Statement of Mark V. Sykes
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Before the Subcommittee on Space
United States House of Representatives

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Mr. Chairman and Members of the Committee, thank you for the opportunity to appear before you today. My name is Mark V. Sykes. I am CEO and Director of the non-profit corporation Planetary Science Institute, which celebrates 42 years of active participation in American solar system exploration. PSI supports more than 90 PhDs in 21 States and the District of Columbia and is involved with almost every NASA solar system exploration mission. I have been a member of the planetary community for more than 30 years and have had the honor of serving as Chair of the Division for Planetary Sciences of the American Astronomical Society, a member of the NASA Planetary Science Subcommittee, and as a founding Steering Committee member and Chair of the NASA Small Bodies Assessment Group from which I have now rotated off. I am also a Co-Investigator on the NASA Dawn mission to Vesta and Ceres and a member of the Board of Advisors of Planetary Resources, Inc. The views I express today are my own, and do not necessarily represent those of the Planetary Science Institute, the NASA Small Bodies Assessment Group, or any other organization or committee.

Summary of Comments to the Committee

The Committee has requested my testimony on several issues facing the planetary science community. My key points to the Committee are:

Draft SBAG Recommendations

- SBAG recognizes that the Asteroid Redirect Mission does not effectively advance our knowledge of asteroids or planetary defense strategies and that the uncertainties in our knowledge of the near-Earth object target population result in significant cost and schedule risk. I would further say that cost figures of $1.25B lack credibility and that this proposal undermines any long-term human exploration objective by this country.

- Achieving the Congressional goal of detecting and characterizing 90% of NEOs with diameters greater than 140m is achieved in the shortest time by a dedicated space-base survey telescope (and is not likely to meet the 2020 goal). SBAG has endorsed such a system several times since 2010 and recommended a competitive process for its selection. Concern has been expressed by SBAG that NASA's ability to move forward has been sidetracked by reliance on a private initiative seeking private funding. It notes that this company has been unable to meet schedule milestones under a Space Act Agreement with NASA.

- SBAG supports the recommendations of the planetary decadal survey with specific regard to the recommended cadence of missions in the competed Discovery and New Frontiers programs. Other findings include concern over maintaining US planetary radar capability and the need to establish a Planetary Defense Coordination Office within NASA in support of recommendations made in the 2005 and 2008 NASA Authorization Acts.

Concerns Regarding NASA Planetary Science Funding Levels

- US planetary science capability is significantly sustained by its research and data analysis programs. Instability in the funding of these programs threatens US leadership in this area. The $35.4M increase in the President's FY15 proposed budget for Planetary Science Research reflects a reorganization of budget lines and not a net increase in funding to these programs. The House proposes to increase this by a few million. A cut of $40.3M proposed by the Senate Appropriations Committee would have very negative consequences, which could include ending curation of NASA mission data and lunar and other samples or a sharp reduction in all new planetary research awards in FY15.

- The Planetary Science Division should be applauded for planning to fund new awards in its ROSES14 research programs within 6 months of proposal submission. This represents a new commitment to the GPRA standard. The new large amalgam program, Solar System Workings, is the exception at 1 year. This seems to be to afford the opportunity to shift funds for new awards from FY15 to FY16, thereby freeing up these funds for other purposes. Delays strain researchers and reduces funds for new awards in FY16.

- The pursuit of two flagship missions (Mars 2020 and Europa Clipper) is not possible within a budget profile of ~$1.3B/year. Even less so if planetary decadal recommendations for the competed Discovery and New Frontiers programs were followed, along with the recommendation for research and analysis programs. Cost overruns will cause even more disruption.

- A baseline budget for NASA planetary science should be established that consists of stable and modestly growing research and data analysis programs, technology programs, Discovery and New Frontiers missions at the cadence recommended by the planetary decadal survey, and continuing missions in flight. With the exception of continuing missions, all other baseline programs would be competed, ensuring the best return on taxpayer investment. Funding for any flagship mission or missions or their development should be an appropriation over and above this baseline budget.

Commercial Asteroid Resource Extraction

- Sustainable, long-term human activity beyond low-Earth orbit is not possible without the identification and cost-effective exploitation of resources, primarily water, from near-

Earth objects. Commercial exploitation requires a reasonable period of time between investment and return on that investment. The development of an NEO ISRU infrastructure is beyond the scope of private enterprise. No such infrastructure can be developed until we embrace a long-term vision requiring these resources and engage in a program to identify their sources, process them, develop the means to use them, and demonstrate that it can be done more cheaply than bringing everything up from the surface of the Earth.

