management leadership should remain with civil service staff to guarantee project accountability.

Recommendation: To maintain a strong research base, the CICT program should continue to encourage a close connection between researchers and the external research community by, for example, encouraging its researchers to attend conferences and serve as journal editors.

Benchmark Datasets and Problem Sets

In addition to funding research projects, CICT management could leverage the work of external NASA researchers by providing appropriate benchmark datasets or problem sets. In this way, the work being conducted outside NASA would be relevant to NASA-specific problems at little or no additional cost. The release of such datasets would facilitate quantitative comparison of different research techniques, as well as encourage the broader community of researchers who are not funded directly by NASA to consider NASA-relevant problems in their work.

The task NSF Collaboration, under the ITSR Automated Software Engineering Technologies element, is a good example of a task where such activity has taken place. Under this task, the PI jointly funded the creation of a reliable software testbed. This was also discussed earlier under the section on the CICT research portfolio in relation to parallel programming tools.

FACILITIES, PERSONNEL, AND EQUIPMENT

During the site visits and during various interactions with researchers throughout the course of the review process, the CICT review panel found the qualifications of the CICT scientific staff to be very good and easily comparable to those of world-class researchers. As noted in the previous chapter, the external investigators that the CICT program has employed are also world-class and of high renown.

The facilities and working environment are in a very good state of repair and on a par with other government laboratories and facilities. All researchers appeared to have the tools and equipment they needed, in very good working order. The panel site visits did not observe unnecessary duplication or poor use of NASA- or contractor-furnished equipment or facilities

In the case of JPL in Pasadena, California, some of the laboratory space was cramped for the number of researchers working there, but the CICT panel understands that laboratory space at JPL is very limited. And, in any case, the panel found that inadequate laboratory space did not impede their technical progress. In the view of the panel, NASA has done an excellent job.

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4

Report of the Panel on Engineering for Complex Systems

INTRODUCTION

The Engineering for Complex Systems (ECS) program is one of three programs under NASA's Pioneering Revolutionary Technology (PRT) program. The ECS program, funded at $24 million for FY2002, comprises three broad, level 2 projects: System Reasoning and Risk Management (SRRM); Knowledge Engineering for Safety and Success (KESS); and Resilient Systems and Operations (RSO). The projects are then divided into level 3 elements and, further, into a total of 52 level 4 and level 5 tasks. Program organization and budget are presented in Table 4-1.

The goal of the ECS program is to "achieve ultra-high levels of safety and missions success by fundamentally advancing NASA's system life-cycle approach through the infusion of advanced technologies" (Gawdiak, 2002a). The program's intent is to accomplish this goal by addressing areas of need, including increasing NASA's ability to conduct system and trade-off analyses and NASA's understanding of organizational risk and of knowledge acquisition and communication and improving its system control strategies and status assessment.

TABLE 4-1 Engineering for Complex Systems (ECS) Program Organization and Budget, FY2002-2003

	Budget (million $)	
	FY2002	FY2003
ECS program, total	23.6	27.4
Level 2 projects		
Knowledge Engineering for Safety and Success (KESS)	4.9	5.9
Resilient Systems and Operations (RSO)	11.9	13.7
Systems Reasoning and Risk Management (SRRM)	6.8	7.8

SOURCE: Gawdiak (2002b) and Andrucyk (2003).

REVIEW PROCESS

The National Research Council's Panel on Engineering for Complex Systems (referred to as the ECS panel in this report) conducted its review in two phases. The first phase was to gain an understanding of the top-level objectives of the ECS program as the program relates to overall NASA needs. This phase was completed at the first meeting of the ECS panel, June 10-13, 2002, at NASA Ames Research Center at Moffett Field, California. The second phase of the review was aimed at understanding the quality and technical merits of individual tasks being conducted under the auspices of the ECS program. To accomplish this task-level evaluation, the ECS panel gave ECS management a one-page questionnaire, which the management distributed to some 52 level 4 and level 5 managers and principle investigators (PIs). The ECS panel then evaluated the individual tasks, referring to the questionnaires, conducting follow-up site visits, reviewing technical publications, and talking directly to PIs, as needed. A copy of the questionnaire can be found in Appendix E. Subpanels of the main ECS review panel visited six sites:

- Goddard Space Flight Center in Maryland (June 24, 2002),
- Jet Propulsion Lab (JPL) in California (July 1, 2002, and April 17, 2003),
- NASA Headquarters in Washington, D.C. (July 22, 2002),
- Glenn Research Center in Ohio (July 23, 2002),
- Johnson Space Flight Center in Texas (July 23-24, 2002), and
- Kennedy Space Flight Center in Florida (July 30, 2002).

The findings and recommendations of the ECS panel are presented in this chapter. This report section initially discusses the top-level issues that are relevant to the ECS program as a whole. Other sections concentrate on issues that apply individually to the three projects within ECS and discuss tasks specific to these projects. It should be noted that not all tasks within the ECS program are discussed in this report. If a task is not discussed, the ECS panel deemed that the effort is good work and should continue per the current ECS program plan.

GENERAL OBSERVATIONS

Programmatic Risk Management

The ECS program has been in a state of flux since the program's formation some 6 months before the start of this review. At the end of this review process, the program is still in the early stages of developing a critical mass—a large enough effort to make a difference within NASA and the external community—of research in programmatic risk management with the limited budget available to it. The programmatic risk management concept is a comprehensive, probabilistic process with full development and delineation of uncertainties. For a given operational system, programmatic risk consists of the probability (with uncertainty delineated) of achieving each of the following system requirements:

- System safety (probability of crew survival),
- Reliability (probability of system completing its designed mission),
- Performance (probability of achieving the design parameters of system performance),
- Cost of the program (probability of staying within the budget), and
- Schedule for system delivery (probability of meeting the schedule).

Such a programmatic risk management concept, from early design to mission completion, would contribute to the comprehensive programmatic risk management approach that is under development and being applied by safety organizations within NASA. Given the limited resources of the ECS program, it will not be able to create such a comprehensive plan in the foreseeable future. Given the dynamic physical environments in which NASA operates, and in light of the Mars robotic exploration failures and the Columbia tragedy, it is critical for ECS to aggressively contribute to the development of NASA's programmatic risk as defined above.

The headquarters group responsible for implementing large-scale safety protocols at NASA is the Office of Safety and Mission Assurance (Code Q). Code Q performs research, both in house and by contract, on the development and application of methods for managing program risk. Code Q also provides guidance to NASA programs in the application of these methods.

The other groups responsible for safety are located at the NASA centers. The safety and mission assurance organizations at the NASA centers assist NASA programs in the application of programmatic risk technology. Improved cooperation between the ECS program and Code Q in developing and applying risk methods is strongly encouraged. The panel did see evidence of varying levels of ECS program involvement at the NASA centers visited (listed earlier in this chapter).

Finding: NASA has a critical need for a comprehensive risk management program that can be implemented throughout program life cycles. The ECS program should contribute to the development and application of such a program for NASA.

Recommendation: In light of the Mars exploration failures and the Columbia tragedy, the ECS program should aggressively contribute to a comprehensive programmatic risk management program that would develop the probability (with uncertainty delineated) of achieving each of the following system requirements:

- System safety (probability of crew survival),
- Reliability (probability of system completing its designed mission),
- Performance (probability of achieving the design parameters of system performance),
- Cost of the program (probability of staying within the budget), and
- Schedule for system delivery (probability of meeting the schedule).

NASA's efforts to rework the overall ECS program plan during the course of this review (from June 2002 through April 2003) are appropriate given the program's central importance to NASA's mission. The overall ECS effort has the potential to make advances in complex systems engineering. The ECS program has succeeded as a role model for NASA by recognizing the need for and conducting multidisciplinary human and organization factors research. The goals of the ECS program are consistent with NASA goals of achieving safe and reliable systems and missions (Venneri, 2001). As such, the mission and work of the ECS program are critical for NASA.

Finding: The current ECS program, as formulated and funded, will not by itself develop a comprehensive programmatic risk management program in the foreseeable future, yet this ECS risk management work is important for NASA.

Technical Quality

ECS work in individual tasks is, in general, good. The ECS program appears to address the right problems using correct justification (i.e., hypotheses) through a multidisciplinary research approach. However, at the start of the review in June 2002, there were many gaps in the ECS portfolio that weakened the effectiveness of the program, including the following:

- Integration of mission- and organizational-risk-oriented tasks within the ECS program overall,
- A need for benchmarks in the ECS program overall,
- Addressing NASA-specific problems in software development within Resilient Systems and Operations (RSO),
- Modeling organizational risk under Knowledge Engineering for Safety and Success (KESS), and
- The absence of a coherent roadmap or strategy for the development of System Reasoning for Risk Management (SRRM) technology, which could apply across many NASA programs,

Many, though not all, of these gaps have been addressed since the start of the review. The remaining gaps are discussed under the individual projects and in the section "Challenge Areas."

The tasks have been reasonably distributed among NASA PIs, academia, and industry, and the balance of fundamental research versus user-driven research is appropriate, considering the early stage of the ECS program. The ECS panel anticipates that the current ECS program will have small but positive incremental impacts on limited areas or individual missions within NASA. However, the panel believes that more fundamental research should be conducted if the ECS program is to have a profound and revolutionary impact on the NASA culture. Since NASA systems typically are more dynamic and more complex than other systems, such as those in the nuclear industry, fundamental research is required to enable adequate modeling of the interactions between hybrid-dynamic systems (systems composed of hardware, software, and human elements) operating in high-energy and hostile environments.

To balance the risk of not successfully completing a task against the payoff when a high-risk task succeeds the ECS program has an appropriate mix of tasks (Gawdiak, 2002a, and based on response questionnaires). The majority are oriented to direct applications in missions and organization risk mitigation and risk analysis. A program such as ECS can be said to fail if no other NASA programs use the tools and methodologies developed by the program. The payoff, on the other hand, is a revolutionary change in NASA culture, whereby mission and organization risk management become an integral part of the design of spacecraft and missions.

The panel was also concerned with several tasks in the ECS program. The ECS panel acknowledges that NASA has constructed a competent team of in-house and external expertise. Moreover, it agrees with ECS management that top-down, functional decomposition to identify research requirements is important, that the starting point for the ECS program did not permit an optimal decomposition, and that there is a continuing challenge to integrate top-level research requirements with lower-level tasks.

Since the program is still evolving, it is important for the ECS program to continually seek external guidance to help maintain the focus and quality of the program. This is especially so for the SRRM project, which has only recently come into clear focus (ECS, 2003).

Finding: ECS work in individual tasks is, in general, good and focused.

Finding: The ECS program is in the early stages of developing a concentration in programmatic risk management within NASA, which should prove to be central to the NASA mission.

Finding: In general, the ECS program appears to address the right problems through a multidisciplinary research approach.

Finding: The ECS program, and SRRM in particular, is still evolving. External reviews can help the program maintain focus and quality.

Recommendation: The ECS panel recommends that the ECS program make use of external, independent reviews to maintain the focus and quality of the program.

The personnel working under the ECS program are, with few exceptions, of very excellent quality and represent NASA well. The program generally makes use of contractors and contractor facilities efficiently.

Challenge Areas

The ECS panel has identified two general areas that cut across the ECS program where, if changes are made, the ECS program as a whole would benefit. The first is the institution of benchmarks by which to measure success and the second is an analysis of the system-level impacts of task products. At the management level, for instance, a benchmark of success for the SRRM project could be the successful deployment of the risk workstation that is currently being developed, as detailed in the SRRM review below.

The implementation of benchmarks (so-called "measurables") is also very important at the individual task level. In general, benchmarks refer to quantitative goals or expectations that serve as technical measures of success. For instance, a measure of success at the task level under SRRM might be successfully engaging a target number of NASA mission program offices and having them use the defect detection and prevention tool or the technology infusion, maturity assessment process. The SRRM project is already doing this informally, but it is important to list these criteria as formal benchmarks of success. The ECS panel did not see evidence that measurables have been integrated into all of the tasks. The panel acknowledges that the ECS program is relatively new but believes that measurables should be part of the planning for all individual tasks.

Recommendation: The ECS program should implement clearly defined benchmarks at the individual task level in order to judge task progress.

The ECS panel was impressed by the top-level vision that the ECS managers presented (Gawdiak, 2002a; Jones, 2002; Pallix, 2002; Prusha, 2002). This vision could be strengthened by analyzing the impact these tasks will have on NASA missions. This system-level analysis is important to guide NASA managers on the scope of their tasks and the impact they may have on large, distributed physical systems (computers and hardware) as well as organizational systems for management and decision making. It was not apparent to the ECS panel that these plans are currently in place.

Recommendation: Each ECS task should have a system-level analysis of the impact it could have on NASA missions in order to determine the overall scope and the possible agency-wide impact of each ECS task.

SPECIFIC TASK DISCUSSIONS

The review panel evaluated individual tasks based on presentations from researchers during site visits and questionnaires completed by individual researchers. If the ECS panel had follow-up questions, individual members of the panel contacted the researchers directly or NRC staff requested additional information, such as published reports. Individual tasks are discussed under their projects, below.

The ECS panel found some tasks within the ECS portfolio that were world-class efforts. Tasks falling under this category have the potential to yield significant results for NASA and possibly for communities outside NASA. The panel points out some of those tasks below as examples.

The ECS panel found other tasks within the ECS portfolio that could be strengthened to make the overall ECS program better. The tasks identified as "Worthy Efforts That Could Be Strengthened" should not be discontinued but should be altered in a way to further benefit NASA.

While most of the ECS program is good, goal-oriented work that supports the NASA mission, the ECS panel identified two level 5 tasks that no longer appear to be relevant. These are discussed in the SRRM and KESS sections.

SYSTEM REASONING FOR RISK MANAGEMENT

The System Reasoning for Risk Management (SRRM) project consists of two elements, Integrated Risk Management Technologies and Integrated System Modeling and Reasoning. The objective of SRRM is to develop a comprehensive risk management approach for safety and system failure, to be used in the design and development of NASA aeronautics and space systems (Prusha, 2002). If successful, this work will yield a risk management framework that can be applied throughout program and system life cycles to achieve greater safety and dependability of operation than achieved by traditional design approaches still in use today.

SRRM research is critical to future NASA missions and has the potential for cross-NASA applicability, as well as applicability in the broader technical community outside NASA, but only if managed and implemented effectively. The introduction of a comprehensive risk management approach in the preliminary design phase rather than afterwards would enable design trade-offs between performance, cost, schedule, and safety throughout program and system life cycles. If NASA considers safety early in the design cycle, it can halt or alter the development of unsafe systems early on. Improving safety later in the design process can be extremely expensive.

The risk management framework supporting system design and development can help achieve a proper balance among system performance, schedule, cost, and safety, such that all of these parameters are at an acceptable level. System safety, in terms of the probability of catastrophic failure on the one hand and the probability of mission success on the other, will be provided, along with appropriate confidence bounds to delineate the uncertainties inherent in the estimates. A risk management framework may also be used to access the risk (probability and associated uncertainty) of achieving various system performance levels, system delivery dates, and cost.

The SRRM objectives appear to be both worthwhile and achievable with a reasonable investment of time and resources, considering the current state of the art in the various disciplines of design, mission, and organizational risk management. Improved system performance and safety appear to be within reach. Currently, programmatic risk assessment and risk management research outside NASA is scattered. With the expertise available to NASA in the SRRM project, it would be appropriate for the SRRM project to assume a greater role in the risk management process and tool development efforts within NASA. In addition, by leveraging work conducted outside NASA, SRRM has the opportunity to integrate best practices that are suitable to NASA missions and needs.

Finding: The SRRM project, conducted under the ECS program, is addressing critical issues in programmatic risk management. The project has the potential to advance the state of the art for NASA-wide and external applications.

The ECS panel has attempted to assess how the SRRM project has fared in terms of translating its top-level objectives into a set of well-defined, achievable goals. The panel has also examined how well these goals have been addressed in specific project tasks, as well as how these tasks as a whole will achieve overall program objectives.

The ECS panel understands that the SRRM project seeks to further the state of the art in important areas of systems engineering for aeronautics and space systems. It is assumed that the project would then organize its activities in a systems engineering fashion, with top-level project objectives flowing down to lower-level task objectives.

Initially, the ECS panel could not see much evidence that this orderly process was taking place, but to be fair, it could also see serious programmatic reasons for the delay in top-level project formulation. At first, the project was called Design for Safety (DFS), but very quickly, before significant project developments could take place, its objectives and impact shrank significantly owing to a reduction in budget. The panel assumed that a considerable realignment of activities took place because of these changes mandated from above.

The ECS panel saw evidence that the SRRM project included some tasks carried over from the DFS program together with some newly defined tasks. Thus, in assessing the work being conducted under the SRRM project, it would be unfair for the panel to ignore the background situation that has directly affected the program mode of development.

The SRRM management team proactively streamlined its efforts during the course of this review by twice revising its work breakdown structure. In this sense, the panel has been reviewing a moving target. With that said, the panel appreciates ECS management efforts to identify and start implementation of needed changes to the structure and organization of the SRRM project.

Between June and October 2002, there were indications that the ECS management tried to put a more disciplined approach into effect, especially in the SRRM project. As of April 2003, the project plan for SRRM (ECS, 2003) appeared to be sounder and better organized than at the beginning of the panel's review. In fact, the material presented in April 2003 by NASA demonstrated to the panel that the reorganized SRRM project, as well as the goals and objectives, was greatly improved and more in line with NASA's mission. The panel also saw evidence that the SRRM project had taken into account and acted on guidance provided by the PRT committee in the interim letter report issued in January 2003 (NRC, 2003). During the April 2003 revisit, NASA reported that the SRRM project had participated in the internal NASA review of the Columbia accident. Development and implementation of a mature risk management process might provide a framework for accident investigation, as well as a process for safety improvement.

At the start of the review, in June 2002, the ECS panel had concerns about the ad hoc flavor of the tasks (Gawdiak, 2002a; Prusha, 2002). However, after the April 2003 follow-up review of the redirected SRRM project, the panel found that the changes made to the project are good and appropriate. Details of the project's new makeup and comments on specific tasks are presented below.

Finding: The ECS panel concurs that the changes made during the course of the review to the SRRM project under the ECS program were correct and appropriate.

The ECS panel wishes to point out some of the key concepts that ECS management appears to have embraced. Specifically, the SRRM project has developed a coherent strategy and roadmap for the development of technology that will potentially apply to many NASA programs. In this process, ECS management identified how the individual tasks and research elements fit into a broader concept of risk management that incorporates metrics of risk in the early design phases and carries them all the way through final design and even implementation and operation. The panel wishes to emphasize that all activities under the SRRM project should be carried out in close consultation with the risk assessment technical community at large, as discussed in the next section.

Connections to the External Community

As part of the review process, the ECS panel was asked to examine how well the technical program was interacting with the community outside NASA. It found that the interaction of the SRRM project with the risk assessment and management community outside NASA has been limited and can use improvement. As presented, the tasks within the SRRM portfolio appeared to be based mostly on internal NASA work and knowledge despite the wealth of information available outside NASA that would only enhance the program. The panel observed little leverage of external work in mission and organization risk management. This has been especially true for the interaction (or lack thereof) of level 4 and level 5 task PIs with other communities.

As one example, the Risk Management Colloquium is held every year in Palo Alto, California, in the summer or early fall. This colloquium is cosponsored by NASA headquarters Code Q and NASA Ames Research Center. Several members of the SRRM team presented at, and took part in, this meeting in 2002. The panel encourages SRRM to take an even more active role, such as organizing a special session on SRRM objectives and needs. Since most of the NASA risk management and assessment community from all of the NASA centers would be present, along with some invited outside experts, such a session would expose a wide NASA audience to the SRRM project and goals.

In general, the ECS panel noted that the SRRM dialogue with the risk assessment community at large appears to have been limited and unorganized. It believes that there are ideas and expertise in the external technical community that could greatly benefit SRRM activities if better lines of communication and cooperation are established. There is some anecdotal evidence that SRRM connections to the external community are improving. The panel encourages SRRM to continue pursuing formal and informal interactions.

Recommendation: SRRM project management and task principal investigators should formally engage the internal NASA community as well as the external technical community as often as possible in order to gain exposure to external expertise and benefit from it.

Every technical community has a concept of what risk means to it. These communities also have their own methods for risk mitigation. To become the center of gravity for multiple communities, including NASA, and to maintain credibility in those design communities, ECS must demonstrate that it has knowledge of the state of the art and best practices in each of those communities. NASA can move toward demonstrating this understanding and credibility through the use of the Cross-Enterprise NASA Research Announcement (NRA) open solicitation process.

Recommendation: The SRRM project, conducted under the ECS program, should ensure that all task deliverables (engineering tools, etc.) can be used across the diversity of NASA projects and missions in order to maximize their effect. This broad applicability will require that researchers in the SRRM project become familiar with the challenges faced and methods used in many NASA projects and missions.

Research Portfolio

The ECS panel found that the level 4 and level 5 tasks appear to reasonably cover important areas of need, as defined by NASA and top-level ECS goals. At the start of this review, the ECS panel had concerns that the SRRM project was attempting to fund too many projects at a funding level too low to yield significant results. During the course of this review, the SRRM project correctly decided to reduce the number of tasks it was supporting (ECS, 2002; Penix and Jones, 2003). This appropriate action has yielded a much more effective but leaner SRRM organization. The ECS panel believes that this limited research, in view of limited funding, is appropriate.

As already stated, it is important for NASA to remember that SRRM results should be applicable to all NASA projects and missions. The project should also ensure that even during the initial development of the risk workstation that NASA has planned and is conducting, serious efforts should be made to capture and include all of the major risk factors in the trade-offs encountered in mission or system design. The ECS panel recognizes the difficulty of this undertaking. However, if not done carefully, incomplete design trades may produce misleading or incorrect information, resulting in a false sense of security in the final product.

The tasks being conducted under the SRRM project appear to be defined with an appropriate level of program risk in that they have a reasonable chance of successful completion. The ECS panel emphasizes that SRRM research inherently needs to proceed from a solid basis of previous developments, and in an orderly and systematic fashion. Therefore, the risk associated with development and application of the risk mitigation work has a medium chance of successful completion but, if completed, a high payoff for the NASA missions. SRRM is approaching and managing its risk appropriately, mainly by assigning the right people to the task in question or by soliciting help as needed.

People and Facilities

A positive aspect of the work being conducted under the SRRM project is that the project is bringing together people from multiple disciplines and perspectives to jointly develop a common vocabulary and achieve common goals. SRRM has apparently assembled this group in a very deliberate manner. First, it found the people in NASA it wants to work with and it has also developed a program management structure. SRRM has made an attempt to bring in people from the outside through the use of various NRAs. These are leaders in their fields who can easily fill any gaps in expertise. The facilities were in good condition and appropriate for the work being conducted.

Methodology

SRRM personnel have approached mission projects within NASA in order to explore and possibly create technology transition opportunities for SRRM products. The ECS panel commends the SRRM project for its successful use of this methodology. Once NASA missions take on SRRM products or processes, the SRRM work can reach a higher TRL more easily by virtue of its real-world use by NASA missions.

Of several notable successes of the SRRM project, one success is the use of the Technology Infusion, Maturity Assessment (TIMA) process by the Primary Atomic Reference Clock in Space mission. The same TIMA process was also used to validate the LabView software architecture for use in space. Finally, the Defect Detection and Prevention (DDP) program is being used by the Mars Science Laboratory mission, slated to fly in 2009. DDP is being used as a tool for program management, which is based on an underlying risk mitigation process.

These successes prove that SRRM has taken steps to ensure that the methods and techniques it develops can be migrated out to a user community. SRRM should continue to make certain that the applications propagate beyond these few programs to allow ECS to become a center of gravity for NASA in probabilistic risk-based design.

Recommendation: The SRRM project, under ECS, should further concentrate on migrating its developed techniques and methodologies, such as Technology Infusion Maturity Assessment (TIMA), to a user community within NASA in order to make ECS a center of gravity within NASA for risk assessment.

Quality of Work

At the start of the SRRM project, the ECS panel found that there was a mismatch between the types of tasks being conducted under SRRM and the SRRM project objectives (Prusha, 2002). Since then, SRRM has reduced the number of tasks being pursued and has also apparently made active use of the NRA process. As a result of these program changes, the SRRM project improved dramatically over the course of the review. The panel has determined that the current work being conducted in the SRRM project is on the right path and could become excellent quality and meet accepted peer-review standards.

Observations on Specific SRRM Tasks

World-Class Tasks

There are no tasks in this category for SRRM.

Completed Work or Areas Not Supporting the NASA Mission

Probabilistic Analysis of ISS Power System This task is being conducted as an SRRM level 2 task. Previous work under this task gave some very good and useful results on the power usage of the International Space Station. The follow-on efforts aim to bring the power system model to a higher resolution. The ECS panel believes that there is a diminishing rate of return on this activity and that there is no need to go beyond the current understanding of the ISS power system. In this sense, the work conducted under that task was very good, but it is complete. ECS management has canceled this task and the ECS review panel agrees with this decision.

KNOWLEDGE ENGINEERING FOR SAFETY AND SUCCESS

The Knowledge Engineering for Safety and Success (KESS) project concentrates on system-level model and methods design for system and human organizational risk and failure analysis. The project is divided into two level 3 elements: Human and Organizational Risk Management (HORM) and Knowledge Management (KM).

Human and Organizational Risk Management

This level 3 element aims to identify, model, predict, and mitigate technical and program risks as a function of the structure and processes of teams and organizations. This is a very ambitious undertaking. It involves the creation of a novel synthesis of observational, cognitive, technical, and organizational methodologies in the service of critically important NASA goals. If successful, this element could become a core competency within NASA.

The ECS panel found that the element summary presented by ECS management is well written and provides clear directions for the research to follow. Generally, the portfolio mix of beginning versus mature technology tasks appears to be appropriate. The ECS panel based its conclusion on the technology readiness level (TRL) system that NASA employs.

Key parts of this effort include the development and deployment of a useful multilevel model of risk perception and management while optimizing organizational performance. Some parts of this important set of tasks are easier to conceive and execute than others. While improvement in techniques and processes often appears to be straightforward, the approach on fundamental issues is more daunting.

For instance, developing the organizational portion of the risk management model would clearly be a major outcome. However, given the current state of organizational modeling, the early development and validation of such a model (to be completed in FY2003) seems overly optimistic. Important questions that remain to be answered are these:

- What are the research hypotheses?
- What would a model look like?
- How would such a model be used?
- In what sense would the model be computational?
- How would it be validated?

The ECS panel knows that the research team is well aware of these issues, yet seemingly infeasible milestones are presented as part of the overall plan. It is possible that the short development time may have been imposed from above the element level to foster coherence across programs. While timeliness is also a desirable goal, the ECS panel believes that the development of novel models and validation methodologies should be given enough time.

Recommendation: The Human and Organizational Risk Management level 3 element within the ECS program should establish a feasible time frame for the development of novel models and methodologies in order to allow researchers an appropriate amount of time to generate measurable results.

The ECS panel commends NASA for taking on such a challenging task as developing complex organizational models. A critically important aspect of the modeling research is its multidisciplinary nature. The ECS panel believes that the model development effort can be improved by having social scientists work on the same team with engineers, systems/equipment operators, and computational model developers. This type of interaction should be in addition to engaging in collegial dialogues. The diverse group could integrate the individual expertise of its members into the joint enterprise and together be responsible for the outcome. Closer ties with the multiagent systems researchers in the CICT program would also help to create computational models of organizations that could be used to predict risk factors.

Recommendation: To improve the model development process, the Human and Organizational Risk Management level 3 element within the ECS program should increase the diversity of the research team by including engineers, systems/equipment operators, and computational model developers. The effort within ECS should also establish closer ties to the NASA CICT program to help in the creation of computational models.

One place where this multidisciplinary approach is being tried is in the empirical analysis of problems that may occur during shift handovers, as identified in the level 5 task Understanding Shift Handovers in Mission Control, for which a new choice capture tool has been implemented in the level 5 task Choice Capture Technology for Mission Control. The work is important in that it could improve the success of shift handovers, as well as provide a means to evaluate the usability and utility of the developed tool. Other aspects of multidisciplinary research that could be incorporated into the Human and Organizational Risk Management element include, for example, results from artificial intelligence (especially multiagent systems), distributed cognition, and natural language processing, particularly in the design and deployment of controlled languages as they are being developed by Boeing Corporation.

Since organizational dynamics figure prominently in mishaps, it is very important that more effort be devoted to understanding and modeling organizations in tasks under the Human and Organizational Risk Management element. For instance, organizational metrics are not as well understood as team metrics and depend on an organizational model for relating team and organizational performance. The ECS panel believes that it is important for NASA to increase its knowledge of the organizational processes, climates, and information flows in order to detect and repair latent pathogens in complex systems (Reason, 1990). These so-called pathogens were cited in recent accidents, including the

Columbia tragedy. Multiorganizational processes and relationships, such as interorganizational relationships and dynamics, should also be studied. Finally, some account needs to be taken of the role of management priorities in setting safety/risk policies and in their assessment. At the start of the review, the ECS panel was concerned that the technical and management links between the three elements within ECS were not strong. As of April 2003, it appeared to the panel that the ECS program has established those linkages. The panel believes that these linkages should be strengthened, especially between Human and Organizational Risk Management and SRRM.

Recommendation: In terms of overlap between areas of expertise within ECS, it would be desirable for the Human and Organizational Risk Management element to be more explicitly integrated with the products coming from the System Reasoning for Risk Management project.

Knowledge Management

The Knowledge Management level 3 element offers a number of exciting, well-integrated research efforts that aim to capture and represent knowledge of the structure and function of spacecraft hardware throughout that hardware's life cycle. The results of these efforts will be an integration of contractor and agency knowledge bases that could be used by designers, operators, and maintenance personnel, as well as by automated diagnostic systems. Two examples of this work are the level 4 task Wire Integrity Research and the level 5 task Hybrid Reflectometer.

The ECS panel deemed that the Virtual Iron Bird level 4 task is especially notable. The goal of the task is to build a detailed virtual model of the space shuttle as a means to visualize and interact with a virtual shuttle before attempting operations on the vehicle itself that have a high risk of damaging the shuttle. This project is showing the way forward for other spacecraft designs.

Also notable under the Knowledge Management element are two database collection tasks, Lifecycle Systems Integration and Digital Modeling, that will enable NASA to capture technical measurements and specifications of the shuttle and other spacecraft designs. These efforts will allow NASA to take into account uncertainties in structure and function that are created by wear, maintenance, and undocumented modifications.

There is some general overlap between the Knowledge Management element within ECS and another PRT program, the Computing Information and Communications Technology (CICT) program. The ECS panel looked carefully at this issue and found that stronger collaboration between the two groups would create an even more goal-directed program.

Recommendation: The Knowledge Management element within ECS should work together with Computing, Information, and Communications Technology (CICT) program researchers to prioritize research on computational tools that underlie the Knowledge Management element's efforts.

The ECS panel noted that one level 4 task, Inter-Organizational Process Analysis, and its two subtasks, Socio-Technical Approach for Identifying Ground Processing Errors and Human Factors in Inter-Organizational Process Analysis, appear not to fit the focus of the Knowledge Management level 3 element because these two subtasks involve characteristics of organizations, which are supposed to be incorporated under the level 3 element Human and Organizational Risk Management. Moreover, the ECS panel believes that the level 4 task, as currently executed, is not likely to produce the desired general knowledge.

Recommendation: The level 4 task Inter-Organizational Process Analysis should be integrated under the Human and Organizational Risk Management level 3 element with better designed subtasks in order to make the effort more effective.

