

Testimony for the House Committee on Science and Technology's Subcommittee on Space and Aeronautics

"NASA's Space Science Programs: Review of Fiscal Year 2008 Budget Request and Issues"

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Chair, Astronomy and Astrophysics Advisory Committee**

Mr. Chairman, members of the subcommittee, thank you for inviting me to testify. I am a Professor and Astronomer at the University of California, Santa Cruz and the University of California Observatories/Lick Observatory. I am the Chair of the Congressionally-chartered FACA committee, the Astronomy and Astrophysics Advisory Committee (AAAC). This committee was established to assess and make recommendations regarding the coordination of astronomy and astrophysics programs of NSF, NASA and DOE and progress on the National Academy National Research Council's (NRC) Astronomy and Astrophysics Decadal reports. As required by the enabling legislation, the AAAC generates an Annual Report in March of each year (the 2007 AAAC report is at <http://www.nsf.gov/mps/ast/aaac.jsp>). As Chair of the AAAC, the recommendations of that committee underpin this testimony.

In addition to responding to the questions from the Chairman, I would also like to highlight some issues that were a concern of the AAAC and will increasingly impact science at NASA unless rectified. Arguably science is the crown jewel of NASA. The science missions give NASA great return through their frequent and exciting results that capture the imagination of the public. They are equally a frequent demonstration of our nation's scientific and technical capabilities. However, that jewel is becoming tarnished by the effective reductions in the NASA Science Mission Directorate (SMD) budget.

SCIENCE AT NASA AND THE CURRENT NASA BUDGET PROJECTIONS

Science at NASA: NASA's science program has been an extraordinarily successful enterprise. The scientific productivity of its diverse suite of science missions has made many of them household names. Missions like the Hubble Space Telescope (HST), the Mars Rovers, the very successful Explorer missions like the Wilkinson Microwave Anisotropy Probe (WMAP), the remarkable outer planet images in our Solar System from Cassini-Huygens and Galileo, along with numerous other remarkable missions and projects, are a demonstration of US technological leadership. NASA has shown time and time again that novel technology, driven by great science goals, can dramatically expand our horizons and bring exploration of the cosmos beyond our Earth within the reach of all. NASA's missions have dramatically changed our understanding of the universe – its origin, evolution and structure, the existence of massive black holes, when and how galaxies formed, and the birthplaces of star and planets – our solar system, and our home planet Earth. The value of these science missions is widely recognized for generating enthusiasm for science and engineering and for stimulating the interest of the nation's youth.

Yet this leadership in the scientific and technological arena – with the visibility that it brings to U.S. technological and scientific achievements – is clearly at risk in the coming years. The breadth and balance within NASA's science program is a major factor in this visibility. The substantial budget changes envisaged for the coming five years are already having a major impact on the future science program. The resulting major restructuring of the long-term science program is a great concern to the science community and will, over time, significantly change NASA's perceived value to the nation. NASA has had

extraordinary successes over the last decade from its challenging, ambitious science missions, combined with continuing, broadly-based research support that produces stunning science return from a diverse portfolio of programs. In ten years as we look back, will we be able to make the same statement? There will be highlights, but will we feel that NASA's science program has had its golden era? I feel very strongly that we all do not want that to be the case, but if we are to explore our universe and our earth through the unique capabilities that NASA brings, then we must step up to the plate and commit the resources needed.

The problems that are visible in SMD flow not just from NASA trying to implement the Vision for Space Exploration, but also from the recovery from the loss of Columbia and major impacts such as Katrina. Science at NASA suffered a major hit when ~\$3B was removed from SMD in the FY07 5-year projected budget request. The SMD budget is now down 7% in inflation-adjusted FY06 dollars by 2012 in the FY08 request, instead of growing as in the FY06 request. The reduced SMD budget stems from the overall problems of the NASA budget and it's disconnect with its current mandate. This is discussed further below, after the discussion of the role of NASA science in the American Competitiveness Initiative (ACI).

Innovation, Competitiveness, ACI and NASA: Research is essential to innovative activities and underpins a technologically-competitive society, as enunciated in the NRC report *Rising Above the Gathering Storm*. The inclusion of ACI increases in the FY07 budget request for NSF, DOE Science and NIST was a very strong response to the challenges faced by the nation in staying at the forefront of scientific and technological development. The continuation of the ACI in the FY08 budget request demonstrated the Administration's commitment to building a robust R&D base in the physical sciences. Congressional support for NSF research and DOE science in the FY07 appropriation through the Joint Funding Resolution was a further key step in strengthening science and technology through the Congressional Innovation and Competitiveness effort. However, the exclusion of NASA science from the ACI contrasts with the inclusion of DOE science. There is no question that NASA is at the cutting-edge of science and technology research. This exciting and highly visible research contributes to the vitality of the national skill set and has encouraged young people to move into science and engineering. The Congressional interest in Innovation and Competitiveness enables a fresh opportunity for enhancing NASA science. The AAAC in its Annual Report strongly encourages Congress to consider enhancing the support for science at NASA explicitly to improve innovation and competitiveness, as has been done for NSF and DOE science.