H.R. 5063 – American Space Technology for Exploring Resource Opportunities In Deep Space (ASTEROIDS) Act

- The definition of property rights regarding resources obtained in outer space is useful. However, the provisions "Freedom from Harmful Interference" and "Relief from Harmful Interference" appears to be an attempt to lay the basis for a private company to assert property rights beyond "resources obtained in outer space." The scope of this right is unknown and could actually slow and discourage commerialization. Protection against actual damage or the demonstration of imminent damage to private property in space (e.g., a spacecraft or a resource recovery facility) would be of value, but only with language narrowly tailored to achieve that objective. This bill does not accomplish that.

When Nature is Inconvenient to Strategic Planning in Science

- The discovery of seasonal running water on Mars and the potential oceans on Ceres and what the Dawn mission will see when it arrives raises useful questions about how rigid we are in our thinking about important issues that feed into our strategic planning.

Draft SBAG Recommendations

Asteroid Redirect Mission

At the time this statement is being composed (August 28, 2014) the SBAG findings from its 11th meeting on July 29-31, 2014, in Washington DC, are still in draft form. The posted language is close to final form. The findings on the Asteroid Redirect Mission (ARM) refer to a draft report of the SBAG Asteroid Redirect Special Action Team (SBAG ARM SAT). SBAG was asked by NASA to create this team to provide specific input "on the likely physical composition of small (<10 meter diameter) near-Earth asteroids, the likelihood and nature of boulders on asteroids, relevant information gained from meteorites, the properties of asteroid regolith, and the potential for science, planetary defense, and resource utilization." This report may be undergoing significant revision.

At SBAG 11, an entire day was devoted to the discussion of the ARM mission. The overall finding [draft] is

> "The portion of the ARM concept that involves a robotic mission to capture and redirect an asteroid sample to cis-lunar space is not designed as an asteroid science mission and its benefits for advancing the knowledge of asteroids or furthering planetary defense strategies are not compelling and will be limited."

For a mission that is not a science mission, there is much effort to suggest potential science and planetary defense benefits. The finding indicates the degree to which SBAG finds that compelling.

There was much discussion about how our lack of knowledge of the physical characteristics of potential ARM target bodies in both the "capture and asteroid" scenario (Option A) and "pluck a boulder" scenario (Option B) equated to significant risks to the mission as well as to its expense. It was only short time ago that small asteroids were monolithic, strength dominated bodies. Now we know they could be rubble piles, even held together by Van der Waals forces with great uncertainty about the consequences of mechanical interaction. There is also the basic question of target mass, which still seems difficult to resolve with existing ground-based and space-based assets to with a factor of 4 from the earlier Target NEO 2 Workshop. The draft finding on these uncertainties is:

> "Limits in the current knowledge and large uncertainties in the properties of near-Earth asteroids contribute significantly to schedule and cost risk and ultimately to the likelihood of success or possibility of failure of either Option A (redirect an entire small asteroid) or Option B (capture and return a large boulder from a larger asteroid) of the robotic ARM concept. Current surveys, observing programs, and other projects are not positioned to sufficiently bridge this knowledge gap within the allotted schedule."

It is proposed to sharpen this up to relate identify the risk as the risk of mission failure.

One question for which a finding was not made was whether ARM is a part of the critical path for sending humans to Mars. While much was said about ARM having some relevance to future human Mars missions, no statement was made that I recall asserting that ARM was essential or even the most cost-effective way of demonstrating technology necessary for a future human Mars mission.

There is ongoing discussion on the SBAG ARM SAT report. The focus is primarily on whether ARM has anything more than notional relevance to planetary defense and resource utilization. My version of the summary regarding ARM and ISRU (which is a substantial rewrite of the draft report) is:

"Summary Findings: The value of ARM to ISRU is very limited.

- Detailed knowledge of a range of asteroid compositions within a given target taxonomic class is critical to developing a practical method of extracting a particular resource. ARM returns a single target.