Observations on Specific KESS Tasks

World-Class Tasks

Organizational Risk Perception and Management This level 4 task, placed under the level 2 KESS project, is a well-written overview of the domain and task directions for risk perception. The scientific background and research progress of this task is also impressive.

Virtual Iron Birds This level 4 task under the level 2 KESS project appears to be an innovative approach to allowing users to visualize and interact with information about the structure and function of complex systems. The results will support maintenance and modification operations.

Worthy Efforts That Could Be Strengthened

Organizational Metrics At the start of the review cycle, this level 5 task under the level 2 KESS project demonstrated a good beginning to an effort that consists of NASA-relevant work. A weakness identified at that time was that the project researchers should have been more cognizant of the literature in the high reliability field in order to give the technical community at large more confidence in the ultimate result of the task. Also, the panel felt that the PI should consider collaborating or consulting with the high reliability community. As of April 2003, in conjunction with the Human and Organizational Risk Management element, the team had conducted a literature search of organizational culture, safety culture, and high-reliability organizations. It also apparently cross-linked this effort to other researchers pursuing similar topics. The panel commends ECS for taking all of these steps.

Interorganization Process Analysis At the start of the review, the panel was concerned that, given the current working plans, this level 4 task under the level 2 KESS project was not likely to produce the general knowledge that is being sought. In early 2003, the task was rescoped by ECS management and placed under the direct support of the Digital Shuttle task. The panel concurs with this action.

Human Factors in Interorganizational Process Analysis This level 5 task under the level 2 KESS project would benefit by involving more basic science principles. As described in the PI's response to the questionnaire, the task "builds on the methods and tools of psychology, anthropology, linguistics, and communication sciences," and presumably employs "field and observational methods to characterize technical operations." However, this level of generality is not helpful for understanding the strengths and weaknesses of an effort at TRL 4-6, where the expectation of near-term positive outcomes needs to be based on earlier basic science. For the work to succeed, it is critical to specify the conceptual frameworks and corroborated results from the previous research on which the task will build and to which the task should be expected to contribute. The absence of such well-articulated starting points is of concern to the panel.

Wire Integrity Research This level 4 task under the level 2 KESS project and the level 5 task under it, Hybrid Reflectometer, are good work and should continue. The effort aims to develop techniques that would automatically diagnose the state of health of wiring on aging spacecraft and is directly applicable to NASA's missions.

At the beginning of the review, the ECS panel believed that this research activity might be generalizable beyond the shuttle and that it therefore needed to be more basic. Also, the panel found that the project should include more physics-based measurement research on techniques applicable to multiple systems. To strengthen the project, NASA should take more of a leadership role in industry and academia in this technology field than it has been taking. The ECS program, apparently the leader at this point in wire integrity research within NASA, could expand its efforts, specifically by involving NASA's Office of Safety and Mission Assurance (Code Q).

As of April 2003, the panel was satisfied that the task PI and management have significantly improved their work by more actively engaging the external community and participating in the research on physics-based measurement techniques. The ECS panel commends the members of the Wire Integrity Team for their good performance given the limited amount of funding.

Areas Not Supporting the NASA Mission or Completed Work

Sociotechnical Approach for Identifying Ground Processing Risk This task is being conducted under the level 2 KESS project. The private contracting company that NASA employs has little demonstrated familiarity with sociotechnical systems theory. Also, it has no approach for integrating social and technical risk. It is not clear whether the off-the-shelf tool the contractor developed and the experience it has with medication errors in hospitals will generalize to the NASA-specific task. There would not be opportunities for validating the resulting error estimates, especially the interdependencies between errors, using the task's current plan. In general, the activities rely too heavily on unverifiable judgments.

As of April 2003, this task was still ongoing. The review panel understands that it intended to provide support to the Digital Shuttle effort. However, the work conducted under this task could seriously jeopardize the overall success of the Digital Shuttle effort. Unless drastic changes are made to the task, including changing the support contractors, the review panel recommends that it be discontinued, as the quality of work is not up to NASA standards.

Recommendation: The ECS panel recommends that the level 5 task Socio-Technical Approach for Identifying Ground Processing Risk be discontinued or its support contractors replaced with more qualified personnel.

RESILIENT SYSTEMS AND OPERATIONS

The Resilient Systems and Operations (RSO) project was formed to address safety and stability issues in mission software, autonomous systems, and human-machine interfaces (Pallix, 2002). The project comprises two level 3 elements, Intelligent and Adaptive Operations and Control and Resilient Software Engineering. The Intelligent and Adaptive Operations and Control element encompasses four level 4 tasks. The Resilient Software Engineering element includes two level 4 tasks and seven level 5 tasks.

The RSO project appears to be broad in scope, and its two principal components appear to be central to the NASA mission. The project has engaged some of the nation’s best talent in human-computer interaction, especially in software engineering. The ECS panel noted particularly exemplary efforts in JAVA Pathfinder and in the establishment of the NASA Ames Research Center testbed for computer code verification. With this said, the ECS panel did have concerns about the direct applicability to NASA of some of the tasks, as discussed in the next section. As of April 2003, the RSO project had made significant improvements, including becoming more customer- and application-oriented.

Intelligent and Adaptive Operations and Control

The Intelligent and Adaptive Operations and Control element has a very well-balanced portfolio of level 4 tasks in terms of their probability of success. There is a good mix of TRLs across the tasks. The TRL range is from 1 to 2 for the task Autonomous Propulsion System Technologies and 4 to 7 for the task Adaptive Flight Control Research. Overall, the tasks are comparable to and competitive with academic work in the same category. From discussions it had with management of this element, the ECS panel anticipates that the element’s work will take the positive step of concentrating more on autonomous operations for systems than on system components, as it does now.

Resilient Software Engineering

The use of software is pervasive throughout all the NASA missions. There has long been a challenge for both NASA and industry to produce reliable software in a timely, cost-effective manner. As such, the Resilient Software Engineering element is central to NASA’s mission.

The potential benefits of better software development techniques are very great: They could improve the prospects for successful missions and ensure that missions meet their cost objectives. However, it was difficult for the ECS panel to determine if the tasks being conducted under the Resilient Software Engineering element are likely to have a significant impact on software development within NASA given the long history of research and work in these areas outside NASA and the lack of specificity in identifying NASA software challenges in task descriptions.

Software engineering in general and dependable systems in particular are well-developed research disciplines ubiquitous in academia and industry. Resilient Software Engineering element research in these areas follows several currently popular approaches. However, the rationale for NASA selecting these specific approaches over others was unclear to the review panel at the beginning of the review, as was the applicability of the tasks to NASA’s most pressing software problems.

At the start of the review, academic participation in High Dependability Computing research, a level 4 task within Resilient Software Engineering, appeared to have been heavily skewed toward the conventional software engineering community and did not give sufficient weight to researchers from other important communities. These other communities include real-time computing, dependable computing, and static program analysis, all three of which could make valuable contributions.

As of April 2003, the work under the Resilient Software Engineering element had greatly improved. While there is much work to be done, the top-level view of the collection of tasks was much more focused and the element’s goals appeared to be relevant to NASA or, at a minimum, to other work being done within the ECS program, such as the SRRM project. The element has also apparently expanded its involvement within

NASA by making contact with researchers at Marshall Space Flight Center and outside NASA with the United Space Alliance. It has also added at least one task involving network dependability to the mix of tasks being pursued. Finally, the element adjusted its mix by dropping some tasks that were complete or not performing to expectations. The panel concurs that all of these actions should improve the overall Resilient Software Engineering element.

Finding: The Resilient Software Engineering element tasks, while showing improvement over the course of the review, appear to have limited participation by other software-related communities such as real-time computing, dependable computing, and static program analysis.

Research by the Intelligent Software Engineering Tools task exhibits considerable overlap with that by active academic and commercial research, as many of the problems are shared by industry, including the high cost of software development, the difficulty of large-scale collaboration, and the need to ensure high dependability. Since funding for the NASA effort is scanty, the panel questions the benefit of duplicative efforts. To maximize the impact of this work on NASA, the panel recommends focusing the research on problems of high priority to NASA that are not adequately addressed by outside research and commercial tools.

Recommendation: The Intelligent Software Engineering Tools component of the Resilient Software Engineering element within the RSO project of ECS should continue to identify which of NASA's high-priority software problems are unique to NASA and which high-priority software problems are not being addressed by industry research or the academic community. Based on this identification, management should then shift resources under the Resilient Software Engineering element as needed. The ECS panel encourages NASA to continue to seek involvement from other software-related communities, such as dependable computing and static analysis.

The high dependability testbed included in the level 5 tasks High Dependability Computing, Testbed for Reusable Flight Software, and Dependable Networks for Flight Testbed has the potential to make significant contributions to NASA and beyond. Collaboration with academics by making testbeds available to them is an excellent and relatively new approach that allows academic researchers to work on problems immediately relevant to NASA. The result of such a testbed collaboration should be flight-qualified or near-flight-qualified software that is made available to the general research community. Such software is often difficult to obtain from other sources.

Recommendation: The ECS panel suggests that ECS management make the flight software code developed at the Ames Research Center testbed available to the general research community.

The ECS panel notes that the Ames Research Center established such a software dependability laboratory in the past—namely, the Digital Flight Control Systems Verification Lab (DFCSVL), a facility that existed at Ames about 20 years ago. The panel hopes that the current effort can be sustained, since past similar work was discontinued. While the ECS panel is enthusiastic about the establishment of such a laboratory, it urges project managers to carefully plan for sustainability of the lab.

Because it involves complete systems, software development under the Resilient Software Engineering element is very complex. Examples of this complexity are evident in the Jet Propulsion Laboratory's (JPL's) Mission Data System (MDS) software, a project for managing telemetry, flight, and experiment data onboard a spacecraft, and the code for the Center Terminal Radar Approach Control (TRACON) Automation System (CTAS), a system providing controllers with various aids for more effectively directing the flow of aircraft at busy airports.[1] In fact, the software may be too complex for this (or any other) testbed to perform the intensive verification and validation needed for high-dependability software. It is unclear what benefit could be gained from unit or subsystem testing of the MDS or CTAS, because the development organizations (JPL and Ames) are already performing such testing. The reported goal of the new Ames laboratory is to perform integrated testing.

It appears that the level 4 tasks Autonomous Propulsion System Technologies and Adaptive Flight Control Research, which do not have a significant verification and validation component, could definitely benefit

[1]See <www.ctas.arc.nasa.gov/CTAS>.

from the activities of the Ames laboratory. These tasks, if successful, would be subject to extensive certification and qualification requirements if the technology developed is eventually applied to civil aircraft. The ECS panel therefore encourages the project to concentrate the laboratory testbed research on these two level 4 tasks as these two tasks will in all likelihood benefit the most.

Recommendation: The ECS panel suggests that NASA management concentrate the Ames Research Center software verification and validation laboratory testbed on the level 4 tasks Autonomous Propulsion System Technologies and Adaptive Flight Control Research being conducted under the Resilient Systems and Operations level 2 project.

Observations on Specific RSO Tasks

World-Class Tasks

Advanced Software Verification and Testing Tools This level 5 task is contained under the RSO level 2 project. It concentrates on statically analyzing software to find bugs and has a clear motivation and approach. The work also builds clearly on previous work at Ames Research Center.

Worthy Efforts That Could Be Strengthened

Empirically Validated Software Dependability Model The ECS panel questions whether this level 5 task, under the level 3 Resilient Software Engineering element, will yield the desired results. The validity and usefulness of the software dependability model that is being used have not been explored, but the model is slated to be transferred directly to development efforts. The TRL listed for the task is based on high expectations that the model will work in a new domain.

Perhaps the most important question here concerns the data being used. Without adequate data, this research, like much of the research that preceded it over the last three decades, has a far lower probability of success than the 75 percent estimated in the task description. Much of the data that might be used in this program originated in the NASA Goddard Software Engineering Laboratory (SEL). NASA has a wealth of other data that could be made available to this effort. The highly qualified lead on this task will have a much higher likelihood of success if a concerted effort is made to provide the research team with data and the background information necessary to assemble such data.

Dependable Networks for Flight Testbed This level 5 task under Resilient Software Engineering is being developed to assess the applicability of dependability technologies to potential next-generation ISS onboard information technology capabilities. At the start of the review, the panel found that the task could be improved by involving researchers from the distributed systems and reliable computing communities. These communities have been working on similar issues for many years. As of April 2003, ECS had taken significant steps to involve researchers in these other areas. The review panel is comfortable that the effort should yield useful results.

Intelligent Software Engineering Tools The comments the ECS panel has for this level 4 task under the level 3 Resilient Software Engineering element also apply to the level 5 tasks under it except for the level 5 task Advanced Software Verification and Testing Tools, which the ECS panel deems was world-class (criteria listed in Chapter 2).

At the start of the review, the ECS panel found that the goals of the general tools projects could have been more solidly formulated and given a clearer motivation. The panel was unable to determine at that time if there were any novel ideas or approaches for these tasks. In particular, it was unclear why NASA needed to develop its own tools in light of the many commercial and open-source projects cited in the project descriptions.

Specifically, the motivation for the level 5 task Formal Specifications Database, to create a database of formal specifications, was not clear, nor did the panel understand how such a database would be searched or who the end user community would be.

During the course of the review, the ECS management canceled the level 5 task Collaborative Software Engineering Tools and descoped the other two tasks: Formal Specifications Database and Formal Enough Notations for Computer System Engineering. The ECS panel believes that the ECS management took the correct action in this case. Furthermore, as discussed in the preceding section, ECS has taken additional positive steps by engaging the internal NASA community to identify those NASA-specific problems that need to be addressed.

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5

Report of the Panel on Enabling Concepts and Technologies

INTRODUCTION

NASA's Enabling Concepts and Technologies (ECT) program was created as one of three subprograms of the Pioneering Revolutionary Technology (PRT) program by the Aerospace Technology Enterprise (ATE) in October 2001. The program consists of several elements that were previously funded under separate programs throughout NASA. ECT is described as the "front-end of the technology pipeline that feeds the focused development and validation programs of the NASA Enterprises" (Moore, 2002). ECT is described by the same source as the arm of NASA that performs fundamental research and development of "high-risk, high-payoff cross-cutting technologies with broad potential application to the needs of multiple Enterprises." According to Moore, the program objectives for ECT are these:

- Explore revolutionary aerospace system concepts to enable the grand challenges and strategic visions of the NASA Enterprises and to expand the possibilities for future NASA missions.
- Develop advanced technology for sensing and spacecraft systems to enable bold new missions of exploration and to provide increased scientific return at lower cost.
- Develop advanced energetics technology to provide low-cost power and propulsion for enhanced mission capabilities and to enable missions beyond current horizons.

The ECT program is divided into three main projects, which map to these goals:

- Advanced Systems Concepts, which includes three elements: Technology Assessment Analysis (TAA), Revolutionary Aerospace Systems Concepts (RASC), and the NASA Institute for Advanced Concepts (NIAC),
- Energetics, which includes Advanced Energy Systems and On-Board Propulsion elements, and
- Advanced Spacecraft and Science Components, which includes four elements: Advanced Measurement and Detection (AMD), Distributed and Micro-Spacecraft (D&MS), Resilient Materials and Structures (RMS), and Space Environmental Effects (SEE).

An organization chart for the entire PRT program can be found in Appendix C. Projects are located and managed at four NASA centers: Glenn Research Center, Goddard Space Flight Center (GSFC), Langley Research Center, and the Jet Propulsion Laboratory.

The ECT program's projects and elements were funded at the levels[1] reported in Table 5-1. External

[1]NASA research budgets, until the recent release of the proposed FY2004 budget, were not presented in full-cost accounting form. As a result, budget figures presented here do not reflect full-cost accounting.

TABLE 5-1 Enabling Concepts and Technologies (ECT) Program Organization and Budget, FY2002 and FY2003 (million $)[a]

Project/Element	FY2002	FY2003
Advanced Systems Concepts project	13.0	34.6
Technology Assessment Analysis element	0.0	1.6
Revolutionary Aerospace Systems Concepts element	8.0	8.0
NASA Institute of Advanced Concepts element	4.0	4.0
NASA Technology Inventory and Miscellaneous	1.0	1.0
Space Architect	0.0	20.0
Energetics project	17.7	16.6
Advanced Energy Systems element	13.1	n/a
On-board Propulsion element	4.6	n/a
Advanced Spacecraft and Science Components project	18.5	23.2[b]
Advanced Measurement and Detection element	10.2	13.1
Distributed and Micro-Spacecraft element	2.8	3.9
Resilient Materials and Structures element	4.0	4.7
Space Environmental Effects element	1.5	1.5
Space-based NRAs	40.0	40.0
Congressional earmarks	3.6	n/a
ECT program, total	92.8	114.5

[a]Program organization and budgets for FY2005 and future years are currently under planning and as a result are not presented in this table. Preliminary information indicates that further changes will be made to the ECT program at this time, including possible refocusing and defocusing of several program elements.

[b]This entry reflects the sum of projects and elements within ECT that were organized within the Advanced Spacecraft and Science Components (ASSC) project in FY2002. During FY2003, projects were organized in a slightly different manner, which is not reflected in this chart. Components of the ASSC project were broken into three new projects: Revolutionary Spacecraft Systems (including Distributed and Microspacecraft and Space Environmental Effects), Advanced Measurement and Detection, and Large Space Structures (including Resilient Materials and Structures and a new Large-Aperture Technology element). A third reorganization is anticipated in FY2005.

SOURCE: Adapted in part from Moore (2002 and 2003b).

NASA Research Announcements (NRAs), also referred to by the program as the Space-Based NRAs, are funded at $40 million per year. This broad set of NRAs, discussed in a section to follow, was designed to infuse innovative technology into NASA from various experts, both foreign and domestic. Two future program elements, Revolutionary Spaceflight Research and Multi-technology Integrated Systems, were not evaluated by the panel since they are not scheduled to begin until FY2005.

The ECT program is also designed to promote a transition between fundamental research and mission-oriented, applied research (see Figure 5-1). The goal of the program is to fund 50 percent in the exploration phase (TRL 1-3) and 50 percent in the transition phase (TRL 4-6). Furthermore, the intent of the exploration phase is to promote the development of ideas from outside NASA via NRAs and other contractual mechanisms. The transition phase is used to promote new technologies to other NASA enterprises. The cofunding of projects is emphasized in this phase.

REVIEW PROCESS

The Panel on Enabling Concepts and Technologies was constituted in early June 2002 as one of three panels supporting the Committee for the Review of NASA's Pioneering Revolutionary Technology (PRT) Program. Its charge was to review all projects and elements within the ECT program. The ECT panel met June 10-12, 2002, at NASA Ames Research Center in conjunction with the Computing, Information, and Communications Technology (CICT) and Engineering for Complex Systems (ECS) panels. At this first meeting, panel members received broad overviews of the PRT program, the research within ECT, and the elements and tasks within the ECT projects. After this initial meeting, members of the panel visited various

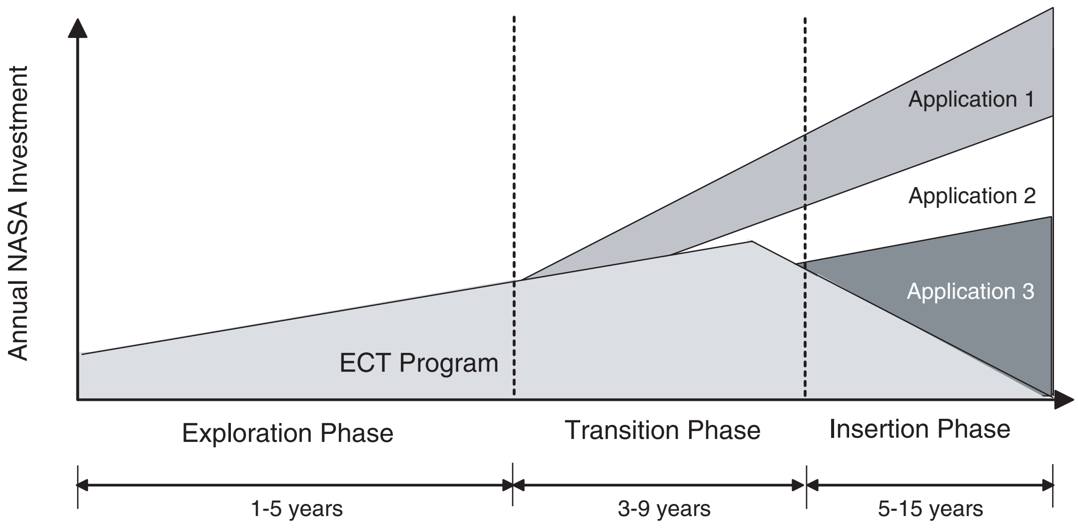

FIGURE 5-1 ECT program implementation strategy. SOURCE: Adapted in part from Moore (2002).

NASA field centers to interact directly with the researchers and to delve more deeply into specific project areas (see Appendix D).

Parallel to the site visits, panelists received responses from questionnaires designed to elicit information on specific tasks within the ECT program (see Appendix E). Information on the research's tie to previous work, potential customers for the technology, roadblocks being faced, and other information were obtained.

ECT panel members then met in Washington, D.C., for a final panel meeting to report on site visits, teleconferences, and other information-gathering activities. Subgroups held meetings to come to consensus on final observations, findings, and recommendations, and the complete panel addressed similar topics from a global standpoint. After the final meeting, the systems subgroup of the panel held a final teleconference on October 3, 2002, with NASA PRT and ECT managers to discuss the status of systems analysis and to address issues that had arisen during the open sessions with NASA in this area.

GENERAL OBSERVATIONS

The following subsections present general findings and recommendations that apply to the ECT program as a whole. More detailed findings are presented in subsequent sections that discuss individual projects and elements within ECT.

Goals and Research Portfolio

The appropriateness of each research project was evaluated based on (1) the relevance of the tasks to the overlapping NASA strategic plans[2] (Goldin, 2000; O'Keefe, 2002), its science themes, and the derivative missions and (2) the criteria for PRT research within the charter and strategic plan of NASA's Office of Aerospace Technology (Code R) (Venneri, 2001). The ECT panel also evaluated each project in terms of the degree to which it is revolutionary or evolutionary, its risk, and its orientation to fundamental science or applications. To distinguish evolutionary from revolutionary, the panel assessed whether the work was (1) a natural extension of known methods applied to the same problem (evolutionary) or (2) a departure from traditional methods, or used methods from another area or discipline not normally applied to this field, or involved the discovery or utilization of new physical discoveries and theories or phenomena (revolutionary).

The panel understands that terms such as "revolutionary" and "pioneering" can be subjective and unclear in the context of this review. In the area of spacecraft technologies, concepts can appear very revolutionary and generate significant visibility for

[2]The PRT program was formulated under the NASA Strategic Plan 2000. The program began operating under the new Strategic Vision in April 2002, just a few months before the review began.

themselves yet provide little or no benefit to actual space systems when flight engineered to the spacecraft system level. For example, a new propulsion device may be very efficient at accelerating propellant in the laboratory and therefore seem attractive in terms of reducing power and propellant requirements. However, if this same device requires other high-risk, high-impact subsystems, the additional requirements must also be considered in the evaluation of the device. Therefore, the following guidelines were adopted for use in the review:

- "Revolutionary" or "pioneering" technologies are technologies that could have orders of magnitude benefits for a spacecraft or space mission. Specifically, the panel recognizes a technology as revolutionary if it has the potential to remarkably improve satellite and space mission performance, cost, or simplicity, taking into account the issues associated with development, qualification, and insertion into flight systems.
- Conversely, seemingly revolutionary new concepts that do not consider the systems applicability and impact are not *automatically* highly regarded by the panel.
- Since both cost and performance are principal drivers in new technology development, revolutionary new concepts are also evaluated in terms of their total life-cycle costs, supportability, and test and evaluation requirements.

The ECT program is intended to include both revolutionary basic research and evolutionary basic or transitional work that meets NASA's needs. The balance of this research should be consistent with top-level program goals. In analyzing the entire portfolio of ECT, the panel felt that the ratio between evolutionary and revolutionary work should be reevaluated. It seems that the program's top-level goals (Hanks, 2002) emphasize revolutionary work while the program itself actually consists of both revolutionary and evolutionary research. Placing an emphasis on research labeled "revolutionary" might wrongly imply that evolutionary work has less value. What NASA appears to really need is excellent quality, high-impact research.

A consideration in achieving such excellent quality is the degree to which the research is (and should be) connected to an application. The ECT program properly includes research across this spectrum. There are applied projects well connected to specific missions, balanced by other projects more oriented to solutions that can be generalized. ECT includes both basic and applied research.

Finding: To carry out its mission of both innovation and transition, projects with varying degrees of risk and maturity must be part of the ECT program.

Recommendation: Value should be attached to excellent quality research that will have (or could have) a substantial impact on NASA missions, independent of whether it is perceived to be revolutionary or not.

Recommendation: Regular critical reviews of the progress of projects (both in-house and out-of-house) should be performed, with periodic quantitative reassessment of their relevance and system benefit to proposed high-level NASA mission priorities and comparison with competing technologies.

At the same time, several elements within ECT should reevaluate their portfolio and goals and consider riskier, even revolutionary approaches. For example, the Resilient Materials and Structures element should consider more tasks that embrace the ECT far-reaching vision of resilient materials and structures (Hanks, 2002), which involves concepts such as self-assessment, self-healing, and multifunctionality. It is also important to note that the onboard propulsion Energetics work has been purposefully chosen to be more evolutionary in nature than other NASA programs in onboard propulsion. (See additional discussion on page 72.) The most rigorous way of choosing a research portfolio should be through a systems analysis that considers the realistic potential of proposed technology developments. NASA should require, for example, that research in radically new approaches consider performance goals in relation to the current state of the art. The performance of a new technology sometimes begins behind that of a state-of-the–art technology, but over time, the new technology should overtake and exceed the old. The panel recognizes that there is not always a way to rigorously represent new technology in a systems analysis since appropriate performance metrics may not yet be available. In this case, managers should use their current knowledge of potential technological advances in concert with systems analysis in order to not miss potentially revolutionary work.

This is particularly the case for research into mission-enabling technologies that might not necessarily provide cost, weight, or power saving but instead might enable missions that were previously technologically impossible.

Program goals for the ECT program are well connected to both PRT-level goals and Aerospace Technology Enterprise (ATE)-level goals that ultimately feed into the NASA-wide goals developed in the 2000 NASA Strategic Plan (Goldin, 2000). However, little top-down strategic planning within ECT connecting these top-level goals with the actual research being performed was seen. The top-down direction may be lacking simply because ECT inherited projects and NRA work from the previous management of various individual PRT components. The panel notes that NASA managers plan to develop future portfolios within this program using strategic planning tools and processes, such as the Technology Assessment Analysis (TAA). The panel supports a systems approach, but notes the current direction of TAA may not provide this capability (see TAA section).

The panel also observed that many of the elements focused on individual technology advancement without an overall look at the effect those individual components had on an entire spacecraft system or a specific mission. While increasing the performance of individual components is important, the impact of various choices on the entire system must be considered. The panel was troubled by the lack of even simple (i.e., first-order) systems calculations to support technology investment decisions. For example, the Stirling work in Energetics (see page 76) has promise, but the panel did not hear of an adequate assessment of the effects of vibration on the entire spacecraft or a comparison with other technological solutions in development at other research organizations. The panel also saw several routine thermal projects within ECT that address the lowest mass and cost elements of small satellites and therefore would be expected to have little impact.

Panel members note that most activity within ECT focuses on space systems, yet the scope of the objectives could also apply to planetary probes, rovers, and other space exploration and development technologies. Other NASA technology development programs that are not within the purview of this review overlap the ECT technology areas but are managed and funded within other NASA enterprises. However, the basic research being conducted by these other programs should also be considered during ECT program portfolio selection. For example, a NASA-wide microspacecraft technology roadmap would enable better coordination between related technology development programs throughout NASA.

Finding: Many ECT tasks do not include a systems-level viewpoint in their research. Systems analysis was lacking in many areas and at various levels of the ECT program.

Recommendation: Systems analysis should be strengthened as a crucial part of the portfolio management and project selection process to support investment decisions in the technology areas needing development. This process should recognize the priorities NASA has for its missions and the potential impact the research projects have on enabling and enhancing those missions. This process should also be applied to individual tasks and used by individual researchers as a mechanism for ensuring that research goals retain their original desired relevance. However, it should not be so rigid as to disallow serendipity and ideas of opportunity.

Technical Quality

Most of the tasks within the ECT program were deemed either good or excellent on an individual basis. A few projects had poor methodology, limited experimental setups, and/or lack of planning, but they were generally funded at relatively low levels. ECT panel members judged approximately 20 percent of the ECT program to be world-class (criteria listed in Chapter 2). Areas (and individual tasks) of world-class quality singled out by the panel were these:

- Hall, ion, and pulsed plasma thrusters in electric propulsion, advanced photovoltaics technology, and advanced energy storage work, all within the Energetics element,
- The radio frequency/terahertz (RF/THz) thrust, the focal plane thrust, microshutter arrays, and microthermopile arrays within the AMD element,
- Modulation Sideband Technology for Absolute Range (MSTAR) and formation flying work within D&MS, and
- Experimental and Analytical Methods for Characterization of Gossamer Structures in RMS.

The SEE element also provides a unique and much-needed service to the spacecraft design community. These areas of research are discussed in more detail in the individual project and element sections below.

Finding: The panel judged approximately 20 percent of the ECT program to be world-class. Specific areas of world-class quality within the ECT program include the radio frequency/terahertz thrust, the focal plane thrust, the microshutter arrays, and the microthermopile arrays in Advanced Measurement and Detection; electric propulsion, advanced photovoltaics technology, and advanced energy storage in Energetics; modulated sideband technology and formation flying in Distributed and Micro-Spacecraft; and gossamer structure characterization in Resilient Materials and Structures.

Generally the panel found good quality researchers in all programs. There were, as for any program, researchers at all levels of capability, experience, and quality of work. Many of the top researchers also had a firm grasp of what needed to be considered for a technology to be adopted by a mission or transitioned for other uses. Such an understanding is not always found in the research community or reflected positively in the mission orientation and end goals of the ECT program. In other cases, the ECT panel observed a lack of connection between the researchers and their customers. The role of on-site support contractors in the ECT program was not made clear to panelists during site visits or other briefings. Most support contractors work seamlessly with NASA civil servants on a day-to-day basis.

There were a few instances of researchers pursuing concepts that they had invented and patented, such as electric propulsion hollow cathodes, microelectromechanical system (MEMS) Stirling coolers, and intercalated graphite shielding. These tasks were funded by the Energetics project, albeit at a relatively low and appropriate level. In some instances a case could be made that these research projects were out of scope and better moved to another NASA center. However, the ECT panel found this to be an excellent practice when it comes to developing and retaining top researchers. Scientists need the flexibility to pursue their new ideas. Good managers provide these scientists with a reasonable amount of time and funding to encourage innovative concepts that can lead to pioneering, revolutionary technology. Such "blue-sky" ideas may mature into valuable and much-used technology.

The panel also noted instances where researchers appeared overburdened with marketing and advocacy activities that competed with existing and new research for valuable time and resources, although the need to "sell" a program is recognized.

Recommendation: Since flexibility and serendipity are key elements of basic research programs, the ECT program should continue to allow its top scientists small, short-term amounts of funding to pursue ideas that may not be entirely within the rigid scope of the program or that may at first seem to provide little return on investment.

Facilities at all locations were deemed excellent for the types of work performed, the main exception being the inability to test chemical propellants at NASA Glenn. The E-beam lithography lab at the Jet Propulsion Laboratory (JPL) and the Polymer Rechargeable Battery Lab and the electric propulsion test facilities at NASA Glenn are all world-class facilities. More specific discussion of facilities can be found in the program element sections below.