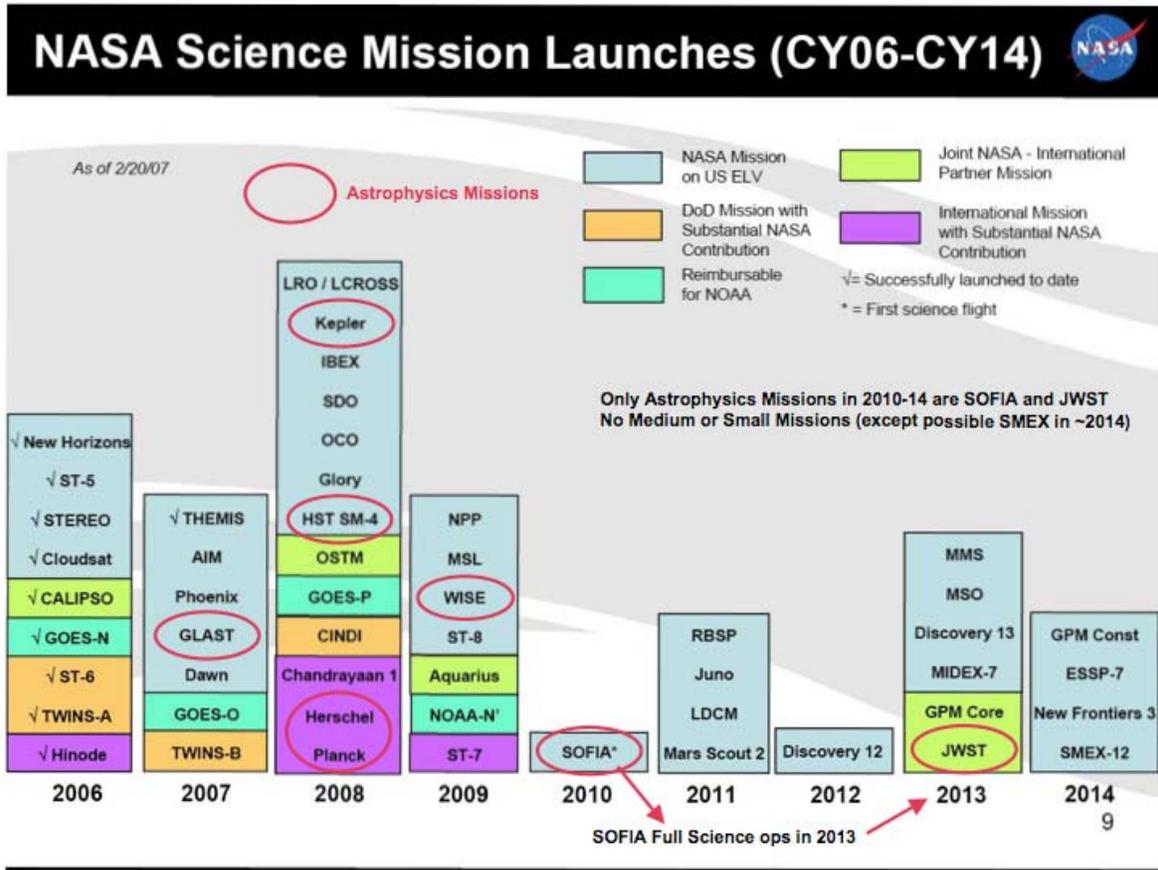
Funding for NASA for the Vision for Exploration: Before discussing the science program further I would like to comment on the overall context in which the NASA science budget is developed. It has become widely recognized that NASA is significantly underfunded for the mandate that it has been given to implement the Vision for Space Exploration. No discussion of the budget challenges for science at NASA can take place without acknowledgement of the challenges that face the agency overall. The challenges of transitioning within the current NASA budget to a new generation of space capabilities in the framework of the Exploration Vision, with no new funding, are obvious. NASA's overall budget has remained essentially unchanged through the last three budget requests. Yet in that timeframe the real costs of the transition to a new human spaceflight structure have been recognized. As a result, the balance among the needs of Space Shuttle (STS) operations and ramp-down, International Space Station (ISS) completion and operation, Exploration Systems development and a robust Space and Earth Science program has come under great strain. I recognize the support that the Administrator has to transition the agency from being driven by the vestiges of its past program – one that was devised in the 1970s – into a new, forward-looking set of objectives. Broadly I support the goals of transitioning the human spaceflight program into a new set of capabilities. A nation as technically-advanced as ours, with such human, technological and fiscal resources, should be able to explore beyond the Earth. Furthermore, these new capabilities will benefit science missions and scientific “exploration”. But to ask NASA to transition and develop these new capabilities, while undercutting its most innovative and productive component, its science program, is unwise. NASA needs more resources if it is to explore in a “feet on the ground” sense through a human spaceflight program, and to explore our universe by unearthing its secrets through a vibrant science program.