- Knowledge of surface and subsurface mechanical properties are critical to a resource extraction processes.
- Small (ARM-class) targets provide limited information about the surface and subsurface mechanical properties of the larger asteroids that would be industrial ISRU targets.
- Once a technique is developed for extracting an identified resource from a likely range of compositions of target objects overlapping the composition of the returned ARM target, the ARM target could be used as a site for testing the deployment of a bulk processing demonstration, recognizing that it would not be testing for the range of potential mechanical properties of multiple ISRU targets.
- ISRU will require autonomous robotic operations. It is unclear on what timescale such a system can be matured for demonstration once the ARM sample is returned.
- ISRU experiments on ISS are essential precursors to any bulk processing experiments on the returned ARM sample."

Finally, I would note that at the meeting there was some vigorous opposition to ARM, but no complementary support by the non-ARM attendees at the meeting. I raised the concern that the only reported commitment of the ARM mission is to succeed in unfolding its solar panels – which might more cost-effectively be achieved in Earth orbit. Failing to commit to actually returning an asteroid to retrograde lunar orbit avoids the need to discuss levels of acceptable risk to mission success and how uncertainty in the target population of asteroid could drive cost in accommodating that uncertainty. NASA contends that the ARM mission will cost less than $1.25B (for comparison the OSIRIS-Rex mission will cost ~$800M). I think it is fair to say that this figure lacks any credibility with the SBAG community.

The Need for a Near-Earth Object Survey and *The B612 Sentinel Project*

Achieving the Congressional goal of detecting and characterizing 90% of NEOs with diameters greater than 140m is achieved in the shortest time by a dedicated space-base survey telescope (and is not likely to meet the 2020 goal). SBAG has endorsed such a system several times since 2010 and recommended a competitive process for its selection.

Concern has been expressed by SBAG that NASA's ability to move forward has been sidetracked by reliance on a private initiative seeking private funding (the B612 Sentinel project). Under the B612 Space Act Agreement with NASA, the schedule and milestones include: (1) Sentinel mission contract start date with Ball, November 2012; Preliminary design review, October 2013; Critical design review, October 2014; Launch, December 2016. There has been no PDR, without which a CDR cannot be held. The SBAG finding noted that this company has been unable to meet schedule milestones under the Space Act Agreement.

Other Findings

SBAG supports the recommendations of the planetary decadal survey with specific regard to the recommended cadence of missions in the competed Discovery and New Frontiers programs. Other findings include concern over maintaining US planetary radar capability and the need to establish a Planetary Defense Coordination Office within NASA in support of recommendations made in the 2005 and 2008 NASA Authorization Acts.

Concerns Regarding NASA Planetary Science Funding Levels

The budget for the NASA Planetary Science Division has been in difficult straights since the Administration suddenly reduced it by 20% from $1.5B to $1.2B from FY12 to FY13, using the difference to fund other agency priorities. Congress improved this situation in FY13 by appropriating $1.415B to planetary, but the Administration reduced this to $1.27B in its Operating Plan. Congress succeeded in raising this to $1.345B in FY14, which the Administration proposes to reduce to $1.28B in FY15. All this is in the context of the sudden desire of the Administration to commit to a new flagship mission, Mars 2020, and the desire of some in Congress to see a new start on a flagship Europa mission. There is also broadly declared support for the Planetary Decadal Survey, which calls for the restoration of the decimated Discovery program and an additional New Frontiers mission this decade in addition to growing funding for Research and Analysis programs. There is even a planetary balloon program that expended ~$12M in FY13 and continues in FY14 at ~$6M that appears nowhere in the President's budget. I am greatly concerned about the sustainability of our planetary programs. Even at $1.5B/year, there are insufficient funds to do everything. There is great unease across the planetary community about the future of the United States solar system exploration program.

Of particular note is the overwhelming focus on what flagships will prevail in a time of limited resources. Many people will to take the position that we need to have flagships (primarily), New Frontiers and Discovery missions under development because the decadal survey calls for "balance." I take the position that the decadal survey does not contemplate a suicide pact. Balance requires resources and the decadal gives specific guidance about what to do when there are insufficient funds to do everything

> "It is also possible that the budget picture could be less favorable than the committee has assumed. If cuts to the program are necessary, the first approach should be descoping or delaying Flagship missions. Changes to the New Frontiers or Discovery programs should be considered only if adjustments to Flagship missions cannot solve the problem. And high priority should be placed on preserving funding for research and analysis programs and for technology development."

