Report of the 2008 Senior Review of the Astrophysics Division Operating Missions
April 22 – 25, 2008

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Overview

The Astrophysics Division (AD) convened the 2008 Senior Review Committee (SRC) with the charge to rank the scientific merit of the following ten missions on a "science per dollar" basis, based on the expected returns from the projects reviewed during 2009 and 2010:

Chandra	Suzaku
GALEX	SWIFT
Gravity Probe-B	Warm Spitzer
INTEGRAL	WMAP
RXTE	XMM-Newton

The AD also asked the SRC for advice regarding whether the funding level for the Research and Analysis Program was appropriately balanced with the funding for missions, and whether funds should be set aside to support missions of opportunity through the proposed SALMON Program.

The SRC reviewed proposals from each of the mission projects, which included budget requests according to three scenarios: (1) "nominal", i.e., the minimum budget needed to carry out the project; (2) augmented; and (3) mission close-out (for the two Great Observatories). The SRC members read all of these proposals before convening. During the SRC meeting, representatives for each mission project presented an overview and responded to intensive questioning by the SRC.

The SRC members discussed these presentations and then carried out a first straw poll in which "overall science return" was the guiding criterion. They discussed the missions further in the light of the budget constraints. They also discussed what was meant by the metric of "scientific merit per dollar", and decided that "scientific merit" should include not only the stand-alone merit of a particular mission, but also its incremental benefit to NASA's overall astrophysics portfolio. Subsequently the SRC conducted a second poll. The resulting rank-ordering, which differed only slightly from the first poll, is displayed below.

1. Swift
2. Chandra
3. GALEX
4. Suzaku
5. Warm Spitzer
6. WMAP
7. XMM-Newton
8. INTEGRAL
9. RXTE
10. Gravity Probe-B

The SRC unanimously endorses this rank-ordering.

Detailed summaries of the scientific merits of each mission and recommendations for funding follow. In ranking these missions, the SRC was cognizant of these details and it placed particular emphasis on the science that would be accomplished during FY09 and FY10, in anticipation that the 2010 Senior Review of Operating Missions will reconsider the scenario for FY11 and FY12.

The SRC notes that, if sufficient funding were available, the top nine missions in the rankings would be certain to deliver unique data of high scientific value. In the case of GP-B, however, which failed to reach its sensitivity goals, the SRC considers it unlikely that the proposed analysis will result in significant additional constraints on theories of gravity.

The 2008 Senior Review was particularly difficult for two reasons: (1) the projected budget envelope provided to the SRC made it clear that the Astrophysics Division would not be able to continue supporting missions that the SRC regarded as having high scientific value; (2) the distribution of mission budgets was highly skewed as a result of the inclusion of two Great Observatories. The budget of Chandra exceeded the sum of the budgets of all the other missions, and the budget of Warm Spitzer was comparable to the sum of the budgets of all the remaining missions.

Consequently, there was some sentiment in the SRC that an improved science yield might be attainable within the projected fiscal envelope if some funds were removed from the budgets of these Great Observatories to enable continuation of some of the smaller missions. However, the SRC did not believe that it had sufficient information to assess the consequences of doing so, nor did it have a charge to do so. Therefore, the SRC decided to rank the proposals based on the nominal budgets as proposed by the projects.

According to the rank-ordering by the SRC, Warm Spitzer falls at the break point at which projected funding runs out. If the Astrophysics Division adheres strictly to this rank-ordering, it will be impossible to support Warm Spitzer at the budget proposed by the project. Moreover, any recommendation to fund the project fully would likely have dire consequences for many of the smaller missions under review. As described below, the SRC regards Warm Spitzer as having great scientific merit, and recommends that the Astrophysics Division work with the project to develop a plan that will enable Warm Spitzer to carry out a scientific program optimized within the fiscal constraints. The final plan should allow for limited funding of the smaller missions as recommended below.

In establishing the rank-ordered list, the SRC was aware that participation by US scientists in two international missions, XMM and Integral, will be severely curtailed by loss of GO funding. Therefore it is imperative that funding of the ADP program (see below) is sufficient to support high-priority proposals for data analysis by US scientists for these missions after the GO funding for the missions is terminated.

The SRC endorses the plan presented by the AD for the R&A program budget. An assessment of the value of the program follows below.

In the absence of any specific proposals for participation in missions of opportunity, and in view of the severe budget constraints described by the AD, the SRC does not recommend setting aside funds for the proposed SALMON program, as discussed below.

The SRC has three recommendations regarding the process for future Senior Reviews:

1. As was done for the current Senior Review, mission teams should respond explicitly to recommendations in this Review in their proposals for the 2010 Review.
2. NASA should require the proposals for the larger missions to address the impacts of a specific reduction (e.g. 10%) below the in-guide target. Having this information would help the SRC address the scientific tradeoff between reducing capabilities of the larger missions versus extending the lifetime of the smaller missions.
3. The SRC Chair should assign primary and secondary reviewers in advance of the meeting. Primary and secondary reviewers could collect and organize questions to the presenters to make the discussion proceed more efficiently.

Finally, the SRC commends the Astrophysics Division staff for their efficient and thorough support of the Review Process.

SWIFT

Swift, launched in 2004, is a multi-telescope MIDEX mission designed to detect and perform follow-up studies of gamma ray bursts (GRBs). Its combination of gamma ray, X-ray, and optical-ultraviolet telescopes, autonomous fast slewing capability, and rapid transmission of results has made it into the premier instrument for observing and studying transient high energy phenomena.

Spacecraft/instrument health & status:

Swift has been active for GRB monitoring for over 96% of the total available time. The BAT wide-field gamma ray detector's measured backgrounds are lower than predicted. A micrometeoroid hit on the X-ray telescope (XRT) CCD has caused a loss of 1% of the detector area, and the XRT is showing expected small effects of aging due to radiation damage. The UVOT ultraviolet telescope is fully operational.

