

**NASA Astrophysics
Research, Analysis &
Enabling Technology
2011 Review Panel
Comments**

I. Introduction

This panel was created to consider questions about the NASA Astrophysics Research and Analysis (APRA) programs posed by NASA HQ in response to internal and previous committee queries. Panelists selected by NASA, listed in Appendix 1, held three face-to-face meetings in Seattle (January 9, 2011) and in Washington, DC (March 24 & 25; April 28 & 29) that were supplemented by teleconferences. Thus this report was prepared on a compressed timeline. Agendas for the in person meetings are included in Appendix 2. We also note that the panelists were chosen in part because they had first hand experience with APRA (which usually we will take to include Enabling Technology) program research. At some point in their careers, all the panel members received funding from an Astrophysics Division (or its predecessor) program; these connections were discussed openly. The panel was non-FACA, and so provided no recommendations or consensus conclusions.

The panel gathered background information through written materials, most notably information on research budgets and recent grant competitions supplied by NASA headquarters, the NRC/Fisk report, and an informal report on metrics by Eric Smith. Additional information was obtained by presentations from the Fermi, Spitzer Space Telescope, Hubble Space Telescope, and Chandra GO programs. We requested that these presentations focus on explaining each program's view of supporting its mission and the community, with the objective of learning about best practices that could be applied to the NASA astrophysics R&A programs.

Briefings on the relevant Astrophysics program elements provided perspective on current practices. The focus in these discussions centered on understanding the bases for program structures and how metrics or other feedback were incorporated into decisions about program balance or structure.

The committee was impressed by the scope and impact of the APRA programs, including enabling technologies. A general observation - one that repeatedly came up during each of the sessions - was their remarkable cumulative breadth and depth. Their activities cover wavelengths from the gamma-ray to the far infrared, and objects from the early universe CMB to exoplanets to black holes to asteroids, not to mention stars and galaxies. Its detector and telescope technologies perforce spanned just a great a range of variety. The programs also included support for the significant theoretical work that underlies missions, and laboratory astrophysics to provide a real-world context. Thus these APRA programs provide a critical opportunity for cross-fertilization of ideas relating to astrophysics and its

supporting technologies, and for competitive selection between them, that helps NASA insure the most effective and innovative future missions.

Virtually every mission that was discussed by the panel, including the Great Observatories, owed a significant heritage to these programs. This included technology (infrared detectors, for example), and the training of the leadership: many of the instrument PIs on these NASA missions (indeed some mission PIs) had trained with APRA supported programs as graduate students or postdocs. Other graduates of the APRA programs went on to productive careers in related industries, for example with Ball and Lockheed-Martin. The committee emphasizes that this variety is a fundamental plus for NASA, not a weakness. We appreciate in particular the role these programs play in training future scientists and engineers, whether or not they continue their careers with NASA.

The role of some APRA programs likely will need to expand in the future as the number of space observatories with major guest observer (GO) research and grant programs declines, and possibly becomes zero in the pre-JWST time period. The panel therefore paid special attention to how ADAP and ATP, as well as other programs, could be positioned to fill the GO-funding gap in ways that are effective for science and in positioning NASA with technologies for the future.

This report consists of a main body that responds to the questions posed by the NASA Astrophysics Division, and in effect provides a summary of the panel's discussions.

Additional material and most of the detailed comments are contained in the white paper Appendices 3-8. These consist of results from studies by working groups, each composed of a few panel members with special interests, experience or expertise in each area. The white papers, while reviewed by the panel as a whole, present the views of each working group and thus also provide insight into the diversity of perspectives among panel members. This review was a highly time- and resources-limited process; as such, there were numerous details that we did not attempt to address, but which are subsumed in the general conclusions.

II. Responses to Questions

[Questions from NASA are in roman typeface and the panel's response in *italics*.]

Review of Astrophysics programs for Research, Analysis and Enabling Technology
This comparative review should assist NASA to increase the effectiveness of its Research, Analysis and Enabling Technology programs. The purpose of these programs is to maximize the scientific productivity from NASA's current and future missions, in the context of the science goals, objectives and research focus areas described in the Science Mission Directorate's Science Plan, and the Astro2010 Decadal Survey of Astronomy and Astrophysics. The review will use readily available data to assess the effectiveness of the programs.

The panel found that the data made available to it were not always adequate to fully assess the effectiveness of the APRA (hereafter this refers to APRA + Enabling Technologies) programs. We suggest the collection of additional information to inform future reviews (Appendix 4); such information can be used to develop and support criteria against which program progress can be judged. The panel also strongly urges the NASA Astrophysics Division to develop statements of goals and related evaluation criteria and associated metrics (see Appendix) that can be used as fundamental criteria for judging success for various elements of the APRA programs.

The review was asked to address the following specific questions that we have divided into seven main areas.

1. The Astrophysics Research and Analysis program (APRA) funds enabling technology, suborbital payloads and lab astrophysics. Does the APRA program:

- Balance appropriately between suborbital flight opportunities (both for science and for advancing technology) and the development of enabling technology and of detectors?

Answering this question requires a clear definition of the program goals and their associated evaluation criteria. It is not clear that these currently are available in a form that would allow an external panel to make detailed judgments on the relative balance between these two key areas. In their efforts to probe further, the study group examining this area, found from their analysis that, yes, the current balance is appropriate. This conclusion, however, would have been stronger if it had been supported by better data. For the cumulative APRA programs, we suggest that NASA HQ investigate an ongoing process to collect better quantitative and qualitative data on performance (internally, through subcontracting, or some other mechanism).

Panel discussions on this topic emphasized the need to sustain the process whereby new technologies could progress to flight trials and eventually to compete for orbital missions. As discussed in Appendix 5, detectors clearly are one critical factor for future missions, and have been identified as such by previous reviews.

The panel also points out the central role of APRA funding for this entire development stream, which also encompasses part of laboratory astrophysics. These are not areas where commercial or university partners are likely to make the investments required for future NASA purposes.

- Make initial investments in technology that are appropriate to NASA's future strategic missions?

This issue received considerable attention, especially in the context of providing the technological and workforce capabilities for NASA space astronomy of the future (Appendix 3). Results from previous technology investments (described in Appendix 5) illustrate NASA's past successes in developing the means to address key astrophysical problems that later matured in major missions.

Such investments are key to foster innovative new approaches; it is rarely effective simply to scale up existing technologies. But the committee had serious concerns that the number of opportunities for students and young scientists to participate in developing new capabilities is declining. In part, this reflects the growing sophistication of the experiments that will produce new science. Ballooning, for example, offers a test bed to pioneer new technologies, advance the scientific forefront, and train students in a thesis timeframe.

Program balance therefore requires that NASA not let this area lag. NASA should consider what level and range of technical development it would need for possible future missions, and NASA should implement programs that, as a secondary goal, if not a primary one, help to train people with the skills and interests to carry out the work.

- Allow PIs to develop technology to the level of readiness required for an Explorer proposal?

We see this as a problem area. APRA is currently well structured to fund technology readiness levels (TRLs) 1-3, but a gap then exists at TRLs 4-6. Since a TRL of 6 is needed for Explorer submissions, this raises a serious concern. The panel also recognizes that resource balancing is involved in providing this support, and therefore believes this is an area where

implementation of metrics tied to orbital missions as a criterion will be important.

- Fund laboratory astrophysics in a way that optimizes interpretation of data from current and future space missions?

The panel reflects the community in considering laboratory astrophysics to be a part of the foundations for future astrophysical missions. Since much of our data consist of spectra for photons or other cosmic messengers, understanding the physical basis of for the formation spectral features is essential. This type of work tends to involve long-term projects by specialized research teams; funding consistency therefore is especially important.

Optimizing this funding depends on understanding what constitutes a satisfactory investment. This again leads to a need for NASA to quantify requirements. For example, is the interpretation of data from current missions being substantially limited due to a lack of critical laboratory data? If so, is it reasonably feasible to obtain this information? Are the laboratory databases sufficient to support future major missions?

- Offer a range of award sizes suited to meet the challenges in these areas?

Determining the range of award sizes is a complicated matter. One issue is funding efforts to cross the TRL3-6 gap, where current programs are viewed as not providing sufficient funding. On the other hand, the panel feels that over-consolidation of programs has to be resisted. Too few opportunities will have a chilling effect on efforts to maintain a program that is diverse and vigorous enough to support future missions and insure the training of a technologically able workforce.

In its discussions, the panel recognized the difficulties inherent in balancing the need for substantial funding for programs to progress to TRL 6, with the need for a diverse base at the lowest TRL levels. This presents a management challenge for which well-defined goals are especially important in informing decisions.

- How should the APRA program change to complement activities in the Office of the Chief Technologist?

The OCT Technology Roadmap plans are still under NRC review, and thus the panel did not spend time on this issue. Appendix 5, however, contains a discussion of some ideas developed by that study group.

2. Is the Astrophysics Theory Program appropriately targeted to facilitate

interpretation of results from current missions, and aid in developing concepts for future missions?

- What are appropriate metrics to judge whether too large a fraction of the Astrophysics budget is spent on theory, or too little?

This topic was explored in the context of asking what constitutes a successful NASA Astrophysics Theory Program (ATP)? Appendix 7 expands on the views expressed in the panel and presents a full list of metrics. The basic conclusion is that theory can open new ways to study the cosmos (e.g., big bang cosmology) as well as providing tools that are critical for the interpretation of astrophysical data (e.g. stellar oscillation modeling applied to stars observed with Kepler). We expect this trend to continue as evidenced by the high priority placed on measurements of baryon-acoustic oscillations with the proposed WFIRST mission.

Appendix 7 lists a number of metrics that could be applied to ATP. At the present the proposal pressure factor stands out: ATP is more over-subscribed than other Astrophysics R&A funding opportunities. This suggests that some imbalance might be present. The panel also is well-aware that a single metric may not provide a complete picture, and urges that the ATP situation also be examined in terms of other criteria, such as the ability of proposals to support or extend the science of ongoing or planned NASA missions.

- Is the range of award size suited to the range of theory challenges to be addressed?

The panel felt there could be value in making a separate call for larger "research network" types of ATP proposals. Here we drew from the positive experiences of GO missions that incorporated large programs by providing fenced off amounts of observing time; i.e. changed the observing time allocation mix by policy. In this way large projects can compete with each other on a level playing field, and neither large nor small scale-efforts necessarily exclude the other.

This type of division, however, requires that NASA decide in advance on a set of criteria to assess the effectiveness of different scale programs. While "science-per-dollar" is one such criterion, there may be others, such as impact on future mission planning.

3. The Astrophysics Division funds analysis of data from its missions in two ways:

- The Astrophysics Data Analysis Program (ADAP) funds analysis and interpretation of data in the public archives of NASA missions, and of international space missions such as XMM, CoRoT and Herschel. Most are multi-year awards for investigations using data from multiple missions.
- Guest Observer (GO) awards are associated with specific operating missions; they fund analysis and interpretation of data from proposed observations. These are typically single-year awards, with funding released only when the observations are taken.

The panel notes that GO missions have departed from the single year funding paradigm in order to optimize scientific return. This change appears to have been based on their use of data to determine program effectiveness. The Spitzer Space Telescope program and the Herschel Space Observatory program, taking advantage of some unique characteristics of their arrangements, have even gone to providing fixed cost budgets with an extended performance period, a move that greatly simplifies the award process, solves numerous bureaucratic headaches for university-based PIs, and has very low overhead cost.

- What are the strengths and weaknesses of these two funding models, and what is the appropriate balance between them?

As discussed in Appendix 6, GO and APRA programs are complementary. GO programs provide resources for properly planning to acquire and then analyze data. They therefore seek to maximize the scientific returns that depend on active mission resources. The GO programs also have smaller associated funding streams for theoretical and archival research that similarly feed into this primary objective.

ADAP ensures that data placed in archives, key legacies of the successful NASA missions, are fully exploited to maximize their scientific return and fully achieve post-mission operations goals. Such projects tend to work across wavelength or other experimental boundaries for the broadest possible picture, and can involve the development of new software tools that also are brought into the public domain. Archival research also serves a very important NASA manpower goal by training students and new scientific staff, and by acting as a kind of scientific shock absorber by sustaining scientific activity between missions. Trained people do the science, and thus it is in NASA's interest to help support an appropriately sized and skilled scientific workforce.

4. The Origins of Solar Systems (OSS) program is run jointly with the Planetary Science Division; the Astrophysics element supports exoplanet detection, from space or from the ground. The Astro2010 Decadal Report emphasizes NSF's role in enabling ground-based observations.

The subpanel report on OSS is contained in Appendix 8. The panel viewed exoplanet research as a key emerging area of astrophysics. This field is strongly interdisciplinary, thereby connecting with and broadening the scientific scope of both Astrophysics and Planetary Sciences. It fits well into the multi-disciplinary approach manifest in the other programs within APRA-RAETS. Research on exoplanets is exciting to the public and scientific community and thus offers an opportunity to enhance support for NASA's research activities.

- How should the OSS program change to complement NSF's role?

The NASA/NSF partnership is extremely beneficial to this area, which will continue to gain from combinations of ground- and space-based observations. For example, Kepler will yield thousands of candidate exoplanets whose confirmation largely will come from ground-based radial velocity measurements. The OSS working group therefore strongly endorses NASA's continued support for ground-based exoplanet research, and suggests a more coordinated approach to working with NSF, rather than a division based on ground- versus space-observations. We note that current regulations exclude several active NASA exoplanet communities from applying for NSF support.

- Should the OSS program be continued to foster interdisciplinary collaboration with Planetary Science?

This question generated a great deal of discussion by the panel. Initial opinions tended towards a separate astrophysics program, but on reflection moved back to appreciating the benefits of a partnership between the Astrophysics and Planetary Science Divisions. These two divisions bring complementary skill sets to OSS, thereby strengthening its ability to address fundamental issues concerning the origins and evolution of planetary systems. Examples include the Planetary Science Division's investments in understanding planetary atmospheres and Astrophysics Division's in developing the capabilities to measure properties of exoplanet atmospheres. Other examples are included in Appendix 8.

Within this framework the Astrophysics Division is encouraged to play a more active role as a partner. Especially important is attention to the makeup of review panels where a perception exists that planetary science can be over-represented among the panelists.

Regarding the query about workforce, we have added more general comments on workforce development as a separate item at the end of this section, and details are included in Appendix 3.

Exoplanet research is inherently interdisciplinary, and therefore will have a presence across APRA. Thus, for example, exoplanet studies should be explicitly included in requests for Astrophysics Theory Program proposals, while also seeking to maintain an appropriate balance on the OSS selection panels, as noted above.

Given the interdisciplinary and cross-division/agency nature of exoplanet research, developing a metric portfolio and associated evaluation criteria is very important. The panel suggests that in this area outcome-based metrics, e.g. progress towards the goal of obtaining a census of exoplanets and publication of results, are important, as well as tracking the use of resources, such as telescope time allocated for this type of research with ground and space observatories.

5. The Fisk Report also points out the importance of funding research that presents high risks but offers high potential returns.

- What metrics might be appropriate for the program's effectiveness in this area?

The committee agreed that this is an important area, across the board, but especially with regard to initial steps towards new technologies and techniques. With new technologies APRA needs to beware of excessive risk avoidance and, as discussed in Appendix 5, recognizing that a substantial failure rate is likely. The same principles may be applicable to other areas where review panels might be instructed to identify a few high risk, high return proposals for consideration of funding. In these cases, however, the definition of "high return" must be clear to reviewers. As with other areas of APRA, the panel supports the development of clearer metrics and a mechanism to accumulate the necessary evaluation data.