This is a remarkable, fiscally responsible position, but it is largely rejected by vested interests and others. If the decadal recommendations for flagship missions were actually followed, we would probably be contemplating the modest Uranus Orbiter Probe option if any.

Over the years, I have advocated for competition in missions as a means of controlling cost and maximizing return. When the Europa Orbiter missions and Pluto-Kuiper Belt Express missions were cancelled because of huge cost overruns, I advocated along with others for NASA to set what it considered to be a reasonable cost and compete them. It did just that for the Pluto mission, which resulted in New Horizons mission, which won the competition, and the New Frontiers program. The first competed mission program was Discovery, which in its first decade selected two missions for flight every two years (excepting the initial NEAR and Mars

Pathfinder missions). We would do well to restore the vigor of the Discovery program and to ensure the twice a decade cadence of New Frontiers.

When it comes to the planetary budget and its management, my first concern, however, is always the small research programs, which are also competed. In a survey I conducted for the NASA Planetary Science Subcommittee in 2010, nearly half of the planetary community relied on these programs for at least half of their salaries. This makes sense. Modern planetary science is a creation of NASA, it has only modest penetration into academia, unlike astronomy. United States solar system exploration has not limited itself in its reach, sending missions from Mercury to beyond the edge of the solar system. Planetary science is incredibly rich in range of subjects it addresses across worlds – atmospheric science, geophysics and geology, celestial mechanic, particles and fields, and much more. The community engaged in this work has organically grown over the decades. The research programs are the primary means by which we support training the next generation of scientists, and they support the research we use to define the problems best addressed by missions and to analyze that mission data and integrate it back into our broader knowledge base. While missions are the peak of our endeavors, they rest on a mountain of work supported by the research programs. This is why the research programs are given probably the highest priority in the decadal survey – strive to keep stable under adverse economic conditions, and otherwise slowly grow.

Over the remainder of this decade, most of the NASA missions operating today are expected to come to an end: all of our Mars assets, MESSENGER, Cassini, Dawn, LRO and others. Perhaps Voyager will still be sending back signals! This will increase pressure on our research programs and the question must be raised about whether we are going to maintain our national capabilities, manage a retrenchment, or allow a more chaotic resolution. I anticipate the last.

Another pressure on the community is declining research program selection rates, which have dropped nearly 50% over the past five years, with more dramatic declines in specific programs, while average award sizes have stayed fairly constant and total numbers of proposals have actually declined. I think part of the problem has been the use of "rephasing" to free up money in the research programs in one year by shifting obligations of an award to the next year. Without money being added to the program to cover the obligations, funds available for new awards are reduced.