External peer review seems to be used effectively in selecting the external work funded under Space-Based NRAs and in the external NRAs within the SEE element. However, the panel observed little evidence of comprehensive external peer review of internal NASA work in the ECT program. The panel notes that PRT-wide reviews are performed by the PRT subcommittee of the Aerospace Technology Advisory Committee (ATAC) and the PRT Technology Needs Council; however, these reviews focus on programmatics and not necessarily on technical quality. Peer review is used at one of the ECT centers, NASA Langley Research Center, to evaluate its own organization. However, this review is not taken into account by the PRT management at NASA Headquarters in making programmatic decisions or evaluating technical quality. Specific comments on the usefulness of the reviews and the review process at NASA Langley are also outside the purview of this panel's work.

Publications can be an excellent way to evaluate and ensure continued excellence in a research program. The panel did observe that ECT researchers for the most part had had a large number of conference papers published. However, in many cases the researchers did not take the extra step of preparing their work for peer-reviewed journal publication, apparently because such publication is neither encouraged nor explicitly supported by NASA management. The number of publications and patents in some specific areas of excellence was, however, commendable, and is noted in the individual project and element sections that follow.

NASA should maintain an environment that nurtures and rewards intellectual leadership and technical excellence. Expectations should be aligned with the metrics of excellence and leadership that apply within the broader technical community—for example, acceptance of work in refereed publications and the award of patents. Metrics like these should be encouraged in addition to, not in place of, metrics for measuring progress toward technology maturation and transition to NASA flight programs. The highest-quality tasks managed to do both. The ECT panel does note, however, that it is sometimes difficult to publish articles on technology under patent and undergoing the licensing process.

Recommendation: ECT managers should implement a set of criteria, used either in a critical assessment or in an external peer review, for assessing the quality of in-house or external research. The assessment should be carried out for ongoing projects and proposed new efforts. Criteria should be adjusted to reflect the expectations of different fields and should include the number of peer-reviewed journal articles, the number of patents, and the number of missions adopting the technology and its impact on those missions.

Such assessments will not burden the staff of successful programs since their delivered hardware and publications are already a measure of their excellence.

Management and Strategic Planning

There is a general need for better strategic planning within the ECT program. The panel saw little top-down direction for the program in this area. With the exception of the Advanced Measurement and Detection (AMD) element and some new developments in the Distributed Spacecraft Systems area, there was little evidence that the portfolio and future work were planned in a truly strategic manner. In part, this is due to the circumstances that brought portions of the ECT program together into a single program. These programs were originally conceived and begun in different areas of NASA, often at different field centers and sometimes with different goals, objectives, and management structures. Some of this dispersion of strategic intent remains in the program.

Many managers admitted that they were awaiting the technical and portfolio assessment capability touted in the Technology Assessment Analysis (TAA) element within the Advanced Concepts project. This capability, which would, in concept at least, provide valuable information for strategic planning, has not yet been advanced to a point where it can be effectively and confidently used. As recommended by the PRT committee in Chapter 2, systems analysis and research technical assessment capabilities should be developed and would be useful tools for strategic planning.

Approximately 20 percent of the ECT budget is devoted to Advanced Systems and Concepts (ASC); this funding is supposed to serve as seed money for new technologies. This is a reasonable portion of the budget to devote to exploration, but it is disconnected from the actual technology research and development. In other words, little of the funded ASC work actually stimulates a research program. It might be more appropriate to use some of this money to explore outside-the-box ideas—for example, 10 percent of the ASC funding could be used at the overall ECT level unconstrained by project area and another 10 percent used by the individual ECT element managers to explore outside-the-box technologies and concepts within their elements. Another alternative is to measure the success of ASC by how many of the ideas are transitioned to projects in ECT and to fund future ASC work based on past success.

These issues in strategic planning are due in part to the lack of consistent objectives and funding and even to management structure within NASA over the last decade. A link can be shown between the stability of an individual project and the project's technical performance over a long time horizon. This is especially so for the more fundamental and challenging research tasks, in which basic advances in science and engineering are required. The ECT program is fundamental research, and fundamental research often takes a long time to bear fruit. However, the ECT program (or at least those parts that were in the Space Technology program) has undergone frequent and sometimes disruptive restructuring and reorganization. Most elements of the ECT program (earlier, the Space Technology program) have been managed by five different enter-

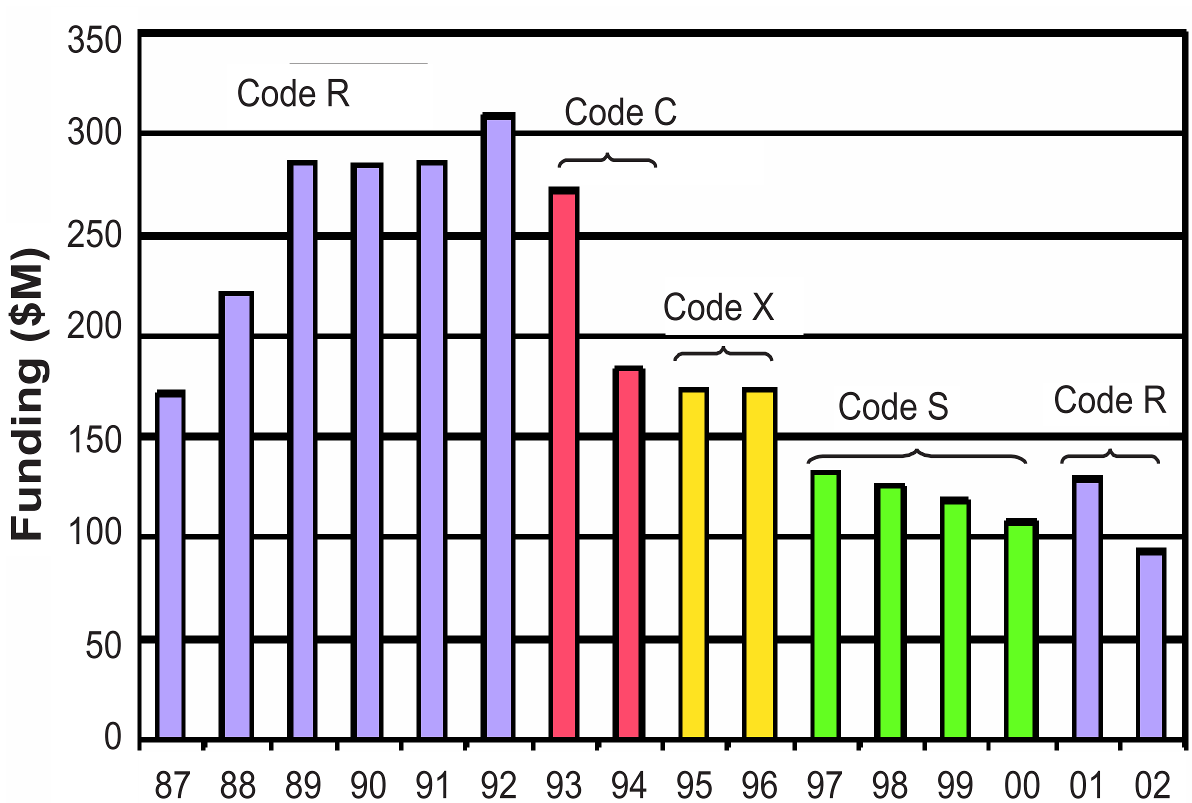

FIGURE 5-2 Space technology program funding history. Legend: Code R, Aerospace Technology Enterprise; Code C, Commercialization Enterprise; Code X, Advanced Technology Enterprise; Code S, Space Science Enterprise. Current NASA Codes X and C are not the same organizations listed above. SOURCE: Taken in part from Moore (2002).

prises within NASA in the last 10 years (see Figure 5-2). The panel recognizes that certain program time spans are imposed by the Office of Management and Budget (OMB). However, these OMB constraints involve 5-year time horizons, while parts of the ECT program have experienced 1- and 2-year lifetimes between reorganizations. As a result, top-down planning and direction (not to mention funding) were difficult to sustain. The panel found, however, that the most successful elements within ECT had managed to perpetuate long-term research in spite of rather than because of the changing program structure at the top. If current plans for the FY2005 ECT program are implemented, the ECT program will have undergone three top-level organizational changes within the course of this review. While the panel understands that many of the research projects within these programs will continue, this is yet another example of constant churning in the program.

Finding: The ECT program and its previous incarnation, the Space Technology program, have undergone frequent and disruptive restructuring and reorganization over the past decade, which has affected top-down planning and direction. This disruption has undercut the long-term support necessary for fundamental research.

Recommendation: NASA should commit to and provide a stable management environment that will encourage and support long-term research within both the agency and its community of collaborating industrial, academic, and other government researchers.

Managing risks in a basic research program is a difficult task. By definition, portions of a research program should contain a reasonable amount of risk due to the uncertainty and serendipity that inhere in such programs. High-risk efforts should have risk-reduction mechanisms built into their structure in order to drive risk down to an acceptable level. The panel notes that many individual areas within the ECT program address risk satisfactorily. The AMD element employs a realistic assessment of risk and addresses it well. The D&MS testbed work inherently addresses risk while testing integration issues before technologies are pursued further. The Energetics project performs excellent work in many areas, but the panel saw little treatment of risk. By design, any work in systems analysis, if done properly, will address risk. However, risk assessment was not a primary consideration in the ASC project.

A portion of strategic planning and management should involve a determination of which portions of the program should be performed in-house and which portions of it outside. The ECT program in FY2002 comprised over 51 percent externally funded work (Moore, 2002), most of it through a set of Space-Based NRAs. While this statistic appears to demonstrate numerical parity between in-house and outside work, it should not be interpreted to mean there is an effective mix between NASA and non-NASA personnel in the projects. Collaboration outside NASA ranged from excellent to good to, in some cases, poor. This means it is not possible to draw general conclusions about the level and quality of ECT-wide collaboration with efforts outside NASA. Instead, the matter is discussed as necessary in specific sections below. Most technologies within the ECT portfolio could (and should) be open in some way to external research. The panel notes, however, that NASA must continue to maintain expertise in many technology areas where industry or other government agencies do not have an interest or overlapping missions. There are also areas where NASA must continue to maintain a knowledge base in order to successfully plan missions and incubate new technology. Examples of such areas are these:

- Energetics project
  —Radioisotope powered devices
  —High-specific-impulse electric propulsion (<2,500 s)
  —Radiation-tolerant solar power
  —Spacecraft batteries and fuel cells
- Distributed Spacecraft element
  —Ultraprecision formation flying with large baselines (100s of meters)
  —Control of large constellations/clusters of formation flying satellites
- Microspacecraft element
  —Technologies and integration of innovative microsensorcraft
  —Technologies for microspacecraft in hostile environments (i.e., solar proximity, outer planets, etc.)
  —Miniature propulsion for control of large gossamer structures
- Advanced Systems Concepts element
  —Systems analysis tools
- Resilient Materials and Structures element
  —Gossamer structures
  —Space-durable materials
  —Deployable telescope technology

Conversely, there are areas in which NASA should involve top external researchers in order to get new ideas. The SEE element does this very successfully, using \$1.1 million of its \$1.5 million FY2002 budget to fund competitive research whether in-house or outside. Of the \$1.5 million total, \$927,000 is for work performed completely outside NASA. Other areas within the ECT program rely on Space-Based NRAs to fund external work. The Energetics program, however, could easily expand its interaction and cooperation with external or other in-house NASA efforts.

A systems analysis and technical assessment capability, such as proposed by the TAA, is an essential capability that NASA should have in-house so it can properly judge its portfolio. While expertise from the outside (i.e., from universities and industry) can supplement this capability or help in the creation of tools, it is important that the knowledge and a significant portion of the analysis be performed within NASA so NASA managers have the understanding necessary to make sound decisions about technology balance and content.

Finding: The TAA element within the ECT program is an important area for NASA to continue investing in. However, the panel believes that the element has not been given the emphasis it needs.

Revolutionary Aerospace Systems Concepts (RASC) and the NASA Institute of Advanced Concepts (NIAC) are parallel activities, the former in-house and the latter outside. Having an ability to generate advanced concepts both within and outside NASA is important and should be maintained. However, as is pointed out in the specific sections on these project elements below, both the RASC and NIAC activities should be tied closely with NASA's technology portfolio as well as the missions it hopes to perform, because if the advanced concepts resulting from them are not relevant, they will be ineffective no matter where they are generated.

Recommendation: NASA should maintain internal research and development activities and expertise in areas unique to NASA's mission where commercial or defense interests are limited and for items that are on the critical path for future missions.

The sections that follow address each of the technical areas within ECT. Within each section are specific observations, findings, and recommendations that apply to the respective areas.

NASA CROSS-ENTERPRISE TECHNOLOGY RESEARCH ANNOUNCEMENTS

During FY1999, the Office of Space Science at NASA released a NASA Research Announcement (NRA) entitled the Cross-Enterprise Technology Development Program (NRA-99-OSS-05).[3] The NRA's goal was to infuse the agency with research at a low level of technical maturity (i.e., basic research) to conceptualize and develop revolutionary new technologies. NASA centers, JPL, and other organizations were all allowed to compete under the announcement (NASA, 1999). Management of the awards was shifted to the ECT program in FY2002.

Ten technology thrust areas were chosen in a broad search: Advanced Power and On-Board Propulsion; Breakthrough Sensor and Instrument Component Technology; Distributed Spacecraft; High Rate Data Delivery; Thinking Space Systems; Micro/Nano Sciencecraft; Surface Systems; Ultralightweight Structures and Space Observatories; Next Generation Infrastructure Systems; and Atmospheric Systems and In-Space Operations. The effort ultimately awarded $40 million per year to 111 awardees selected from 1,229 proposals. Each award was for 3 years. The selection proceeded as follows: First, 43 separate external technical peer review panels[4] evaluated all submitted proposals according to criteria listed in the solicitation announcement. Then, the top-rated proposals (which numbered 428) were evaluated for relevance to the needs of NASA's various enterprises, with 111 of the them being selected based on various criteria. Table 5-2 shows the selection in various thrust areas.

The NRA was advertised as "NASA's primary vehicle for undertaking basic research within the Agency to conceptualize and develop revolutionary new technologies" (NASA, 1999). The panel saw little evidence of that boldness in the list of awardees.

Despite the lack of detailed information on *all* the research performed under the NRA, the panel saw many good ideas. However, across the awards, one could question the degree to which they were "revolutionary new technologies." For example, radioisotope power sources, hot electron detectors, solid state microrefrigerators, and thermochemical research on sensing materials appear to be topics that are either already covered within the internal ECT portfolio or not necessarily truly new ideas. The panel recognizes that the process for selecting proposals was challenging because of the large number of proposals and the wide range of technologies and applications the NRA was trying to support. The large number of technical review panels make it difficult to normalize results across so many panels and technical areas.

The panel observed that the management of the NRA was problematic. The NRAs had been transferred from the Space Science Enterprise to the Aerospace Technology Enterprise when the Enabling Concepts and Technologies (ECT) program was formed. This management change, coupled with the broad focus of the announcement, has led to a general lack of integration of the projects with NASA programs and centers. However, the NRAs associated with the research topics within the Resilient Materials and Structures (RMS) element appear to be well integrated into the ongoing research program. The element should maintain its current procedure for integrating the Cross-Enterprise NRAs.

This general disconnect between NASA programs and the NRA awards is due in part to the competitive environment set up between the awardees and the NASA researchers who did not win awards. Effective competition enhances productivity and quality. However, the winning teams are now competitors for funding and can no longer freely exchange ideas and findings. For example, an excellent NRA contract may be awarded to an outside group for a new thruster design,

[3] Also referred to as the Space-Based NRAs.

[4] The names, affiliations, and expertise of the external reviewers and the content of nonawardee proposals were not available to the panel due to procurement sensitivities.

TABLE 5-2 Cross-Enterprise Technology Development NRA Awards

Technology Thrust Area	Proposals Reviewed	Proposals Selected	Percentage Selected	Percentage of Total
Advanced Power and On-Board Propulsion	172	13	7.6	11.7
Breakthrough Sensor and Instrument Component Technology	308	40	13.0	36.0
Distributed Spacecraft	73	7	9.6	6.3
High Rate Data Delivery	90	13	14.4	11.7
Thinking Space Systems	114	10	8.8	9.0
Micro/Nano Sciencecraft	106	10	9.4	9.0
Surface Systems	80	2	2.5	1.8
Ultralightweight Structures and Space Observatories	140	9	6.4	8.1
Next Generation Infrastructure Systems	99	6	6.1	5.4
Atmospheric Systems and In-Space Operations	47	1	2.1	1.0
Total	1,129	111	9.8	100.0

but if the awardees have a firewall between their basic research and the NASA Glenn test and analysis capability, more may be lost than gained from the competition. The panel believes that the NRA work could have a higher payoff if individual NRAs were solicited in various thrust areas and managed directly by the PRT project most closely related to the subject matter, allowing increased cooperation and interaction between NASA researchers and those winning the NRAs.

The panel observed that NASA has showed little ownership of the NRA work. As mentioned previously, this is probably attributable to two factors: (1) allowing NASA centers to compete for awards and (2) no clear mechanism for evaluating progress during the award's duration. The lifetime of the NRA awards, while excellent for stability of research funding for the outside contractors, seemed to cause problems with their management by NASA. Awarding 3-year-long NRA contracts every 3 years with no rotation of awards or overlap of award tenure causes NASA management to be locked into certain technology choices. A more staggered approach to funding the NRAs should be considered. It is the panel's understanding that PRT/ECT management plans to restructure the NRA solicitation in the coming year to address these concerns. NASA managers have proposed that, eventually, a rotating set of technical topics be used each year, allowing for research at various stages to be in progress at any given time. To begin this process in FY2004, a portion of the NRA funding will be used to transition the most promising work into various enterprises in NASA. The first set of rotating topics will include advanced measurement and detection technology, large-aperture technology, and low-power microelectronics technology (NASA, 2003a).

The panel agrees that the technical concept behind the NRA is good. It will allow NASA to contract with leaders in various fields external to NASA and could prove to be an effective way to infuse many new and revolutionary ideas into the NASA program with very little risk and at relatively low levels of funding. However, the panel feels that the collaboration and management of the NRAs could be improved in several ways. Since September 2002, ECT management has held "reviews" of the NRA work related to AMD, Energetics, RMS, and D&MS in order to better integrate the research in the ECT program. There are, however, no current plans to review the NRA work that is directly related to the CICT program. While such reviews are a good start to improving the integration of external research into the program, future NRA management should expand opportunities for collaboration between the awardees and NASA researchers.

Panel members briefly reviewed materials available from the NRA reviews. They found the overall scientific quality of the work to be good. In the Energetics area, however, the research was not always aligned with NASA's mission and did not always adequately evaluate system-level payoffs or identify the mission-enabling drivers of such technology. Further collaboration between the winning teams and NASA will do much to improve this, as suggested above.

Finding: The ECT panel observed a general lack of integration of Cross-Enterprise NRA research with NASA programs and centers, limiting the overall return on investment.

Finding: The NRA structure in which NASA centers compete with universities, industry, other agencies, and with one another has put NASA in a competitive position from which it can no longer freely share technical information with other researchers. This significantly reduces the payoff from the NASA investment in research at a low TRL.

Recommendation: The research performed under Cross-Enterprise NRA contracts should be managed as an integral part of in-house PRT research activities, with individual program elements being responsible for the performance of the contract, including contract deliverables and milestone monitoring. Element managers should participate in defining technical objectives for the NRAs, which should also be released on a more regular basis. Element managers most closely related to the subject matter should also participate directly in the selection of proposals along with outside experts. Element managers should be responsible for ensuring that NRA contracts further the NASA mission, but NASA centers should not be allowed to compete for NRA funds.

ADVANCED SYSTEMS CONCEPTS PROJECT

The Advanced Systems Concepts project of the ECT program consists of three elements: the Technology Assessment Analysis (TAA) element, the Revolutionary Aerospace Systems Concepts (RASC) element, and the NASA Institute of Advanced Concepts (NIAC). The first is meant to develop a tool to evaluate the ECT technology portfolio, while the last two elements are focused on creating new system concepts. The following sections discuss specific issues in the three elements and overarching systems analysis issues.

General Observations

It is difficult for the ECT panel to apply the same review criteria and the same review process to the Advanced Systems Concepts project as to the other ECT projects and elements. As a fundamental research project, Advanced Systems Concepts does not meet many of the standards and expectations of the other PRT projects—for example, refereed journal publications, patents, and insertion directly into flight programs. The RASC and NIAC elements are meant to incubate new concepts, and some have indeed served this purpose. The incubator analogy is used because, as in business start-up incubators, many ideas are supported but only a few are successful. The TAA element is at an early stage of development, making it challenging to effectively judge its merits. However, the panel does highlight areas of concern that should be addressed as the project continues. Because of its nature, Advanced Systems Concepts is judged more on how it develops concepts and how it evaluates technology portfolios.

This, however, does not diminish the importance of the Advanced Systems Concepts project to the PRT program and to NASA as a whole. As noted earlier in the report, the PRT committee observed gaps in systems analysis capability throughout the PRT program, from top (management) to bottom (individual research team). Systems analysis was judged to be a NASA weakness in two previous National Research Council reports (NRC, 1997, 2001). Even further back, the Report of the Advisory Committee on the Future of the U.S. Space Program (the Augustine report) recommended that "a systems concept and analysis group reporting to the Administrator of NASA be established" (Augustine et al., 1990). Currently, many entities within NASA are trying to fulfill such a need, but they lack coordination. The Advanced Systems Concepts project suffers from this same lack of coordination and communication in terms of coordination within Code R itself and within NASA as a whole. However, this flaw should be viewed not as a reason to eliminate an area but rather as an opportunity to provide a much-needed capability for NASA.

During the course of this review, NASA created the position of Space Architect reporting to the NASA Administrator. One primary role of this position is to direct long-term strategic planning for space technology research at NASA (NASA, 2002). Systems analysis should be a key part of this endeavor. The latest budget of the Advanced Systems Concepts project within the ECT program earmarks $20 million to the Space Architect for FY2003 (Moore, 2003b). It is the panel's understanding that this money will not be used by the ECT program but solely at the discretion of the Space Architect. However, the panel did not have the opportunity to review this new effort within the program and cannot comment on whether it will provide the needed systems analysis capability.

Recommendation: NASA should support a well-defined, coordinated, centralized systems analysis capability that will work toward an agency strategy for technology development.

The diversity of projects in the Code R program and within the PRT program specifically makes it a challenge to create common metrics for comparison of technologies. NASA's Space Science Enterprise (Code S) suggested a set of criteria (Thronson et al., 2002) that have uniform applicability: revolutionary aspect, credibility of technology infusion plans, applicability to several missions, and criticality and relevance to mission set. These criteria may be an appropriate starting point for Code R's metrics.

In addition to this set of criteria, it is important that performance metrics also be established for similar types of technologies. For example, in power performance, metrics typically include expected mass savings and improved efficiencies. Expected development time and cost should also be considered metrics. It was evident to the panel that while metrics are being used in some areas, there is little uniformity and consistency across similar technology areas. Often metrics are created by the technologists themselves, with limited review of their credibility by both internal and external parties. The TAA tool could provide this independent verification capability, but since it is not yet ready for use in this manner, it is difficult to determine if this independent verification capability will be achieved. It is also important to consider the full impact of a technology on the performance of the entire system, not just the individual subsystem. For example, if a new power technology leads to greater efficiency but creates higher thermal loads it must be determined whether this power advantage outweighs the potential thermal issues. So, rather than prescribing an exact set of general and specific metrics, TAA should work with each of the technology areas to determine the best metrics.

Recommendation: A common set of technology metrics at the system or mission level, used to judge all technologies, and metrics specific to each technology should be determined. An independent assessment must be conducted to verify a technologist's claims against this set of metrics; this should be the job of TAA. The full system impact of a technology should be understood and considered in research portfolio management.

While pockets of excellence in systems analysis were observed in areas of ECT, the use of systems analysis to guide decision making and to evaluate technologies was not pervasive. Systems analysis can be an effective tool for technology portfolio management. To illustrate, a very simple systems analysis was applied to the microspacecraft (MS) work in the D&MS element. The panel assessed the portfolio of MS work by comparing relative cost and mass of basic elements of small satellites. These data were obtained from a survey of satellites weighing less than 500 kg (Bearden, 1999; Sarsfield, 1998), both earth-orbiting and interplanetary. Figure 5-3 presents a historical cost and mass distribution for small satellites. Although the distribution of cost and mass varies somewhat between subsystems, the average shows the relative contribution of each subsystem to a generic satellite's cost and mass. The panel then compared these distributions with the relative number of MS tasks, addressing the cost and mass of each spacecraft subsystem (Figure 5-4). The portfolio in September 2002 did not develop technologies for some of the high-payoff subsystems and overemphasizes some lower-payoff subsystems. For example, the power and structure subsystems are the heaviest and most expensive parts of a spacecraft and yet there are relatively few tasks in the portfolio addressing these areas. Similarly, the thermal subsystem historically has been the least costly and least massive satellite subsystem, and yet there is a relatively large number of tasks in this area.

Because of the interdependencies of spacecraft subsystems it is unsound to use the mass and cost contributions of the subsystems on their own as the sole basis for choosing a balanced portfolio. Some subsystems will have a multiplying effect on cost and mass. For example, a reduction in the mass of a subsystem's components will also allow a reduction in mass of supporting structure and propellant, which needs to be accounted for in these assessments. An example of how these considerations might lead to a different investment strategy is the thermal control problem. Smaller satellites with higher performance will likely have to contend with thermal control problems owing to the reduction in radiator area. One might conclude that the solution is to develop more capable thermal control technologies. On the contrary, it may be more effective to develop low-power-dissipation elec-

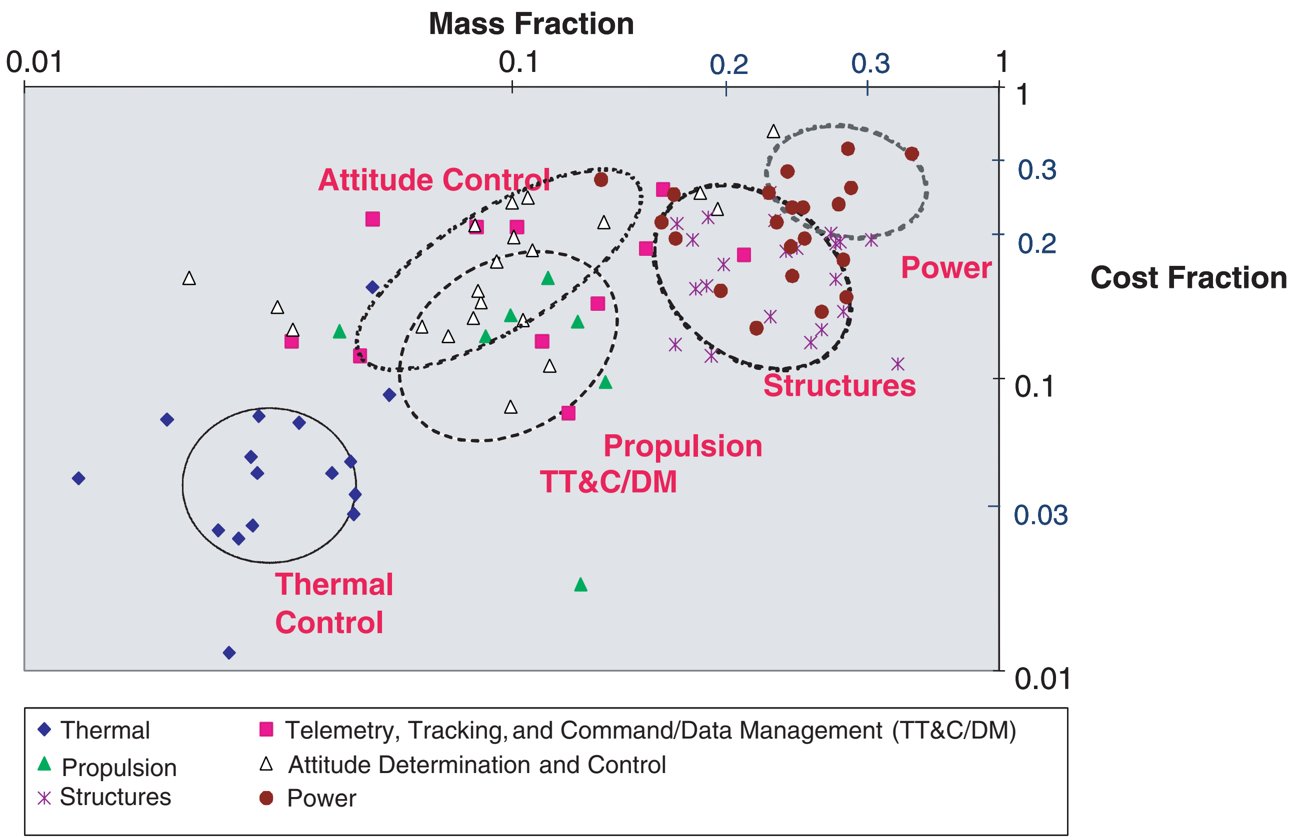

FIGURE 5-3 Historical cost and mass distribution of small satellites. SOURCE: Adapted in part from Bearden (1999) and Sarsfield (1998).

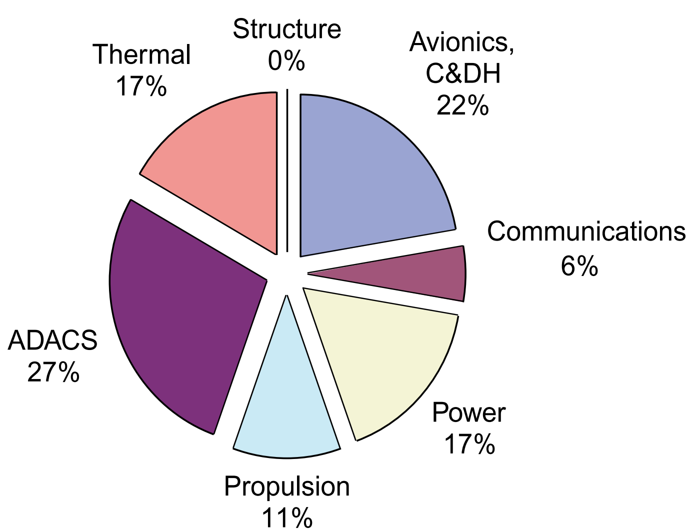

FIGURE 5-4 Distribution of NASA ECT microspacecraft technology projects.

tronics that reduce the thermal problem and also pay dividends by reducing the power subsystem requirements. This illustrates the need for higher-fidelity systems analysis of technologies so one can decide if thermal subsystem investments make sense even though, as pointed out, they contribute little to cost and mass at the gross subsystem level. Second-order multiplier effects not seen at the basic analysis level will be uncovered only if a more detailed systems analysis is performed.

Some efforts have been made to develop the tools needed to improve the assessment of technology portfolios (Feingold, 2002; Weisbin, 2003; Bearden, 1999; Sarsfield, 1998), but further work is needed. This should be a focused effort of TAA, as discussed in a subsequent section. A similar survey, performed nearly a decade ago by NASA Langley (Ferebee et al., 1994), could be used as a starting point.

Recommendation: To understand the state of the art in systems analysis and technology assessment tools, TAA should perform a survey of tools and processes both within and outside NASA.

