SBAG ASSESSMENT OF DECADAL SURVEY AND RECOMMENDATIONS FOR ITS IMPLEMENTATION

The Planetary Decadal Survey process is designed to develop a comprehensive strategy for NASA's Planetary Science Division to continue advances in the growth of new knowledge about our solar system in the coming decade. It reviews the status of planetary science in the United States and presents a program of science and exploration and recommendations to achieve this goal. The latest survey, *Visions and Voyages for Planetary Science in the Decade 2013-2022* (V&V), was released in March 2011. The NASA Small Bodies Assessment Group (SBAG) has been tasked with providing input to the NASA Planetary Science Subcommittee (PSS) to review the impact of the decadal survey on small body science and make suggestions on how the recommendations of the survey may be implemented.

In general, the Decadal Survey vision and recommendations should have a very positive impact on the future of small body science. It recognizes the fundamental importance of planetary research programs and the Discovery program where much of the small bodies research is currently undertaken. Two of its five recommended New Frontiers missions involve small bodies (Comet Surface Sample Return, Trojan Tour and Rendezvous). Its recommendation for investment in technology development has the potential of increasing opportunities and science return from missions to small bodies across all mission classes. Its support for ground-based observations highlights an essential activity required to characterize the diversity of asteroids, comets and other objects comprising the different populations of small bodies. Such observations are required for designing cost-effective missions to these small bodies and provide the critical informational context necessary to understand and interpret the data returned from these missions.

The Decadal Survey made its recommendations in the context of positive budget projections for NASA that deviate substantially from current expectations. The Survey did recognize the potential for this and fortunately provided some guidance for understanding how its recommendations might be implemented. However, such guidance is limited, so when detailed implementation of language would contradict the goals and objectives identified by the Survey for that language, an implementation that advances the goals and objectives should be followed.

FINDING #1 – Current budget expectations for PSD are such that Flagship missions are not possible in the next decade. Pursuit of Flagship missions in this budget environment may jeopardize the implementation of decadal recommendations for smaller missions, research and analysis programs, and technology.

This is consistent with the V&V recommendations that Flagships be descoped or dropped depending upon mission expense and budget. The Survey recognizes that there may be insufficient funds to initiate any candidate Flagship missions in 2013-2022. In this event the Survey recommends continued technical studies and technology investments in the hope that this might enable Flagship missions in the future.

FINDING #2 – Future Flagship missions should be funded by Congress as new starts in the NASA PSD budget. This allows for the possibility of high science return missions identified within V&V as meritorious, but outside the budget range considered practical. This would require revisiting Flagship recommendations in an open and transparent process.

Flagships are included in a balanced portfolio supported by the Survey. However, the Survey does not demand that the existence of such a balance be implemented to destructive effect. To implement the Survey recommendations, an alternative path to funding Flagships must be pursued.

FINDING #3 – The importance given by the Survey to the R&A programs argues for an implementation of the Survey recommendation on R&A funding that would not allow these programs to fall below their FY2010 funding levels.

The Survey recommends increased funding "for fundamental research and analysis programs." Its means of accomplishing this was to increase funding by 5% "above the total finally approved fiscal year, FY2011, and growing at 1.5% per year above inflation for the remainder of the decade. This presumed only scenarios in which the FY2011 budget for these programs would be above FY2010.

It makes sense that the Survey does not couple research funding to a fixed percentage of the PSD budget or funding for missions. As the Decadal Survey and many other NRC reports recognize, these programs are foundational to the solar system exploration enterprise where funding stability is essential and funding growth should be measured. Even if it were decided that all new missions were to cease for the next decade, this would not motivate a reduction in support for planetary research. It would have the opposite effect. Funding would have to be bolstered to ensure that American core capabilities were maintained until such time that mission activity could be resumed, in addition to continuing to provide a return on tax-payer investment in past and ongoing missions.

FINDING #4 – To "maintain the original goals of the Discovery program" and

provide a "regular, predictable, and preferably short (< 24 month) cadence for Discovery Announcement of Opportunity (AO) releases and mission selections," NASA needs to have calls on 18-24 month timescales, selection of two missions per call, and accept higher mission risk. This would ultimately translate into an average of one Discovery mission launch per year.

Science return from Discovery missions has been consistently high and diverse. The Decadal Survey's exclusion of launch vehicle costs from its recommended $500M mission cap is important given the volatility of launch vehicle costs and ongoing changes in the launch vehicle market. An increased cadence of Discovery missions would increase the pace of technology readiness that would benefit New Frontiers and Flagship class missions as well. Increasing cadence would also encourage the selection of cheaper, riskier missions (recalling the success of the Hayabusa NEO sample return mission for under $200M).

FINDING #5 – The recommendation that the Discovery program call allow space-based telescopes to be proposed should include both survey facilities (analogous to WISE) and user facilities (analogous to HST and Spitzer).

For decades, significant planetary science has been achieved using space-based telescope facilities designed primarily to conduct astrophysical investigations. The potential return will be even greater when space-based missions are designed for planetary objectives, whether it is populational surveys such as has been endorsed for identifying NEOs optimal as targets for future human missions and low-cost robotic sample return mission or synoptic studies of planetary atmospheres or comets or many other possible important objectives. The range of astrophysics missions from SMEX to the Great Observatories offer a range of capabilities and experience that can be plumbed to the advantage of future planetary facilities. We note that Kepler was initially selected as a Discovery mission along with Dawn in the 2000 call.