The AAAC hopes that Congress can work to rectify this problem, since the recent fiscal year requests have not provided the resources to enable NASA to carry out its mandate in the Vision. Adequate funding is critical over the next few years when NASA is trying to support Shuttle operations and ramp-down, completing the ISS, initiating new launch and transportation capability, and carrying out a comprehensive science program. Long-term impacts to both science and human spaceflight will accrue if the funding is not adequate during this period. The AAAC recognized the issues with its highest-priority recommendation in its Annual Report in the discussion re NASA: **“The lack of growth in the NASA budget to respond to the Exploration Vision is stressing all the agency's activities. The AAAC is deeply concerned about the growing impact on the space and earth science program.”** The AAAC was also concerned about the potential out-year impact of the reduced funding for NASA overall in the FY07 appropriation if this funding is used as a base for the FY08 appropriation. The science community appreciated that the FY07 Joint Resolution budget for NASA explicitly designated and made statutory only a small cut (~1.5%) to science compared to the FY07 budget request level, but remains concerned that further cuts may arise if the FY07 base is used. The AAAC noted: **“The AAAC is concerned that the appropriation for FY08 and beyond may lead to a further cut by using the FY07 appropriation as the base for future budgets, and recommends that the FY08 request be the base to preclude added impacts on science at NASA.”**

The issues faced by NASA are so challenging that they really require broad consensus between the Administration and Congress on the nation's goals for its space endeavors. I hope that some form of high level discussion forum, such as has been recently proposed both in the House and Senate comes to fruition, and provides clear guidance for NASA and enhancement of its budgetary framework.

Astrophysics – an overview: If one takes a near-term view, and looks forward with a horizon around 2009-2010, the mission mix in Astrophysics looks fairly good. Over the next ~5 or so years Astrophysics will have a reasonably well-balanced program, i.e., one with a mix of small, medium and large missions in operation covering a diverse range of scientific areas. The launch of a mid-size mission, the Gamma Ray Large Area Space Telescope (GLAST – in late 2007), a Discovery mission Kepler (in 2008), an Explorer mission, the Wide-Field Infrared Survey Explorer (WISE – in 2009), and participation in two powerful European Space Agency (ESA) missions Herschel and Planck (2008-9) strengthens the program. Astrophysics is operating three Great Observatories, Chandra, Hubble and Spitzer, and providing significant funding for data analysis for those missions. The next Hubble Servicing Mission (SM4) and the instrument upgrades will rejuvenate Hubble. The Stratospheric Observatory for Far-Infrared Astronomy (SOFIA) is moving towards its first science demonstration in 2010 and full science operation in 2013. NASA is progressing on an extremely powerful Great Observatory-class mission, the James Webb Space Telescope (JWST). NASA is also planning for a possible Beyond Einstein mission that would begin to be funded for development in the same time frame (though its launch would not be until the middle of the decade or beyond). These elements of the program are consistent with community-developed strategic plans such as the National Academy Astronomy and Astrophysics Decadal Survey.

So why is the astronomy and astrophysics community so concerned? And why is this concern reflected so strongly in the AAAC annual reports, and the reports and discussions of the NASA Advisory Council (NAC) Astrophysics Science Subcommittee, and the NRC committees (Space Studies Board – SSB; Board on Physics and Astronomy – BPA, Committee on Astronomy and Astrophysics – CAA)? First, the cuts that have occurred in the Research and Analysis (R&A) funds are a very serious issue for the community. R&A funds support theory and modeling, training of students and postdocs, and development of new technologies, and so are of great future value to NASA as well as the community. Second, it is when we look up from the immediate future and look down the road that we see that the new mission pipeline is strikingly empty beyond 2009. This is a major issue. This can be seen in the Figure below. The next few years look good because we are benefiting from the achievements of the past decade, or even longer. The missions from the 1990s and early 2000s are operating or coming to fruition – but the dearth of new small and medium



missions initiated in the last few years is reflected in the next decade. SOFIA does not come into full science operations until 2013. JWST, when it launches in 2013, will be an amazing observatory, as dramatic in its way as Hubble was in 1993 (when its optics were corrected), or when the Hubble Advanced Camera was installed in 2002. In contrast to these major programs there is nothing else in the years 2010–2014, except for a possible Small Explorer (SMEX) in Astrophysics in ~2014 (from the recent SMEX announcement of opportunity – AO).

How limited the options are for Astrophysics can be seen in the Table below. In real terms the Astrophysics Division suffers a precipitous decline in FY09 (down by 23% in constant dollars relative to 2006) that worsens in the out-years. Even though a number of important and productive missions will be operating into the next decade, the long lag between inception and launch will lead to a period with far fewer operating missions by the middle of the next decade, unless this budget trend is reversed.

	FY06	FY07	FY08	FY09	FY10	FY11	FY12
Actual Year \$M	\$1,553	\$1,540*	\$1,566	\$1,304	\$1,269	\$1,266	\$1,393
% change**		-0.8%	1.0%	-16%	-18%	-19%	-10%
Inflation Adjusted***	\$1,553	\$1,495	\$1,476	\$1,193	\$1,127	\$1,092	\$1,167
% change**		-2.3%	-5.0%	-23%	-27%	-30%	-25%

*1.5% decrease from FY07 request **relative to FY06 ***in FY06 dollars with 3% annual inflation

Need for better cost estimates and the use of “lifecycle” costing: I have emphasized the impact of the projected budget decreases for the science program at NASA, with particular emphasis on the situation in Astrophysics. But I think we all recognize that there is another aspect that has impacted our ability to plan ahead – and that is the unrealistic and incomplete costs estimates that have been used in the past for science projects by NASA and the community. The AAAC has strongly encouraged the adoption of a consistent and common approach to mission costing by the community and NASA, and advocated that the baseline be “lifecycle” costs (from conceptual development through the end of operations – from pre-Phase A through Phase E). Doing so would eliminate some of the uncertainty that has surrounded cost numbers in community discussions and lead to more realistic costs. In addition, better cost estimation is needed for the Phases, utilizing independent cost estimates as a cross-check. The transition to full-cost accounting at the NASA Centers also is resulting in more realistic cost estimates for missions.