Science strengths:

Swift has proven itself as one of the most productive GRB missions ever flown. Its ability to locate the associated X-ray emission precisely has led to a plethora of follow-up observations that have dramatically improved our understanding of GRB phenomena. Swift has yielded arcsecond positions of short gamma ray bursts, suggesting that they result from mergers of compact objects (NS-NS, NS-BH or BH-BH); it has probed the early universe with bursts detected out to a measured redshift of at least $z = 6.3$; it has collected data to probe the connection between GRBs and supernova explosions; its capability for rapid follow-up observations of afterglows has shown that the duration of the GRB's central engine activity may persist for as long as hours.

In addition to making important contributions to GRB science, the BAT instrument observes a large fraction of the sky daily to produce the most complete all-sky survey of the hard X-ray sky, with a sensitivity approaching 0.5 mCrab. Swift is accommodating an ever-increasing number of target of opportunity observations with its XRT and UVOT instruments, covering a wide range of science including supernovae, CVs/novae, AGNs, galactic transients, active stars, and comets.

Swift's unique ability to precisely locate and follow transient sources is an essential complement to GLAST's GeV sensitivity and the increasing TeV sensitivity of ground-based air Cerenkov telescopes. Continued observations of GRBs will provide crucial inputs to the enhanced gravitational wave sensitivity expected from LIGO in the next 2-3 years. Swift will be an essential and active contributor to multi-wavelength campaigns involving multiple telescopes for the foreseeable future.

Relevance to NASA priorities:

Swift directly addresses science goals in the NASA Science Plan relating to black holes, the formation of the first stars and galaxies, and the structure of the universe. Its supernova observations address the cycle of elements, and its observations drive an extremely effective education/public outreach program.

Proposal weaknesses:

The Swift team proposed that it augment its high-z capability by providing funding to instrument an array of modest 2-meter class ground-based infrared telescopes. Scientifically, the idea to obtain complementary ground-based IR measurements makes excellent sense. Given the current funding environment, however, rather than constructing a suite of new instruments, the SRC recommends that Swift survey existing facilities in order to find collaborators with available IR capability that can be directed to the burst follow-up that Swift needs.

Overall assessment and recommendations:

Swift has been an extraordinarily successful mission covering a wide variety of scientific topics and promises to be a central component of NASA's high energy astrophysics program for a number of years. It is the highest rated program in the suite of missions considered by this Senior Review. The SRC recommends that Swift funding continue at its nominal proposed level through FY 10, augmented by $400 K in FY 09 to permit the project to develop tools for increased automation of mission operations. The SRC also recommends continued funding at the in-guide level through FY 11-12, subject to the 2010 Senior Review.

Chandra X-ray Observatory

The Chandra X-ray Observatory is one of NASA's four Great Observatories, providing high spatial X-ray imaging and spectroscopy in the 0.06-10 keV photon energy range. Its complement of instruments include the ACIS-I and ACIS-S CCD detectors with energy resolving power of R=10-50, and the High Resolution Cameras HRC-I and HRC-S, with essentially no energy resolution, but with 15 microsecond timing resolution, and a modestly smaller point spread function than the ACIS cameras. The dispersive gratings – the High Energy Transmission Gratings (HETG) with R = 60-1000 in the 0.5-10 keV range, and the Low Energy Transmission Gratings (LETG) with R = 30-2000 (0.06-6 keV range) – enable high-resolution spectroscopic investigations.

Spacecraft/instrument health & status

Nine years following launch, the spacecraft and observatory operations remain nominal. The photon sensitivity of the ACIS-S detector below 1.0 keV has decreased since launch due to contamination on the detector surface; this effect has stabilized and is calibrated. The thermal blanket degradation and its effect on the IEPHIN detectors, which protect the safety of the cameras by monitoring the solar particle flux, have been addressed through limiting scheduled pointing durations within certain sun-angle constraints. The long-term degradation of the thermal blanket is a concern, and a plan for mitigating its effects is needed.

Science Strengths

Chandra’s small PSF, instrumental versatility, and low and stable background capabilities continue to produce groundbreaking scientific results across a range of fields. Chandra has recently produced – and is expected to continue to produce – results which change our view in areas as diverse as dark matter, dark energy, and clusters of galaxies. It has measured the relativistic broadening of X-ray lines near the horizons of black holes; the metalicities and activities of stars, and charge exchange between the solar wind and comets;

Relevance to NASA priorities:

The instrumental flexibility of Chandra enables leading studies in a broad range of science, responsive to a number of NASA Strategic goals, including studies of dark energy and dark matter, the evolution and environmental impact of AGN, the nature of gravity, phenomena near black holes, and the structure and evolution of stars.

Data Accessibility:

Guest Observers receive calibrated data following data verification, on average, 1-2 days after the observation is taken. Data are placed in the public archive 1 year following observation. All data taken under the Very Large Programs are made public immediately following verification. These procedures provide highly accessible data to guest observers and archive scientists.

Proposal Weaknesses:

The SRC felt that Chandra, as a mature operating mission with an experienced user community, should be in a position to identify further economies in operations beyond that presented to the SRC. Such economies might come about in various ways, some of which were discussed during the oral presentation. While not in position to make specific recommendations, the SRC considers it likely that savings of a few percent over the next two years could be found without greatly affecting the science return or reducing the number of supported observing modes. Anticipating that budget constraints will continue to be tight, the science impact of reducing or eliminating support for specific observing modes should be discussed as part of the next senior review.

Overall assessment and recommendations

The SRC recommends funding at the in-guide level for FY09-FY10, and planning for mission extension at the in-guide level through FY11-12.