6. The December 2009 Fisk Report "An Enabling Foundation for NASA's Earth and Space Science Missions" notes (Box S.1) that Research and Analysis programs should enable a "healthy scientific and technical workforce" for NASA's science missions.

- When should this be a consideration in evaluating and selecting proposals?
- What metrics might be appropriate for the program's effectiveness in this area?

The future of NASA Astrophysics and training of people with relevant skills are intertwined issues that emerged in almost every area of discussion. A detailed summary of comments is in Appendix 3.

Currently the majority of Astrophysics Division funding for scientific research (as distinct from technology areas) flows through GO programs. As in the past, these modest-budget research programs support a large number of graduate students, post-doctoral fellows, and scientists in a variety of institutions throughout the U.S. The community therefore is concerned about the upcoming decline in active missions, the associated decrease in GO funding, and their impact on the astrophysics workforce. We appreciate that major increases are being sought for the R&A budget to help blunt this change. The panel wishes to reinforce the importance of this step: it is needed to insure that scientists with proper skill sets and interests are present to support NASA in new (major) missions as they appear in the second half of this decade.

Training and sustaining the workforce in technology development areas is widely recognized as being a serious concern. This is especially true in Astrophysics, where opportunities for instrument development, and specifically space-based instrument development, have declined. It seems clear that NASA will benefit from wider student involvement in its technology-based activities. As noted, the current Great Observatories reaped tremendous heritage benefits from earlier NASA-supported technology development.

To this end, the panel suggests that NASA study ways to increase student involvement in technology-oriented projects. For example, plans for wider student involvement could be added as part of future AOs for larger suborbital and Explorer class missions. Existing NASA-based programs, would benefit from being reexamined and where possible enhanced to meet current requirements for student support. Another avenue for enhanced involvement could be through internships to facilities, and industries, that do not routinely have access to students. .

We noted that the emergence of high performance balloons could play an important part in engaging students in developing capabilities. Balloons in combination with rockets offer a hands-on environment, and with their longer duration, balloon-based projects enhance the range of activities that can be considered for student projects. Projects can be completed within the career of a graduate student.

The panel also suggests consideration of new NASA technology fellowships. This possibility, described in Appendix 5, would have special conditions to allow post-graduates to join teams actively involved in developing instrumentation for NASA. One component could be a Hubble/Chandra - like fellowship for postdocs with a duration of 2-3 years to be taken to an existing laboratory and would end upon acceptance of a faculty offer.

A second track would include an NSF Career - like opportunity for junior faculty or non-tenure track permanent positions of 3-5 years. These would come with substantial funding, and would serve to jump-start the careers of young faculty and researchers. Only those newly employed in tenure track or other permanent positions would be eligible to propose for this technology fellowship option.

A third aspect of this area involves raising public awareness. The Great Observatories have done an excellent job in engaging the public, but they present only one side of NASA. The excitement in developing new capabilities to study the universe, however, and the process to pioneer new technologies, does not come through in these focused outreach programs. It would be very useful to see if ways can be found to engage the future instrument builders as effectively as we have engaged future observers, modelers, and theorists through targeted outreach efforts.

7. The review should also:

- Identify any options to add new proposal opportunities, or to remove existing opportunities.
- Identify areas of Research, Analysis and Enabling Technology where NASA could fruitfully partner with NSF, DoE or other agencies.

These two bullets present difficult questions. We do not have much information available to us on bullet 2, and bullet 1 depends on a strong vision of the future, which we view as the purview of NASA Astrophysics management. However, there was unanimous agreement that with the decrease in active major astrophysics missions, coupled to the explosive growth in high-quality archival databases, enhanced support for archival (and in appropriate cases, associated ground-based) research would be extremely valuable. The panel also has noted the importance of sustaining technology efforts, both in terms of people and growth of capabilities.

- Identify any ways in which we could improve the mechanics and quality of our reviews.

In discussing proposal reviews, the panel generally found the situation to be reasonable. The primary area receiving comments related to the need to be explicit in requests for proposals as to included areas (e.g., exoplanets in APT), and then following through by selecting reviewers who cover these areas in balanced ways.

More generally, the panel also discussed the wider issue of reviewing APRA. The GO programs impressed the panel with the multiple ways in which they have engaged their communities. One form of engagement is via high-level reviews. The panel felt that external reviews of APRA are essential, but were split as to whether these should be a standing group or one convened every few years. However, continuity across reviews was recognized to be a positive factor, and the membership terms of, for example, participants in a review committee therefore should be staggered so that the history is not lost between meetings.

GO programs, like APRA, have proposal review committees drawn from the community, but also connect via Users' Committees. The panel therefore considered the possibility of the APRA divisions having something akin to Users' Committees. It was not clear to us how this might best be managed under the current Federal guidelines, but having a standing group of community members supported by and engaged with APRA would be a very valuable way to build stronger working relationships with the astronomical community. For example, a sequence of User Committee meetings for various programs could be interleaved between the major reviews as a way to sustain community contact.

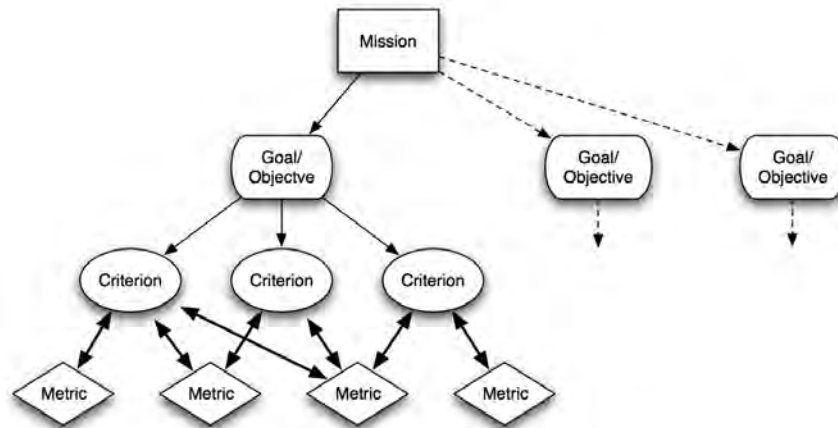
8. Finally: this review should suggest appropriate program metrics, and a mechanism for future review of the Astrophysics Research, Analysis and Enabling Technology program. What data could most usefully be collected to assist future assessments of the program?

This was a major focus for the panel: trying to describe what is needed to allow future reviews to more effectively assess the success and cost-performance of the Astrophysics research award programs.

The Panel built its vision for metrics beginning from the Fisk report and informal 2010 Smith study. The Fisk report sets the stage for the use of metrics in implementing NASA programs and for providing accountability for their use of human and financial resources. Most of their proposed metrics are qualitative in nature. The Smith study emphasizes quantitative metrics based on an analysis of several years of proposal inputs to NASA. Our concepts for extending and implementing the concepts from these reports are in Appendix 4.

A clear and well-documented mission statement for each Astrophysics Division research program is a key first step. Communicating how each goal adds value to science and society is also essential because it helps maintain support for the Astrophysics Directorate's activities among stakeholders, the scientific community, and the wider public. Together with evaluation criteria, they provide a key framework within which future reviews will operate to assess program quality.

The panel therefore suggests that NASA Astrophysics develop a set of quantitative metrics that can be combined to produce criteria for making management decisions. This process is illustrated below.



We agree with the Fisk report on the importance of connecting metrics and criteria to demonstrate how programs enable NASA's missions. This led the panel to suggest that metric portfolios should be associated with major program elements. These can include quantitative data collected before and after proposals, including information from the published literature, along with qualitative assessments by program managers. This approach also should allow NASA to determine in a rational way what information it already has and what additional information is needed to support its metrics.

Similarly, criteria for program success need to be formally defined, and many likely already are contained in various components of Astrophysics Division long range plans. Thus the panel envisions a process whereby future reviews can readily see how decisions relate to stated objectives and in turn connect to the overall objective of supporting missions and their associated scientific returns.

In implementing metrics the panel cautions that care is required not to overburden PIs with additional administrative tasks: the focus of proposals should very clearly remain on the relevant scientific and/or technological issues. We also caution that one set of metrics cannot be applied equally to all program elements.

The details of our proposal are presented in Appendix 4. The proposed metrics are divided into four major categories:

- A. Measurements associated with science value of the supported research.*
- B. Indicators of program relevance to NASA's stated goals.*

- C. Assessments of the wider impact of programs on the public, scientific community, policy makers, and other stakeholders.*
- D. Degree to which programs are addressing workforce concerns.*

Examples of proposed metrics include appropriate numbers of scientific publications, citation rates relative to community size, student support, geographical distribution of funds within the United States, identifying major research breakthroughs, technological spinoffs, evidence of career advancement, and noting any associated prizes or awards. But not all metrics will apply to all Astrophysics program elements. For example, number of publications can be essential for the Astrophysics Theory Program but may be of minor importance for a technology development effort, where the "science value" may lie in what could be enabled in the future or in technological breakthroughs rather than research papers. Each Astrophysics program element should decide how to select and weigh metrics to define success criteria.

While the proposed metrics in area A are largely quantitative, those in other areas can be qualitative. For example, key issues in technology investments include the degree to which such efforts influence the design or justification for future missions, or aid in defining opportunities for scientific follow up via missions. We suggest that each Astrophysics program element metrics portfolio may have quantitative and qualitative components, which can be linked to success criteria and presented as a component of program reviews as well as to stakeholders and the public.

A final suggestion is to identify a mechanism to obtain the statistical data for evaluating performance against metrics. Several of the Great Observatory presentations included impressive demonstrations of this capability from their current program structures and staff. Building on this work, or subcontracting directly to an existing group, would be sensible. The committee felt based on the GO experiences that, with encouragement, enough program PIs would be cooperative to make this fully successful. The panel also notes that GO programs allocate significant resources to this type of activity, typically a few FTE in each GO program. Metrics are valuable but will not come for free.

III. Concluding Comments

NASA's engagement with the astronomical community has been mutually beneficial for more than 50 years. Now we are ending an extended period where multiple major space observatories have been in operation, and in the process, supported great science and a large and active community of scientists that in turn developed the following generation of NASA missions, as well as engaged the broader public. In the coming years, various

components of the NASA Astrophysics Program will likely need to play different and sometimes larger roles.

In our research for this review we have found that numerous opportunities exist for NASA to foster development of enabling technologies, as well as increased productivity from archival research and science based on moderate-to-small scale missions. During this time of change we encourage NASA to seek wider contact with its various communities. We also suggest that the collection and organization of data from Astrophysics programs, with a goal of building metric portfolios and success criteria, will be increasingly valuable in guiding the continued evolution of APRA.

John S. Gallagher III, Panel Chair

APPENDICES

Appendix 1: Committee membership table p2-3

Appendix 2: Agenda from meetings p4-6

Appendices 3-8 study group white papers

3. Workforce p7

4. Metrics p10

5. APRA and Enabling Technology p14

6. ADAP p23

7. ATP p31

8. OSS p35

Appendix 1: Committee Membership

John Blondin
North Carolina State University
Department of Physics
2700 Stinson Drive
Raleigh, NC 27695-8202

Steve Boggs
University of California
Space Sciences Laboratory
7 Gauss Way 7450
Berkeley, CA 94720-7450

Dennis Ebbets
Ball Aerospace & Technologies Corp.
Civil Space Systems
1600 Commerce St RA4
Boulder, CO 80301-2734

Miriam Forman
Stony Brook University
Physics and Astronomy
100 Nicolls Rd 3800
Stony Brook, NY 11794-3800

John (Jay) Gallagher
University of Wisconsin-Madison
Astronomy
475 N Charter St
Madison, WI 53706-1507

Tom Greene
NASA's Ames Research Center
Space Science Division
Bldg. N245, Room 148 MS 245-6
Moffett Field, CA 94035

Mary Beth Kaiser
Johns Hopkins University
Department of Physics and Astronomy
3400 N Charles
Baltimore, MD 21218-2608

Tom Loredò
Cornell University
Dept. of Astronomy
Space Sciences Building
Ithaca, NY 14853-6801

Amber Miller
Columbia University
Physics
550 W 120th St. 5247
New York, NY 10027-6601

James Neff
College of Charleston
Physics & Astronomy
58 Coming St. 101 SCIC
Charleston, SC 29424-1418

Joe Nuth
NASA's Goddard Space Flight Center
Astrochemistry Laboratory
Greenbelt Road Code 691
Greenbelt, MD 20771

Sterl Phinney
California Institute of Technology
Theoretical Astrophysics, 130-33
1200 E California Blvd
Pasadena, CA 91125-0001

Howard Smith
SAO
OIR
60 Garden St. MS-65
Cambridge, MA 02138-1516

Chris Walker
University of Arizona
Astronomy
933 N Cherry Ave
Tucson, AZ 85721-0009

Appendix 2: Agenda from meetings

Telecon 1:

December 13, 2010-Planning and introductions.

Face-to-Face Meeting 1: January 9, 2011, Co-scheduled with the American Astronomical Society Seattle Meeting

Review of Astrophysics programs for Research, Analysis and Enabling Technology: AAS Agenda

12:00 pm Assemble & get some lunch

12:20 Introductions of panel members & guests—all

12:30 Overview of Panel Charge & Process—Gallagher & Sparke

1:00-2:00 Fermi Guest Investigator program, ~20 minutes for a presentation
Remainder for questions & discussion.

2 – 2:15 break

2:15 – 3:15 NASA Origins of Solar Systems research awards program, ~20 minutes for a presentation
40 min for discussion & questions.

3:15 – 3:30 break

3:30 Discussion of Panel process & information requirements.

- What kinds of information do we need, from whom, & what are we lacking?
- How do we organize to optimize our effectiveness—subgroup taskings.
- What kind of communication between now & the March meeting—another telecon? When? What?
- Are there specific requests for materials before & at the March meeting?

4:30 Other business or issues?

5:00 pm or earlier -- adjourn

Telecon 2:

March 10, 2011- Goals of review, updates on status, working group assignments.

Face-to-Face Meeting 2: March 24 & 25, 2011, Washington, DC

Thursday 24 March

8:45am: Gathering and introduction

9am: Working group reports

Origins of Solar Systems: report from Loredó & Nuth

Appendices: 2011 Astrophysics Research, Analysis and Enabling Technology Review

Metrics for Research Programs: report from Ebbets & Forman

10am: Discussion of issues around the Theory program, Astrophysics Data Analysis, and APRA.

10:30am: Break

11am – noon: Astrophysics Theory Program

12 noon - 1pm Lunch

1:15 - 2:15pm Astrophysics Data Analysis Program

2:15pm Break

2:45pm to 4:15pm APRA

4:15pm Break

4:30pm Technology development programs that may interface with APRA: SBIR, Office of Chief Technologist, etc.

Adjourn by 6pm

Friday 25 March

8:30am - 9:30am Spitzer GO program

9:45am - 10:45am Hubble GO program

11am - noon Chandra GO program

Noon - 1pm lunch

1-2:30pm Panel discussion:

-- Successes of the Guest Observer/Guest Investigator programs

-- Technology development: what is required, at a time when few new missions can be started?

-- Plans for the April meeting and how to write the report

2:30pm action items and assignments

3pm adjourn, dash for airport

Telecon 3:

April 18, 2011: Update on working group reports; discuss April DC meeting agenda.

Face-to-Face Meeting 3: April 28 & 29, Washington, DC

Review of NASA Astrophysics Programs for Research, Analysis and Enabling Technology

3rd panel meeting: Residence Inn Marriott Capitol at 333 E Street, SW Washington, DC 20024
Senate Conference Room

Thursday 28 April 2011

8:45am Gathering and introductions: review agenda

9am Working Group Report: Origins of Solar Systems

9:30am Working Group Report: Astrophysics Theory Program

10am Working Group Report: APRA

10:30am Break

11am APRA funding over the past 5 cycles: Wilson-Hodge

11:20am Guest Observer/Guest Investigator Programs

12 noon - 1pm Lunch

1:15pm Technology development

1:45pm Summary of public comments: Wilson-Hodge

2:15pm What does the panel want to say to Jon Morse?