The Decadal Survey recommendations are typically implemented over 10-15 years. This is therefore the timeframe over which we should be costing our missions if we are to match our recommended mission suite to likely budgets. The full costs of JWST, the Space Interferometry Mission (SIM), and SOFIA over that 10-15 year timeframe were not appreciated because the costs used for planning in the Decadal Survey and elsewhere were typically construction or Phase C/D costs (and also were not subject to an independent cost study). This “undercosting” (to use the NASA Administrator's very appropriate word) has led to a gap between what we wanted to do and what we can do. Fortunately, both NASA and the astronomy community have recognized the problem that this approach has caused. We do not want to repeat this mistake and so ways to improve the mission and project budgets are under serious discussion for the next Decadal Survey.

One important step in being more realistic about mission costs is to ensure that we understand the “lifecycle” costs of our currently operating missions. These estimates have significant uncertainty, given the very different situations under which the missions were developed. Nonetheless, they will allow us all to compare new, current and old missions in a more uniform way. Some examples are (for lifecycle costs in current dollars in a full-cost accounting environment, including design and technology, construction, launch and operations): HST: ~\$7.5-9B (including SM4 plus 5 years of added operations); Chandra: ~\$3.4B (15 years of operations); Spitzer: ~\$1.3B (with operations through 2011); Cassini-Huygens: ~\$3B (including ESA and DOE contributions); JWST: ~\$4.5B (assuming 2013 launch and 10 yrs of operations); SIM: ~\$3B (uncertain since launch date unclear – 12 yrs of operations); SOFIA: \$3.4B (with 20 yrs of operations).

The Decadal Survey numbers were traditionally “construction” costs. These were typically underestimated and this, in combination with the change to lifecycle costs, led to some dramatic increases. JWST (2000 survey as NGST) has gone from \$1B \Rightarrow \$4.5B, but such cost growth is not rare. Chandra (1980 survey as AXAF) went from \$500M \Rightarrow \$3.4B. SOFIA (1990 survey) went from \$230M \Rightarrow \$3.4B. SIM (1990 survey as AIM) grew similarly \$250M \Rightarrow ~\$3B. Correcting for inflation changes the factors a little, but the growth is still very large. The examples of SIM and SOFIA, both of which were moderate-size ~\$250M missions in the 1990 Survey, but which grew to be \$3B programs lifecycle, have made us aware of the challenges. JWST was a major surprise when it grew to \$4.5B lifecycle, but given that we now understand that, in current dollars, with full-cost accounting, Chandra is a \$3.4B program and HST is over double that, the lifecycle cost of JWST, while high, is not extraordinary compared to other major programs.

The discrepancies clearly indicate the need for better cost estimates for each of the project Phases by both NASA and the Decadal Survey, and the use of lifecycle costs for planning. Great cost detail is not necessary (nor is it possible), but knowing that JWST would be an ~\$4B program lifecycle instead of a \$1B program, or that SIM and SOFIA would be ~\$3B lifecycle instead of \$0.25B, would certainly help the development of a more robust Decadal Survey, and subsequent planning and roadmapping. It is already clear that developing reliable lifecycle mission cost estimates is considered to be very important for the next Decadal Survey – both NASA and the community are learning from our previous mistakes.

Summary: The key points from this discussion are:

- NASA's science program has been an extraordinarily successful enterprise. NASA has shown time and time again that novel technology, driven by great science goals, can dramatically expand our horizons and bring exploration of the cosmos beyond our Earth within the reach of all.
- The exclusion of NASA science from the ACI contrasts with the inclusion of DOE science; the AAAC encourages Congress to consider enhancing the support for science at NASA explicitly to encourage innovation and competitiveness, as has been done for NSF and DOE science.
- The lack of growth in the NASA budget to respond to the mandate of the Exploration Vision is stressing all the agency's activities. The AAAC is deeply concerned about the growing impact on the space and earth science program and strongly endorses efforts to increase NASA's budget to allow it to undertake the transformation envisaged in the Vision, without imparting serious damage to the science program.
- The decline in the Astrophysics budget in real terms by ~25% (from 2009) greatly reduces the opportunities for new missions following the next Decadal Survey in 2010. Even though a number of important and productive missions will be operating into the next decade, the long lag between inception and launch will lead to a period with far fewer operating missions, with scientific and productivity impacts, by the middle of the next decade, unless this budget trend is reversed.
- The cost growth in missions, both moderate and large, clearly indicates the need for better cost estimates for each of the project Phases by both NASA and the Decadal Survey, and the need to use lifecycle costs for planning and roadmapping. It is already clear that developing reliable and robust lifecycle mission cost estimates is considered to be very important for the next Decadal Survey – both NASA and the community are learning from our previous mistakes.

I would also like to add that the changes in SMD under the new Associate Administrator Alan Stern are being viewed very positively. His efforts to add to the many very experienced people in SMD with new people to strengthen the scientific focus of the Directorate is being well received in the community.