GALEX

The Galaxy Evolution Explorer, a NASA Small Explorer UV survey mission launched in 2003, is in its fifth year of operation. The GALEX team is imaging galaxies and selected Galactic fields in the UV using FUV and NUV detectors centered on 1500 and 2300 Angstroms, respectively. There is also a grism mode for low-resolution spectroscopy. The GALEX project has a vigorous GO program that is enhancing its scientific return.

Spacecraft/instrument health & status:

All flight and ground systems are currently healthy. The data quality from the UV detectors and instrument performance meet or exceed pre-launch expectations and the instrumentation is stable. A high-voltage (HV) current anomaly continues to recur in the FUV detector, but the project has learned how to recover through a procedure of HV cycling. The observing time efficiency during the past two years has been more than 75%.

Science strengths:

As the first wide-area UV imaging survey mission, GALEX continues to return unique images of nearby galaxies that effectively isolate the young OB star population and yield important information on the star formation rates, clustering, and evolution in the nearby universe. New discoveries continue to be made. Noteworthy is the imaging of UV-luminous wakes and gas tails associated with AGB stellar winds, including evidence that this type of activity contributes in an important way to the formation of interstellar cirrus. The GALEX Extended Mission surveys will be a valuable database for understanding the UV sky and a critical dataset for modeling galaxy structure and evolution when combined with X-ray, optical, and IR data from Chandra, HST, and SPITZER, and on-going and future H I surveys and future molecular line observations with ALMA. The Project has responded completely to each recommendation of the 2006 Senior Review: The prime mission was completed in 2007 September, four large data releases have been delivered to the MAST, and progress was made toward completing observations for the extended mission and a backlog of GO observations.

Relevance to NASA priorities:

GALEX observations are contributing to our understanding of star formation, galaxy evolution, and the interaction between the mass loss from evolved late-type stars and the ISM, which support the NASA Astrophysics Division's core goals for investigating Cosmic Origins and the Physics of the Cosmos.

Data accessibility:

Processed and calibrated GALEX data are available through the MAST archive. Four data releases from the primary mission have been delivered. Many GO observations are also now part of the public archive. Community use of the GALEX archive is increasing and accounts for more than 30% of the MAST searches and 10% of the downloads.

Proposal weaknesses:

A nine-year plan for completing several surveys was proposed. While it is clear that the incremental gain of an additional nine years would be great relative to the primary mission, the SRC would have preferred to see a proposal focused on the science that could be achieved in the next two years. The proposal lacked strong quantitative justifications for obtaining the deeper exposures. The SRC was skeptical that the time spent on the Time Domain Survey (TDS) would produce useful or ground-breaking results and would have preferred to see some samples of targets of interest and a preview of information on variability already obtained. The SRC is concerned that a completely uniform sampling in the TDS might preclude front-loading the legacy surveys aimed at obtaining deeper images of the most important areas of the sky.

The GALEX FUV detector continues to experience a current anomaly. Even though the GALEX operations team has developed a procedure for recovering from such events with minimal downtime, the SRC is concerned that the events could become more frequent and that the recovery technique might not continue to be successful.

The proposal did not clearly show how the GI program complements the goals of the project. The SRC felt that the proposal would have been stronger if it had contained a more detailed summary and discussion of some of the GI projects that are currently in progress and examples of projects that could be tackled in future GI programs.

Overall assessment and recommendations:

GALEX remains a unique facility, and continuing its operation has tremendous legacy value for astronomy, since no other US mission with wide-field FUV imaging capabilities is planned. The SRC appreciates the attention that the GALEX Project gave to the concerns of SR06. The proposal puts forward an ambitious plan for legacy surveys. While the SRC strongly endorses continued operation at the proposed funding levels for the next two years, we recommend that each year be considered as potentially the last year, and that the team prioritize its plans accordingly. This will undoubtedly affect the uniformity of the TDS sampling, but will result in the acquisition of the deep UV observations sooner, which the SRC regard as more important.

The SRC is enthusiastic about the potential science gain from continuing GALEX for two more years, but is less optimistic about the feasibility of the full nine-year plan for the surveys while carrying through a significant GO program. Further continuation beyond 2010 will require a much stronger quantitative justification for the incremental science gain from the additional depth, area, and time coverage from the major surveys, and the appropriate balance of benefits from these surveys and the GI program.

The SRC recommends that funding of GALEX be extended until 2012 at the proposed minimal budget level, with the proviso that recommended funding in 2011 and 2012 should be revisited by the 2010 Senior Review.

Suzaku

Suzaku is a joint Japanese-US X-ray mission launched in July 2005 as a replacement for the failed Astro-E satellite. The primary goal of the mission was to enable spectral studies of X-rays with unprecedented resolution using the XRS calorimeter instrument. The XRS was complemented by a suite of instruments (XIS, HXD/PIN and HXD/GSO) to provide very broadband coverage (0.2-600 keV) of sources using detectors designed to have very low backgrounds. Because of the limited cryogen gas supply, mission design and planning focused on use of the XRS for the first mission years. The loss of the XRS soon after launch caused a major reorganization of mission priorities and resources, with a shift of focus to the broadband instrumentation suite. This, together with problems in the broadband suite (the overall XIS experienced contamination issues, the XIS-2 CCD failed, and the initial HXD background models were not as good as hoped), led to a significant loss of scientific productivity during the first year or so of scientific operation. The problems with the broadband instruments now appear to be under control, and Suzaku is poised to fully exploit its unique and powerful capabilities.

Spacecraft/instrument health & status:

The broadband instrument suite now appears to be in good health. The worrisome XIS contamination appears to have stabilized, and the HXD background appears stable. The background model works down to the hoped-for 1% level. Charge re-injection in the XIS CCDs is effective, giving Suzaku the best energy resolution among currently flying CCD instruments. Adequate instrumental data analysis tools now appear to be in place, as evidenced by a significant jump in the number of recent publications.