2:30pm Jon Morse (Director, Astrophysics Division)

3:15pm Break

3:45pm Astrophysics Data Analysis Program

4:15pm Revise draft reports

Adjourn by 5:30pm

Friday 29 April

8:30am Working Group Report: Workforce

9:00am Working Group Report: Metrics

9:45am Assess Progress on Review Charge

10:15am Break

10:45am Revise draft reports

Noon - 1pm lunch

1pm action items: what do we need to do to finish this report?

3pm adjourn, dash for airport

Appendix 3: Workforce

Primary authors:

John Blondin (NC State University)

Dennis Ebbets (Ball Aerospace)

James Neff (College of Charleston)

Howard Smith (SAO)

Christopher Walker (University of Arizona)

Introduction

The National Academies report *Rising Above the Gathering Storm* draws attention to the need for a STEM-educated workforce in order for the United States to successfully compete in the global community of the 21st century. The Executive Summary states "Having reviewed trends in the United States and abroad, the committee is deeply concerned that the scientific and technological building blocks critical to our economic leadership is eroding at a time when many other nations are gathering strength." The report provides some chilling quotes from industry leaders (Intel, GE, IBM) about the need to improve the STEM education pipeline.

NASA is in a unique position to contribute to the nation's future technical and scientific workforce. Indeed, many, if not most, of today's engineers and scientists have been motivated by NASA exploration initiatives. In addition to this very public, global role of attracting young people into STEM careers, NASA has a responsibility of maintaining a core level of technical expertise to directly support future missions. The research supported by the RAET programs, especially APRA, plays a key role in attracting students into STEM careers.

Funding patterns in the past have been generally successful in maintaining a stable workforce in support of NASA missions. Analysis of the employment of Ph.D. astronomers has shown a healthy, stable workforce production over the past two decades (Metcalf 2008). We consider two subgroups.

R&A: Much of the funding from GO programs and from ATP and ADAP supports graduate students and post-doctoral researchers. As an example, the HST GO program reports that roughly 50% of its awards go to support graduate students and post-docs. This funding model ensures a healthy pipeline of scientists with requisite skills to maximize the science output of NASA missions. These programs have also played a key role in sustaining mid-career scientists in non-permanent positions. The committee calls attention to the widespread concern within the community that the decline in GO program support as operating missions phase out over the next half decade, without corresponding increases in R&A opportunities, will severely affect this constituency while at the same time leaving NASA at risk of the future without a trained workforce for future missions, or, for that matter, to take advantage of the increasingly large and unexamined database in its archives.

Technology and Instrumentation: There is serious concern that workforce training in the critical areas of instrument and detector development is not sufficient, as currently supported by APRA. Both the 2010 Decadal Review and the Fisk Report point to the challenges of maintaining a workforce skilled in detector development. The Decadal Review specifically points out “opportunities for training students in instrumentation have declined precipitously over the past 20 years.” This committee notes that the Great Observatories, which presented to the group, owe much of their scientific and instrumental successes to parentage from the RAET programs (or their earlier analogs), and from APRA in particular. Spitzer technology, as one example, traces its heritage directly back to RAET's programs; most of its PIs were trained under one or more of these small NASA projects.

Findings

NASA can best leverage this in-road into the future workforce by involving students at all levels in ongoing investigations. The panel recommends that future APRA AO's should encourage proposers to include, if possible, a plan for student involvement. Consideration of such a plan from the outset is likely to increase the effectiveness of student participation in successful proposals. PI's at companies or institutions that do not have students or areas of study relative to the proposed investigation may consider funding student participation through internships (*e.g.* over a summer or semester). Many examples of such programs exist and could help students make a smooth transition from academia to the workplace. Larger, mission oriented programs (Suborbital and Explorers) are in a position to provide students with insights not only into specific science and technological areas, but also into systems engineering and the interactions between members of an extended team. The ability to work effectively, as a team is essential to mission success and to many life experiences.

NASA has had successful Graduate Student Researchers, Internship, and Space Grant programs for many years. Recently, the Office of the Chief Technologists has initiated the NASA Space Technology Research Fellowship program, with the objective to motivate a new generation of engineers and technologists through working directly with NASA personnel in a mutual area of interest. Programs such as these should be encouraged and expanded. The committee calls attention to the fact that the current size of these awards is insufficient to cover tuition plus a normal student stipend at most universities, discouraging students from applying.

A central database of student demographics, areas of participation, and outcomes should be maintained in order to assess the impact of NASA programs on the work force.

NASA must develop a means of anticipating the qualifications of a future workforce needed to support missions in the near and distant future. (For example, one might envision a growing need for training in laboratory astrophysics to enable mission development and science output from a mission such as IXO.) 'User Committees' can help with this strategic planning of workforce.

Fisk Report Summary (Excerpt from Report; p. 37-38)

“The development of a capable workforce, both for NASA and for the outside space program community, is the third guiding principle articulated above and a singularly important task for the mission-enabling program. However, it is also one of the hardest for which to establish a metric. The metric should be based on the demographics of the scientific community and on a projection of workforce needs during future decades. Such an assessment will require considerable thought on the part of the science managers of NASA, in coordination with the science community. (See Chapter 4; Fisk Report)

The metric for the workforce development component of mission enabling should include the following:

1. A statement of the importance of workforce development to the SMD division, based on a realistic analysis of the demographics of the community and expectations for future mission opportunities.
2. The means by which the division intends to satisfy its workforce needs, including providing funding for graduate fellowships, ensuring that both undergraduate and graduate education can occur in universities that actively participate in SMD programs, and supporting hardware programs that will provide hands-on opportunities to train experimentalists.
3. A quantitative measure of the extent to which the demographics and the scientific and technical competence of the science and engineering communities, including the relevant NASA workforce, are being improved and maintained.
4. An appropriate allocation of resources, based on the stated workforce need and the means to satisfy the need.”

Decadal Review (Except from Report; p149)

“The current distribution of activities and grants funding poses particular challenges for maintaining a workforce skilled in instrument and project development. Although properly funded programs for space and ground facilities often provide significant support for the training of new data analysts, the opportunities for training students in instrumentation have declined precipitously over the past 20 years. Training for the next generation of instrumentalists is most efficient when there is a steady-state hierarchy of project sizes, so that people can progress from relatively smaller, simpler, and faster projects to responsibilities in larger and more complex activities. Despite existing NASA and NSF funding mechanisms that can support technology training, the data gathered by the survey’s Infrastructure Study Groups show that fewer than 5 percent of students recently receiving Ph.D.s from astronomy departments classify themselves as belonging to “instrumentation and methods” subfields. If there are to be enough young instrumentalists to spearhead the ambitious new instruments and facilities of the coming decade, more must be done within graduate astronomy programs to educate and train them. The growth of astrophysics research within physics departments can help in this regard. Some of the input Astro2010 received in white papers submitted by the community discussed the need for increased emphasis on instrumentation within U.S. astronomy and astrophysics Ph.D. programs. It is important that universities recognize

the value of skilled instrumentalists, and that they continue to provide opportunities for early-career training. Further, the scientific community must value the intellectual contributions of instrumentalists as an integral part of the astrophysics endeavor.”

Appendix 4: Metrics

Dennis Ebbets, Miriam Forman and Howard Smith

Assessments of NASA’s R&A programs will benefit from the identification and appropriate analysis of metrics. Four concerns are: measures in addressing NASA’s goals; measures of the intrinsic value of any one program; balance between different program elements; and the effect of NASA R&A programs on workforce. The December 2009 Fisk Report addressed the creation and use of metrics. A statistical analysis by Eric Smith in 2010 provided quantitative data about specific programs. In addition, the committee heard metrics presentations in the reports from all of the great observatories and some other missions.

The task for our subcommittee had three components:

1. Summarize the recommendations from the Fisk and Smith reports
2. Pick out points which could be helpful
3. Identify additional useful metrics and criteria for assessing programs

The Fisk report suggested that metrics would be useful to (a) help justify the selection of new projects, explain why they are important and provide a basis for the allocation of resources, (b) measure progress towards NASA’s established goals, (c) measure the quality, relevance and leadership of projects, and (d) provide objective means for evaluation of success, often retrospectively. Even before the actual metrics, however, the Fisk report suggests in Chapter 3 that metrics need to follow from a clear mission statement for the division’s mission-enabling activities in general, and from mission statements for each element of the division’s mission-enabling program, which is to be assessed with metrics. We agree with this approach, and feel it would be helpful for the Astrophysics Division to develop such statements. Quoting the report:

*Each **division’s mission statement for its mission-enabling activities** should provide a rational framework for assessing how its portfolio ensures support for the full range of activities. Multiple possible components...may include technology-readiness enhancement, development of a junior workforce, sustainment of a mid-career non-faculty workforce, and maintenance of critical physical infrastructure.*

We suggest including developing new science ideas that could lead to new technology requirements and new missions, in addition to new techniques. Fisk 2009 also says that in the statements:

Programmatic relationships of mission-enabling activities to spaceflight programs should be clearly communicated.

The Astrophysics mission-enabling program is described only very briefly in the 2010 SMD Science plan section on “Astrophysics Research.” It would be useful for metrics to place there a Division-level statement along the lines Fisk 2009 suggests. The ROSES calls for mission-enabling activities each year might be a good place to repeat it, and to put the statements for each element in their sections. Fisk suggests that each program element needs evaluation of success against its mission statement, and justification for the resource allocation. These are more problematic than the first two. It appears that these metrics may be largely narrative and qualitative, and less well suited for objective and quantitative assessment of progress against numerical criteria if such exist.

“Evaluation” here seems to us to require some sort of data collection not currently done, at the proposal stage, in interim and final reports, and possibly later. There must be hard choices of what data to collect, how to evaluate it across all grants in each program element and for different program elements, and how to organize the resources to do the data collection and the evaluation on which the justification rests. Nevertheless, we agree with Fisk that the importance of the mission-enabling programs is so great and so easily overlooked, that we suggest a modest start to data collection and evaluation on each award. These data would be collected not to evaluate each award, but combined to understand, explain and evaluate the performance of the mission-enabling program elements in the division. It would be counter-productive to overly burden proposers and managers with more reporting and paperwork than is needed to justify the programs they use and manage. We agree with Fisk that metrics must be applied with care, appropriately and somewhat differently to each program element. We are also aware of the dilemma that in fact metrics may be used to compare program elements and even between divisions and so on (as Jon Morse pointed out to us on April 28).

We suggest that metrics as detailed below could be gathered without imposing onerous burdens on PIs and their institutions by selective addition of simple fields in the input data, and by requesting it as part of final reports. In particular information about the total number of individuals involved in the projects, net FTEs, students and post-docs would allow assessment of workforce impacts. PIs could be requested to provide simple updates for several years after the conclusion of their projects to allow tracking of publications and other outcomes from the sponsored research. We strongly urge NASA to implement a metrics collection and evaluation capability for all these programs. We note with approval the metrics abilities and experience of the current NASA great observatories and their GO programs, and suggest considering subcontracting this task to one of them, to AdS, or to one of the professional societies.

We liked the numerical metrics compiled by Eric Smith. His use of “proposal pressure” is a one useful independent variable for quantitative assessment of balance between competing elements that NASA seeks. Smith’s data come from several years of proposal inputs, and therefore allow tracking of trends over time. On the other hand,

most or all of the data are from the initial submissions, and are not amenable to post-facto assessments of outcomes. We suggest that additional useful information could be gathered without imposing onerous burdens on PIs and their institutions by selective addition of simple fields in the input data, and by requesting it as part of final reports. In particular information about the total number of individuals involved in the projects, net FTEs, students and post-docs would allow assessment of workforce impacts. PIs could be requested to provide simple updates for several years after the conclusion of their projects to allow tracking of publications and other outcomes from the sponsored research.

In many cases metrics will be used by people who are not intimately familiar with the technical details of the research programs, but who may influence future policy. The metrics will be most useful if their meaning and relevance is easy to understand, if they are easy to visualize through graphical presentation, if they allow fair comparisons of different programs and if they fairly track parameters of interest over time. Depending on the goals of the assessment the metrics may be rigorously numerical, or more anecdotal and descriptive. We identified four families of metrics.

Some of the metrical data we suggest could be part of each proposal, saved for selected proposals. Some (especially in categories A, C and D below) could be entered by the PI or their administrator during the performance period, and/or could be part of the final report, which is captured by NSPIRES or other database (e.g. grants.gov). Some kind of summaries of the metrical data might be made available to NASA program managers whenever they need them, and the program managers might be asked by the division head for mission-enabling programs to discuss and informally assess the deposited metrical data regularly (e.g. quarterly) until enough insight is gained to start using it seriously for evaluation.

The AdS database and tools can facilitate the identification of publication data that might be assembled now for recent selections by searching the journal databases for acknowledgements to specific grant numbers or classes of grant numbers. This will only apply to journals that allow such acknowledgements, but many do. The AAS would probably be happy to ask their journals to cooperate. The division could look into what journals would cooperate, and the value of counting grant acknowledgements in cooperating journals only.

The use of metrics for the enabling technology programs will be a learning experience. It should not be expected to work right away, but to take a while to mature into an efficient useable tool. Some resources and management time may be necessary to begin, but could be kept to reasonable levels if the idea is accepted soon and developed in the context of possibly critical times near for space-based astrophysics research.

We suggest four groups of metrical data be collected:

A. Metrics of science value of the supported research at the end of the grant.

These could be reported in the final report, in a form captured by the NSPIRES or other database. Items 1) through 4) are objective and quantitative, while items 5) through 8) may be more anecdotal. These items are largely measured and reported by the investigators who received the awards and performed the research.

1. Publications
2. # Refereed (List of publications also submitted, with links)
3. # Non-refereed, including conference proceedings
4. # Citations (weighted by year since publication)
5. Invited and contributed talks given or other professional dissemination of results
6. Students (undergrad-graduate-post-docs) who were supported
7. # Thesis produced using these programs (titles and links also submitted in database)
8. The number (and names) of States in the US involved in the program (for Congressional testimony)
9. Results that resolved previously identified, outstanding puzzles or that helped to uncover a major new puzzle
10. Spinoffs (for technology), including applications outside of NASA (patents pending)
11. Career advancement achieved by the participants (e.g., tenure) based on the work
12. Recognition given through professional awards or prizes based on the work

A. Metrics indicating relevance to NASA's stated goals.

These factors may be more subjective, and may be better identified and described by the discipline scientists within NASA who are most familiar with the research and its impact.

1. Influence of project on other programs, as identified in the other program's proposal
2. Influence in designing or justifying future missions
3. Influence as follow-up science for other missions
4. Impact on programs in other NASA Directorates/ Divisions
5. Training PI's or Co-I's for other NASA programs or missions
6. Cited in Decadal or NAS reports
7. Category B could be explored with a "network" study tracing the names common to flight missions and mission-enabling programs and to PI's, and others. For this, "flight programs" should include recent highly rated but not selected Explorer proposals, and flight missions recommended in the decadal survey.

C. Metrics indicating influence on policy makers and public attitudes.

Creating and maintaining healthy perceptions of the value of NASA's work is important for assuring continuing support. Some metrics may be strictly numerical, whereas others may benefit from subjective and anecdotal narratives.

1. # Newspaper or media mentions, # key citations: NYT, Nature, Science, (other?)
2. # Press briefings
3. # Website hits, including inquiries, public and otherwise
4. # Government inquiries (from Hill, etc.)
5. Impact of EPO projects associated with the research, as reported by the investigators directly or by the facilitators of the activities.