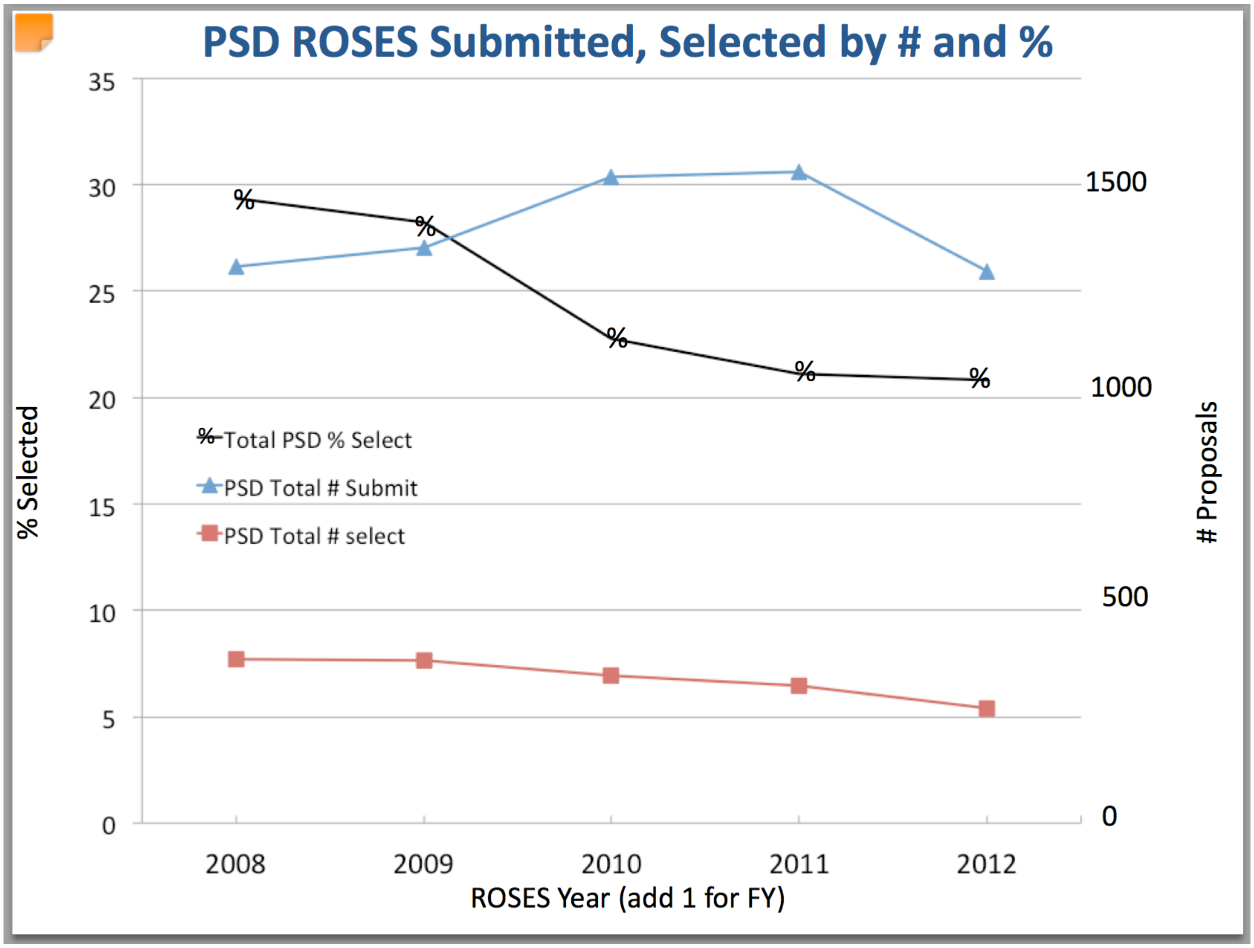


Figure from Jonathan Rall (NASA HQ) presentation to SBAG 11 (7/29/14). MVS calculation of success rates for planetary ROSES13 proposals, using data from sara.nasa.gov, is a new low of 19.5%.

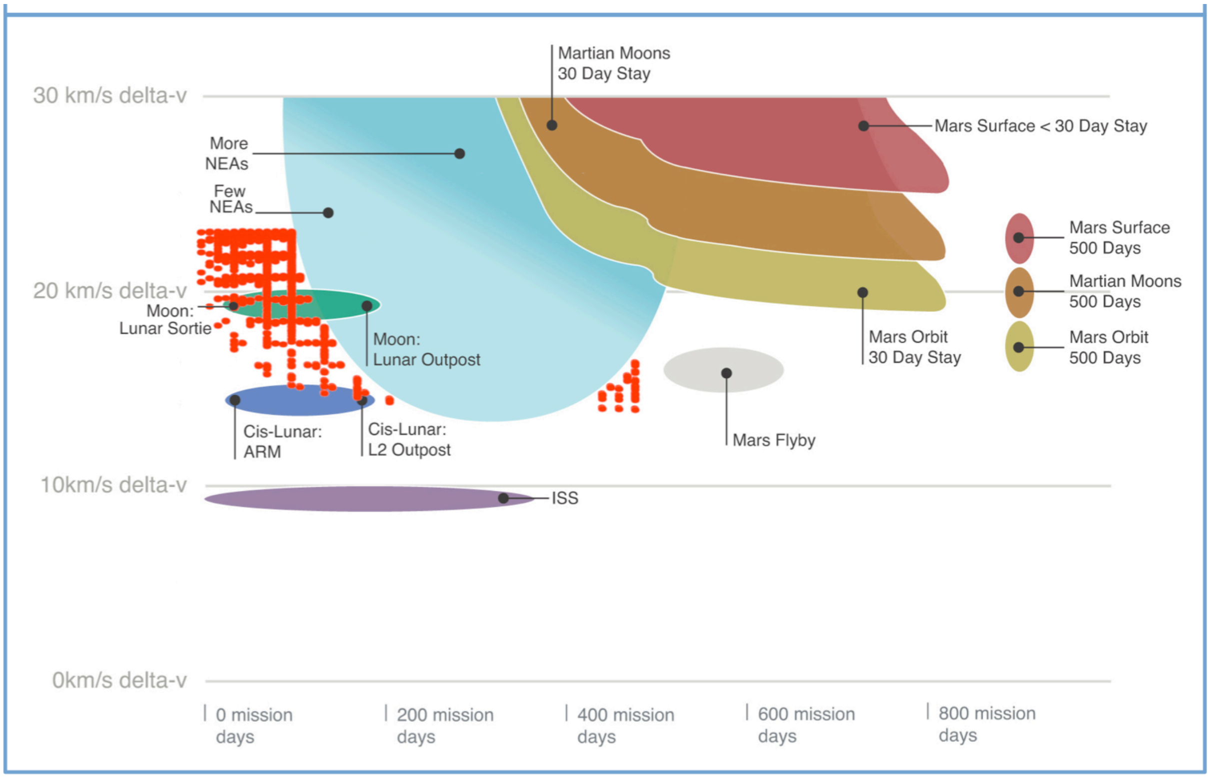
The current ROSES Year (2014) has marked the reorganization of established basic planetary research programs to in part align them more with the themes of the planetary strategic goals. The utility of the reorganization was not clear and estimated funding levels were not provided (though the funding levels of their merged programs were known), raising concerns at public venues (e.g., LPSC, AG meetings).