Both space-based survey and user facilities should be considered given the number and diversity of the small body population within the solar system. Many small bodies have yet to be discovered and are not adequately detected via ground-based observations (e.g., NEOs). In addition, detailed characterization of small bodies from ground-based systems is limited by Earth's atmosphere and diurnal cycle. Having additional access to space-based systems would enable both small body science and enhance solar system knowledge in general.

FINDING #6 - The New Frontiers mission line provides important opportunities for the Small Bodies community to pursue scientific objectives whose feasibility is beyond the scope of Discovery class missions. Savings by forgoing Flagship missions should support the recommended selection of two New Frontiers class missions in the coming decade, in addition to supporting the recommendations for the Discovery program.

It is important for there to be a regular, predictable cadence of both Discovery and New Frontiers missions. As with Discovery, this is assisted by the Decadal Survey's decoupling of launch vehicle costs from the recommended $1B cap for New Frontiers missions.

FINDING #7 – Implementation of the Survey's recommended technology development program requires that a significant fraction of this program be dedicated to the development of technology that enables and enhances science return from the frequent Discovery class missions. The balance should be invested in potential technologies that enable and substantially reduce the expense of high-value New Frontiers and Flagship missions.

The Survey states: "the technology program should be targeted towards the planetary missions that NASA intends to fly…" In the past, NASA has tended to focus technology investment almost exclusively in areas benefiting infrequent Flagship-class missions. The most frequent missions are Discovery class, followed by New Frontiers class, and this should be taken into consideration when balancing the investments within technology development.

FINDING #8 - Within NASA investments in potential technologies that enable and substantially reduce the expense of high-value New Frontiers and Flagship missions in this decade and beyond, priority should be given to developing technologies required for a future Comet Cryogenic Sample Return Mission. These include: 1) collection of cryogenic samples from depths up to 3-meters below the nucleus surface, preferably intact core tubes; 2) containment of samples at cryogenic temperatures, ~ 125 K, during return to Earth and through re-entry and recovery; 3) analysis techniques for handling and studying cryogenic samples in the laboratory.

These technologies have applications to many other NASA missions, such as a Mars permafrost or polar cap sample return, or sample return from a main-belt comet, from a Trojan asteroid, and/or from a Galilean satellite.

FINDING #9 – The Survey recommended that mission instrument teams be required to generate high-level derived data products as well as low-level products before the completion of the project. This cannot be implemented without the segregation of funding for data product generation and archiving in a manner that protects these activities from mission cost overruns.

FINDING #10a – Recommendations for continued NASA support of large ground-based facilities (NASA IRTF, Keck, Goldstone, Arecibo, and VLBA) should be implemented with a corresponding share of time for solar system observations. . In addition, NASA should work with NSF to ensure that appropriate time is provided for planetary studies on NSF facilities. Such time allocations are required to aide research of the numerous small bodies within the solar system and help support spacecraft missions to these objects. These allocations are

important for the general study of solar system objects, but are more so for observing targets of opportunity and transient events (e.g., Impacts on Jupiter, asteroid collisions, close NEO approaches, comet outbursts, etc.).

FINDING #10b – The Decadal Survey recognizes the importance of the LSST for solar system studies and encourages the timely completion of this facility. NASA should ensure that an appropriate amount of LSST time be allocated by NSF for the detection and characterization of solar system objects that is commensurate with the level of funding contributed for NASA's share of the observations.

FINDING #11 – In times of fiscal limitations, NASA does best to focus on those recommendations that promote and solidify infrastructure supporting planetary science.

Our human resources (researchers, engineers, and students) and their corresponding knowledge and skills cannot be easily or quickly replaced if lost from the field. Increasing opportunities through the R&A program, technology development programs and Discovery-class missions will best serve the maintenance of a workforce essential to preserving our national core capabilities to undertake the exploration of the solar system. It also sustains investments made in the creation of this workforce and avoids inefficiencies and duplication of expenses inherent in restoring areas of expertise.

FINDING #12 – Decadal Survey support of NASA engaging in international partnerships, when appropriate, to enable missions unlikely to be pursued by single countries or single space agencies, should be implemented in part via participation by NASA in standing planning groups such as the International Primitive Bodies Exploration Working Group (IPEWG). In addition, the process required for NASA participation in international missions needs to be streamlined, so that the Agency can respond to potential missions of opportunity more effectively.

Space missions are increasingly expensive enterprises relative to available budgets. International collaborations are key to enabling the most ambitious missions supported by the Survey and for enhancing international participation in potential missions of opportunity. The IPEWG was formed to help address the issues related to international participation, but unfortunately there was a significant delay on the part of NASA to sign the charter. NASA was unable to sign the IPEWG charter document more than a year after its initial formulation, while other national agencies and aerospace companies signed it within a month of the charter's proposal. The process by which NASA approves participation in such international collaborations should be examined to understand the reason for these delays and determine mechanisms by which such delays can be mitigated in the future.

FINDING #13 - The Science Mission Directorate (SMD) and the Human Exploration

and Operations Mission Directorate (HEOMD) should evaluate and cooperate on future NASA robotic precursor missions and human expeditions to Near-Earth Objects (NEOs) with respect to synergies for science, exploration, resource utilization, and planetary defense. This requires a standing committee tasked to bridge these Directorates to provide awareness and input on these synergies. The SBAG is the appropriate committee to undertake this role.

The Survey states that although most of the key scientific lunar and NEO exploration goals can be achieved robotically, “The committee urges the human exploration program to examine this decadal survey and identify—in close coordination and negotiation with the SMD—objectives where human-tended science may advance our fundamental knowledge.” This will also benefit from reciprocity – when SMD examines HEOMD objectives and identifies relevant data generated by SMD that can advance those objectives and human exploration, it will also identify human-tended science that can advance our knowledge.