RESPONSES TO THE QUESTIONS FROM THE CHAIRMAN

1. What are the AAAC's concerns and recommendations with respect to NASA's astrophysics program?

The AAAC noted a number of concerns in its report. The broadest issues concerning the NASA budget (“too small for the mandate it has been given”) and ACI (“NASA science is equally as important for the nation as DOE, NSF, and NIST science”) were discussed above. The AAAC is very concerned that the NASA science program has been seriously impacted and that further stresses lie ahead for a science program that has been such an effective demonstration of U.S. science and technology leadership. These broad concerns led directly to two of the AAAC's 2007 recommendations: **“NASA's science funding outlook should be restored. Doing so would be entirely consistent with the commitment to innovation and competitiveness already demonstrated by the Administration and Congress for the NSF and the DOE Office of Science.”** and **“The AAAC strongly encourages Congress to consider enhancing the support for science at NASA explicitly to improve innovation and competitiveness, as has been done for NSF and DOE science.”**

Beyond the budget question (but obviously related) the central issue is the trend in the mission mix in Astrophysics. It is clear that Astrophysics at NASA is living off the past and the mission pipeline will, with the exception of JWST, largely run dry post-2009. JWST will be a remarkably powerful observatory, as dramatic in its impact as Hubble was in the 1990s, but astronomy and astrophysics encompasses much more than the science enabled by JWST. The only other new opportunities are SOFIA, a possible SMEX by 2014 and a possible Beyond Einstein mission by the middle of the decade. Serious problems with cost growth, both from underestimates and from not using lifecycle costs, have occurred in a wide range of programs from Explorers through Discovery to large missions like SIM, SOFIA, JWST and HST SM4. The cost growth has combined with the budget changes to leave the future looking bleak.

Other areas of concern and recommendations in the AAAC 2007 Annual Report are summarized here (and discussed in more detail in the 2006-2007 AAAC report at <http://www.nsf.gov/mps/ast/aaac.jsp>):

Research and Analysis (R&A) funding. The widespread concerns in the community about the cuts and trends in R&A funding were reflected in the report. R&A encourages creative extension of archived data, theoretical studies that can cross traditional disciplinary boundaries, laboratory studies that provide fundamental measurements, and new instrumentation and sensor technologies that pave the way for new science initiatives. With its strong academic emphasis R&A is a key factor in the scientific training and development of younger members of the community – reductions will certainly impact their involvement and run counter to the overall goals of ACI. The R&A program is broader than mission-specific data analysis, and has significant direct value to NASA for science planning and future flight opportunities. A strong R&A program will result in greater productivity from the mission investment at NASA.

The AAAC would very much like to see recovery (and enhancement) of the very valuable R&A program. However, we recognized the great strains on the Astrophysics budget in the near-term due to SOFIA re-instatement, HST SM4 delays, preparing for GLAST, Kepler and WISE launches and ensuring JWST stays on track, so we were reluctant to recommend an “unfundable activity”. In the end we recommended that R&A be given high priority if any additional funds became available in the near-term, and if not, that R&A be considered for recovery in the 2009-2010 timeframe as part of the “wedge” that opens up as HST servicing mission activities are ramped down and as JWST construction funding ramps down. We recognize that incrementing R&A competes with the “Beyond Einstein” and the “Decadal Survey” wedges, but that exemplifies the very serious problems faced by Astrophysics.

Competed, cost-capped missions. The Explorer and Discovery mission lines have been very productive. The AAAC believes that a similar program of larger cost-capped missions, the Einstein/Origins Probes (analogous to the Planetary Division’s New Frontiers line), would be particularly valuable for Astrophysics. Several concepts for Probes are being discussed, including the Joint Dark Energy Mission (JDEM). The AAAC felt that development of this concept and discussion with the Decadal Survey about their potential broad value to Astrophysics would be a valuable step and recommended that the Probes be discussed as a mission line for Astrophysics.

Current major programs in Astrophysics. The AAAC discussed a number of the major activities in its report because of their importance to the Astrophysics program.

- The AAAC was very encouraged by the results of the JWST Technology Non-Advocate Review. Technically, JWST appears to be in excellent shape, with all major technologies at TRL-6 (flight readiness). The added contingency provides a better buffer too. JWST is a major, cutting-edge project and we are not naive enough to expect a completely smooth progression to launch, but the committee, like the community at large, hopes that its cost-growth problems are now in the past.
- The committee is very supportive of HST SM4, even more so now that the ACS has failed. A modern camera is needed to restore Hubble’s imaging capability. Accommodating the costs of servicing remains a major challenge, especially budgeting for the four-month launch delay in 2008. This further reduces the flexibility within the Astrophysics program.
- The Navigator program is under stress, with two large missions, TPF and SIM, given the recognition that SIM is in reality a ~\$3B program. Guidance from the ExoPTF and the Decadal Survey is needed on how to move forward on the study of other planetary systems.

Major mission technology and conceptual development. It is crucial that programs under consideration for implementation by the Decadal Survey process reach a level of maturity that is characterized by a well-defined architecture with well-vetted costs. The AAAC has emphasized that consistent support, roughly at the \$10M level, would make a significant difference in the robustness of the mission selections in the next Decadal Survey. The AAAC recommended that early phase development funds for the major missions in

Beyond Einstein (Constellation-X; Con-X and the Laser Interferometer Space Antenna; LISA) and in Navigator (Terrestrial Planet Finder; TPF) should be continued if possible until the Decadal Survey re-evaluates the mission suite in the Astrophysics arena.