During review panel activities, it became clear that it would be difficult to get a complete handle not only on the technologies within PRT but also across NASA as a whole. As the NASA enterprise responsible for technology, it makes sense that Code R should have an integrated database that would give users the information they need on technologies in the PRT program and preferably for NASA as a whole. Not only is information on the technology itself important, but there must also be information on how to contact the technologists performing the work. The NASA Technology Inventory is supposed to serve this purpose, but it was clear to the panel that this database is not meeting that purpose nor is it widely used. As a result, many other areas of the agency have created their own technology databases, which inevitably leads to a lack of integration. Lessons can be learned from NIAC's virtual institute approach as well as the Office of Biological and Physical Research's (Code U) separate online database.[5] It is also important that NASA be able to understand related technology being created by other government agencies. NASA databases should be coordinated with other government databases where appropriate.

[5]See <http://research.hq.nasa.gov/taskbook.cfm>.

Recommendation: NASA should develop a complete, integrated, online, public database of technology projects. This database should include not only Code R PRT projects, but also NASA-wide projects. It should be integrated with other government databases as appropriate.

Implied in these suggested improvements is improved coordination with the other parts of NASA. In order to understand how this technology portfolio relates to the missions of the other codes, ATE must work hard to improve communication. It was suggested by many of the enterprises (Thronson et al., 2002) that ATE fails to coordinate and involve the other parts of NASA in its initial planning. It was said that Code R often waits until after plans are created and then asks the other enterprises to help with justification (Thronson et al., 2002).

Recommendation: The Aerospace Technology Enterprise should strive to improve communication and coordination with the other codes. It is especially important to involve the other codes in initial planning activities.

Technology Assessment Analysis Element

TAA's purpose is to strategically assess the Office of Aerospace Technology's ECT technology portfolio, quantifying the value and progress of product lines and their potential benefit to future missions (Ferebee, 2002). The TAA activity solicits study topics from the Earth Science, Space Science, Space Flight, and Biological and Physical Research Enterprises of NASA. TAA is led and integrated by NASA Langley Research Center and owing to its early emphasis on space and earth science, also involves JPL and Goddard Space Flight Center. It hopes to leverage the existing Code R Technology Inventory database as well as the mission design efforts of Goddard and JPL. TAA, a new activity for FY2003, was originally funded at $3 million, with plans to increase funding to $4 million per year; however, its funding was scaled back to $1.6 million for FY2003.

Throughout the review, TAA was an undefined process set to officially begin in FY2003, yet many areas of ECT looked to it for direction and prioritization during the summer of 2002. It is a capability that does not yet exist. The panel recognizes that TAA has been in a state of flux and the project is just now becoming defined. Despite this serious lack of definition during formulation, the importance of this area is significant to the PRT program, Code R, and NASA as a whole. It merits much stronger consideration than it has received to date.

Finding: The Technology Assessment Analysis element within the ECT program is an important area for NASA to continue investment. However, the panel feels that the area has not been given the emphasis it needs.

Recommendation: TAA must go beyond the early planning stages and become an actual capability. It should receive the attention and support that this critical capability merits.

Throughout the course of the review, it was unclear how the TAA studies would be performed. Some utilization of existing capabilities and tools at JPL and Goddard was alluded to by the ECT management, but it was uncertain how they would be used. As a whole it was unclear exactly who would perform the work and how any of the TAA effort would be completed in light of the changing definition of this proposed new area for FY2003. In March 2003, panel members received an update on plans for the TAA. It is now focused on four pilot mission studies actually selected by and performed in conjunction with personnel associated with different NASA enterprises: (1) large telescope systems (Code S), (2) lidar observatories (Code Y), (3) space power systems (Code M), and (4) automation of microgravity research (Code U) (Moore, 2003a).

TAA's focus is currently on mission scenarios chosen by other NASA enterprises and staffed by individuals associated with those enterprises. Each pilot study uses tools already developed and utilized by other NASA enterprises. Each pilot study is scheduled to run for 6 months so that results can be used in planning the FY2005 ECT program and NRA topic selection for future years. The top-level approach presented for TAA (i.e., progressing from desired science goals and capabilities to identifying potential technical concepts to determining system-level benefits of new technologies and finally using a prioritization process to optimize the technology portfolio) is sound in concept. However, there was no clear indication that TAA, as structured for FY2003 with pilot studies, will ever develop a true portfolio analysis tool set. NRC panelists also saw no plans for the future development of new tools under TAA.

Rather than perform narrow mission studies, as proposed, TAA should focus more broadly on how technologies support the NASA mission set and on evaluating competing technologies. Code R's mission is to develop technologies across the entire agency, not to fund pilot studies for other NASA enterprises. The panel recognizes that knowledge of mission enterprise needs is key to effectively using scarce technology development resources. However, Code R's basic research should be funding cross-agency enabling technology and the tools needed to evaluate its applicability across the agency.

One example of technology assessment and prioritization is the recent work done for the NASA Integrated In-Space Transportation Planning (IISTP) Phase I activity (Farris et al., 2001). Conducted in 2001, the IISTP activity involved a NASA-wide team of more than 100 engineers and scientists assessing and prioritizing in-space propulsion technologies. In a 6-month period, the IISTP effort evaluated primary propulsion systems for transportation between various in-space destinations for nine potential missions selected from the NASA mission set that included the Earth Science Enterprise, Space Science Enterprise, and Space Flight Enterprise missions. Seventeen propulsion architectures were evaluated and priorities assigned to the technologies according to their ability to meet mission requirements, schedule, cost, and other selection criteria. Thirty-one figures of merit were selected, scored, and balanced using Kepner-Tregoe and Quality Function Deployment techniques. Cost-benefit analysis was also performed and used with a figure of merit rating to prioritize these technologies.

While one can debate if this exact process is the proper one, TAA should emulate the characteristics of a focus on technology, a broad view across the NASA mission set, a review of a technology type with a common set of merits, and performance of cost-benefit analysis. If TAA finds itself short of funds to perform a review of the complete ECT portfolio, pilot studies on a few specific technology types should be completed. This is strongly preferred over the mission and enterprise focus currently proposed.

Recommendation: To develop TAA capability, the proposed pilot studies should focus on specific technology types rather than on missions or enterprises,

as currently planned. TAA's process should also be characterized by a broad view across the NASA mission set, a review of a technology type with a common set of merits, and the performance of cost-benefit analysis.

Revolutionary Aerospace Systems Concepts Element

The RASC element, formed in 2001, is largely operated at NASA Langley Research Center, where 60 percent of the $8 million annual funds in FY2002 were spent. Funds ranging in magnitude from $375,000 to $700,000 were distributed to several of NASA's other centers, including Glenn, Goddard, the Jet Propulsion Laboratory (JPL), Johnson, and Marshall. Allocation to centers is a function of the specific studies selected every year. RASC is largely an internal NASA activity but does include some universities in its work. During 2002, the purpose of RASC was to formulate revolutionary mission approaches, develop revolutionary aerospace systems architectures/concepts, and provide related technology requirements that would enable these missions to be implemented to enhance NASA's technology investment strategy (Troutman, 2002). Its focus was on helping to develop approaches and scenarios that will achieve NASA's science objectives 20+ years in the future.

In early 2003, RASC was reformulated and integrated with the Agency Aerospace Systems Analysis (AASA) project to consider alternative (instead of revolutionary) approaches and systems (Troutman, 2003). The 20+ year time frame was dropped as an objective and the aeronautics portion of the effort was removed. The newly appointed NASA space architect plans to use the capability to develop technology roadmaps and gap analysis to guide strategic planning. Many FY2002 activities and planned FY2003 selections were modified, transitioned to other programs, or canceled.

RASC is in many ways similar to NIAC, but for NASA internal competition. It solicits internal ideas via a request for information. Forty-five ideas for possible projects were submitted for FY2003; however, these projects have been greatly modified due to the project's reformulation. Three of the 17 total projects originally funded between FY2001 and FY2002 are undergoing further study and funding by NASA. Panel members disagreed on whether this number of transitions constitutes success. NASA should evaluate this 3 in 17 success rate to determine if it is acceptable.

The RASC Academic Linkage (RASCAL) program has been created to enable university participation (RASCAL, 2002). A forum to discuss various project ideas was held in May 2003. As originally formulated, RASC intentionally included both aeronautics and space themes; however, this did not guarantee that all enterprises were represented. During the 2003 reformulation, the aeronautics portion of the activity was dropped. As a result, NASA will now depend upon Code R's Intercenter Systems Analysis Team (ISAT) for concept development related to aeronautics. Efforts need to be made to have RASC viewed as a NASA-wide activity, but it is not suggested that the RASC budget simply be evenly distributed among NASA centers.

In summary, RASC originally focused on the following: (1) concepts that relate to long-term mission themes, (2) concepts that create critical pulls on technologies, (3) space and aeronautical themes and scenarios provided in the NASA request for information, and (4) new and wide-ranging concepts. Another important criterion, which should go without saying, is that the concept does not violate the laws of physics (Troutman, 2002). While the technology pull criterion attempts to relate RASC concepts to the NASA technology portfolio, it could be strengthened by overtly specifying its direct relationship to the NASA technology database. No criteria for the revised project had been presented to the panel by time of publication of this report.

Recommendation: RASC should improve its relationship to the NASA technology database. Better integration is necessary to ensure an actual connection exists between RASC and the NASA technology database.

RASC studies tend to be concept studies within a certain NASA enterprise area. The panel notes that although efforts have been made to distinguish current studies from past work, further effort is necessary. The panel suggests that RASC should emphasize work that crosses enterprise lines to strengthen the idea that it is wide ranging. Examples of such work might include understanding the synergy between human and robotic missions, NASA-wide future communication needs, and the synergy between high-speed aeronautics and launch vehicle technology. The panel felt that NASA should revisit its 20+ year time frame since in some cases this criterion might have unintentionally directed ideas too far into the future. Almost all projections tend to underestimate how soon a project will begin. If this change is adopted, it may address some minor criticism about RASC relevance. The panel notes that the 20-year time frame was subsequently dropped during program reformulation in early 2003.

Recommendation: RASC should reconsider the criteria it uses to select studies, giving more weight to cross-enterprise studies. Care should be taken with the long-term focus so as not to make RASC projects so far off that they become irrelevant.

NASA Institute for Advanced Concepts Element

NIAC's purpose is to be an independent source of revolutionary aeronautical and space concepts that could dramatically impact how NASA develops and conducts its mission (NIAC, 2001). Its ultimate goal is to infuse NIAC-funded concepts into future NASA plans and programs. NIAC is operated by the Universities Space Research Association (USRA) as a virtual institute using Internet technology to distribute its solicitations, receive proposals and reports, and review proposed projects. In 2002, NIAC's fifth year of existence, funding was provided at a level of $4 million per year. NIAC solicits proposals only from non-NASA sources and strives to use non-NASA reviewers to maintain independence. The panel was impressed by the diversity and experience of the reviewers as expressed in general statistics.[6]

If one agrees with the purpose and premise of NIAC—namely, to be a technology incubator—then NIAC has had some success infusing interesting new ideas into NASA. In its 2000 annual report (NIAC, 2001), NIAC identified 12 projects out of the approximately 100 it has funded as having been infused into NASA. (NIAC's definition of infusion is having other NASA sources provide funding for concepts developed by NIAC projects.)

There is evidence that NIAC-funded work is disconnected from the NASA centers. In addition, NIAC reviews and awards contracts without involving the NASA centers. Conversely, NASA centers do not always consider NIAC results in choosing their new research directions. For example, NIAC-funded research that should have been relevant to the mission of the Energetics project, such as antimatter propulsion, spinning tethers, high-acceleration laser sails, magnetic sails, electron-spiral toroid propulsion, and space elevators, was not considered by Energetics management. Although NIAC's continued independence from NASA is important if it is truly to act as an external incubator, research funded under the program should be considered in light of NASA needs and current investments.

Finding: While striving to maintain some independence, NIAC needs to become better connected to the NASA researchers and centers.

A majority of the proposals for NIAC projects come from the university and small business communities (64 and 90, respectively, of the 172 received in the 2001 call for Phase I proposals). Thus a majority of the awards go to these same institutions (11 of the 18 Phase I grants awarded in 2001 went to universities, with small business receiving 5 of the remaining 7). Initially many of the projects in NIAC emphasized human exploration and development of space or space science. In recent years, however, NIAC has actively solicited proposals in other topic areas related to Earth science and physical and biological research. While the effort is still not balanced, all areas are now represented. NIAC should be encouraged to continue this positive trend toward proposals representative of all of the NASA enterprises.

Recommendation: NIAC should continue its efforts to solicit quality proposals from all NASA enterprises to better provide funding to a diverse set of technologies. However, if quality proposals are not submitted in a given area, NIAC should not feel obligated to select proposals simply for the sake of balance.

The current criteria for Phase I NIAC selection include three questions (Hirschbein, 2002; Cassanova, 2002): (1) Is the concept revolutionary or evolutionary? To what extent does the proposed activity suggest and explore creative and original concepts? (2) Is the concept for an architecture or system, and have the benefits been qualified in the context of a future NASA mission? (3) Is the concept substantiated with a description of applicable scientific and technical disci-

[6]Reviewer names and specific affiliations are held in confidence by USRA and were unavailable to the review panel. Information on general affiliations and experience was provided to the panel.

plines necessary for development? Individual reviewers evaluate the strengths and weaknesses of a proposed concept study in terms of these three Phase I criteria. The panel suggests that a criterion be added to the NIAC review process that addresses the relationship between the proposed concept and the NASA technology portfolio. Such a criterion might improve the infusion of ideas into NASA. In Phase II, selection criteria addressing the pathway to development and benefit versus cost are added. In the past, information on the NASA technology portfolio was not readily available to the external research community. An improved technology database would help make this possible, and use of this database in future proposal solicitations should be a requirement.

Recommendation: NIAC should improve its relationship to the NASA technology database and with NASA researchers. It is suggested that this be implemented by adding a technology criterion to the NIAC proposal selection process.

ENERGETICS PROJECT

Introduction

The NASA Energetics project consists of two elements: Advanced Energy Systems and On-Board Propulsion. The Advanced Energy Systems element received $13.1 million in FY2002 to explore spacecraft power generation (photovoltaics, advanced radioisotopes), energy storage (advanced batteries, flywheels, fuel cells), power systems materials and environmental interactions, and advanced power management and distribution (PMAD) technologies. The On-Board Propulsion element received $4.6 million in FY2002 to explore primarily advanced electric propulsion systems, with a lesser emphasis on chemical propulsion systems. Each element is organized into several areas called product lines,[7] each of which may contain several individual research tasks.

General Observations

Overall the panel found the Energetics project to be very excellent and essential to the advancement of important spacecraft technology. The photovoltaics, energy storage, and electric propulsion work were deemed to be world-class efforts and core competencies for NASA. A few tasks were found deficient in important areas; however, these were at very low funding levels compared with the flagship efforts.

Research Portfolio and System Analysis

The Energetics project focuses on advanced energy systems and onboard propulsion. Excellent quality fundamental research in these areas will inevitably have a significant impact on space mission technology needs. The Energetics project is home to three world-class research areas: photovoltaics, energy storage, and electric propulsion. Each of these areas combines cutting-edge basic research, advanced engineering, system-level analysis, and on-site testing and evaluation capability to produce the highest-quality and most well-rounded research and development programs. Each product line can claim major historical success and payoffs (Deep Space 1 Ion Thruster, Mars Lander Batteries, Solar Concentrator Array with Refractive Linear Element Test (SCARLET)), and new, cutting-edge technology with a high probability of future payoff (50-kW Hall thruster, thin-film solar arrays and structures, polymer energy rechargeable systems).

The balance between fundamental and user-driven research is better defined as a balance between near-term, moderate-payoff research and long-term, high-payoff research. A good balance between near-term and far-term research was generally observed throughout the Energetics project, which might be better characterized as a medium-risk, high-payoff research effort. The quantum dot solar cell research stands out as an example of high-risk, high-payoff work (the risk in this case is that the research investment might not yield a scientific or engineering advance that results in new space capability). In spite of the lack of extremely high-risk research, the review panel feels that the balance is correct and commendable. A plethora of high-risk projects in space systems might result in little or no technology ever being transitioned to operational use because of difficulties encountered at the system level. The panel recognizes that the highest risk for a product occurs in the flight qualification stage, and that the payoffs for a successful technology transition can be revolutionary. The Energetics project does an excellent job of choosing technologies that can be flight qualified and have revolutionary impacts to NASA missions. The

[7]The Energetics Project is unique in its use of product lines to further organize its research.

success of the Advanced Measurement and Detection element demonstrates how frequent interaction with user programs promotes transition.

The complicated systems encountered in space energetics research require robust analysis to determine the optimal research balance among the various tasks. The NASA Energetics project does very well on top-level systems analysis. The colocation of power generation, energy storage, and a primary energy user (electric propulsion) within one organization is a clear benefit. Mission analysis has been used to explore a plethora of potential NASA missions and has obviously been used to define research directions within the individual groups.

The analysis personnel are clearly world leaders in their field. They interact to a significant degree with their counterparts in industry and other government agencies. All potential solutions appear to be considered. As a result, their analyses are well respected in the field and carry a great deal of weight with NASA, industry, and Department of Defense programs.

The Energetics project does an excellent job of mission and systems analysis to balance research between power generation, storage, and electric propulsion. However, balance on the subsystem level could be improved. Specifically, electric propulsion systems research must place more emphasis in the power-processing unit (PPU), the dominant cost-driver in electric propulsion systems. Conversely, the Energetics project is the undisputed world leader in hollow cathode development, another major cost driver. Hollow cathodes are used in both ion and Hall thrusters to produce a source of electrons to ionize the xenon propellant and to charge-neutralize the plasma exhaust.

In addition, the colocation of the power generation, energy storage, power conditioning, and power consumption in electric thrusters provides excellent synergy, whereby each product line can stay in tune with its technology neighbor. A striking exception to synergy occurs in the area of high-power electric propulsion research. Thrusters are in development for operation at the 500-kW power level without a well-defined source for the power or facilities to test them. The program is loosely dependent on NASA's Nuclear Systems Initiative[8] (NSI) effort (not reviewed within PRT); however, that program has not yet defined its design goals in terms of power.

Based on the experiments and accomplishments observed by panel members during a site visit, fundamental science is clearly being applied to solve problems. Researchers in the ion propulsion product line are developing a totally new laser diagnostic capability for use in near-field density measurements of energy and propellant losses due to the hollow cathodes used by ion and Hall thrusters as an electron neutralization source. The thin-film photovoltaic group can rightfully brag about developing single-source precursors leading to deposition at low temperature onto plastic substrates.

While not within the scope of this review, related propulsion projects at other NASA centers clearly cast a significant shadow over the ECT Energetics project. Four other NASA centers are currently performing research in onboard propulsion. Examples of such research, not including the research on nuclear power and propulsion proposed for Project Prometheus, include the following:

- *Marshall Space Flight Center*. Tethers, imploding-liner fusion devices, pulsed plasma thrusters;
- *Johnson Space Center*. Magnetoplasma rockets;
- *Goddard Space Flight Center*. Micropropulsion for formation flying constellations; and
- *Jet Propulsion Laboratory*. Ion and Hall thrusters, micropropulsion, vaporizing liquid, colloids, etc.

The panel is concerned that this structure of multiple programs supporting onboard propulsion could lead to duplication of effort.

If we define as revolutionary those technologies farthest beyond the state of the art, then the Energetics project seems to be the least "pioneering." However, in the view of the panel, the project is indeed revolutionary because it is having and will continue to have a significant impact on spacecraft systems. The present advocacy system emphasizes glamorous new concepts at the expense of essential system components and technologies. The result is that programs for mission enabling *systems* are reduced to a subcritical funding level, while funding accelerates on unproven or high-risk *concepts*. Concept development that ignores system development provides NASA with no new capa-

[8]During the course of this review, the NSI effort was replaced by the Project Prometheus effort to research nuclear power and propulsion options for NASA. This program name change does not affect the findings of this panel.

bility. Systems analysis must be used to balance the research investment across the entire propulsion system in order for NASA to gain from the investment. The panel considers the Energetics project world-class, not only because the majority of its research meets the set of criteria described in Chapter 2, but also because of its previous accomplishments and because its current systems analysis has shown the possible revolutionary impact of its research on future space systems.

The Revolutionary Propulsion Element at Marshall Space Flight Center (MSFC) was funded under PRT in FY2002 but was removed from the portfolio for FY2003. As such it was not reviewed by the ECT panel. This research portfolio contained several highly publicized propulsion options that are very effective at accelerating laboratory plasmas; however, it is not clear that they would compare well against ion and Hall thrusters at the system level. These propulsion options have been moved to the Space Science Enterprise (Code S) under the Integrated Space Transportation Plan (ISTP). The Energetics project also includes 500-kW-class electric propulsion that is motivated by the NSI. The panel was not briefed on the specifics of the NSI program (i.e., on power levels and time frame for power availability), so it is difficult to assess this portion of the Energetics project.

The Energetics project researches both low- and high-power generation at inner orbits where photovoltaics are applicable and low-power generation using the Stirling engines and radioisotope power sources (RPSs) needed farther from the sun. A gap exists in the Energetics portfolio for high-power generation (10 kW to 1 MW) at distances far from the sun, presuming this is in NASA's mission. Use of RPSs at these power levels will probably require the use of nuclear fuel, which is not a popular option with the public. Basic research in this area is being conducted at NASA MSFC under a different program and therefore is not in the panel's purview.

Recommendation: NASA should better coordinate its portfolio development among the five different NASA centers working on onboard propulsion research. Each portfolio should undergo a common systems analysis by a nonbiased group to help NASA optimally invest research funding in this area.

The NASA Energetics project has experienced a decrease in funding for the more basic or low-TRL research needed to explore the core physics issues of advanced new concepts. As mentioned previously, the Cross-Enterprise NRAs, which fund the low-TRL external research, do not adequately involve the centers. In a similar way, concepts investigated in the propulsion area under NIAC are not considered by NASA when developing research portfolios for the Energetics project. Further discussion of this topic can be found in the NIAC section of the report.

Research Plans and Mission Direction

The Energetics portfolio tries to address the technology needs of as many future missions as possible and attempts to focus research on generic spacecraft subsystems where improvements will have an impact regardless of the NASA mission chosen. Energetics generally does a good job of avoiding narrow concepts that address very few missions. The electric propulsion research emphasized by onboard propulsion optimizes, or is competitive with, all of the mission concepts analyzed by the IISTP study. For low-power missions farther from the sun, the Energetics project has a strong program in Stirling engines that is projected to decrease RPS plutonium fuel mass by a factor of as much as 4.

Planning in the Energetics project is clearly supported well by various planning processes, and the resulting benefits for the research are clear. The space technologies under development within the Energetics project typically require 10 or more years of research, engineering, and flight qualification prior to becoming available for space applications. However, in the past 10 years the Energetics project has been managed under five different NASA Enterprises. The overall Energetics project deserves hearty accolades for a history of delivering advanced technology that requires 10 years of development despite the turbulent management atmosphere. In addition, NASA does not identify specific future missions in its Vision (O'Keefe, 2002) or its Strategic Plan (Goldin, 2000). Since it is difficult to develop progress metrics in this situation, NASA management needs to provide more focused direction by identifying specific goals. While the NASA Aerospace Technology strategic plan (Venneri, 2001) provides some guidance, it is vague in comparison with other government agencies' plans. This vagueness will inevitably filter down to the project and task level.

Finding: The NASA Energetics project does an excellent job of maintaining a research direction with

a high probability of payoff on future missions despite sparse mission-specific direction from NASA headquarters and having been moved between five NASA enterprises in 10 years. High probability of payoff is maintained by developing improved subsystems that will impact spacecraft in general.

Recommendation: NASA management should strive for increased stability in their organizational structure. In addition, NASA should adopt a mechanism for identifying and ranking future flight programs in the near term and the far term to provide guidance for NASA research programs.

Program goals and objectives were well defined and quantified for most of the groups. However, clear definitions of baselines were not always evident. A twofold improvement over what? was a common question during panel discussions. Improvements are typically quantified. The impact of the research on NASA enterprises is not clear; however, this may be due more to a lack of mission definition by the NASA enterprises than to a lack of understanding by the Energetics project. The Energetics project should also be commended for its strong analysis capability, which has considered a wide range of potential missions to quantify expected payoffs from the research programs.

Program deliverables in the On-Board Propulsion element were clearly defined. The Earth Observation One (EO-1) pulsed plasma thruster (PPT) is an element of the Air Force Research Laboratory (AFRL) portion of the Integrated High Payoff Rocket Propulsion Technology (IHPRPT) Phase I demonstrator for electromagnetic spacecraft propulsion. Component development from that program has fed directly into Phase II PPT development at AFRL. Regular IHPRPT steering committee meetings ensure the involvement of DOD, NASA, and industry in the planning process. Conversely, coordination and deliverables to NASA enterprises or other organizations were not clearly defined in the Advanced Energy Systems element.

Methodology

Three product lines within the Energetics project have basic research, engineering, test and evaluation, and systems analysis together in one group (photonics, energy storage, and electric propulsion). In addition, the analysis is also performed across product lines to ensure, for example, that electric propulsion power levels are coordinated with the goals of the product lines developing advanced power generation. A disconnect does occur for high-power electric propulsion, which relies on power levels to be researched under the NSI program.

Excellent systems-level assessments of photovoltaics coupled with electric propulsion have been performed. The panel members felt, however, that risk management should be undertaken at the level of individual projects, not at the PRT program level.

Regarding plans for future work, both the photovoltaics and electric propulsion product lines have announced near- and far-term goals and assessed the mission impact of their product advances.

Personnel and Technical Community Connections

Research in the Energetics project is performed by an enthusiastic group of top-notch U.S. researchers. The researchers were clearly excited and proud of their work, their laboratory, and their project. The Energetics project also retains a good number of researchers who are considered world leaders in their fields.

There were several instances of researchers pursuing concepts that they had invented and patented, such as electric propulsion hollow cathodes, microelectromechanical system (MEMS) Stirling coolers, and intercalated graphite shielding. These tasks were funded by the Energetics project, albeit at a relatively low and appropriate level. In some instances a case could be made that these research projects were out-of-scope and should have been moved to another NASA center. However, the panel found this to be an excellent means of developing and retaining top researchers. Scientists need the flexibility to pursue their new ideas. Good managers provide these scientists with a reasonable amount of time and funding to encourage innovative concepts that can lead to pioneering, revolutionary technology.

Photovoltaics, energy storage, and electric propulsion researchers have an excellent understanding of the underlying science and technology and of comparable work within other organizations and NASA units. On other tasks the panel was left wondering what specific role NASA was performing in an effort that clearly included a larger research community. Sometimes it was not clear if the work was being performed in-house by NASA researchers or under contract to an outside company. Even for excellent in-house basic research such as the photovoltaics effort, it was difficult to identify how the NASA contribution fit into research and development efforts at other government laboratories or industry. Research efforts today commonly involve multiple researchers and multiple government agencies funding the effort. An exception was the electric propulsion work, which showed a strong and well-defined involvement with AFRL through the IHPRPT program. Photovoltaics seem to show a similar level of coordination, although it was not explicitly described.

The practice of presenting NASA efforts from a NASA-centric viewpoint is archaic and detrimental to the agency's programs. Presenting only the NASA role where NASA plays a small but critical role in a larger effort makes the entire joint research effort appear insignificant. Based on its presentations, NASA appears to hardly leverage its previous research or research performed by another agency. The review panel would be far more receptive to a small research effort that, for example, took solar arrays developed by another agency and worked to modify them for the extended temperature range required for NASA missions. This type of leveraging is common in other agencies and a necessity in the face of today's economic realities. To be frank, based on their personal broader knowledge of the energetics field, panel members recognize that NASA does participate in many strong collaborations with other researchers and agencies. Their concern is that NASA's practice of presenting its programs from a NASA-centric viewpoint will eventually damage it at higher government funding levels, where appropriate cross-agency leveraging of funds and resources is expected.

Facilities and Equipment

The Energetics facilities at NASA Glenn Research Center were found to be excellent. The facilities are well designed to promote interdisciplinary experiments. The colocation of basic research, systems analysis, engineering, test and evaluation, and flight qualification improves the quality of the research and keeps the research focused on critical issues. The electric propulsion, photovoltaics, and polymer batteries laboratories are world-class facilities. Such facilities are expensive to design, fabricate, staff, and maintain. As such they are beyond the means of all but the largest aerospace companies and government laboratories. The Energetics test facilities are a very strong asset for the PRT program and the United States.

Testing facilities for space technologies can be a driving cost in a development program. Photovoltaics, energy storage, and electric propulsion all require advanced, expensive, high-fidelity testing capabilities to support basic research. Excellent-quality electric propulsion test facilities are very expensive (between $1 million and $20 million). The most-used government electric propulsion life test facility is currently at JPL; however, that facility is limited in thruster power to about 3 kW. The NASA Glenn Chamber No. 6 is a critical test facility for the United States—its physical size, low back-pressure, and high pumping speed are far superior to those found in competing government or industry laboratories. The capability is unique and needed to test the next generation of high-power electric thrusters. Similarly, testing of photovoltaics, space environmental effects, and energy storage devices all require well-equipped laboratories with special capabilities.

Finding: The Energetics project test facilities and personnel are a valuable, critical asset for the U.S. government. For PRT programs they have a significant synergistic benefit to the basic research. The Energetics project also makes these test facilities available to industry, which helps balance the competition between small and large contractors. For future NASA flight programs, this increases competition, lowers risk, and reduces cost.

Recommendation: NASA should strive to maintain the Energetics project's world-class testing capability. This includes maintaining both the facilities and the expertise.

Advanced Energy Systems Element

The Advanced Energy Systems element comprises seven product lines: Advanced Photovoltaics Technology; Advanced Chemical Storage Technology; Power Management and Distribution; Flywheel Energy Storage Technology; Radioisotope Power System Technology; Power System Environmental Durability, Reliability, and Survivability; and Power System Thermal Control Technology. For ease of discussion the product lines have been grouped into three main categories—advanced photovoltaics technology, advanced energy storage, and advanced energy systems.

Advanced Photovoltaics Technology

The Advanced Photovoltaics (PV) Technology product line was found to be a world-class and revolutionary research effort. The researchers were performing cutting-edge and competitive basic research on both crystalline and thin-film solar array cells. Advancements with potential for revolutionary impact in the near term (from a basic research perspective) included the growing of lattice mismatched crystalline cells for deposition on silicon, the development of liquid single-source precursors, and the deposition of thin-film PVs on polymers. In the very far term, research in quantum dots is exploring more revolutionary advances in solar cell technology. PV testing at NASA Glenn is led by recognized leaders in the field and is clearly world-class. Data from the PV testing are used to help develop computer codes in the SEE element. NASA Glenn PV testing facilities are regularly used by industry, as expected for a facility of this caliber.

The program displayed an excellent consideration of system- and subsystem-level issues, trade studies, and in-house test and evaluation to enhance and focus the basic research efforts. Subsystem analysis is used very effectively to help researchers direct research toward optimal solar cell and blanket technologies for various power levels. The research on PV blankets can claim demonstrated success in the SCARLET used on Deep Space 1, and the effort continues to explore new blanket configurations. Advances in the photovoltaics effort will have a major impact on NASA, commercial, and DOD spacecraft operating up to 100 kW.

Advanced Energy Storage (Electrochemical and Flywheels)

The Advanced Energy Storage product area of work within the Advanced Energy Systems element was also found to be a world-class and revolutionary program expected to have a major impact on all NASA, DOD, and commercial spacecraft. The researchers have demonstrated innovation in lithium-ion electrolytes and chalcogenide-based fast lithium-ion conducting glass.

The NASA Aerospace Flight Battery Systems task currently funded under the Energetics project is an essential and excellent-quality national facility and capability. However, the effort is not necessarily a basic research effort and should be transitioned into the mission codes at NASA in the near future. The Polymer Energy Rechargeable Systems task has an excellent new high-tech facility and displays the patents and refereed publications indicative of excellent research.