D. Metrics addressing workforce concerns.

NASA's accomplishments have always been a source of motivation and inspiration for students, scientists and engineers in the early years of their careers. One goal of R&A funding is to encourage and enable such interest, and to ensure that our nation continues to have a healthy pool of workers capable of and interested in performing NASA's future work. While we encourage the formulation of metrics that can track such things, we recognize that there are many issues related to the privacy of individuals that may make these difficult to collect and disseminate. We also recognize that there will be some overlap between this category and those attempting to measure science value.

1. Number of students of all kinds who were involved in the supported research
2. Number of theses, undergraduate, masters, PhD level
3. Number of student-authored papers, both refereed and non-refereed (conference)
4. Number of students then continuing in astrophysics and in other basic and applied science fields
5. Number continuing in other quantitative/technical fields, management and policy
6. Number of program "graduates" participating in subsequent NASA programs
7. Number of graduates that lead/participate in subsequent NASA mission

Appendix 5: Astrophysics Research and Analysis (APRA) and Enabling Technology

S. Boggs, T. Greene (co-editor), M. E. Kaiser (co-editor), A. Miller, C. Walker

The APRA program funds a large number of diverse suborbital investigations, technology development efforts, and laboratory astrophysics research programs. Technologies critical for detectors and optics in COBE, WMAP, Planck, Herschel, Spitzer, HST, GALEX, FUSE, Chandra, NuStar (projected launch date February, 2012), Swift, Fermi, and other high scientific impact orbital observatories were first developed with funding from APRA and its progenitor technology development programs.

Suborbital Program - Balloons

Suborbital balloon investigations funded by APRA have constrained the geometry and composition of the universe, provided insights into the nature of dark energy and have detected energetic cosmic rays, gamma rays, and antimatter from distant cosmic explosions. The wildly successful COBE and WMAP cosmic microwave background Explorer missions were made possible by precursor balloon flights beginning in the 1970s (demonstrating bolometers, HEMT amplifiers, optics, and scanning systems). The currently operating Planck CMB satellite also relied on advances made in these balloon missions. Current polarization-sensitive focal planes employing TES bolometers, polarization modulation strategies, and developing filter technologies are currently being employed on the suborbital balloon experiments, EBEX, SPIDER, and PIPER all searching for signatures of inflation. The most successful of these technologies will very likely be used on the Inflation Probe. The IRAS, ISO, and Spitzer observatories all relied on far-IR telescope and detector technologies that were proven during balloon flights of the 1970s and 1980s. NuSTAR's predecessor, the High Energy Focusing Telescope (HEFT), was a balloon-borne experiment that carried similar multilayer optics and CdZnTe pixel detector technologies. The Nuclear Compton Telescope balloon payload has recently demonstrated high sensitivity, energy resolving gamma ray detectors similar to the technology planned for the Advanced Compton Telescope Satellite for the 2020 decade. This work follows the demonstration of the Fermi satellite's gamma ray Large Angle Telescope (LAT) engineering model balloon flight demonstration in 2001.

The Long Duration Balloon (LDB) flight capabilities from Antarctica and Sweden have dramatically increased access to 'near space' (20 to 42 km above sea level). An LDB flight can now last as long as ~45 days. The Ultra Long Duration Balloon (ULDB) program will push this time aloft to >100 days. The Super Pressure Balloon (SPB) technology used in ULDB will permit flights to operate at mid latitudes, opening up the entire sky to investigators, while providing generous payload weight (up to ~1 ton) and power (~ 1 kW) envelopes. This combination of expanded capabilities will allow investigators to perform science programs from balloons that previously could only be done from an Explorer class mission, but at a fraction of the cost. Current launch facilities can allow up to ~3 such payloads to be flown *each* year.

Suborbital Program - Sounding Rockets

The sounding rocket program revolutionized our view of the universe by opening the door to entirely new portions of the electromagnetic spectrum accessible only from space. Pioneering science discoveries and technological developments included the ultraviolet (UV) radiation emitted by hot stars in Orion (Kupperian et al. 1958); the first extra solar X-ray science (Giacconi et al. 1962); observing the galactic center with a cryogenic infrared (IR) telescope (Harwit et al. 1966).

More recently, sounding rocket projects have extended their electromagnetic bandpasses and flown new technologies, such as grazing incidence telescopes, photon

counting imaging detectors, wide-field spectral imagers, high resolution spectrographs, 3-dimensional X-ray detectors, and polarimeters; many of which have yet to be implemented in orbital missions. These efforts provided first glimpses of X-rays from galaxy clusters, the Lyman alpha bulge of Jupiter, all-sky mapping in soft X-rays, and the far-UV properties of dust, to name a few. Jenkins et al. (1989) flew a far-UV echelle spectrograph with resolution of over 130,000 to observe H₂ absorption lines in the interstellar medium (ISM) between 912 -1120 Angstroms 1120, a record that still stands today (ASRAT, 2009).

Sounding rockets have had a major impact on the development of UV and X-ray missions including aberration-corrected holographic gratings for the UV used with micro-channel plate detector readout systems (HST/COS, FUSE), FUV optical coatings and X-ray calorimeters. Innovations in these areas continue today with programs employing path-finding detector technologies, such as Transition Edge Sensor (TES) micro calorimeters to offer breakthrough science from high spectral resolution observations of extended X-ray sources and, importantly, to advance the technology readiness level of state-of-the-art detector and read-out technologies which are prime candidates for ESA's Athena mission.

Contributions of balloon and rocket payloads to orbital astrophysics missions can be found at http://www.nap.edu/catalog.php?record_id=12862 (Table 3.1, p. 23 – 24 and Table 4.1, p. 35 respectively).

Supporting Technology

APRA-funded supporting technology programs are developing and testing pathfinder wave front control and star light suppression systems (deformable mirrors, holographic masks, occulters, and coronagraphs) for future planetary missions (e.g. TPF-I, TPF-C), pushing novel X-ray mirror fabrication technology, including nanolaminate composites, coating technology, and transmission gratings in support of the next generation of X-ray missions (Athena, Generation X, MAXIM). The development of new types of transmission gratings has potential as a breakthrough technology and could enable dramatically increased scientific yield, reduced instrument weight, cost and complexity. Both grazing and normal incidence large mirror systems have been identified in the NASA Technology Roadmap (November 2010 Draft) as key enabling technology needs. Other critical APRA-developed supporting technologies include the development of high-efficiency silicon diffractive optics for infrared spectroscopy (to be flown in the JWST NIRC_{am} and SOFIA FORCAST instruments), low temperature coolers for several detector technologies, and novel filter and masking techniques for several wavelength regions across the X-ray to microwave spectrum.

Detector Development

APRA funding was crucial for developing detectors for many of the highly successful programs mentioned above, and APRA-funded detectors have also been incorporated into SOFIA and Herschel instruments (e.g., bolometer arrays and heterodyne receivers) and will be key for future flight missions ranging from SPICA and Inflation Probe (TES

bolometers) to photon counting CCDs for exoplanet imaging missions. Several of these new detector concepts use similar cryogenic SQID multiplexing readouts, important for arrays of both TES bolometers and energy resolving high energy detectors, CMOS detector development is being pursued for soft X-ray detectors, but applications extend from the X-ray to the IR. CMOS technology offers many benefits over CCD's, including: lower cost, much lower power consumption, higher levels of integration, higher throughput and long-lived radiation tolerance. FUV/UV detector technology development efforts are also multi-faceted, targeting micro channel plate detectors, photocathodes, avalanche photodiodes, and GaN arrays. APRA has been the principal program funding the development of sub-millimeter heterodyne receivers and arrays, now pushing to THz frequencies. Development of these technologies must continue to allow detection of OI, HD, and other important lines in the far-IR with SOFIA and future balloon-borne and space-based platforms.

Laboratory Astrophysics

Laboratory Astrophysics provides fundamental information (spectra, cross sections, transition moments, etc., related temperature and pressure dependencies) crucial for the direct interpretation of observations or information that is needed to support/amplify/constrain models and test theories that are central to interpreting and placing the observations in solid scientific context.

Over the past 5 years, laboratory astrophysics investigations have spanned the electromagnetic spectrum, extending from the gamma ray region to the sub-millimeter, THz frequencies. Data and models have been generated supporting past and future missions ranging from Einstein, EXOSAT, ROSAT, ASCA, ASTRO-E2, EUVE, HUT, ORFEUS, FUSE, GALEX, IUE, ISO to Chandra, XMM, Astro-H, HST, JWST, Spitzer, Sofia, Herschel and Alma.

The impact of laboratory astrophysics investigations is not always immediately apparent. These can be long-lived activities but with a dramatic and long-lasting impact. Citation rates can lie dormant but then dramatically increase when the appropriate scientific application emerges, IR optical constants of ices were published in 1993. These data were suitable for I/S and Solar System studies with the KAO, the NASA IRTF and Spitzer. The citation rate was initially slow, however in 2004 this paper was cited 106 times (ISI) [101 ADS]. PAH lab work in conjunction with observations showed that PAHs are responsible for the interstellar IR emission bands. The rate of citations to this work continues to increase (2004: 62 citations (ISI) [60 ADS], February 2011: 174 (ISI) [168] ADS). As awareness of applicable laboratory astrophysics results increases, so does the use and citation of these results. Support for and a long view of this important and fundamental field needs to be maintained.

Ground Based Observations

In addition, APRA provides support for ground-based observations that entail a technology development component/demonstration or support the design of a future space mission by investigators ineligible for NSF funds (e.g. scientists employed by

NASA or other Federal Agencies). Recently this program has provided supporting observational data for a wide range of investigations including planetary systems formation, dust in protostellar cores in molecular clouds, high redshift emission line galaxies, and gamma ray bursts. The supporting technology development component for these investigations has included a coronagraphic integral field spectrograph to augment and interpret Chandra, HST, and Spitzer data and to provide test of a technology application for WFIRST and TPF. The RAPTOR network of optical 0.4m telescopes was developed to correlate Swift/BAT and Fermi gamma-ray observations. Support has also been provided in advance of Herschel for the development data analysis strategies to enable efficient use of the (then upcoming) large data volume.

Scientific Database

APRA programs such as laboratory astrophysics generate databases of their results for use by the scientific community. One example is the 'NASA Ames PAH IR spectroscopic database' (<http://www.astrochem.org/pahdb/>). Sustained APRA support enabled the measurement of the PAH data, the establishment of the database, and the development of specialized tools for the manipulation of the spectra so they could be applied by astronomers to their observations. There were 1,200 unique visitors during the first three weeks (2010) this database was available. As of this report, there have been 8,225 visitors from 374 cities and 74 countries.

While many of the APRA suborbital programs produce limited amounts of observational scientific data and publish the products of these observations, other programs have long duration observational campaigns at the culmination of technology development efforts. These technology development programs are not targeted toward providing science data products suitable for inclusion in archival databases. The analysis and interpretation of data acquired through these programs typically requires significant knowledge of the instrument and does not lend itself to archival research without considerable additional work by the instrument team. When warranted by the flight results, programs should have the option to apply for post-flight data processing (i.e. reprocessing) support to produce enhanced data products for archival databases and users.

Technology Advancement and Support

Looking forward, in addition to the near-term, mission-specific technology already under development, the NASA Technology Roadmap (November 2010) identifies five generic technology areas where additional advancements are required for astrophysics. NASA's APRA program over the past 5 years has been supporting multi-pronged technology investigations into four of these key areas: Detectors and electronics for X-ray and UV/optical/infrared (UVOIR); Optical components and systems for starlight suppression, wavefront control, and enhanced UVOIR performance; Low-power sub 10K cryo-coolers; Large X-ray and UVOIR mirror systems (NASA Technology Roadmap, November 2010 Draft).

No other government or commercial interests are investing in these specialized technologies, so funding by NASA through the APRA program is absolutely essential for success. Moreover, this development needs to be continued vigorously and increased if possible in order to supply technologies and precursor science needed for the increased number of Explorers over the next decade recommended by the NWNH Decadal Survey.

Metrics for Success

APRA plays an essential role in providing the technological and scientific seed from which future astrophysics missions grow. It is essential for the APRA program to develop and track clear quantitative metrics for assessing scientific and technological success. These metrics should include the specific mission or technology enabling relevance of projects, the scientific results of suborbital payloads, laboratory astrophysics investigations, and ground-based observations, and the use of APRA-developed technologies in past, present, and future flight missions.

At the same time, APRA must avoid the trap of risk avoidance and accept a substantial failure rate in the development of new technologies. A multi-pronged approach is needed to determine which technology works and has the most optimal performance characteristics. Often what does not work is very informative and can propel research toward what does work.

Metrics for the assessment of a successful APRA technology program should not be based solely, or even primarily, on refereed or non-refereed (instrument) journal publications. Citations are also an imperfect metric for this program. Their benefactors do not necessarily cite technology developments. Often, the more important and appropriate metric for APRA will be its success in developing science enabling technology and advancing it through the required technical readiness levels (TRLs) to flight. This advancement can occur either directly through APRA or indirectly with the help of the newly established Strategic Astrophysics Technology (SAT) program. The ultimate success metric of an APRA program is whether the program enabled scientific discovery by facilitating or directly providing game changing technology or flight readiness to future missions. The NASA Astrophysics Division should maintain a database of such metrics on the products of APRA awards and also use these data to evaluate the contribution of APRA research to NASA missions.

Furthermore, we suggest that the Astrophysics Division (and / or the Office of the Chief Technologist) also maintain a database of strategic technology needs and that it be used in planning solicitations and funding for future technology awards. This database should include technology needs of Explorer missions with high science ratings that require technology development, including missions selected as Category III and those that were not. Of course it should also include technologies that are important for achieving NASA's strategic science goals.

The numbers and types of skilled science and technology workers who first developed technologies and suborbital payloads via APRA and its predecessor programs and later

became key personnel of orbital science missions must be routinely gathered and compiled. The clear presentation of such scientific, technical, and workforce metrics are crucial for the APRA program to demonstrate its continued importance and to also focus long term technology development in areas critical for achieving NASA's strategic goals

Named Technology Fellowship

A core component of NASA's success rests on its ability to attract, train, and retain talented scientists. The participation and growth of these individuals into a stable workforce of experienced technologists/instrumentalists with novel ideas and the skills to successfully propose, develop, and lead those ideas into successful instruments and missions needs to be nurtured.

To facilitate the growth of this stable (e.g. tenured or equivalent), technically creative workforce, we advocate the institution of a named technology fellowship program targeted to early career scientists: mentored post-doctoral fellows, research scientists, and non-tenured faculty. An inclusive program is envisioned providing opportunities and support for early career scientists at a variety of proposing institutions such as universities, industry, FFRDCs, and NASA centers. A balanced program would permit one fellow per institution at a time. To enable the success of this fellowship program the duration of individual awards must be long enough to obtain significant results from the development a new technology component or a major contribution to an existing program for the mentored post-doctoral fellows. Funds for instrumentation should be provided and commensurate with the career phase and scope of the proposed project. Goals of this program would be to mature the mentored fellows to attain research independence with the ability to write successful new proposals and transition from post-doctoral positions into a stable career path (tenure track or equivalent) in astrophysics or related technology fields analogous to the Einstein, Hubble, and Sagan fellowship programs and to launch the ideas of the early career scientists in the permanent positions

Specifically, we advocate a named technology fellowship program that supports two distinct paths:

1. A fellowship, similar to the Chandra/Hubble/Sagan post-doctoral fellowships, with a duration of 3 years and taken in an existing laboratory. These would come with limited funding for instrumentation and would end upon acceptance of a faculty offer.
2. A NSF Career - like fellowship opportunity for junior faculty or non-tenure track permanent positions of 3-5 years. These would come with a substantial amount of money (\$300k -\$1M) depending on the available funds and the merit of the proposal and would serve to jump-start the careers of young faculty and researchers. Only those with tenure track or other permanent positions would be eligible to propose.

