Testimony of

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Mr. Chairman, Ranking Minority member, and members of the Committee, thank you for the opportunity to testify today. My name is Thomas Zurbuchen and I am a professor of Space Science and Aerospace Engineering at the University of Michigan where I am also the Associate Dean for Entrepreneurial Programs. In this function, I am responsible for bringing innovation to large-scale programs within the College of Engineering and also across the university campus. Being the first university graduate in my family, I moved from Switzerland to the U.S. for one reason: to do meaningful work in space within a nation and a university that supports it. Has this ever been a great choice: one of my instruments is currently in orbit about the planet Mercury; two others make measurements near Earth and aid in predictions of violent space weather events.

At the same time, I have also been witness to the progressive decline of the agency that was my childhood dream, and the passion of my academic pursuits. While it is easy to romanticize NASA's past, such as the heroic missions of Apollo, we must remember that our predecessors faced the same challenges we currently face: defining the purpose and meaning behind the exploration of space within a society that does not always see a tangible benefit; being responsible for budgeting resources to protect and serve a nation, while also being responsible to the great quest for knowledge and visionary dreams that unite all of humankind.

We are in a period of limited resources, and so progress will be inevitably limited. This is an ideal time to position ourselves for better times. The way to do this is to ensure that a talented work force will be available, and that innovations and technology breakthroughs are pursued, so that when we come out of this period of limited resources we are positioned to advance rapidly. The talented workforce and the innovations will be developed by universities and by industry, particularly small businesses, not by NASA centers (any more than most large industries are capable of innovating). Hence, we need to pursue a strategy in which universities and industry are fully engaged in ensuring a promising future for the space program.

Let me address the first question about the priorities lawmakers should consider when evaluating future NASA plans, especially plans for NASA's science program.

Science investments should be focused on the following three primary criteria.

1) Science and engineering excellence and leadership: Science needs to be prioritized in time and also to give emphasis to certain areas of NASA research at certain times.
2) Societal benefits: these benefits are in the long term and short term. Some of them relate to NASA missions and needs, but many programs have impact well beyond NASA and open up economic opportunities, contribute to the health and well being of our citizens and in some cases even save lives.
3) Build a talent pool of innovators: NASA's space science missions need access to the best talent; and breakthrough innovations and new technologies are developed by people. Some of this talent is at NASA centers, but most of it is in academia and in industry, especially in small companies. I will get back to this point later.

I believe the first objective - science and engineering excellence and leadership - is well addressed by the advice NASA and Congress receives from the National Academies. I was vicechair of the most recent decadal review focused on solar and space physics and I believe that process really works to identify and promote the best science and also engineering objectives. With regard to the second objective, NASA's space science programs have historically brought tremendous societal benefit. Earth science missions provide vital information in areas ranging from weather and climate to land resources to natural disasters, and spacecraft in solar and space physics are used to make predictions of space weather that can disrupt technologies on Earth. Research that can create such societal benefits strengthens the rationale of those missions and programs.

I am convinced, though, that the third objective – building a talent pool of innovators – in many ways will have the most effect on our leadership position 10 or 20 years from now. We need a workforce whose aspirations in space are not diminished by the doom and gloom sometimes espoused by our leaders as they look back to "the good old days." We need a talent pool of impactful engineers and scientists who are ready to develop the space systems of the future. Top talent does not just hang around and wait for better times – innovators want to start moving the ball now!

I am convinced that a science program at NASA with these kinds of priorities will look different than what we are building today. It would be one that focuses on smaller spacecraft, and rapid turnaround missions. It's not "faster, better, cheaper" – the mantra of a NASA administrator of years past – but it is "faster, cheaper, disruptive". Disruption is good! Disruptive programs overturn old paradigms, create new markets, and engender new value systems. Missions to be developed should be of diverse sizes and implemented at a cadence appropriate to their cost and complexity. Implementing missions in this balanced approach best engages our community and optimizes the science return for a given level of investment. Some of these investigations may be in the suborbital realm, some of them through what we call "CubeSats", some of them through small spacecraft that can be built with an increased risk profile, but all the missions should develop and provide to the space community and industry tools that are game-changing.

How can NASA and its stakeholder community reach consensus on identifying and preserving capabilities necessary for future use?

One of the most challenging aspects of NASA's research program is the mismatch between its strategic objectives and its program. I do not see a way around addressing the issue of consensus without addressing the key question behind each and every organization: What is the most important thing that NASA does? That strategy can be far ranging, but it must have measurable and exciting milestones along the way. Without such a consensus goal, a point in the sky we are marching toward, it is going to be very tough to create consensus – very tough, and perhaps impossible.

I would, however, make a point about the difference between technology and know-how. We can document technology with drawings and with specifications. We can even record in pictures how a particular piece of hardware is assembled. But it turns out that it is almost impossible to reliably build any technology based on such a record. We need know-how that is only achieved through experience, from actually working on the space instrument or the data inversion problem. That know-how is an important part of keeping capability alive, often more important than any engineering drawing or process report.