Science strengths:

The Suzaku XIS instrument has comparable collection area to the CCD instruments on Chandra and XMM but a significantly lower instrumental background, somewhat better energy resolution, and much higher live time for bright sources. For compact object sources, these attributes, coupled with the high-energy response of the HXD, allow simultaneous and unambiguous measurements of the typical source emission components, in particular of the relativistic iron line at ~6 keV used to estimate black hole spin. Suzaku is thus the premier instrument for spectral studies of the emission from moderately bright to bright AGN and X-ray binaries. The low XIS background also makes Suzaku unique for spectroscopy of low-surface brightness regions, enabling, for example, studies of hot cluster gas out to the cluster virial radius. The XIS has the potential to detect and study the WHIM between clusters and measure abundances for hard-to-detect elements like magnesium, critical for understanding the enrichment history of the Galaxy and the IGM in nearby clusters. Suzaku's unprecedented low background in the HXD energy range also allows fast and efficient follow-up of the hard X-ray sources detected by SWIFT and INTEGRAL wide area surveys, and allows rapid tracking of the hard X-ray variability in bright blazar AGN. Overall, Suzaku's broad energy coverage and sensitivity, especially for extended sources, make Suzaku an excellent match for gamma-ray observations by GLAST and ground-based observatories, providing crucial coverage of the hard X-ray synchrotron emission component that typically accompanies the gamma-ray emission. The Suzaku team presented a strategy to concentrate on key projects, which the SRC regarded as an effective strategy for getting the most science out of the mission's remaining years.

Relevance to NASA priorities:

Suzaku directly addresses science goals in the NASA Science Plan relating to black holes, active galactic nuclei, and the final fates of stars.

Proposal Weaknesses:

Other missions are likely to yield more useful data for clusters at redshifts $z>0.5$. There was no detailed discussion of exactly how Suzaku will carry out timely joint observations with gamma-observatories, given that gamma-ray sources are highly variable and that the most interesting sources may not be known before a given proposal cycle. There was minimal mention of the WAM (Wide Area Monitor), which given its sensitivity at energies above the SWIFT/BAT cutoff, makes it a useful complement to SWIFT for GRB studies.

Overall Assessment and Recommendation:

Suzaku is still in the rapid growth phase of its scientific output. It has the potential to produce qualitatively new results rather than add incrementally to prior knowledge. Termination of support for US participation for the mission would significantly hamper the ability of the US community to access its unique capabilities.

Follow-up of transient, serendipitous sources was viewed as one of the key science goals for Suzaku, yet only the 4% director's discretionary time allocation presently appears to be available for this purpose. This may not be sufficient. Effective follow-up also requires tight and continuous coordination between the various mission teams. It was not clear this mechanism was in place. The Suzaku team is strongly encouraged to address these concerns.

Given the low cost to NASA for the mission's capabilities, the SRC recommends funding to support the Suzaku mission at the nominal budget request through 2012, subject to the 2010 Senior Review.

Warm Spitzer Mission

The Spitzer Space Telescope, a NASA Great Observatory, will complete its Cryogenic Mission in Spring 2009. The proposed Warm Spitzer Mission (WSM) would use the remaining imaging capabilities at 3.6μm and 4.5μm to carry out a set of large Exploration Science programs, a (smaller) General Observer program, and a continuing archival science program. The WSM as proposed would extend from FY09 to as late as FY14.

Spacecraft Health and Status:

Overall mission health is excellent. The warmup of the spacecraft and IRAC camera after the end of the Cryogenic Mission should not degrade the performance of the 3.6/4.5 μm imaging. Mission lifetime is limited by degradation in spacecraft communications, but this should not be an issue over the period of the proposed WSM.

Science Strengths:

WSM has sensitivity at 3.6μm and 4.5μm that greatly exceeds what is available from the largest ground-based telescopes, and will be unmatched until the launch of JWST. WISE will survey the entire sky, but to much shallower depths and with lower spatial resolution. The combination of extraordinary sensitivity, good spatial resolution and outstanding photometric stability makes the WSM a powerful and unique facility for projects that require precise photometry, and for deep large-scale surveys at near/mid-infrared wavelengths. There is a large discovery space that the WSM will be able to exploit.

Several large-scale projects are particularly compelling. For extragalactic research, Spitzer's sensitivity at wavelengths longer than 3μm enables deep searches for high-z galaxies, and when combined with observations at shorter wavelengths, constrains the stellar masses and ages of young galaxies. Current observations are limited to small fields and hence suffer from cosmic variance uncertainties. The WSM will also be an excellent way to locate and characterize the stellar content of clusters of galaxies at lower redshift. The extension of the GLIMPSE survey to cover the entire Galactic plane would be a valuable legacy product, and the measurement of rotation periods for numerous pre-main-sequence stars will provide key insights into how the angular momenta of young stars changes with age, stellar mass, and the presence of a circumstellar disk. WSM will be the only mission before JWST that can measure the thermal radiation of exoplanets to constrain models of these objects. The pace of discovery of transiting exoplanets is dramatic, doubling roughly every year, and even more will come from COROT and Kepler. Follow-up observations should be an active and exciting topic for a WSM.

The SRC was impressed with the excellent job Spitzer has done with all aspects of its Cold Mission, including the Legacy surveys, GO support, and archival research support.

Relevance to NASA priorities:

The proposed large surveys of exoplanets, deep extragalactic fields, and star formation in the Galaxy all address key NASA SMD priorities, and have strong synergies with major surveys being carried out with other NASA facilities. The critical role of the WMS for exoplanet research was recently underscored by the AAAC Exoplanet Task Force. The extragalactic surveys would leverage large multi-wavelength datasets, and would help to identify priority science targets for JWST.

Data Accessibility:

Accessibility to the current Spitzer archive is excellent.