One point for which the Planetary Science Division should receive some credit is for planning to fund new awards in its ROSES14 research programs within 6 months of proposal submission. This represents a new commitment to the GPRA standard (Government Performance and Results Act), which calls for 150 days between proposal submissions and notifications of award. NASA procurement usually takes an additional month before the award is made and scientists can commence their work. The new large amalgam program, Solar System Workings, stands out, planning 1 year from proposal submission to funding. This seems to be to afford the opportunity to shift funds for new awards from FY15 to FY16, thereby freeing up these funds for other purposes. A statement by PSD Division Director Jim Green made at the December 3, 2013, "Virtual Town Hall Discussion of Restructuring of the Planetary Science Division R&A," indicated that the positioning allowed for the option of shifting all new awards from FY15 to FY16. Complaints about the artificially long gap (20 month) between the original program proposal due dates and the SSW due date resulted in a shift forward of 7 months of the SSW due date (from late February 2015 to late July 2014). However, by holding off funding for one year, PSD management is maintaining the option to rephase all new SSW awards to FY16. SSW should be held to the same 6 month standard as other programs.

Commercial Asteroid Resource Extraction

The future of human activity in space depends upon our ability to cost-effectively recover and utilize resources from near-Earth asteroids, primarily water, which would be used for propellant, life-support and radiation shielding. A human mission to the surface of Mars is estimated to be ~$1T, including development costs. This contemplates raising all the mass to be used (spacecraft components and fuel) from the surface of the Earth at great expense. This is not a number Congress is likely to fund over the next 20-30 years. To make human destinations beyond low-Earth orbit practical, we need to determine if we can offset costs by establishing an infrastructure that would provide necessary components such as fuel from near-Earth asteroids more cheaply than hauling it up from Earth.

NEOs offer easily accessible, low-energy targets with short mission times. This is illustrated in a figure generated by R. Binzel (below), which is a modification of an original figure from the recent NRC report *Pathways to Exploration— Rationales and Approaches for a U.S. Program of Human Space Exploration* (NRC 2014, prepublication draft). The red points were calculated by Binzel and shows the more accurate envelope of near-Earth object mission scenarios derived from known objects. NEOs can be more accessible than the surface of the Moon and can even be comparable to reaching lunar orbit. While these kinds of calculations have long fueled speculation that NEO in-situ resource utilization is practical, there are many other fundamental questions that need to be addressed in order for that to be demonstrated. However, wishing does not make it so, nor does invoking the magic of the private sector. There is much basic research that needs to be done.