The flywheel product line showed a well-designed experimental setup and laboratory diagnostics. Experiments were correctly focused on the critical issues of vibrations and energy losses. The analysis group performed good subsystems-level analysis to compare the flywheel with batteries and conventional attitude control systems. Lacking from the presentations on flywheels, however, was an adequate picture of how the program fit into other, potentially larger U.S. efforts.

The Regenerative Fuel Cell Systems Technology task focused on ancillary system technology instead of on the actual fuel cell stack technology, where the focus should be. Once the fuel cell stack is optimized, the ancillary technology should follow.

Advanced Energy Systems (Power Management and Distribution, Stirling, Environment, Materials)

The advanced energy systems area of research was considered to be a good research effort overall. The effort to develop a next-generation Stirling engine was excellent work with clear goals, technical challenges, and payoffs. The MEMS Stirling cooler task, invented by a NASA Glenn researcher, demonstrates an innovative idea with clear advantages over thermoelectrics. However, the Stirling cycle team should also perform a systems analysis to determine the effects of vibration (from the reciprocating motion) on the entire system.

The Micro-Loop Heat Pipe in Silicon task is currently performed by contractors. NASA plans to test and evaluate the technology. Since the technology proposes to add mass to the system, NASA should perform subsystem analysis and trade studies to show the expected payoff before much further work is performed. In addition, the NASA Glenn heat-pipe devices must be compared with those of other organizations.

The Power Management and Distribution (PMAD) task on fault detection and intelligent systems addresses a critical need for NASA, DOD, and commercial satellites; however, it was not clear how the NASA effort complemented, fit into, or duplicated other U.S. efforts. The testing capability is adequate for present research.

The Environmental Durability, Reliability, and Survivability product line possesses an enthusiastic group of researchers working on a project critical to all spacecraft. The group shows a strong record of publications and patents. Since the proposed Polymer Erosion and Contamination Experiment (PEACE) cannot return usable data from NASA's Small Self-contained Payload program (Get-Away-Special program), NASA should strive to find the flight opportunity it needs to collect quality data.

Onboard Propulsion Element

The Onboard Propulsion element is composed of five product lines that can be grouped in two main categories: (1) ion, Hall, and pulsed plasma thrusters and (2) high-power electric propulsion and chemical and micropropulsion. Each is discussed below.

Ion, Hall, and Pulsed Plasma Thrusters

The ion, Hall, and pulsed plasma thruster (PPT) product lines within the On-Board Propulsion element clearly entail world-class and revolutionary research. The group can claim many successful products and research highlights.

The Energetics electric propulsion research effort is a flagship international effort with world-class researchers and facilities. A significant fraction of the world leaders in electric propulsion are currently employed at NASA Glenn. NASA Glenn is the leading international capability for electric thruster testing and currently the only U.S. facility capable of accurately testing thrusters over 10 kW. The Energetics project maintains research in two high-specific-impulse thrusters (Hall, ion). Both product lines have well-defined goals, and there is no overlap in applicability. Historically the laboratory has had strong research successes with the arcjet and ion thruster system development. Today the trend continues, with the recent functionality demonstration of a 50-kW Hall thruster.

The PPT task is currently performing a flight demonstration of the first PPT to be flight qualified in 25 years. The flight is returning information on the use of low-power thrusters for attitude control systems that will be referenced by researchers for many years.

The product line maintains an excellent mix of basic research, advanced engineering, in-house test and evaluation, and mission analysis to support an excellent research program. Compared with the other Energetics product lines, the electric propulsion group has the strongest interaction with other agencies, industry, and universities.

On the subsystem level, the panel expected stronger emphasis from the electric propulsion group on subsystems such as the power-processing unit (PPU) and the propellant feed systems (PFS). Historically the PPU has been the dominant cost driver for electric propulsion systems and was the reason NASA failed to validate a 4.5-kW Hall thruster system on a Russian Express satellite. The NASA Energetics project is also in the best position to stress PPU development, because power electronics research is colocated in the Energetics project. It is likely that PPU and PFS efforts are funded under Code S funds at NASA Glenn, so they are not presented as part of the PRT program. NASA Glenn does provide funding through an NRA to investigate the possibility of direct-drive PPU work, but the panel suggests that the system payoff of this work be looked at.

High-Power Electric Propulsion, Chemical Propulsion, and Micropropulsion

Whereas the Hall, ion, and PPT efforts within the On-Board Propulsion element were among the strongest Energetics efforts at NASA Glenn, some of the less generously funded onboard propulsion efforts were not judged as favorably by the panel. These include high-power electric propulsion—pulsed inductive thruster (PIT) and magnetoplasmadynamic (MPD) thruster—chemical propulsion, and micropropulsion.

The high-power electric propulsion efforts are focused on the PIT and MPD thrusters for power levels of 500 kW and greater. The PIT and the MPD thruster were chosen for development based on a 1992 workshop. NASA Glenn should have considered the NIAC research projects for new high-power propulsion concepts as well. The PIT and the MPD thruster efforts were most severely lacking in systems analysis, whereas current research is focused on modeling the thruster to improve performance with little or no attention paid to system mass and reliability. The PIT requires high-voltage, high-power operation. Even if thruster performance is optimized, PPU requirements at the system level may make the thruster impractical for use on a spacecraft. MPD thruster research has been funded for over 40 years. NASA needs to make a strong case for continued funding in light of the considerable effort and absence of significant results. The intent of the panel is not to conclude that the PIT and MPD thruster are poor choices for high-power electric propulsion but rather to question whether the Energetics project fully used its analysis capability before initiating the PIT and MPD thruster research.

Recommendation: Both a systems and subsystems analysis should be performed to compare the PIT and MPD thruster against the high-power Hall thruster used in clusters and other high-power options to ensure that the devices can eventually be made practical and competitive for spacecraft when considered on the systems level.

The GRC Energetic project is attempting to maintain a core capability in chemical rocketry for onboard propulsion. The applications are typically auxiliary propulsion for planetary maneuvers, braking, station keeping, etc. However, the environmental (and perhaps the safety) regulations enforced at NASA Glenn by local and state government prohibit the testing of all competitive and modern propellants. In spite of these local limitations, the Energetics project plans continue research using bipropellant combinations that can only be tested in select locations (such as the White Sands Test Facility) and that pose a potential hazard during launch. Engineering design, safety analysis, and testing of systems utilizing these propellants would need to be extensive. For example, cryogenic oxygen (LOX) and hydrogen (LOH) propulsion systems cannot be launched in the shuttle bay. However, NASA Glenn is considering cryogenic fluorine, which is notoriously more difficult and hazardous to handle than LOX/LOH systems. Another example is the candidate propellant oxygen difluoride (OF_2). It is also a deep cryogen and was developed in World War II by the Canadians as a nerve agent; it is more lethal than cyanide gas. The intrinsic hazards and cost of these candidate bipropellants must be evaluated to judge their mission benefits relative to the complexity of the equipment and handling procedures. Considering such practical concerns, the product line needs more detailed planning on how to proceed.

For decades hydrazine monopropellant thrusters have been used for a wide range of space applications. Now the desire is to replace hydrazine because of its health hazards. This led NASA Glenn to perform research in monopropellants, which are being tested routinely at other facilities. For example, several U.S. laboratories conduct rocket firings using the monopropellant hydroxylammonium nitrate (HAN). However, HAN cannot be tested at NASA Glenn. Information on overcoming the NASA Glenn propellant-testing limitations by working with other organizations was not clear. Perhaps, testing at the White Sands Test Facility or teaming with either the AFRL effort at Edwards Air Force Base or the Navy effort at China Lake, for example, could bring to the NASA Glenn chemical propulsion effort both the needed testing capability and valuable additional expertise. The panel determined that the propellant combinations of interest to GRC were not central to DOD applications. Areas where NASA could contribute include catalyst bed materials, nozzle design and materials, and propellant feed systems. GRC's specific role in national propellant efforts such as the DOD and NASA IHPRPT was also not clear. Important issues of implementation cost and practicality are barriers to use of such propellants but have not been adequately addressed by the Energetics project.

Finding: As presented, the Energetics project efforts in chemical propulsion were deemed subcritical with respect to the facilities and scope of other programs.

Also presented under chemical propulsion was a GRC micropropulsion device using wafer stacks and laser initiation. This effort elicits a mixed response. On the one hand, the thruster was a NASA Glenn invention, so management there deserves some credit for allowing a researcher to pursue his own invention. On the other hand, the program was clearly out of touch and severely deficient compared with strong, aggressive micropropulsion efforts at AFRL, JPL, and universities. Adequate systems analysis to determine total mass impact on the spacecraft at the systems level was not performed. The researchers seemed unaware of similar concepts funded and explored at Princeton University, Honeywell, and the Aerospace Corporation, where the fundamental technical issues surrounding the device were being investigated.

In April 2003, the panel received an update on the Chemical and Micropropulsion work in the Energetics project (Hoffman and Dunning, 2003). Several areas of the project have been redirected, but the effort has not yet addressed important implementation, cost, and practicality issues surrounding chemical propulsion or the Energetics program's role within a larger national chemical and micropropulsion effort both at NASA and within DOD.

Recommendation: The panel recommends that the chemical and micropropulsion research programs within ECT be either terminated or supported at a level where interactions with other groups will keep

the research at the forefront of propulsion research. In any event, researchers in this area should investigate more fully the programs extant in other organizations.

Overall the panel found the On-Board Propulsion tasks to be of the highest quality. Deficiencies in the chemical propulsion and micropropulsion product lines must be balanced against the fact that over 90 percent of the resources are invested in electric propulsion. It is also important to note that the portfolio is well balanced in this regard. Some NASA missions stand to gain the most from advances in electric propulsion and future nuclear-electric propulsion (not within the scope of this review). Chemical propulsion has already realized its major improvements in NASA-relevant performance. Since micropropulsion is also important to future Air Force programs, NASA can leverage DOD programs for its more modest micropropulsion requirements.

ADVANCED SPACECRAFT AND SCIENCE COMPONENTS PROJECT

The Advanced Spacecraft and Science Components project comprises four elements: (1) Advanced Measurement and Detection (AMD), (2) Distributed and Micro-Spacecraft (D&MS), (3) Resilient Materials and Structures (RMS), and (4) Space Environmental Effects (SEE). Each element is discussed in a separate section below.

Advanced Measurement and Detection Element

Introduction

The Advanced Measurement and Detection (AMD) element focuses on the development of miniaturized sensors, advanced active instruments, and nanoscale devices to enable the next generation of remote sensing and in situ sensing capabilities. These technologies most closely address the science requirements of the NASA Earth Science, Space Science, and Biological and Physical Science Enterprises. The scientific requirements of these enterprises demand advances in the detection and measurement of radiation across the entire electromagnetic spectrum. Without measurement, the desired science cannot be performed. Some of the greatest potential for scientific return lies in the x-ray and terahertz (T-ray) part of the spectrum, where quantum-limited and energy-resolving measurements have yet to be attained. For parts of the spectrum where measurement science is relatively mature, research thrusts tend toward either improving efficiency to reduce weight and power consumption or increasing the number, resolution, or range of measurement of passive sensors in the ultraviolet to the visible to the long-wave infrared region. Finally, active laser sensing offers access to new methods of detailing and profiling the planetary atmospheres.

The AMD element was funded at $10.2 million for FY2002. An additional $14.4 million was awarded by the ECT program for external NRAs in this area under the Cross-Enterprise NRA, as discussed previously. The element included seven thrust areas: Focal Planes, Cryogenics, In Situ, Photonics/Lidar, Optics, Radiofrequency/Terahertz, and Nanotechnology.

General Observations

The AMD element is the current incarnation of a long-standing thrust that has succeeded many times and continues to succeed despite the many challenges it faces. A metric of success in this element is the transition of technology to NASA or other agency missions. As stated in the NASA briefings to the panel, “Each task, or group of tasks, has (at least) one target opportunity for future funding in a NASA competitive call for mid-TRL technology” (Krabach, 2002a). There have been a significant number of transitions from basic principles to maturity and integration into instruments for major NASA missions. Recent examples include the microshutter array that is now baselined for the future James Webb telescope and the microthermopile array for the Mars Climate Sounder (MCS) instrument on the Mars Reconnaissance Orbiter (MRO). There are numerous other examples in past history.

In addition, targets for the current research activities include these:

- Diffractive gratings in the MRO imaging spectrometer CRISM (Compact Reconnaissance Imaging Spectrometer for Mars), an Applied Physics Laboratory (APL) instrument;
- Hybrid imaging technology focal plane array (FPA) in MRO entry camera demonstration;
- Micromesh bolometer array in Herschel and Planck telescopes;
- Planar multiplier circuits in Microwave Limb Sounder (MLS) and the Herschel Observatory;

- Superconducting mixers in the Herschel and Planck telescopes and the Stratospheric Observatory for Infrared Astronomy (SOFIA) Casimir instrument;
- 20-K sorption cooler for the Planck telescope;
- Superconducting transition-edge sensor (TES) arrays in Constellation-X; and
- Cadmium-zinc-telluride hard x-ray focal planes in Constellation-X.

There is a well-defined process, described during the panel briefings, that allows a natural transition through mid-TRL instrument development programs such as the Planetary Instrument Definition and Development Program (PIDDP) and Instrument Incubator Program (IIP). One success occurred in the area of uncooled thermopile broadband detector arrays. Research into uncooled thermopile arrays began in what is now called the ECT program in FY1995 and lasted until FY2000. The technology was then transitioned into the Space Science Enterprise through the PIDDP, where focal planes for a waveguide spectrometer based on linear array technology were funded from FY1999 until FY2003. This focal plane technology was subsequently used in the Mars Climate Sounder instrument for the Mars '05 mission based on the thermopile linear detector arrays. The AMD program is now funding the next generation of uncooled two-dimensional thermopile detector arrays, beginning a new cycle of technology maturation and technology graduation (Krabach, 2002b, 2002c). The annex to this chapter provides a detailed table of transition mechanisms and technologies and a figure (5-A-1) depicting the various transition paths used by AMD. A second figure (5-A-2) provides a specific representation of the process used in the uncooled thermopile array example.

Finding: The AMD element is an effective means of pursuing high-risk, high-return research and is a valuable element of the U.S. technology base.

Finding: The AMD element has demonstrated an ability to successfully transition basic research to applications, thus establishing its credibility with the user base and motivating researchers to innovate.

Recommendation: The PRT program should use the AMD element's well-defined transition process as an example for transition in other technology areas.

Research Portfolio

Almost all of the projects in the AMD element are considered good mainstream research, with the truly world-class work making up about 25 percent of the portfolio. The most revolutionary research and development can be found in the radio frequency/terahertz (RF/THz) and focal plane thrusts in projects that could enable near-quantum-limited detection in the x-ray, long-wave infrared, and millimeter and submillimeter parts of the spectrum. Technologies such as superconducting transition-edge sensors, single-electron transistors, hot electron bolometer heterodyne detectors, and monolithic multiplier circuits will most certainly enable future observatories to better view the structure and complexity of the universe.

Important for the advancement of these new sensing technologies is the development of infrastructure or supporting technologies. These can be extremely important for enabling missions, because practical solutions such as reducing weight and increasing power are a major factor in feasibility and instrument selection. Cryogenics research and development—particularly miniature coolers, the adiabatic demagnetization refrigerator, and electrohydrodynamic pumping—are examples of the technologies in this class.

Less revolutionary are the investigations that push technology further, in some cases giving an order of magnitude or more of improvement. Though perhaps not revolutionary, these investigations are very important in achieving better efficiencies in system size and cost or in the number of measurements a given mission might attain. Tasks that investigate the use of microshutter arrays, complementary metal-oxide-semiconductor (CMOS) imagers, quantum well and quantum dot imagers, aluminum foam core optics, and thermopile detector arrays all fall in this category. The work in photonics and lidar, by the researchers' own admission, addresses as much the extension of previously demonstrated capabilities as it does new capabilities. These are heritage programs that support multiple objectives (in both earth and space science) and are being matured in preparation for transition to specific NASA missions.

Finding: The AMD element includes an appropriate balance in the portfolio across technology maturation levels.

Research Plan

As technologies mature, there is a potential for them to transition out of NASA laboratories and into industry; such transitions should be encouraged by the AMD element and the ECT program. In some of the technology areas (e.g., lasers), NASA is keeping the research in-house because it was reportedly more cost effective to do so than to utilize equipment outside NASA. Such decisions or recommendations should be made based on the long-term strategic position of the technology and not be dominated by the near-term development cost.

The transitioning of technologies from research to applications and engineering development is an important factor in the underlying value of the PRT program. Thus, it is not surprising that there are varying levels of technological maturity within the program. While there are a few areas that the panel recommends for near-term transition (such as All-Aluminum Lightweight Optics and Structures, Uncooled Thermopile Broadband Detector Arrays, and laser tasks), the majority of the tasks should remain within the PRT umbrella. The Optimized STAR Structures task appears to be a study of options for deployable structures for a specific mission and not a fundamental research task.

As mentioned previously, the AMD element has been very successful in identifying new sensing modalities, maturing these technologies, and finally migrating them to a mission instrument. The panel believes that the conduct and management of the element were primarily responsible for this success. The other NASA enterprises had taken proactive steps to ensure that they communicated their vision, and the NASA centers helped define the fundamental measurement capabilities necessary to achieve these goals. On both sides, the panel found a strong sense of mission ownership. The fact that NASA has experienced this cycle of requirements definition and development in remote sensing many times in its history has meant that remote sensing technology is one of NASA's key core competencies. The demands on measurement have become so great as to stretch the limits of detection technologies across the electromagnetic spectrum. This has resulted in a balance between technology push on the part of the researchers and technology pull on the part of the enterprises. The transition opportunities between well-defined missions were obvious and were fully pursued by AMD researchers; however, longer term and undefined areas of opportunity both internal and external to NASA were not always acknowledged.

Detectors have applicability well beyond NASA and space applications. Currently important areas of research include the use of quantum dots in computing and terahertz detection for chemical and biological weapons. Such detection technologies are also prime candidates for transition to industry. These are areas where the AMD element could pursue additional leveraging opportunities.

Finding: The AMD element uses a well-defined process that allows a natural transition from basic research through mid-TRL instrument development programs such as the Planetary Instrument Definition and Development Program (PIDDP) and the Instrument Incubator Program (IIP).

Finding: While the process of transitioning technology to missions was well thought out and implemented by the AMD element, the transition to long-term and broader applications was not as well addressed. While some of the technologies are so specialized that there is a limited market, others—such as higher power lasers—should have broader applications.

Recommendation: The transitioning of research to industry should be carefully considered and encouraged.

Necessary to the successful completion of the development cycle is sufficient stability of funding and commitment to sustain the researchers in their quest despite distractions such as reorganizations, redirection, and reprioritization, which would otherwise derail their enthusiasm. The management of the element has done an excellent job of understanding the needs of the other enterprises and engaging the staff so that they understand those needs. They have ensured that the staff supporting the element are highly competent in the appropriate disciplines and have adequate facilities in which to pursue their research. The managers themselves are knowledgeable and informed. They grew up in the AMD element technology area and they have personally experienced a sufficient number of development cycles to make this process work effectively.

The process of bringing technologies from initial demonstration through incorporation into missions has been well defined, as noted above. However, most transition opportunities are available only through competitive calls. By working closely with the science codes, AMD has been able to offer developers and collaborators unique advantages in securing science missions and science payloads in Space Science Enterprise and Earth Science Enterprise announcements of opportunity.

Management, Facilities, Personnel, and Equipment

The desire for revolutionary technologies to enable more capable missions at lower cost and risk brings with it certain management responsibilities. This class of research involves multiyear efforts with schedule flexibility. It also requires a strong team with state-of-the-art facilities. The research teams that made presentations to the panel were of a high caliber and appropriate for the activities. Facilities at both Goddard Space Flight Center (GSFC) and JPL were excellent and appropriate for the research. The ability of the ECT program to provide a stable funding environment over a 3- to 5-year time frame was less clear. A lack of stable funding often introduces substantial inefficiencies into the ECT program.

The diversity of technologies in AMD makes it a difficult area to manage. Effective management demands an exceptionally broad understanding of the driving science requirements, ranging from cosmology, astronomy, and astrophysics to environmental science and human physiology. Because the NASA codes typically focus on the measurement requirements, it is generally left to the researcher and managers in AMD to translate these to the underlying component technology requirements. These component technologies are diverse and broad in discipline, as a more detailed review of the AMD element reveals.

Finding: The panel observed a depth in the very capable research staff in the AMD element. This allowed staff to be moved around while maintaining research continuity. The management was also experienced in the technology development cycle, which facilitated communication between the technology developers and the enterprise scientists.

Finding: The AMD element is well run, and the management has established and embedded a process that shepherds research along a path from initial conception to insertion into missions. Since this process requires several years to almost a decade in some cases, the stability of the element is critical. Setting and following priorities over the long term, as budgets varied, made it possible to protect high-priority research activities in the AMD area.

Finding: Stability of funding over sufficient time to support a critical population of competent staff is crucial. This was a challenge, given the continuing reorganizations at higher levels. (The panel's sense was that the AMD element had been able to maintain funding stability so far, but that in some areas the element funding was dropping dangerously close to the critical threshold.)

Distributed and Micro-Spacecraft Element

Introduction

The Distributed and Micro-Spacecraft (D&MS) element of the ECT program covers the technologies for distributed space systems and microspacecraft. The goal of the element is to develop "technologies to enable revolutionary science collection capabilities through the coordination of multiple spacecraft, and to enable very small, low-cost spacecraft" (Moore, 2002).

Distributed space systems (DSS) are defined as collections of satellites that cooperate to perform a mission in which the known or controlled relative spatial geometry of the satellites is an essential element. The current Global Positioning System is a simple example of such a system, in which the known but loosely controlled relative positions of satellites in the constellation allow it to provide navigation data to the user. Potential applications of DSS are free-flying satellites that perform long-baseline interferometry for high-resolution imaging (Chao et al., 2000), even at astronomical distances, and clusters of satellites in low earth orbit to serve as a sparse aperture remote-sensing system at radio frequencies (Martin and Stallard, 1999) or even in the optical domain. Further applications include sensor webs and dense orbiting constellations to provide a spatial-temporal picture of the near planetary environment (NASA, 2001a, 2001b).

Microspacecraft (MS) are defined as satellites that weigh less than 100 kg.[9] By this definition, many of

[9]This is a broadly recognized definition that is documented in a number of references. Yet, there are other documented uses of the term, which refer to different mass limits, size or volume limits, or a combination of size and mass.

the early satellites would be considered microsatellites, and yet they had very little capability. Thus, the modern term also implies performance superior to that of comparably sized spacecraft of yesterday and today and/or lower mission costs than larger satellites of similar capability.

In FY2002, the D&MS element comprised 14 in-house research tasks with total funding of approximately $2.8 million per year and 18 tasks that were awarded to outside organizations under the Cross-Enterprise NRA solicitation with total funding of approximately $7 million per year. In addition, some of the in-house tasks receive funding from the NASA missions (about $3 million in FY2002).

General Observations

In the view of the ECT panel, distributed space systems and microspacecraft offer the potential for new ways of business that could revolutionize NASA missions. For example, system studies carried out under the Terrestrial Planet Finder program (Beichman et al., 1999) have indicated the potential of distributed space systems to collect new science data. A Mars micromission study (Wilson et al., 1999) and assessments of the Mars micromission architecture (NRC, 1998)[10] showed that microspacecraft could enable affordable and routine gathering of Mars science data. Microspacecraft can also enable new missions that are untenable or unaffordable using larger spacecraft (Moser et al., 2001).

Finding: The Distributed and Micro-Spacecraft element within the ECT program contains many good individual research tasks that represent cutting-edge research with excellent progress and results and enthusiastic researchers. There are, however, opportunities to improve the content of the element, its connection to the mission areas, and the research methodology and management.

The detailed assessment of the panel and the recommendations for improvement are contained in the following sections.

Research Portfolio

The panel agreed that the desired balance in the research portfolio was to have approximately equal numbers of fundamental and applied projects and to have a mix of projects that address the key challenges in each research area, with no gaps. In applying these largely subjective criteria, the panel relied on the expert judgment of its members. To distinguish between fundamental and applied projects, it used the NASA TRL scale, which defines the relative maturity of the technology development. The panel was informed that NASA PRT projects (of which ECT is a component) were intended to develop technology to TRL 4 (Hanks, 2002). The panel found a good mix of applied and fundamental and evolutionary and revolutionary projects within D&MS since approximately half of the individual tasks are TRL 1-2 and half are TRL 3-4. To identify gaps in the portfolio, the panel divided the research into two key areas, distributed space systems (DSS) and microspacecraft (MS), and compared the research in these areas to the stated goals of the program. It identified the following areas of fruitful research for DSS:

- Formation flying control,
- Relative metrology,
- Intersatellite communications,
- Data fusion,
- Constellation control,
- Innovative architectures and concepts, and
- Mission and system design tools.

In reviewing the DSS portfolio, the panel found a balance of tasks in most of these areas. The intersatellite communications, data fusion, and mission and system design tools areas are the responsibility of the CICT program, not the ECT program. In the view of the panel, this could lead to a lack of cohesion and synergy between closely related technology areas and limit the opportunity for the D&MS element manager to balance the portfolio.

[10]The National Research Council report states that "micromissions . . . are fundamental to fulfilling scientific objectives of the Mars exploration program because they can enhance the data return, enable new or unique measurements, provide flexibility to respond to new discoveries, and permit the optimization of surface operations based on experience from relevant preflight tests. In addition, the micromissions . . . provide a potential means of addressing scientific goals not currently included in NASA's architecture (e.g., studies of martian climate change)" (NRC, 1998).

Recommendation: PRT management should include the CICT tasks related to intersatellite communications, data fusion, and mission and system design tools appropriate for distributed space systems in the ECT portfolio to allow the body of research in distributed space systems within the PRT program to be managed as an entity.

The panel identified the following areas of fruitful research enabling very small, low-cost spacecraft:

- Miniature subsystems,
- Multifunctional systems,
- Innovative designs and operations,
- Manufacturing process, and
- Design tools.

In reviewing the microspacecraft portfolio, the panel found that the tasks consisted entirely of miniaturizing traditional satellite subsystems. In the view of the panel, this represents a lost opportunity to include nontraditional approaches to satellite subsystems such as multifunctional components and innovative designs. Furthermore, the area of microspacecraft could benefit from research and technology development focused on manufacturing and mass production of microspacecraft and design tools for microspacecraft. There are also opportunities to reduce the operational cost of such satellites by more tightly integrating the functions of command and control, telemetry collection and processing, and ground operations with the spacecraft design and architecture.

Recommendation: ECT managers should broaden the portfolio of microspacecraft projects to include alternative approaches to reducing size and cost and to provide effective design tools. Research in multifunctional components, innovative design and operations, manufacturing and mass production, and design tools should be considered.

The panel also assessed the balance of tasks in the area of miniature subsystems, as described previously in the Advanced Systems Concepts section of this chapter. According to the assessment, the portfolio in September 2002 was not adequately balanced, since it did not develop technologies for some of the high-payoff subsystems and overemphasized some lower-payoff subsystems. As recommended elsewhere in the report, system analysis tools would help identify these imbalances and would assist in focusing the portfolio on high-payoff projects. Using such an approach, the microspacecraft portfolio could be developed with a solid rationale for which tasks to invest in to achieve the goal, as recommended for the overall ECT program.

Another observation of the panel is that NASA has other technology development programs to miniaturize satellite components. For example, the NASA X2000 program and its follow-on programs were striving to achieve a "satellite on a chip" by developing miniature and highly integrated satellite avionics. The ECT microspacecraft tasks must also be evaluated in the context of this and other NASA activities. The panel was informed that the MS tasks were selected 3 years ago and took advantage of the Systems on a Chip (SOAC) program by pursuing complementary research and leveraging SOAC's technology developments. Subsequently the SOAC and X2000 programs were canceled, leaving a hole in the technology efforts for microspacecraft. An understanding of these relationships and dependencies is important to managing the portfolio. One tool for both insight and advocacy is a roadmap showing how various programs and their planned products can be leveraged.

Research Plan

Distributed space systems (DSS) research tasks have been organized and structured to address the key challenges of a variety of emerging future NASA missions. A survey of the planned missions using distributed space systems, their programmatic milestones, and their technology needs was performed by the D&MS manager (Leitner, 2002). This survey pointed out the relevance of the various DSS tasks and the dates by which the technology would be needed. Furthermore, there is a strong connection between the DSS research tasks and missions such as Starlight and Terrestrial Planet Finder, which have augmented the funding of some of the tasks. This close relationship between technology development and emerging missions helps ensure the applicability of tasks.

A number of mission areas within NASA strongly support miniaturization technology, particularly for the planetary missions and planetary probes (NRC, 2000 and 1994). While microspacecraft technologies generally address this mission need, the connection of some MS tasks to the emerging missions is weak, with little evidence that the tasks are tied closely to mission needs through traceable miniaturization and cost goals or performance metrics. All of the research tasks have well-defined performance metrics at the component and subsystem level, but their relation to a larger strategy and systems perspective is unclear. As one example, the Compact Holographic Data Storage (CHDS) task has established power, mass, and performance goals that provide a significant improvement over the state of the art. Clearly such miniaturization is useful to a variety of applications, but the future microspacecraft missions that will require massive data storage and high data transfer rates at low mass and power are not considered by CHDS task researchers to be possible transition opportunities. However, a number of tasks have identified transition opportunities and have a rational basis for meeting the requirements. Most notable is the Integrated Micropropulsion task, which targets some niche requirements for microspacecraft.

One measure of the relevance of these tasks to the NASA missions is the degree of cofunding from NASA missions and other areas of NASA. Current microspacecraft tasks have no mission funding and are largely funded from Code R. This suggests that ways are needed to more closely align the research tasks with missions. An understanding of why individual technologies are important and how they compare with other technologies is necessary.

Recommendation: ECT managers should develop a microspacecraft technology application roadmap that identifies the performance metrics and missions for potential insertion of microspacecraft technology. This roadmap should help ECT managers and researchers to understand which technologies are important and how they compare with other technologies.

A challenge with such pervasive technologies as microspacecraft is to capture the imagination of mission designers and system developers so that they can better understand how the technology could benefit their mission. Often technologies are perceived to be inapplicable based on preconceived notions about the limitations and capability of microspacecraft. One way to counter these challenges comes from the automobile industry, which develops a concept car to showcase new ideas and stimulate the imagination of the consumer. A notional design that demonstrates to emerging NASA missions how the pieces will come together to achieve the desired cost and mass reduction could be used in a similar fashion within the D&MS element. This idea is not new to NASA, as it is similar to the idea behind the New Millennium Program and the Small Satellite Technology Initiative.

Recommendation: The ECT Distributed and Micro-Spacecraft element should consider a “concept car” approach to stimulate potential applications of microspacecraft technology and to provide cohesion and focus to microspacecraft technology tasks.

The Aerospace Technology Enterprise (ATE) goal for PRT (Hanks, 2002; Venneri, 2001) is “to enable a revolution in aerospace systems.” The D&MS element tasks are intended to enable “radically new aerospace systems” by focusing on “broad, crosscutting innovations” for a number of NASA missions (Venneri, 2001). The D&MS element tasks have these features: they apply to several mission areas within the NASA enterprise, they offer opportunities to enable new science capability, and they represent an approach to achieving these new capabilities that is not incremental or evolutionary. In the view of this panel, the D&MS elements are appropriate research areas for Code R.