Proposal Weaknesses:

The SRC was impressed with the scientific potential for many of the main Exploration Science initiatives, particularly for exoplanet studies, the deep-field extragalactic surveys, and Galactic star formation research, but it was not convinced that a full 4-year project was needed to achieve the maximum scientific return per dollar from a WSM. The SRC considered whether a credible WSM program might be carried out at less than the requested nominal cost, given the greatly reduced complexity of the instrument suite. The SRC also questioned the scientific impact of the NEO project, and noted that the lack of spectroscopy reduces some of the scientific impact of the exoplanet research. JWST will bring yet another leap in capability (in imaging and spectroscopy) to bear at these wavelengths, and the proposal should have clarified better the degree to which JWST will affect the proposed WSM key projects.

Overall assessment and recommendations:

This proposal was unique in this review and presented difficult challenges to the SRC. The cost of the WSM as proposed is high, and there were no funds allocated for it in the in-guide budget given to this SRC. Moreover, the severe budget constraints precluded any meaningful recommendation regarding funding beyond the immediate 2-year FY09-FY10 horizon of this Senior Review.

Despite these severe constraints, the SRC was very impressed with the science value of a WSM, and ranked the science-per-dollar value of the project in the upper half of all missions under review. It recommends that NASA negotiate with the Spitzer project to develop a funding scenario that would allow the highest-priority Exploratory Science projects highlighted above to be carried out in FY09-FY10. Should this be accomplished, serious consideration should be given to an extension of the WSM in the next Senior Review.

WMAP

The Wilkinson Microwave Anisotropy Probe (WMAP) is a MIDEX mission designed to produce all-sky maps at five microwave frequencies (23, 33, 41, 61, and 94 GHz) with high sensitivity and precision. The WMAP team has produced three data releases containing the data and analysis from 1, 3, and 5 years of operations, with the most recent release occurring early in 2008. Current in-guide funding will enable the collection of data for 7 years and the WMAP team presented a proposal to collect and analyze 9 years of data. WMAP has helped usher in the age of precision cosmology, and its scientific impact has been remarkable for a MIDEX class mission.

Spacecraft/instrument health & status:

Mission operations at L2 remain extremely smooth and reliable. There are no consumable cryogens, and fuel reserves are more than adequate.

Science strengths:

WMAP has transformed cosmology into a precision science and continues to address fundamental scientific questions. In this new era even modest gains in sensitivity can have a big impact. At present, WMAP remains unique in its ability to probe large-scale structure and the early history of the Universe. Key areas in which 9-year data would be useful are:

- Improvements on fundamental cosmological parameters, particularly those related to inflation such as scale invariance and departures from Gaussianity. The current data provide tantalizing indications that some of these quantities are incompatible with leading cosmological models, and the additional data may be able to rule out some of these models.
- Unique constraints on reionization, most notably by continuing to improve the low-order multipoles of the EE polarization maps, which are pointing to very early structure formation and a complex reionization history. WMAP provides a unique view of this important epoch.
- Improved 22 GHz maps of the galactic synchrotron radiation for foreground removal. These foregrounds dominate polarization maps on large angular scales, and their accurate removal is key to future Planck and ground based campaigns to measure the B mode signature of gravitational waves.

In addition, continued operation of WMAP allows for improved analysis of all its previous data through, e.g., improved characterization of the beams. Finally, much of the secondary science, such as source counts and spectra at millimeter wavelengths, is of high value.

WMAP results are heavily used by the astronomical community, continue to have a high impact, and are a public relations benefit for NASA.

Relevance to NASA priorities:

WMAP continues to be fundamentally relevant to the NASA Strategic Objective 3D and the NASA Science Plan (Astrophysics) Objective "What is the origin, evolution, and fate of the Universe?"

Data accessibility:

Data are archived and distributed through LAMBDA, which was established to be NASA's central CMB data archive and distribution center. All data products, including spacecraft instrumental monitoring products, are archived.

Proposal weaknesses:

The chief weakness of the proposal was the lack of discussion of waypoints in the analysis, in particular, what could be learned from the 7 year maps, and how they would compare to the projections made for the 9 year maps (the proposal compared results only from 9 year vs. 5 year datasets). This omission is particularly striking given that the 2006 Senior Review raised similar concerns.

In cosmology there will always be some effect at the margins of detection, such as neutrino masses or the evolution of the dark energy equation of state parameter, and of itself this does not make a case for continuation. The proposal did not clearly rank the importance of the parameters that are currently on the bubble, nor indicate which would be adequately resolved with the 7 year maps.

Overall assessment and recommendations:

WMAP continues to deliver high impact, high profile science for a modest cost. While a successful Planck mission will ultimately eclipse the extended WMAP mission in many areas, WMAP has unique capabilities for mapping foregrounds and measuring polarization on the largest angular scales. A 9 year map will allow important constraints to be tightened on key cosmological parameters, on a short timescale and with little risk.

The SRC recommends continued funding of WMAP at their requested minimal budget level through FY10. Funding beyond FY10 should be addressed in the next Senior Review. Results from the 7 year analysis should be made public prior to the next Senior Review, and a detailed assessment made to ensure that the error projections for the 9 year maps are on target.

XMM-Newton

XMM-Newton is a Great Observatory class facility that was a cornerstone of ESA's Horizon 2000 program. XMM has six co-aligned instruments: EPIC, an imaging X-ray telescope; RGS, 2 high-throughput, high-resolution grating spectrometers; and OM, an optical/UV monitor with simultaneous spectral, photometric, and imaging capabilities. NASA-sponsored teams contributed to the reflection gratings mounted on two of the telescopes and to the OM. The XMM Guest Observer program is open to US scientists with ~40% of the accepted proposals having US Principal Investigators and ~70% with US co-Investigators. Journal papers involving XMM data are abundant and typically well-cited.

Spacecraft/instrument health & status:

The XMM-Newton spacecraft and instruments are in good condition. There have been no adverse instrumental anomalies since the early years of the mission. The RGS is losing effective area at low energies, on the order of 30% at the lowest energies, at a rate that is consistent with a buildup of contamination. The loss is understood and is handled in the calibrations. The observing efficiency still remains high at ~70% with only about 4% being lost due to solar flares.