It is recognized within the planetary science community that asteroids are intrinsically diverse. The population of near-Earth objects is also transient. It is fed by source populations throughout the asteroid belt and even by comet populations interacting with gravitational resonances and orbital perturbations by planets. The same gravitational effects result in their being ejected from the near-Earth population on timescales of ~7 million years. We have some idea of their composition from remote spectroscopic observations and by picking up meteorites on the surface of the Earth and analyzing them. However, while spectra provide important clues to composition, they do not necessarily provide detailed information on bulk minerals comprising an asteroid. Spectra measure the outermost microns of an asteroid surface and this may be subject to modification by space weathering, some minerals are not spectroscopically active. Likewise, meteorites represent only a small fraction of the mass of the asteroid entering the Earth's atmosphere and do not necessarily present a complete picture of its composition. These are good starting points, however. They tell us, for instance, that if we are interested in water, we want to look for dark carbonaceous asteroids, preferably those with spectral features indicating the presence of hydrated minerals. There are two missions that will be returning samples from such near-Earth objects: NASA's OSIRIS-Rex and JAXA's Hayabusa 2. These will provide some insight into the composition of the material that normally does not survive entry into the Earth's atmosphere. They will also provide some insight into linking remote spectra to meteorite samples.

Commercial asteroid resource extraction requires an understanding of the composition and mechanical properties of the material to be processed, and an understanding of how to do this under low-gravity conditions. The most dynamically accessible resource objects are likely to be small given that the population increases with number with decreasing size. Industrial level activity will probably require many resource targets and even within a taxonomic class (e.g., C-Type) there will be compositional variation. In fact, it is unknown the extent to which any asteroid is compositionally homogeneous (and there is evidence that some are quite inhomogeneous, such as the precursor of the Almahata Sitta meteorite that fell in Sudan in 2008). Extraction processes will have to be developed that accommodate a range of compositions within a target class. In the case of water, such processes would need to be tested and refined using a range of carbonaceous meteorites and simulants on the International Space Station. At some point there would have to be the demonstration of an autonomous resource recovery facility on a near-Earth asteroid. There is then the need to assess the resource that has been extracted, determine the need for subsequent processing into usable material (e.g., water may need to be purified and then converted to hydrogen and oxygen, liquefied, and stored).

All this basic science and engineering is something beyond the scope of reasonable investment by a commercial entity, because there would be no expectation of return in investment on a reasonable timescales. I expect it would take a couple of decades to get to the point when one could answer the question of whether, with some level infrastructure in place, the marginal cost of processing and returning water from an asteroid would be cheaper than bringing it up from the surface of the Earth. Given the potential long-term benefit of a positive outcome in opening up the solar system to expanded human activity, this is a logical area of governmental investment. Once the basic science is known and basic technologies supporting this effort are developed, this would be the logical time for the private sector to come in and see if it could do things more cost-effectively. They would also be in a much better position to create new industries, building on this infrastructure.

Ultimately, there can be no commercial enterprise without a market. For water, there is no market that exists today. In the near future, the primary customer will be the US government, and there is no and never has been a commitment to a long-term, open-ended vision of expanding human activity in space that would mandate the development of an asteroid ISRU infrastructure.

American Space Technology for Exploring Resource Opportunities In Deep Space (ASTEROIDS) Act (HR 5063)

HR 5063 desires "to promote the development of a commercial asteroid resources industry for outer space in the United States and to increase the exploration and utilization of asteroid resources in outer space." There are components of this bill that would support this, but on the whole I believe this bill could have the opposite effect.

On the positive side, this bill recognizes a personal property right of individuals and corporations in "any resources obtained in outer space from an asteroid." There is no incentive to invest in resource recovery if you do not own what you recover.

There is also a practical need to ensure that if you are engaged in resource recovery on an asteroid, that other private parties are discouraged from damaging your equipment, or interfering with your operations or the return of your recovered material. However, this bill focuses on "harmful interference" with a "right" to conduct operations and activities, not the activities themselves. The scope of this right is not defined and could lead to efforts to claim, and hold hostage, large numbers of potential resource targets long before the capability to exploit those targets is developed, not necessarily by the same parties. Under the current language, I could today take published observations of near-Earth objects by the NASA Wide-field Infrared Survey Explorer telescope, identify those with low albedo (enhancing their probability of being water sources), and lay claim to the 100 objects having the most favorable orbits for low-energy missions with good dynamical opportunities for returns of material to Earth orbit. Resource recovery may be decades in the future, but under the terms of this bill I can make an "assertion of superior right" by being "first in time, derived upon a reasonable basis" to have made that assertion and assuming it is "in accordance with all existing international obligations of the United States." I can effectively increase the future costs of those who might be compelled to pay me for access to "my" asteroids or go to a less dynamically favorable resource target.