These fellowships would be non-renewable. However, this program could include an additional component to facilitate transitioning positions that are permanent but not tenure-track into tenure-track positions. A successful precedent for this is the Planetary

Science Fellowships for Early Career Researchers. This component would permit successful non-tenure track fellows to apply for start-up funding (~<300K\$) when they obtain tenure-track or equivalent positions.

We now address APRA-related questions from the Research Program Review Charter.

A. Does the APRA program:

1. Balance appropriately between suborbital flight opportunities (both for science and for advancing technology) and the development of enabling technology and of detectors?

YES. The examples above illustrate how APRA has contributed broadly to all of these areas.

2. Make initial investments in technology that are appropriate to NASA's future strategic missions?

YES. The examples above show how APRA investments in suborbital (balloon and rocket) payloads, detectors, and supporting technologies have been vital to previous and current strategic missions. Current investments (summarized over 5 years of recent awards) are relevant for future X-ray, exoplanet, gamma ray, UV/optical, and IR, THz, and CMB missions.

3. Allow PIs to develop technology to the level of readiness required for an Explorer proposal?

NO. The APRA program is structured well to fund the development of technologies at TRL 1 – 3 but does not develop technologies well at TRL 4 – 6 (TRL 6 needed for Explorers). The current APRA proposal process is too short term, provides insufficient awards, and is too science focused to develop technologies at TRL 4 – 6. The program needs to provide more continuous funding, place greater value on engineering developments and flight qualification, and increase the size of mid-TRL awards to improve in this area. APRA should also track and evaluate metrics of its funded programs to evaluate their value to future flight missions.

4. Fund laboratory astrophysics in a way that optimizes interpretation of data from current and future space missions?

SOMEWHAT. Laboratory astrophysics research often is applicable over many missions spanning decades of time (e.g., KAO, Spitzer, and JWST. Herschel, SOFIA). Therefore this area must be funded consistently for long time periods. The impact of this research can be dramatic and relevant to missions far into the future. A close tie into metrics therefore is critical for this type of are that involves long time scales to produce results.

5. Offer a range of award sizes suited to meet the challenges in these areas?

SOMEWHAT. The sizes of the current awards have been useful, providing many advances including the above examples. However, APRA awards are too small to raise detector and other technologies beyond TRL 3 (see question A.3). Funding levels have not been sufficient to allow flying highly rated suborbital payloads such as the Planetscope exoplanet balloon mission. Funding for suborbital payloads should be increased if the full scientific potential of LDB and ULDB platforms are to be realized. Current APRA award sizes are completely inadequate to develop the new technologies needed for exoplanet imaging and large aperture X-ray and UV/optical telescopes called out in the recent New Worlds New Horizons Decadal Survey.

The panel was concerned about the temptation to over-consolidate efforts into a few large technology development programs during periods of financial austerity. Despite tight funding constraints, it is important to retain a broad and diverse program that retains a key cadre of technology expertise. It is important to not decrease the number of awards in favor of funding only a few larger programs.

B. How should the APRA program change to complement activities in the Office of the Chief Technologist (OCT)?

It may be helpful if APRA reached an understanding with the OCT, where APRA focuses on supporting low TRL (1 – 3) efforts and informs the OCT of what high priority mid-TRL (4 – 6) efforts should be funded. Hopefully, the OCT can fund these mid-TRL efforts with the stability, award size, and appreciation of engineering focus that is now missing in APRA and other Astrophysics Division programs. The new Astrophysics SAT and TDEM mid-TRL programs have not started well. Their award sizes are too small for the work required, TDEM imposed excessive formalism (milestones, tests, reviews) given the small awards, and the solicitation was cancelled shortly before proposals were due in 2010. Hopefully the OCT can do a better job.

Appendix 6: Analysis of Data from NASA Astrophysics Missions

J. Neff, T. Lored, H. Smith

Data Analysis Funding in the Astrophysics Division

In order to accomplish its strategic goals and to maximize the scientific return from its missions, the Astrophysics Division funds data analysis in two ways. Guest Observer (GO) awards are associated with specific operating missions; they primarily fund analysis and timely interpretation of data from new observations. The Astrophysics Data Analysis Program (ADAP) funds analysis and interpretation of data in the public archives of current and past NASA missions. Most are multi-year awards for investigations using data from multiple missions. We were charged with addressing the strengths and weaknesses of these two funding models and the appropriate balance between them. Our general charge also asks us to address the program's role in enabling a "healthy scientific and technical workforce" and to identify metrics appropriate for assessing the program's effectiveness.

The panel heard presentations from the ADAP program and from several Guest Observer programs, and we reviewed documents from previous presentations and panel reviews. We were particularly interested in learning the GO program "best practices" and the metrics by which they evaluate their effectiveness. Approximately 10% of the HST and Chandra GO funding supports analysis of archival data from these missions that improves on the previous use of the data or addresses different scientific questions than the original programs that obtained the data. Analysis of data from other missions is supported so long as the primary emphasis is analysis of the HST and Chandra archives. Operating missions and the Data Centers are dedicating significant resources to ensure that the archives are readily accessible and contain the highest quality data. We discussed the ADAP program in the broader context of how NASA Astrophysics funds the analysis of data obtained with its missions.

Complimentary Strengths of ADAP and GO Programs

Both GO programs and a healthy archival data analysis program are necessary to produce the maximum return on NASA's investment in space science missions. The panel considers it crucial for NASA to clearly recognize that post-mission archive-based research, while happening second in time, is not second-rate, but complementary to active-mission research. It has the potential for discoveries with impacts as great as those made while a mission is active. Panel members have repeatedly encountered a community perception that the historical level of NASA's financial support for archival research indicates that NASA considers it a second-rate, maintenance activity. Archival data analysis will play an increasingly important role as we transition to an era with fewer missions, but with a tremendously expanded and largely unexplored database. This will require a steadily increasing level of funding as the missions phase into increased archival activity.

The primary strength of the GO programs for missions in which targets are selected by GO's is to ensure that operating missions are used effectively to acquire data of scientific interest. Real-time analysis of data feeds back into a deeper understanding of the instrument's capabilities and often raises new questions that can only be addressed by new observations during the lifetime of the mission (and frequently coordinated with ground-based observatories and with other missions operating at other wavelengths). Other missions operate in a survey mode or observe targets with pre-selected criteria. GO programs for these missions support the timely analysis of the data on targets in support of the mission objectives. The common denominator of all the GO programs is that they maximize new discoveries that depend on active mission resources and capabilities. HST and Chandra fund archive and theory programs that are specific to the missions and contribute to their overall success.

The principal strengths of ADAP complement the GO programs by ensuring that the archives are fully exploited to maximize the scientific return on NASA's substantial investment in acquiring the data. Comprehensive analysis of archival data also enhances the capability of current operating missions and leads to the scientific justification for future missions. Pioneering observations in new wavelength regimes or with unprecedented sensitivity frequently open up entirely new fields of inquiry. The data and knowledge base needed to understand these new results might take years to develop, long after the missions are no longer operating. They often require a multi-mission, multi-wavelength context beyond the scope of any single mission. Data obtained for a specific guest observer program often contain valuable information for entirely different studies (e.g. other spectral lines, other targets in an image). Post-mission recalibrations, and large-scale reprocessing of archival data to produce new databases of derived data products, can enable studies that are not possible during a mission's primary lifetime. Initial interpretations might be discovered to be incomplete or incorrect in the light of later observations or discoveries. New questions arise that were not anticipated during the mission. New data analysis methods can enable us to address new questions, or re-address old ones with improved sensitivity or accuracy. The scientific value of a complete archive might dwarf that of incremental databases available during a mission's primary lifetime. This is particularly true of all-sky survey missions, which might have a short lifetime and a very limited GO program. As we transition into an era with fewer operating missions, a well-funded archival data analysis program will play an increasingly important role in accomplishing the science plan of the Astrophysics Division, maintaining a healthy workforce (for more details, see the workforce section of the full panel report), and generating the science case for future missions.

ADAP Program History and Status

The Astrophysics Data Analysis Program was established in the late 80's to maximize the scientific return on NASA Astrophysics missions (before 2010 it was known as the ADP program). The volume and complexity of the archives has grown enormously since then, and the program has a long history of highly competitive, first-class science that was enabled by a multi-mission, multi-wavelength, science-driven approach. The

number of missions explicitly mentioned in the NRA has grown from 3 to over 30, but for most of the program's history the funding remained level at \$2 million for first-year awards. Until 2008, it supported multiyear programs up to 3 years, though 3-year programs required substantially more justification, so the typical program was proposed for 2 years of funding. Since 2008, 4-year programs have been allowed. In 2005, no funding was available for the program, and only half the historical level was available in 2006. Between 2007 and 2011, the available funding has grown steadily, with \$6 million available for new programs in 2011. The origin of the increased funding level (presumably due to the phasing out of the LTSA program described in the next paragraph) is not entirely clear to this panel, but it is a welcome development. Public comments on our website and solicited by panel members point to strong community support for the expressed goals of the program, but with a nearly universal opinion that the program has been under funded and heavily oversubscribed for many years.

Prior to 2005, a significant amount of archival data analysis (\$2 million available to support new awards each year) was also supported through the Long-Term Space Astrophysics Program (LTSA). LTSA supported individual investigator teams for long periods (up to five years) to conduct comprehensive studies focused around a scientific theme that benefited from archival data analysis, but incorporating whatever input was necessary, including ground-based observations, theoretical models, data analysis methodology research, and even analysis of new data from current NASA missions. There was an explicit effort made to devote about half the funds to "junior" awards. Awards to more senior researchers supported investigator teams. The principal difference was a 3 month/year salary cap on the senior awards. The LTSA program was well funded, highly competitive, and very well regarded by the astronomical community. It could be characterized as "supporting people rather than programs." As the program has phased out, and the ADAP funding level ramped up, it appears that the investigators supported under LTSA have not seen the ADAP as a mechanism to continue long-term funding of their broader research objectives. The objectives for ADAP spelled out in ROSES are substantially different from the LTSA program objectives.

With this background, we looked closely at the proposal pressure and oversubscription rates for the past 5 years as the ADAP program funding ramped up. In the mid-90's there were approximately 200 ADP proposals each year competing for \$1.5M in first-year funds. Between 2006 and 2008, despite a significantly higher budget, fewer than 100 proposals were received each year. A long history of 20 to 30% acceptance rates suddenly shot up to the 40-50% range. For the past two years, the number of proposals has gone up and acceptance rates have decreased. We identified several possible reasons for this short-term fluctuation. In part, it was a community reaction to the program cancellation in 2005 and the low level of funding available in 2006. With a very long history of very low acceptance rates and then the shock of the 2005 cancellation, it is possible that a portion of the community no longer felt the time investment in proposing was worthwhile. There is some evidence this was happening even before 2005. While the ADAP and LTSA funding have been effectively combined, the ADAP program does not cover the same ground as the LTSA program did. This will probably

change somewhat as more 3 and 4-year programs are accepted. Given the low acceptance rates and relatively large time investment in preparing a proposal, it is also possible that a large portion of the community has shifted their efforts toward the GO programs.

Because of the increased funding and reduced proposal pressure, there has been a short-term programmatic flexibility that has enabled funding (through ADAP) of the analysis of newly acquired data from missions that had their GO programs recently terminated. We note that such proposals have very high acceptance rates (they were already reviewed and awarded observations via GO programs), which raises the overall ADAP acceptance rates. We also note that as missions phase into the archives, there will be a brief period of greatly increased proposal pressure from the community of experts associated with that mission. Careful management of the program will benefit by increased feedback from the broad archival "user" community.

The balance of opportunity is shifting. The Spitzer archival program was transferred from GO to ADAP funding in 2009, and the next few years likely will see more missions make the same transition. The current program scientist is making a heroic effort to inform and engage the community. We feel community input is crucial to anticipate and avoid imbalances in "supply and demand" and to guide the program as we transition into a new era in which archival data analysis might eventually produce the majority of the new science in the NASA Astrophysics portfolio.

The new ADAP tools component

Beginning with the current NRA (ROSES 2011), ADAP has introduced a new element supporting projects producing new databases or developing new data analysis methodology and software. These projects must enable new or improved science with archival NASA data, but they need not undertake such scientific investigations themselves. This new component recognizes that the size and complexity of datasets, and the complexity of the scientific questions posed by researchers, in some cases can require reprocessing or information science research at a scale that justifies independent funding, as a valuable science-enabling activity.

Policy background. Astro2010 speaks to the growing importance of data analysis tools research and development (see especially Chapter 5). Noting that archives are "central to astronomy today" and of growing importance, the survey states, "Publicly accessible data archives can multiply the scientific impact of a facility or mission -- for a fraction of the capital and operating costs of those facilities or missions." But to accomplish this, Astro2010 notes, "The data explosion and the long-term need for the ability to cross-correlate enormous datasets require archival data preservation beyond the life of projects and the development of new analysis and data mining tools." The survey goes on to make more specific observations about this need, and recommendations as to the type of research and tools needed and how they could be effectively hosted and maintained.

Findings. In informal communication with the community, panel members repeatedly encountered a perception that the Astrophysics Division has not yet found a successful mechanism to support science-enabling data analysis tools research in a sustained fashion. Short-lived elements of earlier ADAP NRAs, the LTSA program, and AISRP all have attempted varying levels of support of such activity. The panel is concerned that the new element is unlikely to be successful unless lessons are distilled from these past attempts. NASA should undertake a more thorough examination of this issue, both internally and in consultation with the community of astronomers and information scientists in the burgeoning areas of astrostatistics and astroinformatics.

For a number of years, ADP invited "Type 2" proposals that could have a significant information systems component, but which had to build and use tools and databases in the course of a scientific investigation. But the perception among the interdisciplinary astronomy-information science research community is that panels viewed Type 2 ADP proposals with a strong methodological component unfavorably; investigators suggest they had trouble competing with more science-focused proposals. After 2004, support of Type 2 projects ceased. Until this year, the ADAP NRA discouraged proposals with a significant tool-development component, and the only R&A program supporting science-enabling research was AISRP, whose last NRA was in 2008.

AISRP was unique, both in being the sole information sciences ROSES program, and in supporting research across the Astrophysics, Planetary Sciences, Earth Sciences, and Heliophysics Divisions. AISRP supported an incredibly diverse research portfolio. AISRP was also charged with supporting a significant amount of high-risk research (e.g., TRL 1 & 2). The resulting program was impressively diverse, but its funding level was not appropriate for its breadth and other unique requirements. Very few astrophysics tools proposals could be funded in a given cycle (typically one or two at most in the last years of the program). With some of these being high-risk, it is clear AISRP could not be expected to establish, let alone sustain, a significant amount of methodology research supporting data analysis needs in astrophysics.

The 2001 Space Sciences R&A Senior Review reviewed R&A programs at the level of "clusters" that grouped together programs with common science concerns. AISRP was the sole member of the "Information Systems Cluster". The review found important strengths for the program: it supported needed tool development, it encouraged interdisciplinary interaction, and it initiated unique EPO projects involving information system tools and products. But the review also identified two concerns: the program was not supporting enough high-risk/high-innovation projects, and few of the tools produced by the program were in wide or regular use. The review made a number of sensible recommendations focused on addressing the last concern: requiring projects to produce open-source software, creating a visible means of informing the scientific community of new tools and making them easily available, and creating a means to track use of the tools.