SOFIA. The SOFIA program underwent dramatic changes in the last year: the project was first reduced to \$0 and effectively terminated. SOFIA then underwent a recovery and is now part of the Astrophysics budget. SOFIA has had a troubled and costly development history and will not reach full operations until 2013, more than 15 years after the project began. SOFIA has a distinctly different operational model, akin to ground-based telescopes, in that its instruments can be developed to take advantage of ongoing technological developments. Because of this the science opportunities can be high. SOFIA is a major mission, with a full lifecycle cost for 20 years of operations that is \$3.4B (FY08 budget request). From FY09 its yearly cost is estimated to be \$90M, including Institutional costs, broadly comparable to Hubble (excluding servicing costs) and JWST. When fully operational, SOFIA is estimated to provide ~900 hours of on-target time per year for science observations – space missions average significantly more (HST ~2500 hrs; JWST ~6000 hrs). The cost-per-hour of on-target operation is comparable to Hubble and several times JWST, and so the AAAC considers that it is crucial that SOFIA operates as efficiently as possible and fully involves the science community to provide high science returns.

Advisory structure. The AAAC expressed great concern last year in our 2006 report about the lack of an advisory process at NASA, and were very encouraged when the new NASA advisory committees were established. The new structure has, however, lost a valuable role that was once provided by the Space Science Advisory Committee (SScAC). That structure encouraged dialogue, on wide-ranging issues that cut across the SMD divisions, between SMD and a broadly-representative group from the science community. While the AAAC welcomed the re-establishment of the advisory structure at NASA, we noted our concern that dialogue between SMD and a broadly-representative group from the science community is missing in the new structure. The AAAC (and the community more broadly) would welcome an evolution of the current advisory structure that would provide more dialogue with SMD through a more scientifically-diverse group, even as formal recommendations are channeled through the NAC to the Administrator.

Task forces. The agencies have responded very supportively to the AAAC's requests for community-based task forces to advise the agencies on implementation approaches for key scientific areas. NASA's recent support for two interagency activities, the Dark Energy Task Force and the ExoPlanet Task Force was appreciated (in addition to its earlier support for the Task Force on the Cosmic Microwave Background). With the substantial advances on the ground and the recognition of the challenges and cost of major space missions for planet search projects like SIM and TPF, the AAAC recommended last year that NSF and NASA constitute a Task Force to develop a strategic framework for how to move forward on the detection and characterization of planets around other stars. The AAAC greatly appreciates that the agencies responded positively and quickly; the ExoPlanet Task Force (ExoPTF) has been formed and has begun its deliberations. Its report is expected late in 2007. The AAAC also welcomed the decision by SMD last year to ask the NRC to carry out a study to determine which Beyond Einstein mission should go forward if funding became available in a possible FY09/10 funding "wedge" as HST SM4 is completed and JWST passes the peak of its spending curve. The selection of three JDEM mission concept studies for conceptual development by NASA Astrophysics, and the joint support of the NRC Beyond Einstein Program Assessment Committee (BEPAC) study by DOE were also highly welcomed by the AAAC.

National Virtual Observatory (NVO). While this is a very small program, it was considered to be of particular importance in the 2000 Decadal Survey. It is a joint NASA-NSF activity. The agreement on a joint NASA-NSF solicitation for management of the NVO operation has been moving forward at a very slow pace, and the AAAC would like to see this come to closure to minimize the disruption to a small but important activity.

2. What are your perspectives on the balance of the NASA astrophysics program in terms of the mix of mission sizes, R&A, theory, modeling and technology development? What if any adjustments are needed in your view?

A balanced program within Astrophysics has been a consistent goal of the astronomy community. Such a program provides the most cost-effective way to address the great science issues of our time. Some can be addressed through smaller missions like COBE and WMAP (the cosmic microwave background), others require medium missions like Kepler (planet searches), GLAST (the gamma-ray universe) and JDEM (dark energy), while the largest missions (the Great Observatories like Hubble, Chandra, Spitzer and JWST) can address some of the most challenging scientific questions that cannot be answered any other way. The versatility of such Observatories also allows them to be used for follow-up of discoveries with very little time lag. However, where the Observatory capabilities cannot address a particular high-priority science objective the relatively rapid response with small missions provides a means of doing so. The last three astronomy and astrophysics Decadal Surveys have all emphasized the importance of a balanced program of small, medium and large missions, and have given particular emphasis to the Explorer program and to a healthy program of research support (Data Analysis – DA, and Research and Analysis – R&A).

In the near-term, over the next few years, as noted above, Astrophysics will have a range of missions including an Explorer (WISE), a Discovery mission (Kepler) and a medium class mission (GLAST). Data Analysis (DA) funds from the ongoing Great Observatories are supporting a very wide variety of science objectives. The biggest immediate concern is the cut in R&A, which, while modest, had great impact because cuts in a multi-year program are immediately felt by the new or renewing investigators. Another concern that is also vitally important for the future of the Astrophysics program is the current low level of technology development funding. This gets less attention, but it is the “seed corn” for future missions.

However, the clouds on the horizon portend a more dismal future. The future program is dominated by JWST and SOFIA, both of which are large programs (in \$ terms). As can be seen in the Figure above, the dearth of small and medium missions post-2009 is a great concern for the vitality of the field in the next decade. The continuing effective reductions in the R&A budget (in the FY08 budget and by inflation) will further impact the community, unless the trend is reversed. As Spitzer, Hubble and Chandra approach the end of their lives the community will also see reductions in data analysis funds. The DA and R&A funds and smaller-scale missions serve a critical role in supporting the broad fabric of research needed for realizing the science from future missions and in enabling the development of the necessary personnel and skills.