Technical Quality

The panel was impressed with the individual tasks in the D&MS element. In general the tasks represent excellent work that advances the state of art. For example, this element has produced significant results by leveraging MEMS technology to develop microcomponents such as microgyros, which reduce size and weight by a factor of 5 or so, and micropropulsion, which is developing miniature propulsion modules with precise impulse bits. Another example is the excellent progress toward powerful algorithms for the control of formation flying and relative metrology sensors. Many of the tasks are based on innovative designs and concepts and on the use of emerging technologies and devices in innovative applications. In particular, the two tasks Formation Flying Control and Formation Flying Sensor are considered world-class efforts. For example, the Alpha-Voltaic Micropower Source task explores the “old” idea of directly converting energetic alpha particles to electrical energy, using new developments in diamondlike materials and advances in material modeling and simulation. Another example of world-class quality is the Modulation Sideband Technology for Absolute Ranging (MSTAR) task, which takes advantage of advances in optical polymer modulators developed for the telecom industry to allow absolute ranging between satellites to nanometer accuracy.

One objective measure of the productivity and, to some extent, the quality of the D&MS element is the number of publications and patents produced. During the past 2 years, an average of three significant products were produced every year by each task—an impressive accomplishment. However, this ECT element had only a small number of articles in peer-reviewed publications and could benefit from additional attention to publication.

Methodology

The panel also evaluated the methodology to measure the quality of the research. It found the tasks have structured research plans with incremental goals and milestones, and most have well-defined deliverables. The research was initiated based on solid hypotheses and, in many cases, was compared with competing technologies at a subsystem or component level, but there is no evidence that relative progress is tracked or that decision points are in place to abandon the effort if other work shows more promise. Furthermore, in many of the tasks there were no system analyses of the technologies and innovations before or during the task to determine if system implications would overshadow the expected benefits, underlining the need for a systems analysis capability.

The challenge of implementing distributed space systems has been divided into separate thrusts—e.g., metrology, formation control, data fusion—each with several tasks. These tasks have worked collaboratively to develop the requisite technologies. However, unexpected things can happen when technology components are brought together and the interface assumptions are tested. A common methodology in such complex systems is to rely on interface definitions in the early stages. Components are then brought together, physically or virtually, at later stages in the development. DSS tasks could benefit from system-level simulation capability with hardware-in-the-loop testing or a representative testbed or demonstration. The risk of transitioning these technologies could be mitigated by such a means. The CICT program contains a task entitled Object Oriented Simulation of Distributed Observing Systems, which is developing a simulation testbed building on a GSFC testbed for the Global Positioning System (GPS), and the Formation Flying Control task is developing software simulation tools; both could be extended to involve end-to-end testing. There are also flight opportunities that could serve as orbital testbeds for these technologies.

Recommendation: The ECT program should investigate and develop appropriate testbeds to integrate, test, and validate the various components of distributed space systems research. Flight programs should also be consulted in defining relevant testbeds to improve testbed fidelity and reduce their risk.

Technical Community Connections

Another quality metric is the expertise of the researchers and the use of collaborations to build a good research team. The principal investigators (PIs) for these tasks are experienced, and many are recognized experts in their fields. The in-house tasks also include a good number of collaborations with industry and academia, bringing the best expertise to the problem. For example, the formation flying control research tasks have support from professors at four universities known for controls research. The microgyro development effort has partners from Boeing who have a great deal of experience in satellite navigation and the challenges of integrating new technology in satellites, and it leverages an NRA with Nanopower, Inc., to develop an advanced electronic interface for the microgyro. Researchers are knowledgeable about the developments in their area and appear to have selected partnerships with leading researchers in the relevant fields. Some of the tasks are partnerships with other research organizations, such as the Defense Advanced Research Projects Agency (DARPA) and the Air Force Research Laboratory (AFRL). Although in some cases there are related efforts at other government laboratories such as the Department of Energy and AFRL, the researchers are aware of and can articulate the differences between their programs.

Facilities

Another measure of excellence is the research facilities. Many of the microspacecraft tasks leverage unique NASA facilities such as the JPL Micro Devices Laboratory, which has capabilities for state-of-the-art microsystems and microelectronics fabrication and space component and systems testing. Some of the tasks are developing dedicated facilities to support their research, such as the micropropulsion laboratory at JPL.

Resilient Materials and Structures Element

Introduction

The Resilient Materials and Structures (RMS) element within the ECT program is focused on crosscutting technologies for materials and structures and the testing of such materials. The element invests in developing component technologies and validation technologies at TRL 1-3 and performs subsystem and system tests at TRL 4-6. The element was funded at $4 million for FY2002, with cofunding of $960,000 from the Department of Defense (Belvin, 2002).

General Observations

The key objectives of the RMS element are to (1) develop space-durable materials, multifunctional and adaptive structures, and large deployable and inflatable structures to reduce spacecraft mass and launch volume and (2) improve spacecraft performance and reliability in extreme environments. The RMS objectives are appropriate and relevant to missions involving solar sails and large-aperture systems (NASA, 2000a, 2000b, 2003b). The RMS element objectives are clearly defined, and they are connected to the NASA mission and the PRT goal of developing revolutionary technologies and technology solutions to enable fundamentally new aerospace systems capabilities and missions (Hanks, 2002).

The RMS element funds nine tasks. Of these, two were judged world-class: (1) Experimental Methods for Shape/Dynamic Characterization of Gossamer Structures and (2) Analytical Methods for Shape/Dynamic Characterization of Gossamer Structures. The Experimental Methods task has resulted in unique experimental capabilities to characterize deployment dynamics and the shape and vibration properties of ultralightweight, inflatable space structures. This work is complemented by more recent modeling efforts under the Analytical Methods task. The collaboration between these two tasks is excellent and is expected to produce valuable tools for the design of gossamer structures. The Solar Sail Integration and Ground Test task provides a mechanism to validate such design tools. To date, initial dynamic testing of a two-quadrant, 10-m sail has been carried out.

The development of new ultralightweight, space-durable materials is another important aspect of the RMS element. Under the Space Durable Polymers task, an electrically conductive polyimide has been developed without significantly sacrificing optical transparency. This unique combination of material properties is accomplished by using carbon nanotubes. Numerous publications, invention disclosures, and patents have resulted from this work.

A newer task, Lightweight Multifunctional Space Components, seeks to incorporate sensing and actuation capabilities into space-durable membrane structures. The goals of this task are far reaching and could lead to revolutionary materials performance. However, performance metrics for assessing the achievement of goals need to be more clearly defined.

The Dual Anamorphic Reflector Telescope (DART) Precision Testbed Development task is a high-profile project to develop the next generation of large, lightweight deployable telescopes for NASA's submillimeter and infrared missions. A 1.2-m prototype has been constructed and diffraction-limited performance measured. The Membrane Waveguide Antenna task seeks to design a feed network for a large, lightweight, deployable antenna with low electromagnetic losses and bandwidth tailoring. Technology developed in this task could be used by NASA in earth science missions to measure soil moisture. Both tasks could benefit from stronger connections to the materials and modeling research efforts in the RMS element.

Work is also under way to create a materials database for inflatable, rigidizable columns under the Characterization and Assembly of Deployable Structures task. While this work will help develop standardized test methods, it is unclear exactly how the database will be used. This task could benefit from analysis to rank and scale the results. There was also concern that the work was more a service to non-NASA customers than a basic research activity.

A final task, Large Area Membrane Fabrication/Deployment, proposes the use of an origami fold design to package and deploy large membranes. This task is largely based on previous work originating in Japan and is very mature, which does not fit within the stated goals of the PRT program. After it had gathered information for the review, the panel learned that funding for this task has been canceled for FY2003. A new project focusing on fabrication and deployment of inflatable truss structures is now in the portfolio. Other tasks within the portfolio are in general of good quality, but the panel has suggested ways to improve the work or increase collaboration with other efforts, as outlined in the sections that follow.

Research Portfolio

Most tasks fit within the stated objectives of the RMS element. However, some are clearly stronger than others and will have greater scientific impact for future needs of NASA missions. The element tried to bring different disciplines together, beginning with a 60–40 split between the number of applied and fundamental research tasks. However, by having well-established applied components in the element, the risks of individual tasks were minimized, and the whole effort is now moving toward 75 percent applied research and 25 percent basic research.

The balance of technology maturity in the whole element is good. Advancing technology from a lower to a higher TRL is a good decision that will enhance the visibility and impact of the element. For even better results, the element needs to focus on fewer but better-interconnected tasks, which will also secure better transition of technology. Great benefits are expected from moving the element's focus from lower precision structures to higher precision structures, e.g., antennas and telescopes.

A shift in the balance between more fundamental high-risk, high-payoff research and user-driven, lower-risk, mid-payoff research is also warranted. The overall PRT program has a far-reaching vision of resilient materials and structures (Hanks, 2002) that involves concepts such as self-assessment, self-healing, and multifunctionality. However, little of this grand vision was apparent in the RMS tasks.

Recommendation: A shift toward higher risk research on revolutionary materials and structures and a longer-term vision would greatly enhance the program. One example would be expanded research on multifunctional material systems, active controls, and advanced vehicle concepts, which would shift the research focus from lower precision structures to higher precision structures.

Overall, the quality of the work being done in RMS is good. As discussed above, several of the strongest tasks had excellent publication records and were producing work on a par with efforts in academia, the national laboratories, or large research centers in industry. For example, the majority (about 80 percent) of the publications, presentations, and patent disclosures for the element come from two very successful tasks, Space Durable Polymers and Experimental Methods for Shape/Dynamic Characterization of Gossamer Structures. Other tasks focused more on user-driven research and were less productive in terms of scholarly publications and presentations, but in many cases they had greater relevance to specific NASA missions or applications. The research under these user-driven tasks would also be comparable to that conducted by similar applied research programs in industry and at DOD laboratories.

Most of the tasks in the RMS research portfolio are relevant for future space technology and NASA mission needs. In particular, the ultralightweight, space-durable materials and membrane structure technologies under investigation have the potential to satisfy the technology requirements for missions described in the Space Science Enterprise and Earth Science Enterprise mission sets, as defined in their long-term strategic plans (NASA, 2000b, 2001c). It appears, however, that no relevant systems analysis has been done to quantify the potential payoff.

Research Plans

The RMS element objectives are clearly defined and are connected to the NASA mission and the PRT goal of developing "revolutionary technologies and technology solutions to enable fundamentally new aerospace systems capabilities and missions" (Moore, 2002). The development of space-durable materials, multifunctional and adaptive structures, and large deployable and inflatable structures to reduce spacecraft mass and launch volume and to improve spacecraft performance and reliability in extreme environments are the main objectives of the resilient materials and structures element. These objectives are stated well in NASA's Strategic Plan and its Vision (Goldin, 2000; O'Keefe, 2002). New research goals should be set within the element, focusing on multifunctional material systems, active controls, advanced vehicle structural concepts, and radiation shielding materials, which will move the program from lower precision structures to higher precision structures.

The task deliverables are clearly stated for most components of the RMS element. The element should consider whether the guiding technical metrics of the deliverables are consistent with basic structure stiffness requirements. For example, is a "reduction in mass by a factor of 3" realistic in view of material specific stiffness and deployed structure stability requirements? Is there a fundamental limit to mass reduction given known material properties? Also, a "reduction of the package volume by a factor of 10" is meaningful only if further constrained by the volume of the deployed structural system, which also flows from the deployed structure stability requirements.

The RMS element's key metrics for progress and accomplishments were publications and the mentoring of students. Metrics for quality of research should also include patents, new analysis tools, and innovative experiments. The funding for this element exhibited a flexibility that is very positive for the health of the whole effort. The element portfolio was refocused after the first year, consolidated during the second year, and then expanded in the third year, with an emphasis on the analysis that was needed for the research effort. The analysis group that was added during the third year provided a mechanism by which increased funding could be wedged into the element. The quality of RMS managers has been shown by the way they selectively emphasize some tasks, eliminate others, and introduce new ones. This flexibility was a positive aspect of RMS that should be considered for other ECT projects and elements. Because some tasks are not performing or do not seem to map to RMS goals as well as others, the panel believes they should be consolidated to achieve a more focused RMS program.

Recommendation: RMS management should continue to reevaluate the research portfolio each year in order to most effectively focus the research under the current program's available resources.

There is adequate internal review of the element. RMS program managers evaluate the element yearly, refocusing it as necessary. The recent restructuring is a strong indicator that the review process brings needed reorganization in a timely manner. However, no external review of the element's portfolio is apparent.

The critical personnel and facilities were defined clearly. The experimental facilities are certainly available and adequate. Critical personnel are available for most of the efforts, even though external expertise (outside NASA) is, appropriately, sought in a few areas as required.

Methodology and Scientific Hypotheses

Most of the research plans for individual tasks were well formulated and comparable to work done elsewhere within the government. Little RMS work could be accurately called "academic" or basic research, so such a comparison would be inappropriate. Most of the plans were focused on the application of basic technology to particular structural architectures or materials.

The panel did not observe any scientific hypotheses to specifically support the experiments that were under way. Most were "tests" or "demonstrations" rather than experiments in the strictest sense. In one case (Experimental Methods for Shape/Dynamic Characterization of Gossamer Structures), this was appropriate, because the activities involved sensor and methodology development efforts. However, one might expect that the work on sensor technology should consider specific experimental hypotheses in future activities—for example, a hypothesis on critical load levels leading to particular wrinkle patterns. Experiments should be devised that focus on such an issue rather than on a system-level demonstration.

One of the strong points of the RMS element was the integration of lab equipment, modeling and simulation, and field testing. The element is close to providing a direct correlation between the buckled thin-membrane wrinkle patterns observed in the laboratory and those predicted from analysis with a commercial finite-element model code. However, this comparison will only validate the nominal static stiffness of membrane structures. The research should also address the prediction of dynamic response.

One weak point in the RMS element was the lack of system-level assessments of the research. It seemed that most of the work was directed at membrane structures, but the design goals or performance breakpoints were not quantified. In fact, such structures may be useful only to particular missions, such as solar sails, unless the effects of structural instability and low fiber-volume fraction can be mitigated. When goals were identified, they were generally not linked to system-level impacts. The importance of evaluating system-level impacts applies to all areas of the ECT program and is a major recommendation of the panel. NASA should undertake a series of mission studies that use system-level sensitivities to guide research directions. The element is largely a bottom-up portfolio, based on the local interests of the researchers. A balance of top-down and bottom-up research should be sought.

The RMS element intends to redirect the portfolio into higher precision structural technology over the next 3 to 5 years. This should be augmented to include more aggressively visionary technologies, such as smart materials and multifunctional structural components and systems.

Technical Community Connections

The membrane structures research in RMS overlaps with similar efforts at AFRL. However, the NASA activity in basic test instrumentation for membrane structures appears to be a unique capability. The relatively low level of activity on smart materials appeared to duplicate some of the work being done for the Air Force Office of Scientific Research (AFOSR) and AFRL.

The tasks showed an appropriate interaction with non-NASA experts, particularly those from other government laboratories and industry. Most of the industrial interaction consisted of leveraging NASA Small Business Innovative Research (SBIR) awards or cooperating with a DARPA program. The use of academic researchers was noticeably lacking, with such funding accounting for less than 2 percent of the total RMS budget. The RMS element's outside work is primarily in the Cross-Enterprise NRAs, the Small Business Innovative Research program, and a few unsolicited small university grants. There was some commendable leveraging of SBIR and NRA activities to complement the in-house work.

Researchers are in large part widely published in conference proceedings. They should increase their publication in peer-reviewed journals to enhance their interaction with the broader research community. NASA management should support and encourage this publication and interaction. Also, in the past, travel funds were linked to salary line items. As a result, NASA personnel had difficulty traveling to visit other researchers or to attend conferences. This situation can only be addressed at the highest levels within NASA. In the past year, NASA has moved to a full-cost accounting method, which may change the way travel funds are allocated.

Facilities, Personnel, and Equipment

The RMS element benefits from well-qualified NASA personnel to carry out the necessary research tasks. There is a complementary mix of personnel specializing in experimental and analytical work as well as a broad range of disciplines including materials science, physics, mechanics, and structural engineering. The program also has strong interaction with academia and industry through the Cross-Enterprise NRAs, which have been heavily leveraged by several research efforts.

The equipment that was viewed during the laboratory tours was in good working order and provided the necessary capabilities for the research at hand. NASA Langley clearly has unique capabilities for the testing of large space structures. Its high bay and large vacuum chambers are national resources that should be maintained and possibly enhanced. The state-of-the-art equipment used for the photogrammetric dynamic/shape measurements of gossamer structures is particularly noteworthy and provides a unique measurement capability. The facilities and work environment were also well suited for the research tasks. The facilities at NASA Langley enabled several unique capabilities such as the ability to test 30-m rigidizable columns in compression and 10-m solar sail panels in vacuum. NASA should consider component and subsystem testing of parts of the Webb Observatory as a mechanism for improving in-house test and analysis capability.

Contracts are well integrated with the stated goals and objectives of the RMS element. Based on the limited information available, there appears to be little duplication with other government capabilities. As stated previously, several of the capabilities and facilities used in this program are unique.

Recommendation: NASA Langley Research Center should maintain its unique ability to test large space structures in its high bay and large vacuum chambers, which are national resources.

Space Environmental Effects Element

Introduction

The Space Environmental Effects (SEE) element within the ECT program develops engineering technology products in the areas of electromagnetic effects and space charging, ionizing radiation, meteoroid and orbital debris, and neutral external contamination, among others (Kauffman, 2002). The element is modestly funded at $1.5 million for both FY2002 and FY2003.

General Observations

The Space Environments and Effects (SEE) element, managed by the Marshall Space Flight Center (MSFC), is unique within NASA in that it is the only activity that develops and distributes computer codes, models, tools, and guidelines for dealing with space environment effects on the design of spacecraft systems. The spacecraft design community across the nation extensively uses the deliverables issued by the SEE project to improve the reliability and survivability of future space missions.

The SEE element is currently conducting research and developing codes to predict outgassed material contamination, space plasma interactions with spacecraft, the size distribution and damage impact of space projectiles, deep charge storage in insulators, and risk assessment of solar particle events. The element is completing a highly collaborative 5-year effort with the AFRL Hanscom laboratory to develop a comprehensive revision to NASA's spacecraft charging analysis codes (NASCAP-2K).

The project heavily leverages the activities of over 100 scientists and engineers from industry, academia, NASA, and other government agencies through the SEE Technical Working Group. The scientists and institutions selected to work on the SEE-funded projects are all highly respected within the space science community.

The SEE element is an engineering technology development activity (TRL 4-6) and does not involve a lot of risk. Because it is neither a fundamental research project nor an applied research project, it will not lead to breakthrough results. Rather, the SEE project is a pragmatic and necessary activity that produces reliable, standard design codes needed and used by the entire spacecraft design community. The SEE project is accomplishing its goals. Priorities for future activities are determined by a steering group of NASA/AFRL senior technical and program personnel. The panel does note that the high TRL of this activity means that its goals do not necessarily fit in with the more revolutionary goals of the ECT program. This project should continue to be funded and supported by NASA owing to its importance to the nation; however, it should be considered for placement within another element of NASA funding.

Finding: The SEE element is a unique, pragmatic, and much-needed technology development activity that produces standard design codes, models, tools, and guidelines for dealing with the effects of the space environment on the design of spacecraft systems that are used by the entire U.S. space community. The SEE element demonstrates good cooperation with the AFRL in selecting relevant topics and makes excellent use of NRA opportunities to select the best scientists and engineers in the nation to conduct research. The SEE element is accomplishing its goals and widely distributes the results to the space science and design communities through reports, publications, and symposia.

Recommendation: The SEE element's technology development activity should be continued but should be considered for placement within another funding element of NASA. The concept of technical working groups used by the element's management should be considered for other areas of the PRT program.

Research Portfolio

The SEE research portfolio currently consists of nine tasks that are performed at various institutions by respected scientists in the space science community. All tasks were selected from responses to a NASA Research Announcement (NRA8-31) using a peer review selection process. All tasks are funded yearly starting in FY2002, with options for additional funding up to a maximum of 3 years, i.e., through FY2004. In addition, the SEE project was directly funding the completion in FY2002 of three tasks: Satellite Contamination and Materials Outgassing Knowledge Base, a physics-based Integrated Environments Tool that models micrometeorite environments in interplanetary space, and the collaborative NASCAP-2K code described above.

The panel did observe that the recent and current SEE tasks are more focused on near-earth space environments. While this is an important area for continued research, the SEE element should consider expanding its portfolio to include more basic research in space environmental effects for deep space missions.

Recommendation: Future SEE element activities should consider adding to SEE's portfolio more research tasks dealing with future NASA deep space missions.

Research Plans

All of the tasks in the current SEE element's portfolio contain realistic, measurable goals and milestones. Progress is assessed through quarterly technical reports and reviews. The tasks are all low to medium risk (TRL 4-6) and performed by experienced scientists, so the probability of completing the stated objectives is high. The computer codes, models, and databases provided as deliverables are needed and used by the entire spacecraft design community at NASA, the U.S. Air Force (USAF), and aerospace companies. The funding levels in general are adequate to accomplish the tasks, particularly since the element heavily leverages other funding at the performing institutions.

Methodology and Scientific Hypotheses

The fact that the tasks are competitively selected from the space science community based on NASA priorities determined by the NASA/AFRL Technical and Program Steering Group assures that the right resources and personnel are being applied to the most relevant challenges. The SEE project is highly collaborative, with research being performed at the various USAF research laboratories and activities relevant to NASA priorities being funded and incorporated into the appropriate space environment databases. One of the principal challenges is the resolution of conflicting data obtained from different sources. In these cases additional tests are conducted to resolve discrepancies and to increase the accuracy of the resulting models, codes, and tools. Gaps and weaknesses in current models are used to guide new space and ground data collection activities.

Technical Community Connections

There are approximately 100 people within the SEE technical working groups, including 50 from industry, 12 from academia, 32 from NASA centers, and 6 from other government institutions. Membership and collaborative activities encourage the exchange of knowledge and avoid duplication of research. The SEE element is also collaborating with AFRL Hanscom and the European Space Agency to sponsor the Eighth Spacecraft Charging Technology Conference, to be held in October 2003. Topics such as models and computer simulations, ground-testing investigations and techniques, on-orbit missions and investigations, environment specifications, plasma propulsion, and materials development will be discussed. Participation in conferences such as this provides an excellent opportunity to discuss and disseminate the end products of the SEE element and to learn of new results that can be incorporated in future SEE tasks. The SEE element has funded work resulting in 33 publications since 1994 and eight new models or tools for distribution. (This does not include publications of members of the technical working groups.)

Facilities, Personnel, and Equipment

The SEE element does not possess extensive facilities or equipment but uses instead the resources of the various institutions conducting the contracted research. Through a competitive process involving scientific peer review, the most capable scientists and institutions are selected to perform all of the tasks in the SEE element. This approach assures that the best scientists, test facilities, and equipment are always selected to conduct a task without incurring the overhead and maintenance costs associated with an in-house capability.

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Tim Krabach, Jet Propulsion Laboratory, "Advanced Spacecraft Systems: Advanced Measurement and Detection," presentation to the ECT Panel on June 11, 2002(a).

Tim Krabach, Jet Propulsion Laboratory, "Uncooled Thermopile Broadband Detector Arrays Graduation Path," material provided to the ECT panel on November 6, 2002(b).

Tim Krabach, Jet Propulsion Laboratory, "Graduation Paths for Advanced Measurement and Detection Development," material provided to the ECT panel on November 6, 2002(c).

Jesse Leitner, NASA Goddard Space Flight Center, "ECT Distributed and Micro-Spacecraft Element," presentation to the ECT panel on June 12, 2002.

Chris Moore, NASA Headquarters, "Enabling Concepts and Technologies Program Overview," presentation to the committee and the ECT panel on June 11, 2002.

Chris Moore, NASA Headquarters, "Technology Assessment Analysis," briefing by teleconference to the ECT panel on March 20, 2003(a).

Chris Moore, NASA Headquarters, "ECT Master Task List," material provided to the committee and ECT panel on May 5, 2003(b).

Sean O'Keefe, NASA Headquarters, "NASA Vision," briefing to Maxwell School of Citizenship and Public Affairs on April 12, 2002. Available online at <http://www.gsfc.nasa.gov/indepth/nasavision.html>. Accessed September 4, 2003.

Harley Thronson, Gary Martin, John Mankins, Guy Fogelman, Grant Paules, and George Komar, personal communication to ECT panel on September 16, 2002.

Pat Troutman, NASA Langley Research Center, "Revolutionary Aerospace Systems Concepts (RASC)," briefing to the ECT panel on June 11, 2002.

Pat Troutman, NASA Langley Research Center, "Revolutionary Aerospace Systems Concepts (RASC) Integration with Agency Aerospace Systems Analysis (ASAA)," briefing by teleconference to the ECT panel on March 20, 2003.

Chuck Weisbin, Jet Propulsion Laboratory, personal communication to Todd Mosher, Utah State University, on March 2003.

ANNEX: TECHNOLOGY GRADUATION PATHS—EXAMPLES OF THE MATURATION PROCESS IN THE ECT ADVANCED MEASUREMENT AND DETECTION ELEMENT

The Advanced Measurement and Detection (AMD) element within the ECT program has developed an excellent process for maturing technologies. Each technology is examined for possible overlap with various graduation paths both internal and external to NASA. Figure 5-A-1 shows how that process works. Possible paths include (1) direct insertion into a NASA mission, (2) competitive space and earth science and biological and physical research instrument programs (such as PIDDP, SARA, ROSS, IIP, AEMC), (3) focused technology programs, and (4) non-NASA efforts in both the federal government and industry. The AMD element gave the panel many examples of specific technologies that had followed various graduation paths successfully. Twenty of those examples are listed in Table 5-A-1.

One success occurred in the area of uncooled thermopile broadband detector arrays. Figure 5-A-2 provides a schematic of the technology research funding, the competitive call used to transition the technology to a NASA mission area, and the specific NASA mission on which the technology was baselined. Research into the uncooled thermopile arrays began in what is now called the ECT program in FY1995 and lasted until FY2000. The technology was then transitioned into the Space Science Enterprise through the PIDDP, where focal planes for a waveguide spectrometer based on linear array technology was funded from FY1999 until FY2003. This focal plane technology was subsequently used for the Mars Climate Sounder (MCS) instrument in the Mars '05 mission based on the thermopile linear detector arrays. The AMD program is now funding the next generation of uncooled two-dimensional thermopile detector arrays beginning a new cycle of technology maturation and graduation.

Briefings

Tim Krabach, Jet Propulsion Laboratory, "Advanced Spacecraft Systems: Advanced Measurement and Detection," presentation to the ECT panel on June 11, 2002(a).

Tim Krabach, Jet Propulsion Laboratory, "Uncooled Thermopile Broadband Detector Arrays Graduation Path," material provided to the ECT panel on November 6, 2002(b).

Tim Krabach, Jet Propulsion Laboratory, "Graduation Paths for Advanced Measurement and Detection Development," material provided to the ECT panel on November 6, 2002(c).

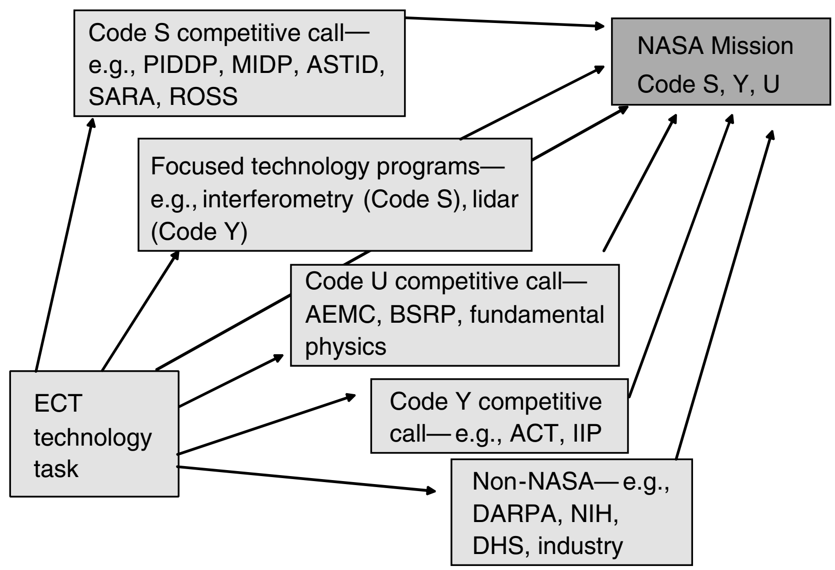

FIGURE 5-A-1 Graduation paths used by the Advanced Measurement and Detection element. SOURCE: Adapted in part from Krabach (2002a, 2002c).

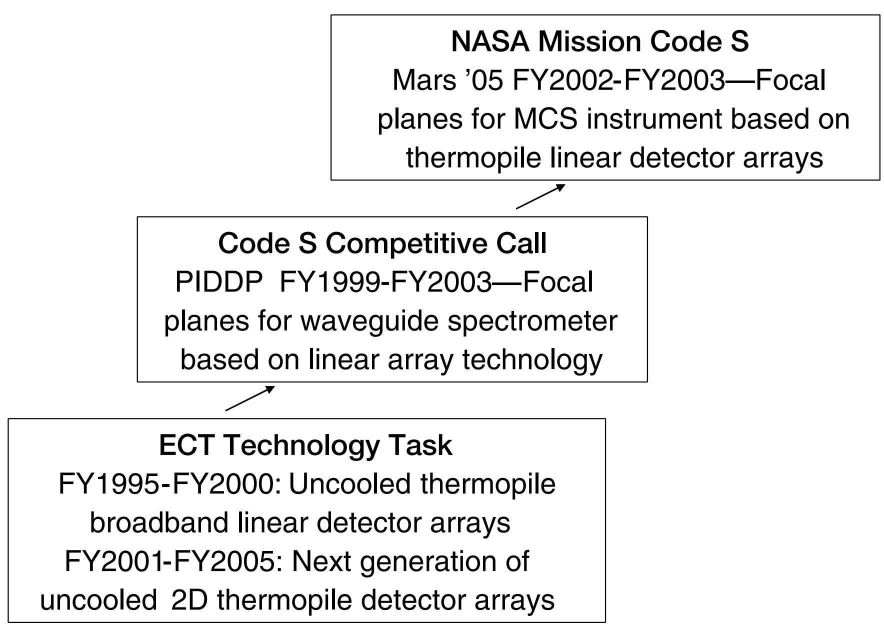

FIGURE 5-A-2 Graduation path for uncooled thermopile broadband detector arrays. SOURCE: Adapted in part from Krabach (2002b).