AISRP funding subsequently decreased, but the program made an effort both to fund more high-risk projects and to set up the dissemination and tracking infrastructure. A

small team received AISRP support to build a code repository, but the support (financial and otherwise) was never sufficient for the task. In fact, the repository server, which has been operating for some time without funding, crashed during the course of this panel's deliberations, and there is no support channel to resuscitate it. Partly motivated by the needs of AISRP, an independent team developed the DASHlink web-based collaboration environment (<https://c3.ndc.nasa.gov/dashlink/>), but regulatory issues have hampered its use by AISRP investigators. There currently is no repository hosting AISRP-developed tools. We urge the ADAP program to develop a specific plan to provide sustainable distribution of tools. NASA should consult directly with the AISRP repository and DASHlink teams to mine their experiences for insights to guide this plan.

We note that all the AISRP-supported scientists we contacted for perspectives on the program's strengths and weaknesses offered unprompted praise for the quality of the AISRP program leadership, particularly in regard to inspiring and encouraging science teams despite dwindling program resources, and efficiently organizing interdisciplinary program reviews, often cited as the most challenging but most rewarding reviews in the experience of participants. The ADAP program should take advantage of the leadership and management experiences of AISRP in planning its support of the new databases/tools element, particularly for planning proposal reviews; effective review of proposals to the databases/tools element may require a separate interdisciplinary panel.

Despite its weaknesses, AISRP can boast of some high-impact projects. The Virtual Astronomical Observatory (VAO) got its start as an AISRP project. The SAOimage DS9 image viewer, widely used by astronomers and with ongoing active development, started as an AISRP-funded major overhaul and rewrite of an earlier viewer, incorporating many groundbreaking features. The Python high-level computing language is becoming increasingly important in astronomy; it is heavily used by HST and Fermi for mission software development, and is the officially supported data analysis language for LSST. AISRP was an early supporter of Python software for astronomy, including partial support for development of the NumPy and SciPy packages supporting general scientific computing in Python. Notably, all three of these developments have very general applicability. This generality provided opportunities for significant post-AISRP support (via NSF and other sources for VAO, via Chandra and HEASARC for DS9, and via a variety of academic and commercial avenues for NumPy and SciPy); such support was probably not available for more specialized AISRP-developed tools. It is also worth noting that the ADAP element as written does not target such general tools; we do not know where projects like this could find a new start with NASA support.

Support mechanisms for databases/tools. ADAP should explicitly address how it can provide a sustainable distribution plan for new tools and enable the astrophysics community to find ADAP-produced tools. There may be a role for the Data Centers here. We suspect other programs (possibly in other divisions) could benefit from such infrastructure, so a centralized effort may be warranted, extending the role of Data Centers to include hosting or at least indexing of databases and software developed by NASA-supported researchers outside of the Centers.

A panelist coined an aphorism: "An un-curated dataset is useless." The same is likely also true of software tools: unmaintained software is doomed to quick obsolescence. ADAP needs to consider how software developed with program support will be maintained. It cannot hope to support all software indefinitely, but it should play some role in making sure its investments in databases and tools pay off over the long term.

A strength of the new ADAP element is bringing expertise from information sciences disciplines (computer science, statistics, machine learning, engineering, applied math, etc.) into astronomy where appropriate. There is great enthusiasm among information scientists for astroinformatics and astrostatistics research. But interdisciplinary research can be expensive when collaborators from information science disciplines are included. ADAP administrators and proposal reviewers need to be aware of interdisciplinary funding practices in order to properly weight them in reviews and selections.

Assessing the Impact of ADAP

Like the other programs being reviewed, the ADAP program is designed to enhance currently operating missions and to enable future missions. Their impact must be assessed in the context of the successful missions. Any measure of the productivity of missions must attempt to credit the mission-enabling activities. Individual investigators often receive support from both ADAP and GO programs, so standard metrics of scientific productivity (peer evaluation, publications, new proposals, etc.) should lead to criteria that share the credit among archival data analysis and operating missions.

But its largest measure of success is that ADAP maximizes the scientific return of previous missions, and thus contributes to the ultimate mission success. The Astrophysics Division expends a tremendous effort through its operating missions and data centers to create and maintain a permanent, public archive of all the data obtained by its spacecraft and the calibration information necessary to extract the best science possible. This is highly commendable and has created and sustains a research community that is actively involved in designing and optimizing future missions. The archives themselves are open to and widely used by scientists worldwide, so ADAP funding helps maintain US scientific international competitiveness.

Science-enabling research may require different support structures and different metrics and criteria for measuring success than more typical astrophysics research. Database and methodology research has features of both "normal" scientific research, and of technology development research, and support mechanisms and evaluation criteria must recognize this. ADAP-supported tool development projects may be expected to produce publications in the information sciences literature. Publication-related metrics must account for differing publication traditions between disciplines. For example, in statistics technical reports are cited much more frequently than in astronomy and are seen as important documents; most departments host archives of technical reports. Some conference proceedings are considered more prestigious publication venues than

refereed journals (e.g., for major conferences where papers are by invitation). The most prestigious publications are "discussion papers" (in journals and proceedings). These papers are not refereed per se; the authors' submission (usually invited) is published as submitted, but accompanied by published commentaries by other experts in the area, often followed by an authors' rejoinder. We suggest that the new ADAP element request grantees to identify "high impact activities" to help the program track such interdisciplinary activity. These could also include non-publication activities, such as organizing special sessions on astronomy topics at meetings in other disciplines, or sessions on information science in astronomy meetings. To the extent that the main product of a databases/tools project may be an online database or software package, publication metrics may need to be down-weighted in evaluation criteria. This will be project-dependent.

Finally, ADAP should deploy metrics specific to software and databases. Examples include lines of code, comment coverage, unit or regression test coverage, quantity of documentation and examples, database size (bytes and numbers of records), as well as usage statistics such as numbers of downloads and forum activity. Criteria must consider that activity will be low for a new product; where possible such metrics should be updated over time well after project funding has ended, to measure enduring impact. If possible, NASA should work with journals and ADS to enable tracking of database and software package citations in published papers as a further measure of continuing impact.

The Future of ADAP

In addition to the multi-mission archival data analysis historically supported by ADAP, the program has expanded in the past 3 years to include several new elements. In 2011, ADAP adds support for databases and new data products/analysis tools and continues support for analysis of new data to investigators with previously awarded observations from RXTE, XMM-Newton, and INTEGRAL, which was added in 2010. Support for laboratory astrophysics databases was added in 2008. At the same time, Kepler, Spitzer, and WISE archives have been added to the list of over 30 space astrophysics missions with public archives supported by ADAP.

The planned budget profile for the next five years includes a 20% increase in 2012 followed by smaller but significant increases in subsequent years. We applaud the foresight this demonstrates and believe that it will enable a healthy scientific program. But as the past few years demonstrates, there is a need for careful strategic planning that involves feedback from the space astrophysics data analysis community. We urge NASA to create a mechanism analogous to a mission-based "Users Committee" to provide community feedback on issues related to archival data analysis. One issue frequently raised is the level of effort required to prepare an ADAP proposal relative to the likelihood of success and funding level, especially compared to the HST and Chandra archival programs. A users committee could provide feedback on streamlining the proposal process; perhaps a 15-page proposal and full budget is not required to justify most archival data analysis requests.

At first glance, the funding of analysis of new data from operating x-ray missions does not appear to be consistent with the established program goals. On the other hand, funding is vital to maintain US competitiveness in these international missions, and it makes little sense for investigators to wait a year for the data to become public and then apply to ADAP to support their analysis. If new observations are supported, the exclusion of support for those obtained as "Class C" is artificial. If this is to become a permanent element of ADAP, careful planning is needed to ensure that sufficient funding is available so that it does not infringe on the primary goals of the program. If success rate metrics are applied in evaluating program effectiveness, this element of the program should be evaluated separately from the strictly archival analysis proposals.

Appendix 7: Astrophysics Theory Program

Primary Authors:

Sterl Phinney (California Institute of Technology)

John Blondin (North Carolina State University)

To quote page 25 of the Fisk report

The science mission teams require the best possible knowledge of mission objectives and their science context if spaceflight missions are to be developed in a cost-effective manner that maximizes the return on investment... Modeling, usually by numerical simulations, and theoretical research are required to turn measurements and observations into physical understanding.

Background and Goals of NASA's Astrophysics Theory Program

NASA supports astrophysical theory to a) facilitate the interpretation of data from space astrophysics missions, and b) to develop the physical understanding that leads to predictions that can be tested with space astrophysics missions.

Examples of (a) are so numerous as to defy any comprehensive listing. A small selection: theoretical modeling of accretion led to the interpretation of mysterious X-ray sources as the discovery of neutron stars and stellar black holes, and the subsequent measurement of their physical properties by missions from UHURU and SAS-3 to RXTE and Chandra. Theoretical modeling of stellar oscillations has allowed the Solar Dynamics Observatory and the Kepler mission to measure the interior properties of the sun and many more distant stars. Theoretical modeling of photo-ionization regions (incorporating laboratory astrophysical data) has allowed missions from Copernicus to HST to be used to determine the properties of the interstellar and intergalactic gas, and the gas around quasars. Theoretical modeling of stellar dynamics has allowed HST data to be used to discover and measure the masses of black holes lurking in the centers of galaxies. Theoretical modeling of exo-planet eclipses of stars motivated and

interpreted the Spitzer and HST observations that discovered the first exo-planet atmospheres.

Notable examples of (b) include: 1) the prediction of fluctuations in the cosmic microwave background, which led to the Nobel-prize winning COBE mission which first measured them; 2) the predictions that the cosmological parameters of the universe could be measured from the angular power spectrum of those fluctuations, which led to the Balzan and Dan-David prize-winning BOOMERanG NASA balloon payload and the Shaw and Gruber prize-winning NASA WMAP mission, which respectively discovered and measured them in dramatic detail.

Missions in development continue this trend: 3) predictions of gravitational waves from a diversity of astrophysical sources drive the LISA mission, while 4) predictions that inflation-produced gravitational waves will produce polarization signals in the cosmic microwave background drive NASA balloon experiments and the proposed CMBPol experiment. 5) Theoretical prediction of baryon-acoustic oscillations and modeling of the growth of cosmic structure motivated the proposed JDEM and WFIRST missions.

The importance of theory to NASA missions is exemplified in the role that ATP-funded theorists play in Mission Definition Teams and Science Working Groups. Current examples from missions in development include Tim Kallman, GEMS Project Scientist; Steve Reynolds, GEMS SWG and NuSTAR SWG;

Current Status

Theory broadly relevant to NASA's mission science and objectives is supported in the ATP program at about \$12M/yr, typically in \$120k/yr grants for 3 year periods, about 35 selected per year. Group proposals are not allowed. Selected proposals are compared against NSF, DoE, Hubble awards to prevent duplicate funding.

The GO programs (Hubble, Spitzer, Chandra) also support theory at the level of about \$2.5M/yr, but this is more restricted to modeling in support of data analysis from the specific missions, and is typically in \$70k grants for a single year, about 30-40 selected per year. Over the past decade, about 25% of NASA's prize postdoctoral fellowships (such as Hubble, Chandra, Sagan fellowships, total about 30 per year) have gone to young theorists, as have about 25% of NASA earth and space science graduate fellowships (total about 8 per year). These correspond to an additional \$2.5M/yr.

The single-investigator model has proven highly successful in driving a diverse spectrum of theoretical investigations supporting the breadth of NASA missions. However, there are key science questions that cannot be adequately addressed in this manner. Many of the theoretical questions posed by NASA observations demand an approach that integrates a diverse range of physics with contributions from theory and large-scale computation. Such complex theoretical challenges demand a focused collaboration of scientists, often spanning multiple institutions.

While the overall funding has risen to match demand in the past decade, the selection rate within the ATP has remained constant at roughly 20%. Such a high, consistent oversubscription has led to a) a large percentage (40%) of proposals ranked E and E/VG that are not funded and b) a negative feedback on the community resulting in fewer people actively pursuing NASA-related theory. Anecdotal comments from the community suggest that highly-regarded, successful scientists are no longer submitting proposals to this program given the amount of effort required to submit an excellent proposal that has a perceived small random chance of being selected.

Suggested Metrics for Success

Quantitative metrics:

- 1a) Proposal pressure (received/funded proposals or requested/awarded funds)
- 1b) Peer review assessments (e.g. fraction of E, VG funded)
- 2a) Publications resulting from funded research (per dollar expended)
- 2b) Citations to those publications (per dollar expended)
- 3a) Workforce development (numbers of students and postdocs supported per dollar expended)
- 4a) Evidence of impact of the theoretical work on mission development supported theorists on Mission Definition Teams, Science Working Groups, Science and Instrument Teams etc for both Strategic Missions and Explorers and suborbital payloads.
- 5.) Distribution of awards across scientific topics relevant to NASA missions

Anecdotal metrics:

- 3b) Workforce -future career success of those funded (as PIs or students/postdocs on grants)
- 4b) Paradigm-shifting theoretical work which leads to new missions or major new discoveries (e.g. those in the initial paragraphs).
- 6) Demonstration of significant progress on major identified theoretical challenges from previous (2000) decadal survey.

In deciding criteria for success based on these metrics, we suggest 2 and 3 should have minimum values for the program to be judged 'good', but 4, 5 and 6 should be the criteria for 'excellence'. Theory may do very well under metrics 2 and 3, e.g., compared to technology or payload programs, but 4, 5, and 6 quantify the direct impact on enabling SMD science. Mark Twain noted "One gets such wholesale returns of conjecture out of such a trifling investment of fact", or the common chairman's refrain to committees proposing to hire experimentalists "but theorists are cheap - they just need a cubicle and chalk." This does not mean that all NASA funding should go to theorists.

To be of scientific value, they need observational puzzles to work from, and observational or experimental tests of their predictions. But theorists provide a huge scientific return for a small additional investment compared to mission costs, and the importance of this is best judged by criteria 4 and 6. Criteria 5 is a means of quantifying a minimum value of 'small'. The NASA Astrophysics Theory Program should be of sufficient capacity to maintain active theoretical investigations in all astronomy sub-fields relevant to current and future missions.

Suggested Changes

Address the impact of higher proposal pressure in ATP (5 going to 6:1) than in any other area of R&A (3 or 4:1), as attempted in the PY 2013 budget plan. Some GO programs (e.g., HST) review theory proposals alongside archival and observing proposals. Those programs has an oversubscription rate more like 4:1, consistent with most other APRA programs. People give up writing proposals at higher oversubscription rates, and this may be happening with ATP.

The solicitation for ATP should call out

- A. A separate (not in competition with the current single-investigator ATP awards), new, large `research network' program in computational theory and data analysis, for programs too large and interdependent for the current single investigator model, attacking a single problem. We support the networking model recommended by the 2010 Decadal survey. We do NOT recommend group proposals on diverse topics.
- B. Exoplanets
- C. Theory in support of Explorers as well as Strategic Missions (current language very confusing).

As in other R&A areas, the funding cycle is often poorly aligned with the hiring and research cycle, so an additional year spending flexibility would be valuable (e.g. N+1 years period of performance for N years funding).

Maintain a regular collection of supported publications and citation statistics (via ADS study of grant numbers, PI names -will be incomplete e.g. when students or postdocs are sole authors excluding a kind PI, or grant number not given in the paper, but better than nothing).

An annual survey of PIs whose grants have finished asking for a

- A. Summary of workforce supported by the grant (undergrad, grad, postdoc, summer salary etc)
- B. Paragraph describing breakthroughs [could perhaps more usefully be asked 5 years after grant end to allow gestation], service on teams such as those listed in 4a above.