As we expand commercial activities in space, we want to create a legal environment that establishes protections against interference with actual economic activity and the jurisdiction to which causes of action can be brought. This would include injunctive relief as well. For example, Company A establishes an automated resource recovery facility on the C-Type near-Earth asteroid 367248 (2007 MK13). Company B announces that it in the coming year it will be launching its own resource recovery facility to the same object and will operate on the opposite side of the asteroid. Company A seeks an injunction against Company A going to 367248, arguing that the debris necessarily ejected from the facility has a high probability of impacting their facility and causing irreparable damage. Company B, in turn, may provide detailed numerical simulations showing that by placing their facility on the opposite site of the asteroid, that the trajectory of all debris they would generate clearly avoids Company A's facility. Independent dynamical experts could be brought in to look at the details of Company A and Company B's simulations and offer their opinions. I would note that it would note that it would behoove Company A to bring its action as soon as possible even though it might be years from launch to the start of operation of Company B's facility. Company A has an obligation to mitigate Company B's losses. Company B may be able to launch its facility to a different target, but once launched towards 367248 it would be faced with total loss.

There is much that needs to be learned about the actual composition and the variation of that composition as well as mechanical properties across the near-Earth asteroid population – even within taxonomic types (e.g., "carbonaceous" C-Types) that are often referred to as primary targets for resource recovery activities. This information is critical to design the processes and methods that would be used for the industrial production of asteroid resources. It is not premature to establish protections against physical damage of private property in space, but we should be very cautious about establishing rights that are undefined and of uncertain scope that could have the unintended consequence of discouraging the development of a new important economic activity.

When Nature is Inconvenient to Strategic Planning in Science

I would like to say a few closing words about the inconvenience of the Universe when it comes to long held ideas in science and our efforts to lay out long term strategic plans in activities such as the Planetary Decadal Survey.

Three months after the release of the decadal survey, the Mars Reconnaissance Orbiter HiRISE team announced the discovery of seasonally flowing water on the walls of some Martian craters. Questions of aquifers arose, later whether it might be do to the accumulation and melting of ice accumulated from the atmosphere, but the first thought to come to most people's minds was whether there could be conditions on Mars today that would support life. Not whether conditions existed billions of years ago, but today. Scientists have dedicated their careers to asking questions about ancient conditions on the planet. This is the lynchpin of the rationale that is used to support the multi-flagship Mars sample return effort, which is their top priority. If put to an open discussion, I think more people and scientists would be excited by the question of life on Mars today, particularly if they thought there was some possibility that the answer might be positive.

In the mid-1990s, the discovery of subsurface oceans on Jupiter's moon Europa by the Galileo spacecraft triggered similar great excitement for similar reasons – could there be life beneath its surface? Does water equal life? Today another spacecraft, Dawn, is heading towards the dwarf planet Ceres, which orbits the Sun between Mars and Jupiter in the asteroid belt. It will be arriving in March 2015. Internal modeling of Ceres, with which I am involved along with other groups, indicates that it could have liquid water oceans in its interior today. The Herschel Space Telescope recently reported the episodic emergence of water vapor from its surface. As Dawn approaches this new world, our team will be looking for evidence of the emergence of mineralized water, geyser activity, and other possible explanations for the Herschel observations. If the Dawn mission discovers evidence for this subsurface reservoir of water, the question is again raised of whether there is life beneath the surface of Ceres. If water erupts and quickly evaporates from its surface, could we send another spacecraft to scoop up some of the evaporitic material and see if there are dead bugs? Now we have a target closer to Earth that is easier to reach and in a less hostile radiation environment. Some of the longer-term missions contemplated for Europa could be more cheaply and quickly executed at Ceres. But, many scientists have dedicated much of their careers and Congress has appropriated much funding for the study of future missions to Europa. Accessible oceans at Ceres is a very big IF, but it does bear contemplating how much flexibility we have in our planning to accommodate important if inconvenient discoveries.

Once a decadal survey is released, our knowledge of the solar system does not suddenly freeze. A mid-term review is good, but what if a major discovery is made right after that? We need to figure out ways of being flexible – both in our thinking as well as planning – when we are confronted with new information and insights.