TABLE 5-A-1 Graduation Paths for Various AMD Technologies

Direct Transfer Examples		
Hybrid Imaging Technology (HIT) task	HIT for Mars '05 Op-Nav camera	Insertion in progress (camera will demonstrate high-accuracy approach navigation)
E-Beam Lithography Development task	Gratings for Hyperion (EO-1), Warfighter, COMPASS, CRISP (Contour) Gratings for upcoming CRISM (Mars Reconnaissance Orbiter) and HSIT (SPIRITT)	Insertion in progress
Silicon Nitride Micromesh Bolometer task	Herschel and Planck missions	Insertion in progress
Superconducting Detector and Mixing tasks	Herschel, Planck, and SOFIA Casimir instrument	Insertion in progress
Code S Technology Call Transfers		
Code R Work	Code S Task/Call	Relationship
Hybrid Advanced Detector for Space Physics Instrument task	PIDDP: compact, low-voltage, high-resolution, robust solar-blind UV imager	Technology development initiated and enabled by Code R
Lidar for Mars Missions task	PIDDP: Planetary Microlidar for Wind and Dust	Technology development initiated and enabled by Code R
Geochronology task and Miniaturized Quadrupole Mass Spectrometer task	PIDDP: In Situ Geochronology System Based on Laser-Induced Breakdown Spectroscopy and Noble Gas Mass Spectrometry	Technology development initiated and enabled by Code R
Microfluidics task	ASTEP: AstroBioLab—A Mobile In Situ Subsurface Biotic Detector and Soil Reactivity Analytical Laboratory	Technology development initiated and enabled by Code R
ELXS development task (finished in FY01)	ASTID: Electron-Induced Luminescence and X-Ray Spectrometer (ELXS) System for Life Detection	Technology development initiated and enabled by Code R
Miniaturized Quadrupole Mass Spectrometer task	ASTID: Measurement of Isotopic Composition of Iron Oxides as a Biosignature on Mars	Technology development initiated and enabled by Code R
Development of Carbon Nanotubes task	ASTID: Detection of Nanoscale Activity (DNA) with Carbon Nanotubes Used as Mechanical Transducers	Technology development initiated and enabled by Code R
Tunable Laser Diodes Development task	MIDDP: Tunable Laser Spectrometers for Mars Scout Mission	Technology development initiated and enabled by Code R

TABLE 5-A-1 (continued)

Focused Technology Programs		Status
Tunable Laser Diodes task	Mars Focused Technology: Tunable Laser Spectrometers for Atmospheric and Subsurface Gas Measurements on Mars	• Development of near-IR tunable laser spectrometers (TRL 4-6) for Mars • Measurement: lander, balloon, cryobot, probe; atmospheric and subsurface (evolved) gases and their isotopic ratios • Science: biogenic signatures, mineral composition, climate history • Emphasis on space-qualifying laser sources and signal processing electronics
Hybrid Imager task	Mars Focused Technology: Optical Navigation Camera	• Mars Exploration Program is planning to use optical navigation for mission-critical guidance in CNES ’07, MSL ’09, and MSR ’13 • The accuracy required for optical navigation is better by a factor of 10 than has ever been demonstrated at Mars (by Viking Orbiters)
Code U Competitive Call		
Code R Work	Code U Task	Status
Nanotube Based Nanoklystron Technology task	BSRP: Remotely Coupled DC Power for Driving Nanotubes	Proposed technology development initiated and enabled by Code R
Antimony Based Lasers task	AEMC: Tunable Diode Lasers for Trace Gas Monitoring	Proposed technology development initiated and enabled by Code R
Microfluidic Technology Development task	AEMC: Microfluidic Lab-on-a-Chip Ion Analysis for Water Quality Monitoring	Proposed technology development initiated and enabled by Code R
Sensors for Electronic Nose task (NRA with NIST)	AEMC: Ground Testing of the Second Generation Electronic Nose for Air Quality Monitoring in Crew Habitat	Proposed technology development initiated and enabled by Code R
Code Y Competitive Call		
Code R Work	Code Y Task	Status
MEMS Transmit/Receive Module for Thin-Film Membrane Antennas task	ACT: Ultra-High Efficiency L-Band Transmit/Receive Modules for Large-Aperture Scanning Antennas ACT: T/R Membranes for Large-Aperture Scanning Antennas	Proposed technology development initiated and enabled by Code R
Solar Blind Detectors	ACT: Development of Large Format Visible-NIR Blind Gallium Nitride UV Imager for Atmospheric Earth Science Applications	Proposed technology development initiated and enabled by Code R

NOTE: See Appendix F for the spelled out form of the acronyms in this table.
SOURCE: Adapted in part from Krabach (2002c).

Appendixes

A

Statement of Task

This project will produce biennial assessments of the programs within NASA's Aerospace Technology Enterprise—the Pioneering Revolutionary Technology (PRT) program, the Aviation program, and the Space Transportation program. The first review in the series will be of the PRT program group; other reviews will follow in subsequent years. Programs within the PRT group are the Enabling Concepts and Technologies (ECT) program, the Computing, Information, and Communications Technology (CICT) program, and the Engineering of Complex Systems (ECS) program.

The committee will assess the overall scientific and technical quality of the PRT program elements. These assessments will include findings and recommendations related to the quality and appropriateness of NASA's internal and collaborative research, development, and analysis. While its primary objective is to conduct peer assessments that provide scientific and technical advice, the committee may offer programmatic advice when it follows naturally from technical considerations or is requested by the NASA Associate Administrator for Aerospace Technology.

The committee will be assisted by three NRC panels that each focus on one of the three elements of the PRT program listed above. Each panel will assess the scientific and technical quality of selected programs in the element under its purview. Each panel will provide input to the committee's report via internal working draft reports to the committee. Panels will meet twice during the study to receive technical presentations about the projects under review by their group and formulate final findings and recommendations. Panel members will also make site visits as deemed necessary in formulating the assessment. Portions of each meeting will be highly interactive with NASA personnel. After completion of its deliberations and investigation, the panel will report to the committee on its findings via internal privileged correspondence and working papers.

The main committee will meet twice during the review: once to plan the review process, meet with the panel members, and discuss the charge to the committee and panels, and a second time to discuss in a closed session the working papers and findings and recommendations. This meeting will also involve interactive discussions with NASA personnel from the program. A final report will be developed from discussion at this final meeting. Before the final report is published, committee and panel members may revisit select programs within the PRT group during a short reevaluation process. This reevaluation will assess progress made by individual programs within the PRT that were initially deemed to be problematic.

While the committee's observations will follow the broad themes of technical and scientific quality and appropriateness of the research, the research performers, and the research plan, the panel assessments should use specific criteria, where appropriate. These criteria are discussed next.

Research Portfolio

- Is the balance between fundamental and user-driven research proper?
- Is research being conducted in the proper areas?
- Are there plausible hypotheses supporting each of the research plans?
- Is far-term research at the forefront of science and determined to be a world-class endeavor?
- Is the proper amount of high-risk, high-payoff research being pursued?
- Is the application of fundamental science to solve real-world problems adequate?

Formulation of the Research Plan

- Are the program's goals and objectives clearly defined and consistent with relevant documents such as NASA's Strategic Plan?
- Is there evidence of a clear understanding of the need by NASA's enterprises, other organizations (e.g., the FAA, DOD, etc.), or the aerospace community at large for the R&D or analysis and the potential benefits? Are the program's deliverables to those organizations clearly articulated, and are those organizations adequately involved in the planning and review process?
- Can the expected benefits be accomplished by the proposed research? If not, is the path for adequately maturing the research clear? Is this planning well supported by sufficient decision points, downselects, customer agreements, and/or unallocated outyear funding?
- Are there sufficient near-term deliverables or progress metrics by which the program can be regularly assessed? Are there sufficient off-ramps or sunsets to ensure that funding is reallocated within the program or to other programs if the program does not make adequate progress toward one or more of its goals and objectives? Are the program's plans for independent and/or external reviews adequate and appropriate?
- Are appropriate scientific and technical objectives being posed, taking into consideration program goals, NASA's strengths, and the time horizon for the project? Are the critical personnel and facilities required to support the program well defined?

Connections to the Broader Community

- What programs or program elements should be performed in-house at NASA and be exempt from competition with industry or academia?
- Is there evidence that the research plan for the area under review reflects a broad understanding of the underlying science and technology and of comparable work within other NASA units as well as industry, academia, and other federal laboratories?
- Is there evidence that the research builds appropriately on work already done elsewhere? Does it leverage the work of leaders in the field? Is the strategy for out-of-house work (competitions, partnerships, etc.) well chosen and managed?
- Is the research being accomplished with a proper mix of personnel from NASA, academia, industry, and other government agencies? Is the program using high-quality research performers, or is there untapped talent outside the program that can be brought to bear?

Methodology

- How well crafted are the research plans for the areas under review? In general, is the use of laboratory experiment, modeling, simulation, and/or field testing appropriate? How well are these methods integrated?
- Have the appropriate supporting system-level assessments been conducted?
- Do both the researchers and managers understand and manage the risks involved to an appropriate level?
- Are the plans for further study reasonable and justifiable?

Overall Capabilities

- Is the scientific or engineering quality of the work (including work performed in academia and industry) comparable to similar world-class efforts at other institutions, and is it appropriate for the goal?
- Are the qualifications of the scientific and engineering staff (including researchers in academia and industry) sufficient to achieve program goals?

- Are the capabilities, quantity, and state of readiness of equipment and facilities sufficient to achieve program goals?
- Are personnel, equipment, and facilities supplied by support contractors used efficiently? Do they fill gaps in government capabilities without duplication?

The selection of criteria for each assessment and the relative weights given to each criterion are within a panel's discretion and can vary from program to program. Neither the committee nor the panels will make explicit budget recommendations to NASA but will instead comment on program content, gaps in technology, and other issues outlined above.

B

Committee and Panel Members Biographies

COMMITTEE FOR THE REVIEW OF NASA'S PIONEERING REVOLUTIONARY TECHNOLOGY (PRT) PROGRAM

RAYMOND S. COLLADAY, *Chair*, is a senior industry executive with extensive experience and demonstrated success in exploiting technology leverage, structuring international alliances, fostering creativity and innovation, and motivating organizations to achieve goals. Dr. Colladay is a retired corporate officer of Lockheed Martin Corporation and formerly held positions as president of Lockheed Martin Astronautics, agency director of DARPA, and associate administrator for aeronautics and space technology at NASA. Currently he serves as president and CEO of RC Space Enterprises, Inc., in Colorado and as a senior associate of Burdeshaw & Associates, Ltd., in the Washington, D.C., area, providing management support, business strategy, and technical services to client organizations. Dr. Colladay is a fellow of the American Institute of Aeronautics and Astronautics (AIAA) and of the American Astronautical Society. He has also served in various capacities on several NRC panels and boards, including chair of the Committee on Advanced Space Technology and member of the Committee on the Space Station, the Aeronautics and Space Engineering Board, and, most recently, the Committee on the Future of the U.S. Aerospace Infrastructure and Aerospace Engineering Disciplines to Meet the Needs of the Air Force and Department of Defense.

BENJAMIN BUCHBINDER has more than 40 years of experience in the development and application of risk assessment methods, in the use of quantitative methods to support management decision making related to safety and programmatic risk, and in the communication of risk assessment results. While working in reliability analysis for the General Electric Company on the Apollo program at Daytona Beach in the 1960s, he served as an adjunct assistant professor at the University of Florida, teaching graduate-level courses in probability and statistical methods. At the National Bureau of Standards (now NIST) Center for Fire Research, he applied decision analysis and risk assessment methods to the analysis of fire risk and the development of fire safety standards. He was chief, Methodology and Data Branch, in the Office of Research of the Nuclear Regulatory Commission (NRC). The NRC developed the modern approach to the probabilistic assessment of risk in the operation of engineered systems. As manager of the Risk Management Program, Office of Safety and Mission Assurance, NASA Headquarters, he pioneered NASA's probabilistic approach to risk assessment in the post-Challenger era. He directed the initial probabilistic risk assessments for the space shuttle and presented workshops on risk assessment processes and consulted on risk assessment methods for NASA programs at most of the field centers. In 1994, he joined the Futron Corporation, where he was responsible for business development and project management in probabilistic risk assessment and programmatic risk management until 1997.

LEONARD H. CAVENY, an aerospace consultant, retired in 1997 from the Ballistic Missile Defense Organization (BMDO), Science and Technology Directorate, where he had served as director since August 1995. While in the Strategic Defense Initiative Organization (SDIO, which later became BMDO) from 1985 to 1997, Dr. Caveny initiated and managed fundamental research and development of high-risk technology. He was the program manger for four space flight experiments on solar power, electric propulsion, and UV signatures. In 1984 and 1985, Dr. Caveny was a staff specialist in the Office of the Deputy Undersecretary for Research and Advanced Technology, at the Pentagon. Between 1980 and 1984, he was program manager for energy conversion in the Air Force Office of Scientific Research (AFOSR), Aerospace Sciences Directorate, Washington, D.C., where he managed the basic research programs on space propulsion, rocketry, and reacting flow diagnostics. Between 1969 and 1980, he was a senior member of the professional staff in the Department of Aerospace and Mechanical Sciences, Princeton University. Dr. Caveny's areas of expertise include space propulsion and power, high-temperature materials, sensors, and space systems. Dr. Caveny served as chair of the National Research Council Panel to Review Air Force Office of Scientific Research (AFOSR) Proposals in Propulsion in 2003. As an aerospace consultant, his present involvements include advanced solid rocket propulsion systems, electric propulsion, energetic materials, combustion, and ignition in high-speed flows. He has authored over 50 refereed technical articles and received 11 U.S. patents. He is a fellow of AIAA and recipient of the AIAA Wyld Propulsion Medal.

SERGIO GUARRO is the director of the Risk Planning and Assessment Office at the Aerospace Corporation. The office provides technical expertise and assistance to a broad spectrum of Air Force and NASA launch vehicle and spacecraft programs in the areas of probabilistic risk assessment, risk management, and reliability engineering. Dr. Guarro has 30 years of professional engineering experience, with more than 20 years spent on both the research and application sides of risk assessment, systems logic modeling, fault diagnosis, and reliability engineering. Before joining the Aerospace Corporation, Dr. Guarro was a project leader with the Nuclear Systems Safety Program at the Lawrence Livermore National Laboratory. During his career, he has had appointments and recognition in academia, as an international Fulbright scholar at the University of California, Berkeley, and a research associate and lecturer at the University of California, Los Angeles, and also in government institutions, as a fellow of the Advisory Committee on Reactor Safeguards of the U.S. Nuclear Regulatory Commission. In the last 13 years he has been specifically working in space systems risk management and mission assurance, making key contributions to major programs and missions such as the NASA Cassini mission and the U.S. Air Force Titan launch vehicle and Evolved Expendable Launch Vehicle (EELV) programs.

DAVID J. KASIK is a Boeing technical fellow. He is the geometry and visualization architect for Boeing Commercial Airlines. His responsibility extends throughout the commercial airplane design, build, and maintenance processes. He is currently bringing to the factory applications based on wireless connectivity. Recently, he acted as the technical architect for the Single Glass project. Single Glass is a unified system that makes over 1,000 applications available to 6,000 workstations in the Puget Sound area. The project required carefully designed system architecture to ensure scalability and extensibility. As the geometry and visualization architect, he routinely capitalizes on his user interface and graphics background. He has been a key developer of the underlying technology needed for 3D graphics and to improve human-computer dialogue sequences through User Interface Management Systems. In his role as exposition chair for advanced technology exhibits for the Association for Computing Machinery (ACM) and the Special Interest Group in Computer Graphics (SIGGRAPH), he has become familiar with a broad range of innovative computing technology. He is the chair of the Battelle Pacific Northwest National Laboratories Information Technology Peer Review Committee and a member of the Technical Advisory Board of the Fraunhofer Center for Research in Computer Graphics. He received a bachelor's degree in quantitative studies from Johns Hopkins University and a master's degree in computer science from the University of Colorado.

DIMITRI MAVRIS is Boeing Associate Professor for Advanced Aerospace Systems Analysis, codirector of NASA's University Research Engineering Technology Institute (URETI) in Aeropropulsion and Power and director of General Electric Aircraft Engine's University Strategic Alliance (USA) Center for Robust Design Methods and Optimization. He received his doctorate from Georgia Institute of Technology in 1988 in the field of aerospace engineering, joining the faculty of the School of Aerospace Engineering in 1995. Dr. Mavris pioneered the Robust Design Simulation methodology as well as a method for technology impact forecasting (TIF), which led to the development of the technology identification, evaluation, and selection (TIES) methodology. All of these methodologies support decision making for complex system design. In 1998, Dr. Mavris worked with Boeing under the Boeing A.D. Welliver Faculty Fellowship. He has 228 refereed and conference publications and serves on many national boards and committees, including as deputy director for AIAA's Aircraft Technology, Integration and Operations Group, chair of the AIAA's Aircraft Design Technical Committee, editor of the International Society of Parametric Analysts' *Journal of Parametrics*, and member of AIAA's Air Transportation and Operation Technology Committee and its Missile Systems Technical Committee. Dr. Mavris has several significant accomplishments in the area of multidisciplinary design, particularly in advanced probabilistic design methodology. The bulk of this work is focused on finding ways to account for uncertainty in the design process and to produce robust designs that are insensitive to changes in the design and/or operational environment. Dr. Mavris is currently a principal or coprincipal investigator of 24 grants worth approximately $13.52 million.

DENNIS K. McBRIDE is president of the Potomac Institute for Policy Studies, a Washington think tank specializing in science and technology policy. Currently serving also as vice president for research (acting), Dr. McBride continues to lead nationally focused technical programs, including significant support to DARPA, the Office of Naval Research, the National Science Foundation, the National Academy of Sciences, and private industry. Dr. McBride is also an adjunct research professor at the Krasnow Institute for Advanced Study, George Mason University. He served previously as executive director, Institute for Simulation and Training, University of Central Florida, and professor (with dual appointments in the Department of Industrial Engineering and Management Systems, College of Engineering and Computer Science, and in the Department of Psychology, College of Arts and Sciences). Dr. McBride completed a 20-year career at the grade of captain, Medical Service Corps, as a naval aerospace experimental psychologist and flight test engineer. Captain McBride served at six Navy laboratories, as program officer for biomedical S&T at the Office of Naval Research, and as program manager (simulation technology) at the Defense Advanced Projects Research Agency. Dr. McBride's formal education includes enrollment at the University of Georgia, University of Southern California, and the London School of Economics. He earned a Ph.D., three master of science degrees, and an MPA. Dr. McBride was a summer scholar at the Santa Fe Institute. He holds professional credentials from the Board of Certification in Professional Ergonomics and in Professional Modeling and Simulation. Dr. McBride is vice president of the Policy Studies Organization, editor of the *Review of Policy Research*, and is a member of editorial boards, including the *International Journal for Human Computer Interaction* and *Human Nature Review,* as associate editor. Dr. McBride has received numerous awards and military decorations, including the Defense Superior Service Medal and the Legion of Merit. Among his civilian awards is the L.P. Coombes Medal, presented by the Australian Institution of Engineers. He has published and presented more than 125 scientific papers, technical reports, and book chapters in the fields of psychobiology, experimental psychology, medical and pharmacological research, engineering science, operations research, complexity science, political science, economics, and public policy.

TODD J. MOSHER is an assistant professor at Utah State University (USU) in the mechanical and aerospace engineering department. His research is in small satellites and payloads, advanced space system concepts, and new design methodologies. He teaches courses in astrodynamics, propulsion, and space system design. He joined USU in 2002 after serving as the associate director of the Space Architecture Department at the Aerospace Corporation. He has 14 years of experience in space systems analysis, especially NASA science missions and space transportation. Dr. Mosher originated and led the development of a lunar Discovery proposal, where he was responsible for both the mission architecture and the organization of the government, industry, and academia team. He has participated in evaluation of many of NASA's programs, including Mars Scout, Mars Sample Return, Discovery, New Millennium, Medium Explorers (MIDEX), Europa Orbiter, Pluto Kuiper Belt mission, Earth Science System Pathfinder (ESSP), and Small Explorers (SMEX). Dr. Mosher has also supported launch programs such as the Advanced Launch System, Atlas, National Launch System, Space Shuttle, and Titan. He also led many of Aerospace's efforts in small spacecraft design and was instrumental in creating new design and cost models reflecting the latest changes in technology and management in the design of these spacecraft. He also served as an instructor at UCLA, teaching the courses Introduction to Space Technology, Spacecraft Design, and Space Hardware Design.

JAMES ODOM recently retired as deputy group manager and senior vice president of the Science Applications International Corporation (SAIC), Inc., Huntsville Group. He currently works part-time with SAIC as a consulting employee. Before coming to SAIC in 1994, he was president and CEO of Applied Research for 5 years, immediately after retiring from NASA. Mr. Odom served at NASA for 33 years and was directly involved in several of NASA's major projects, including the Hubble Space Telescope, the space shuttle, the Apollo program, and the space station Freedom. Mr. Odom's work with NASA began in an engineering position with the U.S. Army's rocket research and development team at Redstone Arsenal; he was later transferred to Marshall Space Flight Center. At Marshall, Mr. Odom was actively involved in the development of early satellites, unmanned space probes, launch vehicles, and propulsion systems. He was then assigned to lead the engineering design and testing for the second stage of the Saturn V/Apollo lunar launch vehicle. Thereafter he was selected to direct the development of the space shuttle's external tank during its initial design phase and saw it through its first six launches. Mr. Odom also played a major role in the development of the Hubble Space Telescope. He capped his NASA career as associate administrator for the space station Freedom.

LEE D. PETERSON is associate professor of aerospace engineering sciences at the University of Colorado, Boulder. He has been an associate professor or assistant professor at the University of Colorado since 1991. Dr. Peterson is also director of the McDonnell-Douglas Aerospace Structural Dynamics and Control Laboratory and is a member of the Center for Aerospace Structures. From 1989 to 1991 Dr. Peterson was assistant professor of aeronautics and astronautics at Purdue University. Prior to his work at Purdue, Dr. Peterson was a member of the technical staff at Sandia National Laboratories, Albuquerque, New Mexico. He obtained his S.B. (1982), S.M. (1983), and Ph.D. (1987) in aeronautics and astronautics from the Massachusetts Institute of Technology. He has authored or coauthored over 100 publications in space structure mechanics, dynamics, control, and design. His research interests are in the development of large, lightweight precision space structures for optical telescopes and interferometers. This includes experimental and theoretical research in the stability of structures and structural components at nanometer scales of deformation.

JOSEPH B. REAGAN (NAE), an independent consultant, is retired vice president and general manager of research and development at Lockheed Martin Missiles and Space and was a corporate officer of the Lockheed Martin Corporation. Dr. Reagan, a member of the NAE, has a strong background in defense technology development, particularly in optics, electro-optics, information software, guidance and control, electronics, and materials. Dr. Reagan joined Lockheed as a scientist, where he led the Space Instrumentation Group for 10 years and was responsible for the development and on-orbit deployment of over 20 scientific payloads for NASA and the DOD. His research interests included space sensors, radiation belt and solar particles, nuclear weapon effects, and the effects of radiation particles on spacecraft systems. As general manager of the R&D Division, he led over 750 scientists and engineers in the development of advanced technologies in optics, electro-optics, information software, cryogenics, guidance and controls, electronics, and materials. Today, Dr. Reagan is chairman of the board of Southwall Technologies, Incorporated, a high-technology company specializing in the manufacturing of thin-film coatings for high-performance residential, industrial, and automotive windows. He is also a director on the board of the Tech Museum of Innovation, where he is the chairman of the Exhibits Committee. He is involved in numerous activities that foster the improvement of science and mathematics education. Dr. Reagan is currently vice chair of the Naval Studies Board.

CYNTHIA R. SAMUELSON is a senior fellow and program manager at the Logistics Management Institute. In this role, she serves as researcher and technical advisor for Information Technology studies and analyses for federal organizations. She came to LMI following retirement as principal director for information management at the Department of Defense and 3 years in private industry. In total, she has more than 20 years of experience in leading and managing complex public and private organizations responsible for providing information technology services. In recognition of her work, she has received numerous awards, including the Secretary of Defense Medals for Meritorious Civilian Service and Exceptional Civilian Service and the Department of Transportation's Bronze Medal.

MARC SNIR has been the Michael Faiman and Saburo Muroga Professor of Computer Science and head of the Computer Science Department at the University of Illinois at Urbana-Champaign since the fall of 2001. Until August 2001 he was a senior manager at the IBM T.J. Watson Research Center, where he initiated and led the IBM Blue Gene project. Previously, he led the Scalable Parallel Systems research group and was responsible for major contributions to the IBM SP scalable parallel system: architecture, parallel operating environment, message-passing libraries, tools, parallel file system, parallel algorithms, and applications. Dr. Snir is an ACM fellow and IEEE fellow. He is on the editorial board of *Parallel Processing Letters* and *ACM Computing Surveys* and serves as co-chair of the NRC's Committee to Study the Future of Supercomputing.

MICHAEL J. ZYDA is the director of the Modeling, Virtual Environments and Simulation (MOVES) Institute, located at the Naval Postgraduate School (NPS), Monterey, California. He is also a professor in the Department of Computer Science at NPS. Professor Zyda's research interests include computer graphics, large-scale, networked 3D virtual environments, agent-based simulation, modeling human and organizational behavior, interactive computer-generated story, computer-generated characters, video production, entertainment/defense collaboration, and modeling and simulation. He is the principal investigator of the America's Army PC game funded by the Assistant Secretary of the Army for Manpower and Reserve Affairs. Professor Zyda was a member of the National Research Council's Committee on Virtual Reality Scientific and Technological Challenges and was the chair of the National Research Council's Computer Science and Telecommunications Board Committee on Modeling and Simulation: Linking Entertainment and Defense. From that report for the Deputy Assistant Secretary of the Army for Research and Technology, Professor Zyda drafted the operating plan and research agenda for the USC Institute for Creative Technologies (ICT). Professor Zyda began his career in computer graphics in 1973 as part of an undergraduate research group, the Senses Bureau, at the University of California, San Diego. Professor Zyda received a B.A. in bioengineering from the University of California, San Diego, in La Jolla in 1976, an M.S. in computer science from the University of Massachusetts, Amherst, in 1978, and a D.Sc. in computer science from Washington University, St. Louis, in 1984.

PANEL ON COMPUTING, INFORMATION, AND COMMUNICATIONS TECHNOLOGY

MICHAEL J. ZYDA, *Panel Chair* (see biography above)

WILLIAM COHEN was a senior research scientist with the Center for Automated Learning and Discovery at Carnegie Mellon University. He received his bachelor's degree in computer science from Duke University in 1984 and a Ph.D. in computer science from Rutgers University in 1990. From 1990 to 2000 Dr. Cohen worked at AT&T Bell Labs and AT&T Labs-Research. Dr. Cohen is currently an associate editor for the journal *Machine Learning*, has served as the action editor for the *Journal of Artificial Intelligence Research*, co-organized the 1994 International Machine Learning Conference, and has served on more than 20 program committees or advisory committees. Dr. Cohen's research interests include information integration and machine learning, particularly text categorization and learning from large data sets. He holds four patents in these areas and is the author of more than 50 refereed publications.

DELORES M. ETTER (NAE) joined the electrical engineering faculty at the United States Naval Academy on August 1, 2001, as the first recipient of the Office of Naval Research Distinguished Chair in Science and Technology. From June 1998 through July 2001, Dr. Etter served as the Deputy Under Secretary of Defense for Science and Technology. In that position she was responsible for the Defense Science and Technology strategic planning, budget allocation, and program execution and evaluation for the $9 billion per year DOD science and technology program. She was also responsible for the Defense Modeling and Simulation Organization, the DOD High Performance Computing Modernization Office, and for technical oversight of the

Software Engineering Institute, a federally funded research and development center (FFRDC). Prior to that she was a tenured professor in electrical/computer engineering at the University of Colorado for 9 years and at the University of New Mexico for 10 years. She also spent a year at Stanford University as a visiting professor in the Information Systems Laboratory of the Electrical Engineering Department. Her academic background includes experience as associate vice president for academic affairs at the University of New Mexico. She received a Ph.D. in electrical engineering from the University of New Mexico in 1979 and M.S. and B.S. degrees in mathematics from Wright State University in 1972 and 1970, respectively.

MARY JEAN HARROLD is the NSF ADVANCE Professor of Computing in the College of Computing at Georgia Institute of Technology, where she is a member of the Center for Experimental Research in Computer Systems (CERCS). Her research to date has involved program-analysis-based software engineering, with an emphasis on regression testing, analysis and testing of imperative and object-oriented software, and development of software tools. Her research is funded by the National Science Foundation, under several of its programs, and by industry, with which she has worked extensively to improve the processes by which software is developed and maintained. She received the National Science Foundation's National Young Investigator Award for her work in testing and analysis of object-oriented software and the 1998 College of Engineering Annual Research Award while at Ohio State University. She serves on editorial boards for *IEEE Transactions on Software Engineering* and *ACM Transactions on Programming Languages and Systems*. She served as program co-chair for the 23rd International Conference on Software Engineering 2001 and as program chair for the ACM SIGSOFT International Symposium on Software Testing and Analysis 2000 and for the IEEE International Conference on Software Maintenance 1997. Dr. Harrold currently serves as vice chair of ACM SIGSOFT and co-chair of the Computing Research Association's Committee on the Status of Women in Computing (CRA-W).

CHANDRIKA KAMATH is a computer scientist at the Center for Applied Scientific Computing at Lawrence Livermore National Laboratory, where she has led the Sapphire project in large-scale scientific data mining since 1998. Specifically, her research investigates the practical applications of large-scale data mining and pattern recognition, image processing, feature extraction, dimension reduction, and classification and clustering algorithms. Prior to joining LLNL in 1997, Dr. Kamath was a consulting software engineer at Digital Equipment Corporation (DEC), developing high-performance mathematical software for DEC Alpha systems. She was responsible for the design, implementation, optimization, and parallelization of the sparse linear system solvers in the Digital Extended Math Library (DXML). Dr. Kamath earned her Ph.D. in 1986 and her M.S. in 1984, both in computer science, from the University of Illinois at Urbana-Champaign. She has filed six patents in data mining and co-edited the book *Data Mining for Scientific and Engineering Applications,* which was published in 2001.

DAVID J. KASIK (see biography above)

ALFRED U. MacRAE (NAE) is president of MacRae Technologies. He is a consultant on communications satellite technology and systems and telecommunications equipment for customers that include satellite manufacturers, satellite system operators, communications equipment developers, and investment bankers. Before this, he was director of AT&T Skynet Satellite Communications Laboratory, with responsibility for AT&T satellite technology, including satellite service development, satellite ground equipment development, satellite design and development, and oversight of satellite manufacture, test, launch, and operations. Prior to the satellite responsibility, he was director of the Advanced Integrated Circuit Laboratory at Bell Labs, with responsibility for the development of SIC fabrication technology and circuit design and their transfer into manufacturing. Honors include election as member of the National Academy of Engineering; fellow, American Physical Society; fellow, Institute of Electrical and Electronic Engineers; Scientific Member, Böhmische Physicalische Society; 1994 IEEE J.J. Ebers Award for contributions to integrated circuit technology; over 50 papers published in refereed technical journals; over 100 talks at professional society meetings and universities; and 18 patents, including a high-revenue-generating patent that was singled out for special AT&T recognition. He serves on several IEEE committees as well as on the Executive Committee of the Electron Devices Society.

DUANE T. McRUER (NAE) is concurrently an independent consultant and chairman of Systems Technology, Inc. (STI). He received his undergraduate and graduate education at the California Institute of Technology. Since 1950, his research has focused on aerospace and ground vehicle and human pilot dynamics, automatic and manual vehicular control, and vehicle flying/handling qualities. He has published more than 125 technical papers and seven books, including *Analysis of Nonlinear Control Systems* (Wiley, 1961; Dover, 1971) and *Aircraft Dynamics and Automatic Control* (Princeton, 1973). He has also been involved with applications of these topics in more than 50 aerospace and land vehicles, and he has five patents on flight control and stability augmentation systems. Besides a career as president and technical director of STI (until 1993), he has been Regent's Lecturer at the University of California, Santa Barbara, and was the 1992-1993 Hunsaker Professor at the Massachusetts Institute of Technology (MIT). His past service for various governmental and professional societies includes terms as president of the American Automatic Control Council and chairman of the National Research Council Aeronautics and Space Engineering Board, the American Institute of Aeronautics and Astronautics (AIAA) Technical Committee on Guidance and Control, and the Society of Automotive Engineers (SAE) Aerospace Control and Guidance Systems Committee. He was a long-time member of the National Aeronautics and Space Administration (NASA) Advisory Council. He is an honorary fellow of the AIAA and a fellow of the Institute of Electrical and Electronic Engineers (IEEE), SAE, and the Human Factors and Ergonomics Society and a member of the National Academy of Engineering. Other honors include the Caltech Distinguished Alumni Award, the NASA Distinguished Public Service Medal, the AIAA Mechanics and Control of Flight Award, the Franklin Institute's Levy Medal, and the Human Factors and Ergonomics Society's Alexander Williams Award.