Overall response to the survey will be better if the community sees the responses to be used, and understands their benefit to the community in terms of defending or increasing theory funding.

Appendix 8: The Origins of Solar Systems Program & Exoplanet Research & Analysis

Primary authors:

Joseph Nuth (NASA/GSFC)

Thomas Loredo (Cornell University)

Introduction

The Planetary Science and Astrophysics Divisions jointly direct the OSS program. This report focuses on how OSS addresses the objectives of the Astrophysics Division (AD). The AD manages OSS investigations aimed primarily at finding and characterizing extra-solar planetary systems, but it has some interests in other OSS-sponsored science and supports such science on an ad hoc basis. The Planetary Science Division (PSD) manages other OSS investigations, including investigations focusing on the formation and evolution of solar system planets, cosmochemistry, and research on the connection between star formation and the formation and evolution of exoplanetary systems. The OSS program supports exoplanet theory with direct ties to data; more purely theoretical research is expected to be supported elsewhere, e.g., by the Planetary Atmospheres, Astrobiology, and Astrophysics Theory programs, or by NSF. The OSS program also supports ground and space based observations of solar system objects that pertain to the formation of our own solar system and of protostars and star forming regions to constrain theoretical studies of star and planet formation and early evolution.

Support of exoplanet research is a large and growing component of the OSS program, yet the program's origin dates to a time before exoplanetary systems were discovered. To set the stage for a contemporary look at the program, this report begins with its historical background, and then summarizes recent policy developments that provide an up-to-date context for evaluation. Only then do we turn to findings, in subsections that begin with a list of highlights, followed by discussion.

Historical Background of the OSS Program

During the 1980s advances in infrared technology and radio techniques presented the possibility of observing the formation of nascent stellar systems while improvements in ground- and space-based observational techniques held the promise of detecting the presence of extrasolar planets around nearby stars. Studies of meteorites, models of

nebular collapse and observation of the details of the modern solar system were already beginning to place constraints on the processes that operated within such nebulae and there was hope that such constraints could shed light both on the origin of the terrestrial planets and on chemical evolution leading to life. It was suspected that the presence of ionized gas and dust could have played a major role in controlling the evolution of collapsing nebulae. The Origins of Solar Systems (OSS) Program started in 1989 as collaboration among the Astrophysics, Earth Science, Heliospheric Physics, Life Sciences and Solar System Exploration Divisions at Headquarters; SSED administered the program. The major goal of the program was to facilitate interdisciplinary communication and research across the boundaries of the NASA Headquarters scientific divisions in order to advance our understanding of the processes leading to the formation of our own solar system, the potential for the formation of others and the possibility for detecting extrasolar planets and even life beyond the Earth. In addition to funding interdisciplinary research projects, the OSS Program through its MOWG worked to establish a Gordon Conference on the Origins of Solar Systems to foster communications between researchers in Astrophysics and Planetary Science that continues to the present day.

Advances during the next decade included the detection of the first extrasolar planetary system (around a pulsar), detection of hot Jupiters, some of which transit their primary on a frequent basis, observation of ever increasing detail in the collapse of protostars, increased precision in the timing of events in the solar nebula based on the analysis of short-lived isotopes in meteorites and continued improvement in the sophistication and degree of detail in models of nebular processes. While research priorities of the Heliospheric Physics and Earth Sciences divisions shifted to space weather and Earth observing, those of Astrophysics were shifting more towards origins, and the division began to pay more attention to the research activities funded by the Origins of Solar Systems program. In 2001, based primarily on its interest in the detection of extrasolar planetary systems and in preparation for missions such as SIM and TPF, the Astrophysics Division began to fund ground-based studies of planet detection as well as some theoretical work related to the origin and evolution of the systems that had been observed to date, both through the OSS program and through allocations of observing time NASA managed at ground-based observatories. While the funding provided to the Origins of Solar Systems program through the Planetary Sciences Division has increased slowly since 1989, the funding provided by Astrophysics since 2000 has grown more rapidly mostly due to the great increase in exoplanet proposals from the Astrophysics community.

Contemporary Policy Context

The primary role of the AD in the current OSS program is support for and management of exoplanet research. We highlight here recent policy statements establishing a special role for exoplanet research in NASA's research portfolio; many of these statements also highlight the importance of broader origins research in NASA's mission.

The 2010 NASA Strategic Plan articulates four "outcomes" (broad goals) to guide NASA's science programs in earth science, heliophysics, planetary science, and astrophysics. The astrophysics outcome is: Discover how the universe works, explore how the universe began and evolved, and search for Earth-like planets. The 2010 SMD Science Plan identifies three objectives for NASA's astrophysics programs emanating from the astrophysics outcome; OSS-sponsored exoplanet research addresses two of these objectives:

Objective 2: Understand the many phenomena and processes associated with galaxy, stellar, and planetary system formation and evolution from the earliest epochs to today.

Objective 3: Generate a census of extra-solar planets and measure their properties.

The Science Plan describes five programs addressing the three astrophysics objectives. One of these is the Exoplanet Exploration Program (ExEP), devoted entirely to exoplanet studies (supported via missions, the Exoplanet Science Institute, and Sagan fellowships). The other four programs (Physics of the Cosmos, Cosmic Origins, the Explorer Program, and Astrophysics Research) are all of broad scope, supporting missions that target a variety of astronomical sources. ExEP is unique among the astrophysics programs in focusing on a single source class---exoplanetary systems---highlighting the special role exoplanet research plays in achieving NASA's science objectives.

The Science Plan identifies exoplanet research as an astrophysics rather than as a planetary science objective. This recognizes that the types of technologies used to detect and study exoplanetary systems, the quality of the available data, and the level of detail of the theory are typical of other current astrophysics research rather than of planetary science research. However, much of the science required to make sense of the data is traditionally in the realm of planetary science, including the study of few-body dynamical systems, planetary structure, and planetary atmospheres. Notably, after significant discussion, the American Astronomical Society placed exoplanet research under the Division of Planetary Sciences (DPS), even though most exoplanet discovery and much exoplanet research at the time of the decision was being done by stellar astrophysicists. Clearly, sound exoplanet research requires strong interaction between the astrophysics and planetary science communities. The OSS program sensibly brings together planetary scientists and astrophysicists to address NASA's exoplanet and broader solar system origins objectives.

President Obama's 2010 National Space Policy assigns a number of responsibilities to NASA. Among them is the responsibility to continue a strong program of space science, including observation, research, and analysis. Two specific goals in the President's policy are to understand the conditions that may support the development of life, and to search for planetary bodies and Earth-like planets in orbit around other stars. OSS-sponsored exoplanet research directly addresses the latter goal of the National Space Policy, and sets the stage for future research that will examine evidence for life on exoplanets, addressing the former goal.

The NRC "Astro2010" decadal survey identified three science objectives to guide the next decade of US astrophysics research: searching for the first stars, galaxies, and black holes ("Cosmic Dawn"); seeking nearby, habitable planets ("New Worlds"); and understanding scientific principles ("Physics of the Universe"). Again it is notable that the New Worlds objective is unique in focusing on the study of a single class of objects, exoplanets. Astro2010 articulated a "complementary effort of space-based, ground-based, and foundational, core research" for addressing each objective. For exoplanets in particular, Astro2010 "strongly supports a vigorous program of exoplanet science that takes advantage of the observational capabilities that can be achieved from the ground and in space." As one highlight, the importance of exoplanet research led the Astro2010 committees to recommend adding an exoplanet component to the previously planned JDEM cosmology mission (e.g., detecting exoplanets via gravitational microlensing); the expanded mission is WFIRST. Astro2010 emphasized the importance of a continued strong investment in ground-based exoplanet observations, especially with the radial velocity technique, to supplement and complement space-based transit observations, and with advanced adaptive optics methods to complement JWST imaging and transit spectroscopy of exoplanets. Astro2010 also highlighted the importance of "a vigorous and adaptive program of theoretical and laboratory astrophysics investigations," both to support the coming decade's exoplanet research, and to guide planning for a future space mission dedicated to studying habitable planets.

Clearly, exoplanet research is a very high priority for NASA. Of the many areas of astronomy covered by NASA missions, it is the only one identified by itself at the level of SMD objectives. But complicating support of exoplanet research is its deeply cross-disciplinary nature. It employs traditionally astrophysical techniques to study systems with physical processes traditionally studied in the domain of planetary science. As a consequence, exoplanet research is supported across multiple programs in the AD and PSD (e.g., Planetary Atmospheres, Astrobiology, ATP, ADAP), and with resources managed across divisions (via OSS, managed jointly by the AD and PSD, and via allocation of ground-based observing resources that NASA acquires from Keck and other major telescopes). Most of these support avenues existed before exoplanets were discovered and have to adapt. Many of the issues that we raise for the OSS program are relevant in varying degrees for other exoplanet support avenues. An overarching issue is: given the unique importance of exoplanets in NASA's mission, should exoplanet research be supported by adapting existing programs, or is a more thorough-going approach needed, perhaps involving creating new programs?

Findings

The findings below reflect panel discussions, as well as input to our working group from the panel's web site and from a handful of exoplanet researchers who directly contacted the working group, partly in response to email solicitations. We emphasize that the number of respondents providing input to the panel was relatively small. Also, the respondents who were not anonymous were all recently tenured researchers, typically

pursuing both observational and theoretical exoplanet research, a common practice in the exoplanet community. The relative youth of the respondents may reflect a predominance of young researchers in this burgeoning specialty. All of the respondents have relied on NASA support for a significant amount of their research, via OSS and other programs; most have also served on OSS and other review panels. There was consensus among the inputs in several areas, but also significant diversity. More thorough interaction with exoplanet researchers is needed to address the issues raised by the panel and the respondents.

A widely held perception, among both panelists and respondents, is that exoplanet research is an "awkward stepchild," not entirely at home in either the planetary science or the astrophysics disciplines. The perception is that this cross-disciplinary aspect, together with the fact that exoplanet science is still a very young discipline, provides hurdles in finding support for this quickly-growing field of research, particularly when researchers must compete directly with researchers in more mature disciplines in a funding climate where supporting an emerging discipline may require sacrifice from more mature disciplines.

Chapter 4 of the Fisk Report discusses the importance of cross-disciplinary research to NASA's objectives, noting "innovative research activities that are interdisciplinary may not fit clearly into one specific SMD science division." It describes two types of funding mechanisms for such research: separate funding via individual divisions, and creation of cross-disciplinary programs managed centrally outside the science divisions. The former mechanism was deemed appropriate for funding specific innovative research projects requiring limited funds. Cross-division programs are warranted when technology development for the cross-disciplinary research can benefit multiple divisions, and when the necessary research requires significant funds. Exoplanet research clearly satisfies these criteria, and exoplanet R&A is currently supported via the cross-division OSS program, and via allocation of NASA's cross-division ground-based observing resources (e.g., at the Keck telescope). However, it is not managed centrally outside of the NASA Divisions, but is instead it is managed within both the Astrophysics and Planetary Science Divisions as separate components of a single program.

Planetary Science/Astrophysics partnership

Highlights

1. The balance of AD and PSD management and support of cross-disciplinary exoplanet research likely needs adjustment to make the AD a more equal partner in the OSS program, reflecting the composition of the exoplanet research community and the prominence of exoplanet science in OSS-funded research.
2. Some consolidation of cross-disciplinary exoplanet research under OSS, or, more drastically, the establishment of a new Exoplanet Research & Analysis program may be warranted but must be done cautiously and probably should not

be too extensive. Both panelists and respondents are wary of extensive consolidation. Significant change in current structures should not occur without extensive interaction with the exoplanet community.

The panel finds a key issue for the OSS program and for exoplanet research is how to best manage research resources across division lines. This requires identifying criteria to guide adjustment of the roles of the PSD and AD in managing and funding exoplanet research (and related research topics) as the field develops over time.

The Astrophysics and Planetary Sciences Divisions each have a vested interest in maintaining the long-term partnership begun by the OSS program. There is a strong origins component to many of the flight projects recommended by the Decadal Surveys for both Divisions. While sample analyses, observation and modeling of minute details of our own solar system and its formation certainly belong in Planetary Science, those same observations, analyses and models complement and inform studies of protostellar evolution, and the detection and characterization of exoplanets. While there are some topics that should be funded solely by Planetary Science (analyses of meteorites, observations of solar system objects) or solely by Astrophysics (detection of exoplanets, observations of protostellar systems) there are other areas where joint funding is appropriate (disk models and nebular evolution, characterization of giant planet atmospheres, spectral studies of terrestrial planets). There is great synergy in the examination of these problems from both the Astrophysics and Planetary Science perspectives.

To most effectively achieve NASA's exoplanet objectives, however, the current OSS program model may not be optimal. The perception of the panelists and most respondents is that the AD should play a more prominent role in managing exoplanet research. A specific concern is the composition of proposal review panels. Historically, except on the AD-managed exoplanet detection panel, the balance between planetary scientists and astrophysicists in OSS proposal review panels that include exoplanet topics appears not to reflect the predominance of astrophysicists as exoplanet researchers, or the essentially astrophysical nature of exoplanet observations. The respondents feel that the historical prevalence of planetary scientists on these panels has in fact served exoplanet science well so far, by encouraging planetary scientists to engage in exoplanet research. But now the AD should play a more prominent role in managing reviews across all exoplanet research areas. The panel raised this issue with OSS program officials and learned that this adjustment may already be happening; community perception may lag reality, but still must be considered as NASA plans the future for OSS.

In adjusting the balance of AD and PSD management of exoplanet research, two alternatives should be considered:

1. Exoplanet research should remain a significant part of the OSS program, but the AD's role in managing it should expand beyond detection of new exoplanets into the characterization of exoplanets and evolving protostellar systems.

2. Alternately, a separate Exoplanet Research and Analysis (ExRA) program should be created, jointly managed by the AD and PSD, with the AD playing a larger role than is presently the case for exoplanet research in the OSS program.

Most of the panel initially found the latter possibility intriguing and very likely justifiable, but upon further consideration came to favor the current model of diverse support, including cross-disciplinary OSS support. The limited resources of the present panel do not enable us to make a strong recommendation one way or another. Notably, the strongest support for establishing an ExRA program in the panel came from astronomers not performing exoplanet research. They see exoplanet science as having reached a "critical mass" where it is a discipline to itself, with a large and growing community of researchers, mature technology, observing, theory, and data analysis components, and unique cross-disciplinary needs. Although no other astrophysics R&A program focuses on a single source class or astrophysical domain, panelists note that recent national science policy and NASA policy (cited above) clearly single out exoplanets as a special, high priority science target. Also, domain-specific programs are typical in the PSD, e.g., the PSD has programs devoted to cosmochemistry, Mars data analysis, and outer planets research. On the other hand, the fundamentally cross-disciplinary nature of exoplanet science argues for continuing support through a variety of support channels. In this regard, the cross-divisional nature of the OSS program makes it particularly valuable for encouraging the desired and highly beneficial collaboration of researchers across the astrophysics and planetary science divisional boundary.

An additional concern of the panel and respondents is the impact of any consolidation on the number and frequency of funding opportunities, an issue we discuss further below.