The program is clearly unbalanced in the future beyond 2009. There are no small-medium Astrophysics missions for many years after 2009. The first mission might be a Small Explorer (SMEX) in ~2014. The unbalance across Astrophysics is but one aspect. There is a need for balance within the very broad areas encompassed by Astrophysics – a single large program in one broad science area and only small missions in another also indicates unbalance. For example, searches for and research on exoplanets will benefit from an ensemble of small-to-large missions complemented by ground-based facilities. A broad, systematic cost-effective approach is needed. The same could be said of a broad science program to explore our universe from its earliest moments to the present day (Origins), and the Beyond Einstein program. Both have very broad goals that together encompass most of the “great questions” within astronomy and astrophysics, and need a suite of missions of different scales to address those fundamental questions.

As much as possible it would be good to not have all our eggs in one basket – especially for space missions. Whole areas of science could be drastically undercut if problems occur. Realistically, there are high priority science objectives where there is no other way than by doing a large space mission, as with JWST’s search for the earliest galaxies in the early days of the universe. However, as much as possible, we should try to accommodate a diverse range of mission and project scales (and to give particular attention to complementing ground-based studies, and collaborations both with other agencies and internationally).

R&A funding needs to improve since it is essential for providing the research base and the development of skills on which future return from missions will depend. Funds for technology development are needed to ensure that optimal choices are made when selecting missions and that the mission options available are broad. There is a crucial need to encourage and support technology development in the science community, as well as at NASA Centers. Core capabilities are required in the NASA Centers, but the Centers might be encouraged to involve the academic community more routinely and directly, possibly through R&D funding that supports more technology development.

I would give particular focus on strengthening the theory and modeling program in R&A. This is remarkably inexpensive for its value to the scientific enterprise. I am not a supporter of acquiring reams of data without concurrent theoretical development. Results drive theoretical efforts and give them relevance, but it is a synergistic and two-way effort, where theoretical developments also help focus observational efforts. It is crucial to have the challenge that comes from having theory \leftrightarrow observations confront each other, and challenge and test each other.

In summary, in my view adjustments are needed to provide a more balanced mission suit across the whole program and also within broad scientific areas, along with support for technology development, and increased support for R&A, particularly for theory.

3. Does the program, in your view, reflect the priorities of the National Academy of Sciences' decadal survey for astronomy and astrophysics? If not, where does the program diverge from the decadal survey?

As discussed above, the Astrophysics program in the near-term, does have a number of launches and a suite of operating missions, and so looks fairly balanced and productive. There are very real concerns, however, about R&A funding, the frequency of small missions (Explorers) and the very limited funds for technology development. The concern grows substantially as one looks further into the future. Moreover, as one takes a longer-term view the program increasingly moves away from the goals of the Decadal survey. The mission mix becomes very unbalanced. JWST will be a remarkably powerful mission, but the mission suite is devoid of other space missions. SOFIA should be operating on the ground, and hopefully a Beyond Einstein program will be under development early in the decade, but launch would be many years away (5-7?). An Astrophysics Small Explorer (SMEX) may be operational by 2014, but other launch opportunities may not arise for years. This is not a balanced program, either scientifically or by mission scale (small-medium-large), and will become increasingly unbalanced as the current Great Observatories begin to end their useful life. This unbalance will be accentuated as the missions launched in 2007, 2008 and 2009 start to approach their end of life towards the early-middle of the decade. The lack of scientific breadth and limited numbers of operating missions will be a serious departure from the breadth of the program envisaged in the Decadal Survey. This will be compounded if the problems with R&A funding and technology development continue.

4. What do you regard as the top 3 risks facing NASA's astrophysics program over the next 5 years and how should those risks be addressed?

The challenge of dealing with a reduction and a dramatic change of slope in the Astrophysics budget, combined with recognition of the costs of the current mission suite have resulted in great concern about future opportunities in Astrophysics. I am assuming that we will develop processes that ensure that we have more realistic cost estimates and that we will use lifecycle costs for programs for planning and roadmapping. I then see the top risks from a scientific perspective as:

1) The lack of small and medium missions beyond 2009. The dramatic drop in the small-medium launch rate beyond 2009 is a major concern. The recently announced Small Explorer SMEX call for proposals later this year could lead to an opportunity for Astrophysics, but the earliest likely launch date would be around

2014. The contrast with the next few years, and with the early part of this decade (when many small and medium Explorers were launched) is dramatic. SOFIA will not reach full operations until 2013. JWST will be a superb scientific mission with wide-ranging capabilities but it alone cannot encompass the science goals of the astronomy and astrophysics community. This becomes especially so since Spitzer, Chandra and Hubble will all be nearing or past their end of life (Spitzer will lose a lot of its science capability by mid-2009 as its cryogen is exhausted). The risk is of greatly reduced scientific returns in the coming decade. An associated risk is that of launch vehicle availability at reasonable cost. This is a serious issue for mission frequency if a substantial fraction of the cost of an Explorer or SMEX is the launch vehicle cost.