<u>What priorities should policy makers consider when evaluating appropriate allocation of resources to maintain balance?</u>

The priorities should be focused on outcomes that are both strategic and worthy of NASA and its proud history. When we come out of this economic downturn, we want to be the most innovative and most impactful economy of any competitor anywhere. I believe that this goal will be achieved through an investment strategy that has both near-term and long-term elements.

In the near term, the first priority is getting the maximum scientific value out of our existing assets. The first aspect of this will be to grow the science output of our investments, building upon our current leadership position and increasing the lead between our competitors and us.

The second priority is growing innovative activities and an entrepreneurial mindset in our science and engineering communities. This priority can be pursued by implementing a diversification of NASA's portfolio to support investments that are focused on disruptive technologies – new approaches to get to space and to achieve success there. These investments should encourage calculated risk-taking and the development of a pool of innovation that enables NASA's long-term strategy, and also attracts and trains the talent we will need to implement that strategy. Investments should go to the entire NASA community, within Centers, but particularly within Academia and Industry. History shows that it requires all three to create the leadership and to get the most economic return from government investments.

The third priority would be to invest in modest-size, and principal investigator led programs—known variously as Discovery-, Explorer-, or Venture-class, depending on the respective community. These missions have provided the best value for the money invested. That's the type of program where the research was conducted that resulted in NASA's first Nobel prize, and that's the type of program that built the spacecraft currently exploring Mercury!

The fourth priority is about big bets. NASA science needs these big bet programs to stretch our imagination. Landing a small car softly on Mars, crossing into our galactic neighborhood, observing the universe all the way back in time to its infancy – these crowning achievements do not come from small assets, but they stimulate our thinking and show leadership worldwide. However, these big-bet projects cannot squeeze out the other three priorities.

If you read the priorities of the recently released decadal strategy of the solar and space physics community, you will notice a deep alignment and consistency with this prioritization.

Thus, the most important dimension of balance is in the scale of programs! The future of space—future talent, future technologies—will be made in low-cost small satellites, such as "CubeSats", and modest size missions, such as Venture class, and Explorer missions. If we fail to recognize balance in scale, we will cede the future to others. I can report to you that a new generation, here in the U.S., is poised to develop the disruptive technologies of the future and lead the U.S. to being the innovative, most impactful competitor in space, in the world. Let's turn them loose. Let's turn them loose to innovate, to take risks, to become leaders. What will follow will be scientific and engineering excellence, what will follow will be new societal benefits, and what will follow, in its own course, will be a broad national consensus of support for our civilian space program.

I cannot forget that it was the work NASA and this committee did thirty years ago that captured my imagination and changed my life forever. I keep an old science book in my office to show to my students. It talks about a new project—Voyager—that NASA was planning to explore the outer planets. This mission—one of the proudest in NASA's history—was in the news again last week. I think it is the dreams and imaginations of our youth that carry us through the difficult choices we inevitably make as adults, and I believe it is the work this committee will do in the next few months that will define the next chapter of space exploration and determine whether the US will continue to be the place that inspires the next great generation of scientists and explorers. My hope is that by focusing on people and disruption, utilizing the best talent here and abroad, and by leveraging the unique strengths of our government, industry, and academic institutions, we can and will.

I look forward to your questions.

Appendices

1) Example of CubeSat mission: RAX
2) Example of CYGNSS, a disruptive mission architecture
3) Information about decline of graduate students to get hands-on experiences in space research

1) Example of CubeSat Mission: RAX

The Radio Aurora Explorer (RAX) is the first NSF-funded CubeSat mission with PI James Cutler (University of Michigan). Two CubeSats were launched in 2010 and 2011 onboard U.S. Department of Space Test Program (STP) missions from the Kodiak Launch Complex in Alaska. RAX is capable of carrying out a mission that was previously only considered to be done with larger satellites: RAX's primary mission objective is to study large plasma formations in the ionosphere, the highest region of our atmosphere. These plasma instabilities can create magnetic field-aligned irregularities (FAI), which are dense plasma clouds known to disrupt communications between Earth and orbiting spacecraft. To study FAI, the RAX satellites utilize a large incoherent scatter radar station located in Poker Flat, Alaska (known as PFISR). PFISR transmits powerful radio signals into the plasma instabilities, which then scatter in the FAI and are received by the orbiting RAX spacecraft. The signals are then processed by RAX's onboard computer and transmitted back to Earth for scientific analysis. [1]

[1] From James Cutler and his entry in Wikipedia, as well as http://rax.engin.umich.edu/

In orbit now for over a year, RAX continues to make novel measurements of the Earth space environment and first results have been published in the scientific literature. During its design and development, RAX has employed over 50 graduate and undergraduate students and provided experiences that already have enabled careers and changed lives.

2) Example of CYGNSS, a disruptive mission architecture

A major uncertainty with hurricane predictions at present is forecasting their intensity. The uncertainty results from a lack of measurements (in particular, surface wind speed) in the inner core of the storm, which is where the dynamical processes occur that determine storm genesis and intensification. The necessary inner core measurements are made today by the NOAA "hurricane hunter" aircraft near the U.S. coast. They are not possible from satellites for two reasons: 1) they require penetration through the intense precipitation that is present in the eye-wall and rain bands; and 2) they require very frequent measurements to capture the rapid intensification phase of the storm (timescale of hours). Current satellite measurements can't see through the heavy rain and they have orbital revisit times of days.