We suggest that NASA study how best to support future exoplanet research and analysis as an inherently cross-disciplinary activity, considering both possible changes within current programs (OSS, and ATP, discussed below), and the possibility of creating an ExRA program. The study should involve broad consultation with the exoplanet research community, and address the following issues:

1. Develop criteria to guide adapting the balance between the AD and PSD for both managing and funding exoplanet research. The balance should be adaptive, responding to the evolution of research. A challenge that must be faced is that, as the field matures, many exoplanet researchers will likely not be easily identified as either astrophysicists or planetary scientists.
2. Ascertain whether formation of a new, cross-division ExRA program will help NASA better achieve its exoplanet research objectives. This could potentially involve both separation of exoplanet research topics currently funded via OSS into a new ExRA program, and consolidation of topics currently funded by other programs (Planetary Atmospheres, Astrobiology, ATP, ADAP, GO programs) under an ExRA umbrella. The panel notes the difficulty of moving programs such as Planetary Atmospheres from the PSD to a new umbrella program in the AD,

and the problems such a move might engender for traditional researchers who study giant planets in our own solar system. Both separation and consolidation could have far-reaching consequences. Several factors need to be weighed in this decision, including:

- Should exoplanet theory be consolidated under an ExRA or revised OSS program? If so, to what extent should it be excluded from other programs? If it is not excluded elsewhere, what explicit criteria should distinguish OSS or ExRA theory projects from other exoplanet theory?
- Should allocation of ground-based observing resources for exoplanet research be consolidated under OSS or ExRA (e.g., with some fraction of Keck time allocated via OSS/ExRA), or should some fraction of it continue to compete with other science? If the latter, how can the opportunities be differentiated?
- If there is consolidation beyond what is currently covered in OSS, the number of funding opportunities per year available to exoplanet researchers would likely decrease. What would be the impact of this reduction in proposal opportunities on exoplanet researchers?
- While not entirely happy with the diluted role for exoplanet research when its support is spread across multiple programs, respondents were wary of extensive consolidation. For a new and quickly evolving discipline, less centralized support may provide more nimble adaptability as the focus of research evolves.
- What would be the impact on the OSS program of having exoplanet research separately managed, rather than competing with other origins science?

Presumably the balance of AD and PSD funding of exoplanet research should reflect the balance of participation of planetary scientists and astrophysicists in the sponsored research. The panel did not have sufficient information to ascertain whether this would require a significant change in current funding allocations. Although the PSD and AD have well-defined roles in managing proposal selection and grant management, they share the burden of funding across the nominal management boundaries. This kind of negotiated balance, potentially capable of adapting to changing research patterns, may remain adequate.

Exoplanet theory

Highlights

1. Theoretical exoplanet research is funded in many programs; this is appropriate, but clearer differentiation between opportunities is needed.
2. In particular, the ATP should explicitly invite exoplanet theory proposals; which types of projects are most appropriate for ATP versus other programs has not been clearly articulated.
3. NASA should consider whether the PSD should play a role in supporting exoplanet theory via ATP.

Exoplanet theory is currently funded in several programs: OSS, ATP, Planetary Atmospheres, Astrobiology, and the Spitzer and Kepler GO programs. Of these, OSS and ATP are in the purview of this panel.

For the OSS program, the findings presented above regarding the balance between the AD and PSD in supporting research apply equally to theoretical, observational, and data analysis research. In the remainder of this section we focus on ATP.

All respondents, regardless of whether their research emphasized observation or theory, and regardless of whether their research heritage was in astrophysics or planetary science, felt strongly that exoplanet research is currently poorly supported by ATP. The panel learned from the ATP that it has never officially discouraged exoplanet theory proposals, but that too few are submitted to justify creating a separate exoplanet theory review panel, so the submissions must compete with proposals in other areas within more traditional panels. Two respondents have served on ATP panels; they feel a combination of factors make the ATP program inhospitable to exoplanet theory proposals. Responding separately, they made similar observations:

1. The ATP NRA does not explicitly invite proposals for exoplanet theory research. Combined with ATP's reputation of limited support of exoplanet research, this leads to few exoplanet submissions to ATP.
2. Exoplanet theorists appear to be underrepresented in the panels compared to the amount of exoplanet theory work being done and the importance NASA policy assigns to exoplanet research (presumably the lack of representation reflects the small number of submissions). The large representation of other areas creates a more favorable environment for support of other theoretical research.
3. The ATP program is heavily oversubscribed; other disciplines with a tradition of ATP support view exoplanet theory as a new competitor seeking a slice from a pie that has not grown in size.
4. Much exoplanet theory can be argued to be in the realm of planetary science, potentially supportable with non-ATP sources; this may lead reviewers to conclude that limited ATP resources should be focused on more purely astrophysical areas.

Of course, these observations are anecdotal (except for the first), but they are plausible and need to be taken seriously. However, before any action is taken to “fix” these problems a more in-depth study should be undertaken to confirm the level of difficulty for exoplanet researchers compared to other theorists. A solution should be proposed that results in the best overall theory program for the Astrophysics Division as a whole rather than twisting an existing program to satisfy the needs of a single (but important) research community.

The panel examined the ATP program and in fact was impressed with its efforts to seek balance among its many constituencies. The issues with respect to support of exoplanet research seem likely to be a consequence of: (1) the difficulty of adapting to fast-changing research patterns in a program with extreme proposal pressure, which produces a climate favoring well-established and possibly conservative research; and (2) the cross-disciplinary nature of exoplanet theory, which creates ambiguity regarding which forums are appropriate for supporting the work.

NASA must clarify how it will support exoplanet research, both in internal policy and explicitly for the community in NRAs. If current program structures are largely maintained, ATP should explicitly invite exoplanet theory proposals in its NRA, and the program should strive to have exoplanet researchers well represented in review panels, possibly with a separate exoplanet theory panel. ATP should work with other programs to establish clear criteria guiding proposers and reviewers, identifying what types of research are appropriate for ATP vs. OSS and PSD-managed programs. Of course, if an ExRA program were created, the possibility of consolidating some exoplanet theory in ExRA would impact the role of exoplanet theory in ATP.

NASA should also consider whether the PSD should have a role in the review and funding of ATP exoplanet theory projects. It may be adequate for program administrators to negotiate a partial role for PSD in supporting specific ATP-selected projects on an ad hoc basis.

Highlight

Until recently, NASA's most extensive contributions to exoplanet discovery and understanding have come through its support of ground-based observing, and theory and analysis interpreting ground-based data. Such observing is also crucial for supporting discoveries by the current Kepler mission, and for planning future missions. NASA should continue to strongly support ground-based exoplanet observations, both through grant programs and through allocations of NASA-acquired observing time at private observatories.

The OSS program has offered increasingly strong support of ground-based observing of exoplanets. In addition, NASA has strongly supported exoplanet observing through allocation of the observing time it has acquired in partnership with important observatories (e.g., Keck, LBTI, IRTF). This significant investment in ground-based

research needs a clear justification from the perspective of support of NASA's astrophysics science objectives and specific space missions. The panel suggests that NASA examine this issue and explicitly address the role of ground-based observing for its exoplanet objectives and missions, to guide future support by R&A programs.

The panel finds there is indeed clear justification for continuing a strong investment in ground-based exoplanet observing. Prior to the recent announcement of Kepler's ~1000 candidate exoplanets, NASA's greatest impact on exoplanet science has been through its support of ground-based observing, both via OSS and through its observatory partnerships. Nearly all pre-Kepler exoplanets were discovered via ground-based observing, mostly using the Doppler radial velocity (RV) method. NASA support has been crucial in pioneering and maturing RV technology, and in supporting large and productive RV surveys searching for exoplanets; Astro2010 specifically recommends continued strong investment in RV technology and observations. Anecdotally, input to the panel indicates NASA support may account for about half or more of pre-Kepler exoplanet discoveries. This work directly addresses NASA's objectives to obtain a census of planets and to understand planetary system formation and evolution. It also has provided crucial input for mission planning.

The Spitzer mission has made groundbreaking contributions to exoplanet science through IR studies of planetary transits and secondary eclipses. Kepler is using transit and eclipse observations to profoundly transform exoplanet science; it's recently announced catalog of transit-based exoplanet catalogs is over twice the size of the entire previous catalog of exoplanets. However, ground-based observing is an essential partner to space-based transit and eclipse observing. RV, adaptive optics (AO), and spectroscopic observations provide complementary information about systems that is crucial for understanding the implications of the space-based discoveries. In particular, the vast majority of Kepler's discoveries are exoplanet candidates that need both confirmation and more complete characterization via ground-based follow-up studies. Exhaustive follow-up of the Kepler candidates alone would require more ground-based observing resources than are already devoted to exoplanet science by both NASA and NSF.

In addition, the panel finds that ground-based observing will be crucial to prepare for future missions, in particular for planning JWST observations, for planning the WFIRST mission and for future coronagraph or star shade exoplanet missions. JWST is the most imminent of these; JWST transit spectroscopy will probe exoplanet atmospheres with unprecedented sensitivity and precision. However, a strong and active ground-based exoplanet program will be crucial for planning JWST exoplanet observations by providing accurate ephemerides for Kepler exoplanets, and by finding more exoplanets in the broad parameter space not probed by the short-duration Kepler mission (especially at long periods). In the time between the Kepler and JWST missions, support of ground-based observing will be the most effective channel for NASA to advance its exoplanet objectives.

NASA/NSF partnership

Highlight

NASA and NSF should continue to fund research based on the relevance of the proposed science to agency science goals, with the technology used to obtain data playing a secondary role in the funding decision. In particular, NASA's support of ground-based exoplanet observing as well as supporting theory and data analysis complements NSF-supported research. Existing cooperation and partnership mechanisms are effective in guiding proper allocation of resources by NASA and NSF.

The issue of NASA support of ground-based exoplanet research touches on the more general issue of partnership between NASA and NSF in support of astrophysics research. NASA and the NSF have a long history in funding projects of mutual interest and in ensuring that their research funding is spent in the most efficient manner. The long-standing tradition that the NSF funds ground-based observations, while NASA funds space-based projects stems from a desire to establish a firm division between projects eligible for NSF and NASA funding. This arbitrary dividing line was never applicable to Planetary Science where long-term, ground-based synoptic observations of targets of solar system missions were required for mission planning and proper data analyses.

Naturally, NSF had no desire to fund studies that are an essential part of specific NASA missions. With the widespread availability of adaptive optics and the construction of large-scale, ground-based arrays, it is possible for projects that utilize both ground- and space-based observational assets to study aspects of both solar system and astrophysical objects in a highly complementary and synergistic manner. NASA and NSF should fund research that proposes the best science (by each agency's own standard) and not worry in excess about how the data are acquired (so long as they are assured that it can be). Funding projects in this manner will require close cooperation between the various NASA and NSF program managers (as already happens in many instances) to ensure efficient allocation of resources among investigators and projects. In a few, rare instances it may be beneficial to work out joint funding arrangements for specific segments of certain projects. However, in most cases where projects were submitted to both agencies, the appropriate program managers can meet to divide highly ranked proposed projects between the programs.

Continuity and Diversity of Funding

Highlights

1. Cost-based metrics for assessing support levels must take into account how the costs of research change with time; inflation-adjusted metrics are insufficient. Nominal support levels should track the actual costs of research.
2. Program-specific demographic metrics, such as proposal pressure, are only useful if interpreted in the context of broader demographic metrics. NASA should collect such metrics; existing work in the astronomy literature and in policy documents provides some of the needed context.
3. Although some consolidation of exoplanet support opportunities may be justified, maintaining multiple funding opportunities per year is important to help maintain continuity of funding for research teams in a competitive funding climate. Longer-duration grants, both in GO programs and in grant programs, would also help maintain funding continuity.
4. Research efficiency will be improved if administrative burdens on researchers and institutions are reduced. Mechanisms for accomplishing this include: longer periods of performance to reduce NCE requests (especially for nominally short projects); shared/consolidated reporting for related small projects (in particular in GO programs); longer-duration or larger awards enabling collaborations to be fully supported with fewer grants.

Panelists and respondents repeatedly raised concerns about ensuring continuity and diversity of funding across nearly every area under the purview of this panel. We highlight here observations made by the OSS/Exoplanet working group panelists and respondents.

Several programs making presentations to our panel used proposal pressure and inflation-adjusted funding metrics to address continuity, consistency, and appropriateness of funding. These metrics are objective and provide important input for examining support levels. But other information is needed to provide a context in which to understand the metrics.

Both panelists and respondents struggled with reconciling reports of roughly constant award sizes in inflation-adjusted dollars with their experience of decreased funding capability per award. It is likely that the cost of supporting research has risen at a rate significantly greater than that of inflation. Certainly educational costs, which impact research through the cost of graduate student support, have risen much more rapidly than inflation. Cities hosting educational institutions are often particularly desirable places to live, with costs of living that rise more quickly than inflation, and this is reflected in salaries. Overhead and benefits rates have increased at every institution we know of; these increases compound the effects of other rising costs. To properly understand inflation-adjusted funding metrics, NASA must gather and report information on how the real cost of research is changing. Other institutions, within government or outside (e.g., the Chronicle of Higher Education), may be sources of the needed

information. Funding metrics should either be presented with an adjustment for research cost, or always in parallel with summaries of how research costs are changing.

Similarly, proposal pressure (ratio of submissions to awards) cannot be interpreted in isolation as a measure of the appropriateness of program funding levels. To some degree, submissions are self-regulating; proposal pressure saturates at a level where a particular community deems the investment of resources required to produce a proposal is not justified by the probability of success. In parallel with such metrics, NASA should also gather and report broader demographic metrics. The recent demographic study of Metcalfe (PASP 2008, "The Production Rate and Employment of Ph.D. Astronomers") provides an example of the kinds of contextual metrics that could prove useful. He gathers data on the number of PhD astronomers produced per year from 1970 to 2006, as well as the number of tenure-track and other jobs for astronomers, using public databases from the AAS and ADS, and a graduate thesis database. Complementary metrics on the distribution of astronomers across scientific disciplines and research type (observation, theory, instrumentation, data analysis, laboratory astrophysics) would also be useful. Astro2010 gathers some such indicators (Chapter 4, pp 4-10ff) and noted that recent trends have important implications for employment and training of PhD astronomers and funding of research. A conclusion of Metcalfe's study is that funding levels drive the PhD production rate, with roughly a 4-year lag time. While funding agencies could set funding levels based solely on internal criteria and let research institutions adapt, surely a more healthy research climate will result if there is a mixture of drive and response from both funders and researchers.

Respondents raised concerns about the frequency of exoplanet funding opportunities. While intrigued by the possibility that an ExRA program could provide a more hospitable home for exoplanet research, they feel it is important to have multiple funding opportunities. They contrasted NASA's multitude of programs with NSF's single-opportunity approach. Program administrators, panelists and respondents, who have served on review panels, all acknowledge that there is a significant "stochastic element" to the review process, especially when proposal pressure is great, so that a high-quality proposal is not a guarantee of selection in a particular cycle. With a full year between NSF opportunities, this creates serious challenges for maintaining continuity of support for NSF-supported students, postdocs and soft-money researchers. The variety of NASA exoplanet opportunities works to mitigate this problem, at least to some degree. But the number of opportunities will decrease as GO programs end for missions doing exoplanet science.

Both the Metcalfe and Astro2010 studies show that the composition of the astronomy workforce changed greatly in recent years, with fewer than half of the permanent positions held by astronomers being secure academic faculty positions. Metcalfe speculated that the shift is due to astronomy moving toward large collaborations and complex projects where service positions are increasingly important. Many of the non-faculty positions are "soft money" research and support positions, amplifying concerns that grant opportunities should be structured in a way to help maintain continuity of funding of quality, mission-enabling research.

Program-specific metrics

Obtaining a census of exoplanets is a top-level AD objective of obvious relevance to OSS. Besides general R&A program metrics (e.g., publication metrics, proposal pressure, funding level history), the OSS program should track metrics tied to this objective. They might include counts or lists of exoplanetary systems discovered and characterized by OSS-funded research, and days or fractions of telescope time devoted to exoplanet observing. Presentations to the panel indicated that OSS staff has already begun some such tracking. We note that journals already track objects associated with publications; if journals and ADS also tracked funding sources, this could help OSS compile such metrics.