Science strengths:

XMM has made many important scientific contributions on topics such as galaxy clusters, relativistic Fe line emission from AGN, supernova remnants, X-ray binaries in other galaxies including ultra-luminous X-ray sources, isolated neutron stars, pre-main sequence stars, stars with planetary systems, and planets within our own solar system. The observing program for XMM remains oversubscribed by about a factor of seven, which is testimony to the continued interest of the astrophysics community in XMM's capabilities. The U.S. GOF has reduced its costs to a bare minimum, with the budget largely supporting the GO program.

Relevance to NASA priorities:

XMM observations directly address NASA scientific objectives in the areas of physics of the cosmos and cosmic origins.

Data accessibility:

X-ray data access via the HEASARC is straightforward and well understood by the community and there are no issues.

Proposal weaknesses:

XMM has produced many interesting and exciting results during its lifetime, but the scientific case for continued funding of the US GOF made in both the documentation and the briefing to the committee was not compelling. The recommendation of the 2006 SR was to plan for a reduction in the GI program in the FY09 and FY10 timeframe. The case was made by the proposers that this is undesirable to the users and the project, and no attempt to plan for reduction was made by the team.

Overall assessment and recommendations:

XMM-Newton remains an extremely valuable resource. The scientific output of U.S. observers is high for a relatively low investment of NASA funds. The SRC's recommendation for a reduction in funding of the US XMM Guest Observer Facility does not indicate lack of support for XMM science. Rather it reflects the reality that a cut to a primarily US-funded mission would lead to serious, potentially irreversible damage to that mission, while a reduction in US XMM funding does not fundamentally impair XMM science, and this decision can be revisited in later years. US XMM science will be strongly impacted by a cut in GO funding, but this impact can be partly mitigated a strong ADP budget, which would enable the best US science to be carried out.

The SRC recommends that this program continue at the proposed in-guide level in FY09 followed by closeout in FY10.

INTEGRAL

INTEGRAL is an ESA mission that was launched in October 2002. It has completed more than five years of successful operations. ESA has recently approved a mission extension until the end of 2012. There are four instruments on the INTEGRAL spacecraft. The two primary instruments cover the energy range from 20 keV up to 10 MeV. Both are coded aperture imaging instruments. IBIS has relatively high angular resolution (12' FWHM) over an 8° FoV with moderate energy resolution. SPI has high energy resolution (using Ge spectrometers), with moderate angular resolution (2.5°) over a larger 16° FoV. The two gamma-ray instruments are supported by two context instruments, one for low energy X-rays and one for optical measurements.

Spacecraft/instrument health & status:

The INTEGRAL spacecraft and its subsystems are performing flawlessly. A 72-hour, highly elliptical orbit ensures an adequate orbital lifetime. There are no consumables or other issues that might impact an extended mission. Fewer than 3% of the detector elements in the IBIS/ISGRI detector array have been lost. Although 2 of the 19 SPI Ge detectors were lost early in the mission, the integrity of the SPI array has remained stable for the past four years. The committee had no concerns about the spacecraft health.

Science strengths:

The unique capabilities of INTEGRAL for observations in the nuclear line region will not be addressed by any other currently-planned mission. The SPI instrument, with its unprecedented sensitivity for narrow line emissions, has made important observations of several nuclear lines, including the ^{26}Al line at 1.8 MeV and the ^{60}Fe lines at 1.173 and 1.333 MeV. These data are consistent with core collapse SNe being the dominant source of emission, but the data continue to improve constraints on the contributions of other sources. Observations of the diffuse 511 keV emission indicate an asymmetric distribution about the Galactic center. The origin of this emission is not well understood, but the distribution bears some similarity to the observed distribution of LMXBs as seen by INTEGRAL.

The IBIS instrument has provided extensive wide-field imaging along the galactic plane, with good sensitivity up to several hundred keV. It has identified more than 400 hard X-ray sources, more than 100 of which were previously unknown. From these data, at least two new classes of galactic sources have been discovered – a class of heavily obscured HMXBs and a class of superfast X-ray transients. INTEGRAL is providing constraints on the contribution of AGN to the cosmic X-ray background CXB), having obtained spectra of more than 100 AGNs spanning the ~30 keV CXB peak.

There is an important synergy between INTEGRAL and the soon-to-be-launched GLAST mission, with INTEGRAL providing a spectral bridge to lower energy X-ray observations.

Relevance to NASA priorities:

INTEGRAL observations are directly relevant to the Lifecycles of Matter goals of NASA's original Structure and Evolution of the Universe roadmap as well as current Astrophysics Division science objectives.

Data accessibility:

Data are provided to US investigators through HEASARC, and user support through the US INTEGRAL Guest Observer Facility (GOF). Issues with regards to data delivery early in the mission have apparently been resolved. The INTEGRAL team has made great strides over the years in making the data analysis more accessible, with continual improvements to the analysis software and the continued support of data analysis workshops.

Proposal weaknesses:

With the exception of the prospect for nearby SN line observations, the proposal offered mainly incremental improvements on previous science. Some of the most interesting new science (e.g., observations of lines from a future nearby supernova, should one occur) is not guaranteed. The size of the US community involved with INTEGRAL was judged to be small in comparison with other missions before the committee.

Overall assessment and recommendations:

Although the committee recognizes the unique aspects of the INTEGRAL mission, much of the science was seen to be incremental. The SRC recommends that NASA continue to support INTEGRAL (with an emphasis on the US GO program) at a reduced level until September of 2010. With the continued operation of INTEGRAL by ESA and the continued access to data through the European ISDC, it is expected that some level of support for US investigators would become available through programs such as ADP.