RICHARD MULLER (NAE) joined the EECS faculty at the University of California, Berkeley, in 1962. His initial research and teaching on the physics of integrated-circuit devices led to collaboration with T.I. Kamins of Hewlett-Packard Laboratories in writing *Device Electronics for Integrated Circuits.* Dr. Muller changed his research focus in the late 1970s to the general area now known as microelectromechanical systems (MEMS) and he joined in 1986 with colleague Richard M. White to found the Berkeley Sensor and Actuator Center (BSAC). Dr. Muller has been awarded NATO and Fulbright research fellowships; an Alexander von Humboldt Senior-Scientist Award; the University of California Berkeley Citation (1994); Stevens Institute of Technology Renaissance Award (1995); the Transducers Research Conference Career Achievement Award (1997), the IEEE Cledo Brunetti Award (with Roger T. Howe, 1998) and an IEEE Millennium Medal (2000). He is a member of the National Academy of Engineering, a life fellow of the IEEE, an IEEE Distinguished Lecturer, IEEE/ASME *Journal of Microelectromechanical Systems* editor in chief (since 1998), a trustee of the Stevens Institute of Technology, past member of the NRC National Materials Advisory Board, and on the board of the Transducers Research Foundation. He is the author or coauthor of more than 200 technical papers and of 16 patents.

CYNTHIA R. SAMUELSON (see biography above)

JUDE SHAVLIK is a professor in the Department of Computer Science and the Department of Biostatistics and Medical Informatics at the University of Wisconsin-Madison. His research centers on developing machine learning systems within the artificial intelligence field, with a primary focus on applications in computational biology. Dr. Shavlik has organized or participated as a panel member at numerous artificial intelligence conferences and is widely published in the field. He is a member of the board of directors of the International Machine Learning Society. He received his Ph.D. in computer science from the University of Illinois in 1988 and his master's in molecular biophysics and biochemistry from Yale University in 1980. He has held positions at either the University of Illinois or the University of Wisconsin since 1983. He served as a member of the technical staff of MITRE Corporation from 1980 to 1982. Dr. Shavlik's current funding is from the National Library of Medicine, to investigate adaptive information monitoring and extraction, and from DARPA, to study pattern discovery in richly interconnected data sources.

SANDEEP SINGHAL, CTO, chief architect and cofounder of ReefEdge, Inc., is a recognized expert in the mobile intranet and internet, handheld computing, and distributed systems. Currently, Dr. Singhal serves as chief product architect, responsible for the company's technical strategy. Prior to cofounding ReefEdge, he was chief architect for IBM's Pervasive Computing Division, with responsibility for IBM's suite of mobile connectivity, middleware, server, and application products for enterprise and carrier customers. He previously served as a researcher in IBM's T.J. Watson Research Center and as a software engineer at NASA. Dr. Singhal's credits include 27 issued patents and dozens of publications, including two books and a featured contribution to the recently published book *Wireless Local Area Networks—The New Wireless Revolution*. He is active in various standards organizations, including the Internet Engineering Task Force (IETF), IEEE, and World Wide Web Consortium (W3C). He holds M.S. and Ph.D. degrees in computer science from Stanford University, as well as B.S. degrees in computer science and mathematical sciences and a B.A. in mathematics from Johns Hopkins University. He is an adjunct professor at North Carolina State University.

MARC SNIR (see biography above)

PANEL ON ENGINEERING FOR COMPLEX SYSTEMS

DENNIS K. McBRIDE, *Panel Chair* (see biography above)

TORA K. BIKSON, a senior behavioral scientist at RAND Corporation since 1976, is recognized for her research on the introduction of advanced communication and information technologies and their effects in varied contexts of use. She recently completed a project to define organizational needs and identify best practices for creating, managing, and distributing digital documents (including compound, multimedia, and interactive documents) among United Nations organizations based in Europe, North America, and South America. In previous projects for clients, including the National Science Foundation, the World Bank, the Organization for Economic Cooperation and Development, the Markle Foundation, and others, she addressed such issues as the factors that affect the successful transfer and implementation of new technologies in ongoing communities of practice, how innovations influence intra- and interorganizational structures and processes, their impact on task performance and social outcomes, and their policy implications. Dr. Bikson has coauthored four recent books addressing such issues: *Sending Your Government a Message: E-mail Communication Between Citizens and Government* (RAND, Santa Monica, 1999); *Teams and Technology* (Harvard Business School Press, Boston, 1996); *Universal Access to E-mail: Feasibility and Societal Implications* (RAND, Santa Monica, 1995); and *Preserving the Present* (Sdu Publishers, The Hague, 1993). She holds Ph.D. degrees in philosophy (University of Missouri) and psychology (UCLA). She has chaired RAND's Institutional Review Board since 1986. Dr. Bikson has also served on special task forces, panels, and planning committees concerned with digital information and communication media for the National Academy of Engineering, the National Academy of Sciences, the National Academy of Public Administration, and the Social Science Research Council.

BENJAMIN BUCHBINDER (see biography above)

PHILIP R. COHEN is professor and codirector of the Center for Human-Computer Communication at the Department of Computer Science and Engineering, Oregon Health and Science University and the Oregon Graduate Institute's School of Science and Engineering. Dr. Cohen specializes in multimodal human-machine interfaces and multiagent systems. His recent projects include multimodal interaction for the command post of the future and robust agent-based systems incorporating teams of communicating agents, both sponsored by DARPA, and multimodal interaction for virtual environments and augmented reality, sponsored by ONR. He recently served on the DARPA ISAT study panel on RAP teams (teams of robots, agents, and people). He received his Ph.D. in computer science from the University of Toronto in 1978 and has been a staff research scientist at Bolt Bernanek and Newman, the Fairchild Laboratory for Artificial Intelligence, and SRI International. Dr. Cohen is a fellow of the American Association for Artificial Intelligence and is a past president of the Association for Computational Linguistics.

SERGIO GUARRO (see biography above)

MYRON HECHT is cofounder and president of SoHaR, a research, development, and consulting firm specializing in computer dependability. He has experience in software and systems reliability and in software fault tolerance. His activities in basic research and development at SoHaR have resulted in new architectures for real-time distributed systems, methodologies for the development and verification of fault-tolerant software, and designs for fault-tolerant distributed systems. Mr. Hecht recently headed efforts for the Nuclear Regulatory Commission on programming guidelines for high-level languages in safety-critical systems and requirements for safety-critical systems. Mr. Hecht currently manages SoHaR's support of the Federal Aviation Administration Office of Air Traffic Maintenance in the analysis of reliability and outage data. He holds a B.S in chemistry, an M.S. in engineering, an M.B.A with a specialty in information systems, and a J.D. degree, all from the University of California, Los Angeles.

JAMES LARUS, a senior researcher at Microsoft Research, leads the Software Productivity Tools research group. His previous research applied programming language and compiler technology to a wide range of problems, most notably efficient program measurement, parallel programming, and fine-grain distributed shared memory. He is now working on applying these technologies to improve software development. His group's research goal is to develop and demonstrate new tools for program design, coding, debugging, and testing that fundamentally improve software development. Prior to joining Microsoft, Dr. Larus was an associate professor in the Computer Sciences Department at the University of Wisconsin-Madison. There, he co-led the NSF and DARPA-funded Wisconsin Wind Tunnel research project, which investigated the design and programming of shared-memory parallel computers. Dr. Larus's master's and Ph.D. were from the University of California, Berkeley.

DIMITRI MAVRIS (see biography above)

RONALD WESTRUM is a professor at Eastern Michigan University with a dual appointment in sociology and interdisciplinary technology. Dr. Westrum specializes in organizational dynamics, technological accidents, and safety, with an emphasis on sociology of science and technology, creativity and invention, and anomalous events. He has been an invited speaker internationally on organizational dynamics in the aviation field and related topics at the Sorbonne in Paris, the U.S. Naval War College, NATO Advanced Research Institutes, FAA- and NTSB-sponsored seminars, and the World Bank seminar on Systems Safety. In the past, he was invited to be one of two keynote speakers at the United Nations International Civil Aviation Organization Regional Seminar in Addis Ababa, Ethiopia. Ron Westrum is also the author of a book on the Sidewinder development team and the creative culture of the China Lake Naval Air Weapons Center.

F. GORDON WILLIS is the founder and president of Vulcan Works, LLC, which specializes in the implementation of high-performance software systems that dramatically reduce time and cost of product development. Prior to founding Vulcan Works, he worked at Ford Motor Company from 1976 until 1999. During his time at Ford, he served as chief engineer for a number of different departments within Ford Motor Company, including automatic transmission engineering, vehicle engineering at the Small and Medium Vehicle Center, and automotive chassis engineering, and as the director for Product and Manufacturing Systems for car product development. Mr. Willis served on the NRC Advanced Engineering Environments committee that evaluated NASA programs in computing and collaboration.

PANEL ON ENABLING CONCEPTS AND TECHNOLOGIES

LEE D. PETERSON, *Panel Chair* (see biography above)

CLINTON A. (ANDY) BOYE is the deputy director of national space programs for Sandia National Laboratories, in the Center for Monitoring Systems and Technology. As the program manager, he is responsible to customers in the national space community. For the past 18 years, he has been involved in the development of space-based electro-optical and radio-frequency systems for remote sensing and communications, systems research in the remote detection and characterization of laser systems, and research on the optical spectra of terrestrial lightning. Prior to joining Sandia, Mr. Boye enjoyed a 10-year career in the U.S. Air Force, working in the areas of electronic countermeasures (ECM) and high-energy laser beam propagation, adaptive optics, and laser antisatellite (ASAT) systems. Mr. Boye is a member of the Optical Society of America, the Society for Photo-optical Instrumentation Engineers (SPIE), and the American Society for Photogrammetry and Remote Sensing (ASPRS).

LEONARD H. CAVENY (see biography above)

STANLEY V. GUNN capped an almost 40-year career at Rocketdyne as program manager for advanced programs in both nuclear thermal rocket propulsion and free electron lasers. Dr. Gunn began his career at Rocketdyne in the aerophysics laboratory and worked through positions of increasing responsibility, serving as program manger for high-energy lasers, gas dynamic lasers, and nuclear propulsion projects. During this period he also evaluated potential applications for advanced launch and space propulsion systems, including advanced chemical rocket propulsion, nuclear rocket propulsion, electric propulsion, and photon propulsion. Lt. Gunn served in the U.S. Army before receiving his B.S. in mechanical engineering from Michigan State University in 1947. Dr. Gunn also received M.S. and Ph.D. degrees in mechanical engineering from Purdue University in 1949 and 1953, respectively. He worked for General Electric in guided missiles before joining North American Aviation (now Rocketdyne) in 1953. Dr. Gunn has been awarded various national awards for his work in nuclear thermal propulsion and his role in the Saturn/Apollo rocket system.

ANTHONY K. HYDER is associate vice president for graduate studies and research and professor of physics at the University of Notre Dame. From 1991 to 1993, he served as associate vice president for research while he continued his teaching responsibilities as a professor of aerospace engineering at Notre Dame. His responsibilities included development and administration of the research activities of the university; strategic planning and formulation of university policy related to research, industrial activities, and research compliance issues; the evaluation of research quality and infrastructure; decisions related to the commitment of university resources to research activities; and representing the university on research and associated graduate-studies matters. From 1991 to 1995, Dr. Hyder also worked as a research fellow for the Space Power Institute at Auburn University. During this period, he completed several research-related activities under way at the time of accepting the appointment at Notre Dame, including editing a book on the nature of the space environment; advising graduate students; and investigating space applications of advanced batteries and fuel cells, radioisotope thermoelectric generators, and high-power microwave tubes. Dr. Hyder also served as associate vice president for research from 1984 to 1991 and was founding director of both the Center for Advanced Technologies and the Space Power Institute while a professor at Auburn University. Since arriving at Notre Dame, he has authored a text on spacecraft power technologies and has edited books on defense conversion strategies and multisensor fusion. Dr. Hyder will also be able to provide expertise in the area of space environments, sensors, and spacecraft power systems.

DIMITRIS C. LAGOUDAS is the Ford Professor of Aerospace Engineering at Texas A&M University, College Station. He is currently serving as director for the Texas Institute for Intelligent Bio-Nano Materials and Structures for Aerospace Vehicles (TiiMS), chair of the Executive Committee of the Faculty of Materials Science and Engineering, and as an associate vice president for research. Lagoudas's educational background includes his diploma in mechanical engineering in 1982 from Aristotle University of Thessaloniki, Greece, and his Ph.D. in applied mathematics in 1986 from Lehigh University. Lagoudas's research interests include active materials and smart structures, theories of defects in solids, micromechanics of composite materials, damage mechanics, and constitutive modeling and applications of shape memory alloys (SMAs). He has published more than 200 papers (more than 90 in archival journals). Lagoudas is the recipient of the Lockheed Excellence in Engineering Teaching Award, the Neely '52 Dow Chemical Faculty Fellow Award, TEES Senior Research Fellow, and the most prestigious Texas A&M Faculty Fellow Award. He is in *Who's Who in America* and *Who's Who in Science and Engineering*. He was selected as the inaugural recipient of one of the two Ford Motor Company professorships and is an associate fellow of AIAA and fellow of ASME.

TODD J. MOSHER (see biography above)

JAY S. PEARLMAN is chief of science and applications for the Advanced Network Centric Operations Systems in the Phantom Works organization of Boeing. His background includes basic research, program management, and program development in sensors and systems. He has played an important role in the development and implementation of new concepts and capabilities for both the military and the civil sectors of the U.S. government. At Boeing, he is working on the Advanced Landsat System as chief scientist and is also developing network-centric applications for government applications. Dr. Pearlman remains active in hyperspectral imaging and analysis as a continuation of his work at TRW and his role as scientist for the NASA EO-1 Hyperion program. Prior to his employment at TRW, Dr. Pearlman was manager of advanced technology commercial applications at Maxwell Laboratories and spent several years at the Department of Energy and Sandia National Laboratories. Dr. Pearlman earned his Ph.D. in aeronautics from the University of Washington.

JOSEPH B. REAGAN (NAE) (see biography above)

NANCY R. SOTTOS is a professor in the Department of Theoretical and Applied Mechanics and Beckman Institute for Advanced Science and Technology at the University of Illinois at Urbana-Champaign. Her research interests include mechanics of complex heterogeneous materials (advanced composites, thin-film devices, smart materials); mesoscale characterization; and autonomic materials systems. Her work at the Beckman Institute addresses issues in the development of autonomic materials systems that have the ability to achieve adaptation and response in an independent and automatic fashion. Dr. Sottos's research group is investigating new experimental methods to quantify autonomic response (e.g., the healing efficiency of a self-healing polymer) and understand this response in terms of the material's chemistry, processing, and microstructure. Dr. Sottos began her career at the University of Illinois in 1991, serving as an assistant professor. In 1997 she became an associate professor, in 1998 she served a 1-year rotating term as assistant dean of engineering, and in 2002 she was promoted to full professor. She received an ONR Young Investigator Award in 1992, Outstanding Engineering Advisor Award in 1992, 1998, 1999, and 2002, the Robert E. Miller award for Excellence in Teaching in 1999, and was designated a University Scholar in 2002. She serves as the senior technical editor for the journal *Experimental Mechanics*, as an editorial board member for the journal *Composites Science and Technology*, and a technical reviewer for multiple technical journals. Dr. Sottos received her B.S. and Ph.D. in mechanical engineering from the University of Delaware. She also serves as the faculty advisor for the Student Chapter of the Society of Women Engineers and as National Student Chapter Coordinator for the Society of Engineering Science.

GREGORY G. SPANJERS has been program manager for the PowerSail Program at the Air Force Research Laboratory (AFRL), Space Vehicles Directorate, at Kirtland Air Force Base since October 2002. He was previously the deputy chief of the Spacecraft Propulsion Branch and Group Leader of the Electric Propulsion Laboratory of the Air Force Research Laboratory, Propulsion Directorate, at Edwards Air Force Base in California, where he directed advanced engineering development for two flight payloads: a propulsive attitude control flight demo on FalconSat3 and the micropropulsion flight demo on TechSat21. Prior to this he was a research scientist with HY-Tech Research Corporation, where he focused on pulse-power simulators for nuclear effects testing, MHD, and plasma processing. Dr. Spanjers holds a B.S. in mathematics and a B.S. in physics from the University of Minnesota, an M.S. in nuclear engineering from the University of Washington, and a Ph.D. in plasma physics from the University of Washington. Dr. Spanjers is an associate editor for the AIAA *Journal of Propulsion and Power* and serves on the AIAA Electric Propulsion Technical Committee. In 2002 he was a member of the National Space Capability Protection Study.

MICHAEL J. STALLARD is senior project engineer in the Space Technology Division at the Aerospace Corporation. He provides strategic planning and technical support to the Space Vehicles Directorate of the Air Force Research Laboratory for advanced space technology, including formation flying and microsatellite research. He also provides support to the MILSATCOM Joint Program Office on advanced satellite communications technologies. Dr. Stallard joined the Aerospace Corporation in 1989 and has been involved with numerous Air Force space missions and flight experiments, providing structural and multidisciplinary analyses, systems engineering, and technology integration. Stallard has several recent publications on low-cost microsatellites, distributed satellite missions, and virtual satellite technologies. He was also awarded a patent for smart docking surfaces for nano- and microsatellites. Dr. Stallard earned his Ph.D. in continuum mechanics from the University of California in 1990 and his M.S. and B.S. in mechanical engineering from the University of California, Berkeley, and California State Polytechnic University.

C

PRT Program Organization

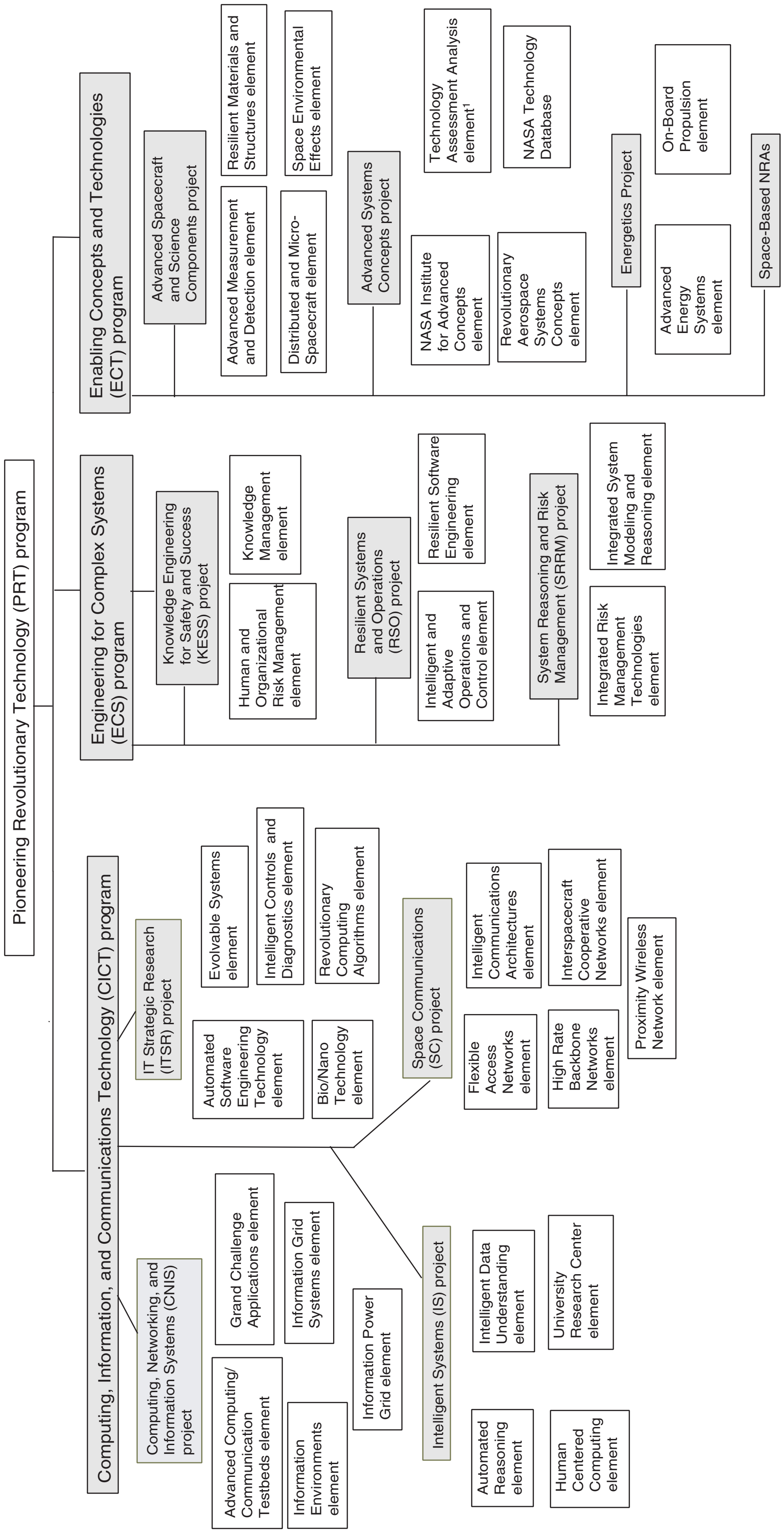

FIGURE C-1 Organization of the NASA Pioneering Revolutionary Technology (PRT) program, FY2002.

[1]TAA did not officially begin until FY2003, but was in formation during FY2002.

D

Committee and Panel Activities

PRT COMMITTEE AND ALL PANELS

June 10-13, 2002	Overview meeting	NASA Ames

PRT COMMITTEE

November 6-8, 2002	Consensus meeting	Washington, D.C.
May 6-7, 2003	Final meeting	Washington, D.C.

ECT PANEL

July 2002		
23	Advanced Measurement and Detection subgroup	NASA GSFC
31	Systems Analysis, RASC, TAA subgroup	NASA Headquarters
August 2002		
5	NASA Institute for Advanced Concepts	Teleconference with Universities Space Research Association
9	Advanced Measurement and Detection subgroup	NASA JPL
14	Distributed/Micro-Satellite subgroup	NASA JPL
20	Resilient Materials and Structures subgroup	NASA Langley
September 2002		
5-6	Energetics subgroup	NASA Glenn
17-18	Panel meeting	Washington, D.C.
March 2003		
20	Systems subgroup	Teleconference with ECT representatives
25	Distributed/Micro-Satellite subgroup	NASA GSFC

CICT PANEL

July 2002		
2	Intelligent Systems and IT Strategic Research	NASA JPL
24	Networks, data management, and space communication	NASA Glenn
September 2002		
18-20	CICTmeeting	Washington, D.C.
April 2003		
14	CICT subgroup	NASA Ames
	HEC Architecture Research	
	Research in Programming Paradigms	
	Persistent Testbeds	
	Biological computing	
	A Model-Based Programming Skunk Works	
	High-Throughput Distributed Spacecraft Networks	

ECS PANEL

June 2002		
24	Integrated Mission Design Center customer perspectives	NASA GSFC
July 2002		
1	ECS subgroup	NASA JPL
	Risk Workstation	
	Highly Dependable Computing Testbed task	
	Organizational Decision Support Tool task	
	Customer perspectives	
	ECS subgroup	Teleconference with NASA Glenn
	Virtual Iron Bird task—SimStation	
	Probabilistic simulation of component reliability	
	Autonomous Propulsion System Technology task	
	Key customer perspectives	
24/25	ECS subgroup	NASA JSC
	Organizational Decision Support Tool task	
	Virtual Iron Bird task: Digital Shuttle	
	Space Station Onboard Information Technology	
	Virtual Iron Bird task: SimStation	
	AERCAM Project—Mobile Sensor Testbed	
	Integrated System Engineering Lab—Risk Workstation	
	Key customer perspectives	
30	ECS subgroup	NASA KSC
	Virtual Iron Bird task	
	Ground checkout and launch software	
	Shuttle maintenance—organizational risk modeling	
	Key customer perspectives	

September 2002		
23-24	Panel meeting	Washington, D.C.
April 2003		
17	ECS subgroup SRRM Project Update Risk management national leadership Virtual Iron Bird yask and Columbia Wire Integrity Research (WIRe) task Intelligent Software Tools task	NASA JPL

E

Task Questionnaires

QUESTIONNAIRE FOR THE CICT AND ECS PROGRAMS

Task Number XXX

Program Name: TRL:_____
Project Name:
Element Name:
Task Name:
PI (name and phone):

1. Briefly describe the project's intent (goals) and what, in your opinion, constitutes success?

2. What is the likelihood of success?

3. On what work does your work build?

4. Who else is trying to do the similar work?

5. What products do you anticipate and who are your internal and external customers? (Please provide names, affiliations, and phone numbers for no more than four.)

6. For TRL 1-3, what is your strategy for transitioning to a higher TRL? For TRL 4-6, what is your deployment strategy/status?

7. Give a full-time-equivalent (FTE) number of researchers and scientists on the effort:

8. List the start date and anticipated end date:

9. List the three most important publications on this project in the last 3 years:

QUESTIONNAIRE FOR THE ECT PROGRAM

Program Name: Enabling Concepts and Technologies (ECT) TRL: ____
Project Name:
Element Name:
Task Name:
PI Name and location: PI Phone:

1. Briefly describe your research project’s intent or goal, how it relates to NASA’s missions, and what, in your opinion, constitutes success?

2. On what work does your work build?

3. What is the key progress to date?

4. What are the key technical issues/roadblocks you are facing?

5. Who else is trying to do similar research/development?

6. What is your plan to transition this research to NASA missions or other aerospace applications? Describe the status of that transition.

7. What products do you anticipate and who are your internal and external customers? (Provide names, affiliations, and phone numbers for no more than four.)

8. Give a full-time-equivalent (FTE) number of researchers and scientists on the effort: 0.0

9. List the start date and anticipated end date:

10. List the three most important publications on this project in the last 3 years. If there were major invited talks or patents on the technology, please list those as well.

F

Acronyms and Abbreviations

ACT Advanced Component Technologies NRA
ADACS attitude determination and control subsystem
AEMC Advanced Environmental Monitoring and Control
AFOSR Air Force Office of Scientific Research
AFRL Air Force Research Laboratory
AIAA American Institute of Aeronautics and Astronautics
AMD Advanced Measurement and Detection
APL Applied Physics Laboratory
ASC Advanced Systems and Concepts
ASEB Aeronautics and Space Engineering Board
ASTEP Astrobiology Science and Technology for Exploring Planets
ASTID Astrobiology Science and Technology Instrument Development
ATAC Aerospace Technology Advisory Committee
ATE Aerospace Technology Enterprise

BEES Board on Energy and Environmental Systems
BSRP Biomolecular Systems Research Program

C&DH command and data handling
CHDS compact holographic data storage
CICT Computing, Information, and Communications Technology
CMOS complementary metal-oxide semiconductor
CNIS Computing, Networking, and Information Systems Project
Code M NASA’s Office of Space Flight
Code R NASA’s Aerospace Technology Enterprise
Code S NASA’s Space Science Enterprise
Code U NASA’s Office of Biological and Physical Research
Code Y NASA’s Earth Science Enterprise
COMPASS Compact Airborne Spectral Sensor
CONTOUR Comet Nucleus Tour
CRISM Compact Reconnaissance Imaging Spectrometer for Mars
CRISP CONTOUR Remote Imaging Program
CTAS Center TRACON Automation System

D&MS Distributed and Micro-Spacecraft
DARPA Defense Advanced Research Projects Agency
DART Dual Anamorphic Reflector Telescope
DFCSVL Digital Flight Control Systems Verification Laboratory
DHS Department of Homeland Security
DOD Department of Defense
DRACO Dynamic Response and Coupling Observatory
DS-1 Deep Space 1
DSS distributed space systems

ECS	Engineering for Complex Systems
ECT	Enabling Concepts and Technologies
EO-1	Earth Observer 1
FTE	full-time equivalent
GEC	Global Electrodynamic Connections
GPS	Global Positioning System
GRC	Glenn Research Center
GSFC	Goddard Space Flight Center
GUI	graphical user interface
HAN	hydroxylammonium nitrate
HSIT	HyperSpectral Imager Testbed
IHPRPT	Integrated High Payoff Rocket Propulsion Technology Program
IIP	Instrumental Incubator Program
IISTP	Integrated In-Space Transportation Planning
IP	Internet Protocol
IR	infrared
IS	Intelligent Systems project
ISS	International Space Station
ISTP	Integrated Space Transportation Plan
IT	information technology
ITSR	Information Technology Strategic Research
JPL	Jet Propulsion Laboratory
JSC	Johnson Space Center
KESS	Knowledge Engineering for Safety and Success
KM	Knowledge Management
lidar	light detection and ranging
LOH	cryogenic hydrogen
LOX	cryogenic oxygen
MCS	Mars Climate Sounder
MDS	Mission Data System
MEMS	microelectromechanical systems
MER	Mars Exploration Rover
MIDP	Mars Instrument Development Program
MLS	Microwave Limb Sounder
MPD	magnetoplasmadynamic
MRO	Mars Reconnaissance Orbiter
MS	microspacecraft
MSFC	Marshall Space Flight Center
MSL	Mars Smart Lander
MSM	Mission and Science Measurements
MSR	Mars Sample Return
MSTAR	Modulation Sideband Technology for Absolute Ranging
NASA	National Aeronautics and Space Administration
NGST	Next Generation Space Telescope (James Webb telescope)
NIAC	NASA Institute of Advanced Concepts
NIH	National Institutes of Health
NIR	near infrared
NRA	NASA Research Announcement
NSF	National Science Foundation
NSI	Nuclear Systems Initiative
OAT	Office of Aerospace Technology
OMB	Office of Management and Budget
OSTP	Office of Science and Technology Policy
PEACE	Polymer Erosion and Contamination Experiment
PFS	propellant feed systems
PI	principal investigator
PIDDP	Planetary Instrument Definition and Development Program
PIT	pulsed inductive thruster
PMAD	power management and distribution
PPT	pulsed plasma thruster
PPU	power-processing unit
PRT	Pioneering Revolutionary Technology
PV	photovoltaics
QDIP	Quantum Dot Infrared Photodetector
RASC	Revolutionary Aerospace Systems Concepts
RASCAL	RASC Academic Linkage
RF/THz	radio frequency/terahertz
RMS	Resilient Materials and Structures
ROSS	Research Opportunities in Space Science
RPS	radioisotope power source
RSO	Resilient Systems and Operations
SARA	Space Astrophysics Research and Analysis

S/C or SC	Space Communications or spacecraft
SBIR	Small Business Innovative Research
SCARLET	Solar Concentrator Array with Refractive Linear Element Test
SEE	Space Environments and Effects
SEL	Software Engineering Laboratory
SOAC	Systems on a Chip
SOFIA	Stratospheric Observatory for Infrared Astronomy
SRRM	System Reasoning for Risk Management
TAA	Technology Assessment Analysis
TCP/IP	Transmission Control Protocol/ Internet Protocol
TES	transition-edge sensor
TOPS	Terrestrial Observation and Prediction System
TRACON	Terminal Radar Approach Control
TRL	technology readiness level
TT&C/DM	telemetry, tracking, and command/data management
USAF	U.S. Air Force
USRA	Universities Space Research Association
USU	Utah State University