2) Inability to respond to the 2010 Astronomy and Astrophysics Decadal Survey. The funding for Astrophysics drops by ~25% in real terms around the time when the new Decadal Survey is released and so the opportunities for ramping-up on the recommended missions will be quite limited. The Decadal Survey will be discussing and making recommendations on many high priority programs that have been under development or discussion for some time. For example, Con-X, LISA, SIM, TPF and SAFIR will all be discussed, as well as a variety of Einstein Probe missions that are being considered in the current BEPAC study. The AAAC ExoPlanet Task Force will likely identify additional areas for development and missions. Some hard choices face the community in the upcoming Decadal Survey. The natural outcome of the more realistic costing that will be part of the next Decadal Survey will be a reduced program, better matched to the available funding. However, the lack of a significant funding opportunity will limit the ability to initiate a strong effort following the survey. This translates to a risk of significantly diminished scientific returns on the highest-priority science questions of the decade. The next generation of missions will also be at risk if technology development cannot be initiated because of the same funding problems.

3) The current lack of technology development and mission development funding and its impact on mission costs and readiness. The very limited funding available in recent times has severely limited the technology development efforts for both current missions in early development (like Con-X and LISA, and now TPF), and also more innovative and speculative technologies for future opportunities. This will have far-reaching implications for mission opportunities in the next decade and is significant risk to future astrophysics missions and competed opportunities. It also increases the risks of cost growth if conceptual development and technology development have been unable to progress steadily.

These areas are identified as risk areas because of two problems. The first is the dramatic change in the budget situation for Astrophysics over the next few years, particularly the cuts in FY09 and beyond. Second, the poor cost estimates in the past have exacerbated our current problems. The agency and community together did not deal very well with the cost estimates and budgets of the missions and programs that we jointly developed. However, it is my view that this situation has changed dramatically with the much more realistic and open approach of the new Administrator and with a more sophisticated and realistic view of project costs and the costs over the lifecycle of missions by the community and the agency. While I think we are now working to deal collectively with the undercosting problem, a solution to the budget problem for science is a more challenging concern for the future. If we are to have a strong, productive and broadly-based science program, additional funding is needed. Recognition is needed that NASA science plays a role as important as that of DOE science, NSF and NIST in the nation's science enterprise.

5. If you could make 3 strategic investments that could benefit the astrophysics program over the long-term, what would those investments be?

Strategic investments are key to positioning the Astrophysics Division, the astronomy and astrophysics community, the NASA Centers and industry partners to be able to extend the limits of scientific endeavor and scientific understanding. To meet the science goals of the community, NASA and the community need to be able to move forward on a variety of missions from large Flagship missions to medium and smaller scale missions, while returning cutting-edge science results from the current missions. I think the following

three areas would be excellent strategic investments to position the agency and the community for a cost-effective program of science missions. The first two are relatively low cost (though still very difficult to fund in the current budget environment), while even the third could be carried out in an Astrophysics budget that is constant at the FY06 dollar level.

1) R&A funding is a strategic investment. This is particularly so for theory, modeling and cutting-edge technology development to complement mission specific data analysis. Clearly R&A and mission-specific DA maximizes the science return from current programs and also maximizes the “return on investment” in space science. Support for such activities is also a strategic investment from NASA’s perspective. A key aspect of an implementable long-range plan is knowing what are key science questions, why they are important, and whether answering them is doable. Exploiting current data, along with theory allows us to set science priorities. Furthermore, exploring novel technologies and strengthening the technological base amongst graduate students and postdoctoral researchers is an investment for the future.

2) Technology development for missions. Astrophysics missions utilize state-of-art technology, and it is essential that that technology be developed and demonstrated to flight readiness levels before a mission enters construction. Retiring technological risk early helps to minimize the likelihood of cost growth. There is another aspect as well. The science community must make strategic choices on how to spend limited funds as wisely and effectively as possible. For this to happen we must understand the level of risks and costs at the time we undertake our Decadal Surveys. We cannot afford to have moderate scale missions at the few hundred million dollar level grow into multi-billion dollar programs. Modest (by comparison with the final costs) expenditures on technology development and on establishing a strong science and management team early in the planning and development process would be money very well spent.

3) Competed, cost-capped missions. These missions, at the medium scale (Einstein and Origins Probes – like New Frontiers), along with the smaller Explorers and Discovery-class missions have a valuable role. Having been a strong proponent of large “Flagship” missions (through personally spending a great deal of my career working to make Hubble a success and NGST – now JWST – a reality in its early years) I do not want to downplay the central role that large missions play in the Astrophysics science enterprise. Flagships, however, are rare and it is essential for the vitality of the field for frequent launch opportunities at the medium and small scale. Cost-capped, competed missions have many attractive features (e.g., focused science opportunities, community involvement, responsive to more current science goals, controls on cost-growth). However, heavy reliance on such quick response, “bottom-up” missions may undercut the benefits of strategic planning through the Decadal Survey. This can be rectified if the Decadal Survey gives guidance on broad areas that the community sees as important and ready for investigation (e.g., searches for planets around other stars – exoplanets; the early universe; dark matter and dark energy). A additional major concern for such missions could be the cost of launch vehicles with the demise of the Delta 2 launchers. This has the potential to be a serious issue for the small-medium scale missions.

As noted, an Astrophysics budget that is constant in FY06 dollars, with the FY06 base, could accommodate all these recommendations. Any growth as part of the legislative Innovation and Competitiveness agency would enable, for example, a new large Flagship mission in the next decade as well.