RXTE

RXTE is a broad-band X-ray telescope that was launched in 1995. Its particular strengths are its large collecting area, sub-millisecond timing capability, and versatility in scheduling. These characteristics and its instrument complement, the All Sky Monitor (ASM), the Proportional Counter Array (PCA), and the High Energy X-ray Timing Experiment (HEXTE), make it a powerful tool to probe the immediate environments of collapsed objects, specifically Galactic neutron stars, stellar-mass black holes and accreting supermassive black holes (active galactic nuclei) in other galaxies. RXTE is complementary to other operational high-energy missions, and has undertaken numerous joint programs with other spacecraft as well as with ground-based observatories.

Spacecraft/instrument health and status:

Instrument status does not seem to have changed materially since the last Senior Review. There is currently no reason to believe that RXTE will not continue to function in a scientifically productive fashion for at least two more years. One proportional counter unit in the PCA continues to show no faults, and the others are on about 20% duty cycles.

Scientific strengths:

RXTE, with its unique capabilities for rapid re-pointing, transient follow-up, and long-term monitoring, continues to excel in the study of bright neutron stars and black holes of all sizes. Recent scientific highlights include the discovery of a 70.5 ms pulsar associated with a TeV source detected by the High Energy Stereoscopic System (HESS), discovery of a rotation-powered pulsar that transitioned to magnetar-like behavior, and detection of intermittent accreting millisecond pulsars. Entirely new phenomena were observed, such as the spin-down of a millisecond pulsar between outbursts with an increase in the orbital period growth that is too fast to be ascribed to gravitational radiation alone. RXTE continues to make measurements of black hole spins and masses (including the now least-massive known black hole at 3.8 solar masses). With combined data from RXTE, Swift, and XMM-Newton, astronomers inferred the radius of the inner accretion disk of a binary in a low-hard state, and discovered that only the low-hard state supports a steady jet. On the extragalactic side, RXTE is tracking the injection of plasma blobs in the jet of BL Lacertae. Contemporaneous monitoring of the Seyfert galaxy NGC 7213 by RXTE and the VLA shows that the X-ray and radio variations are well-correlated, with a delay between them of no more than 20 days. Although Suzaku can replicate RXTE's broad energy coverage, it does not replace RXTE's sensitivity to rapid variability and its broad sky coverage.

Relevance to NASA priorities:

RXTE addresses the goals of the Physics of the Cosmos theme of the Science Mission Directorate and Subgoal 3D of the 2006 NASA Strategic Plan.

Data accessibility:

Data are easily accessible to the entire community through HEASARC.

Proposal weaknesses:

RXTE is a very mature mission (currently in Cycle 12) and recent progress has been incremental rather than transformational. Newer missions, such as Suzaku and Swift, replace in part (but not fully) its unique scientific capabilities. In this extraordinarily competitive situation RXTE is therefore in a weakened position as a stand-alone mission.

Overall assessment:

While RXTE continues to do good science, after more than 12 years of operations its scientific impact is rather less than that of newer missions, despite its scheduling flexibility, sensitivity, and high time resolution. This is unfortunate since: (1) despite the recent lack of funding for a GI program, demand and productivity remain high; (2) the mission's operating costs are very low; and (3) no other high-energy facility is capable of high time resolution observations over a long period. Unfortunately, given the current funding climate, we cannot rank this project highly.

As noted, though, the loss of RXTE impairs our capability to monitor an inherently time variable X-ray sky, where previously unknown classes of transient sources continue to be discovered. This monitoring capability would be particularly useful during the first few years of GLAST. If the overall funding scenario were to improve so that higher ranked missions could be funded at the levels recommended here, the SRC would endorse support of RXTE at the minimum level required to preserve its monitoring and rapid follow-up capabilities.

The SRC recommends that RXTE retain its current in-guide funding profile for a termination date in February 2009.

Gravity Probe B

Gravity Probe B (GP-B) is an experiment in fundamental physics designed to measure the phenomena of geodetic precession and frame-dragging predicted by the theory of general relativity (GR). The magnitude of these processes should be detectable by the precession of an exquisitely precise gyroscope in earth orbit, and their measurement constitutes a direct weak-field test of GR.

Current Mission Status:

The GP-B satellite, which contains four gyroscopes, had a successful launch in April 2004 and a 353 day science mission. In August 2005 the acquisition of science data ended and the planned post-science calibration began, which continued until late September 2005 when the satellite's cryogen was depleted.

The experiment suffered from unexpected complications which compromised the behavior of the gyroscopes. The effects were caused by patch charges and trapped magnetic flux in the gyros, which exert torques in addition to, and greater than, the effects the team seeks to measure. The net result was to produce systematic errors – drift rates and occasional jumps in the gyroscope orientation – that are substantially greater than expected. The GP-B team has engaged in an aggressive analysis of both their science data and their ancillary engineering data in order to model the physical effects that produced these undesirable drifts.

The GP-B team has been able to model these effects to some extent, and have achieved a major reduction in the uncertainty of their measurements of gravitational precession compared to their initial results. The team believes it can reach a precision of a few milli-arcseconds per year in the measurement of gravitational frame dragging and geodetic precession. They seek funding at this time to continue the data analysis for 2 years to reach this level.

Fundamental Issues with the Experiment:

During the long evolution of this experiment, constraints on alternate theories of GR have developed and there have been a number of measurements which have tested various aspects of GR to very high precision. For this reason, the GP-B experiment has been somewhat overtaken by events and now occupies a diminished niche in the field of experimental tests of GR.

Theories that can be expressed in Post-Newtonian (PN) formalism in the weak-field limit are sharply constrained by spacecraft laser ranging, VLBI observations of light bending, lunar laser ranging and radio pulsar timing. In PN formalism, geodetic precession is proportional to $1 + 2\gamma$ and frame dragging to $1 + \gamma + \frac{1}{4}\alpha_1$. Taken together, these observations constrain γ to a precision of about 1 part in 10^5 and α_1 by about 1 part in 10^4. These are about 2 orders of magnitude below the limits that might be achieved by GP-B in the team's optimistic projection of what they can do. Hence, in the most optimistic case, they would fail by 2 orders of magnitude to improve the current constraints on these alternate theories.

For possible theories which cannot be expressed in the PN formalism, it is more difficult to determine whether GP-B can improve our understanding of gravity. A stronger proposal would have discussed such straw-man theories. The SRC believes that, while the potential payoff is great, the likelihood that a weak-field solar system experiment like GP-B could falsify GR in this way is extremely unlikely. Future missions such as LISA directly explore strong gravitational fields at the horizon of a black hole, and have much more power to achieve this via a detailed mapping at high S/N of a Kerr metric (or whatever would replace it in such a theory).

Strengths:

The GP-B team has great technical skills, and has demonstrated considerable ingenuity to date in their analysis of this data.

Relevance to NASA priorities:

The GP-B mission is a direct weak-field test of GR. It is relevant to the NASA Research Objective to "Understand the origin and density of the universe, phenomena near black holes, and the nature of gravity."

Proposal Weaknesses:

The factor of ten increase in precision sought by the team relies on the accuracy and completeness of models of the systematic effects that perturbed the gyros. It will be difficult if not impossible to rule out additional un-modeled or overlooked systematics at the levels they are trying to reach. The gap between the current error level and that which is required for a rigorous test of a deviation from GR is so large that any effect ultimately detected by this experiment will have to overcome considerable (and in our opinion, well-justified) skepticism in the scientific community. The SRC does not believe that the team will be able to reduce its errors to the level necessary to produce a convincing test of currently-untested aspects of GR.

Recommendation:

The SRC understands the desire by the GP-B team to complete the modeling of the systematic errors in their data. However, funding the GP-B proposal would require the Astrophysics Division to further reduce support of missions that are currently producing science of high quality. The SRC does not think that such a transfer can be justified, and therefore does not recommend further funding for this project.

Research and Analysis

The SRC was extremely encouraged by the plans for R&A funding presented by Wilt Sanders. R&A supports instrument and technology development, fundamental theory, data analysis, and the training of young scientists. All these activities are essential for extracting the most science out of the current suite of NASA missions and providing the necessary foundation for NASA's future. Yet the R&A program has been under great stress over the past few years. The plans to correct this by increasing R&A funding gradually to $99M in FY13 amounts to an effective 2.5% annual increase since FY04, an urgently needed contribution to the NASA astrophysics research program. The SRC did not examine in detail the relative distribution of funds to different programs within the overall R&A funding line. As in the past, NASA should involve the astronomical community in setting R&A priorities.

The R&A program constitutes the fundamental basis for developing NASA's future missions and it has supported the development of new technologies that have formed the basis for successful space missions. Instruments as diverse as X-ray calorimeters, microchannel plate detectors, spiderweb micromesh bolometers, room temperature semiconductor gamma ray detectors, and coded aperture imagers have all been initially developed and tested through the R&A program. Laboratory astrophysics investigations yield fundamental knowledge of physical processes needed to interpret data on the cold regions deep inside molecular clouds, the extreme environments around black holes, and the surfaces of solar system objects. Theoretical research supported by R&A is critical to generate the ideas for successful future missions, to generate the simulations needed to design new instruments and missions in the necessary detail, and to ensure that the return on NASA's investments is utilized effectively. Archival research makes it possible to maximize the science return from NASA missions, and suborbital campaigns have enabled direct flight tests of instruments used in space missions including ROSAT, BeppoSAX, Einstein, Chandra, and Suzaku. Balloon flights have produced the first maps of structure in the cosmic microwave background, evidence for positron annihilation near the Galactic Center, and measurements of cosmic ray antiprotons that constrain the nature of dark matter. Finally, the R&A program is a training ground for the future Principal Investigators who will lead the next generation of NASA's space missions, and the students who will be the core of NASA's future research capability.

The SRC anticipates an increased demand on the ADP budget for support of analysis of data from XMM-Newton and Integral as a result of termination funding of the Guest Observer Facilities for these missions.

It is essential for NASA's health to preserve a strong R&A foundation even at a time when NASA's space missions are faced with constrained budgets. The SRC strongly supports NASA's proposed budget for the R&A program, even in the face of the likely need to curtail successful operating space missions.

SALMON

The SRC was asked to assess the recent Stand ALone Mission of Opportunity Notice (SALMON) and to comment specifically on the following four issues: (1) What is the SRC's assessment of the merit of the MO categories? (2) What cost caps are appropriate for the categories? (3) What should be the frequency of Astrophysics solicitations in SALMON? and (4) What should be the annual budget for this part of the Astrophysics portfolio?

The SRC was troubled by several aspects of the SALMON program as presented:

- The prospect of curtailed Explorer opportunities over the next few years, coupled with the suggestion that Missions of Opportunity be supported by the Astrophysics Division's existing budget rather than out of the Explorer program budget, means that flight opportunities for small- and moderate-scale Explorer programs will be infrequent over the near future. This is inconsistent with the repeated recommendations of recent advisory committees that have emphasized the need for frequent opportunities for access to space through the Explorer program.
- It was not clear to the SRC why development projects at the level of $100K per year should be included as part of SALMON rather than in R&A.
- In the funding climate presented to the SRC, with the possibility that on-going successful missions will be terminated, the SRC could not assess the benefit of future unspecified missions with unknown budgets in comparison to the existing missions.

Consequently, the SRC does not recommend setting aside any funds at this time for the SALMON program.

The SRC notes that previous Missions of Opportunity involving collaborations with foreign space agencies have produced excellent science. Any future decision to fund such an opportunity should be subject to a rigorous review by an appropriately constituted scientific advisory committee.