Only hurricane hunter aircraft are a viable solution today – a single spacecraft approach with high accuracy using radars and/or lidars is much more expensive. Hurricane hunter aircraft are one reason why the Hurricane Sandy forecast at landfall near New York City was so accurate. The aircraft were repeatedly overflying the storm as it made its way up the eastern shore of the U.S. Further from land, and in the Pacific Ocean, only conventional satellite observations are available and the quality of the intensity forecasts is much worse.

The CYGNSS EV-2 [2]mission uses a constellation of GPS surface reflection receivers to measure the ocean surface wind speed in all weather conditions, including extreme precipitation levels. This type of remote sensing is fairly new but has been well established on aircraft in hurricanes. The science payload is approximately 2 orders of magnitude lower in price, mass and power than conventional satellite wind sensing instruments, which enables an entire constellation of them to be flown for a low cost. Use of a constellation reduces the revisit time from days to hours, which is what is needed to observe inner core process dynamics. The two requirements for inner core studies (penetration through heavy precipitation and frequent revisit times) are uniquely enabled by this new approach.

CYGNSS should result in a fundamental improvement in our understanding of the inner core process of hurricanes and, as a result, significantly improve our intensity forecast skill. And it will do so not just in the tropical Atlantic near the U.S. coast, but globally. It will also mark the first time that a small satellite constellation architecture is used for space-borne Earth science. This architecture has major cost savings implications for future Earth science missions.

3) <u>Information[3] about decline of graduate students to get hands-on experiences in space research</u>

"Our policymakers need to acknowledge that the nation's apathy toward developing a scientifically and technologically trained workforce is the equivalent of intellectual and industrial disarmament and is a direct threat to our nation's capability to continue as a world

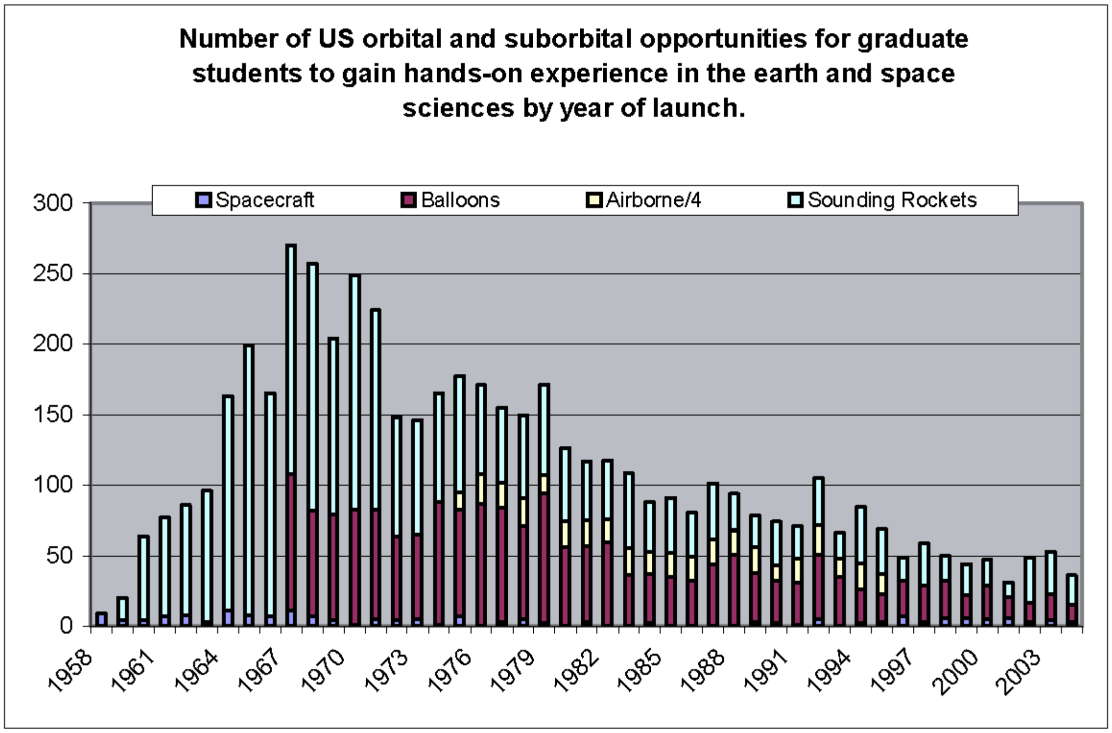
[2] http://aoss-research.engin.umich.edu/missions/cygnss/
[3] This is courtesy of the University Space Research Association

leader.” (The Report of the Commission on the Future of the U.S. Aerospace Industry, November 2002)

"At present, there are insufficient methods for students to acquire hands-on experience in the scientific and technical disciplines necessary for space commerce and exploration." (Commission on Implementation of United States Space Exploration Policy (the Aldridge Report), June 2004)

There is a significant deficit of scientists and engineers in the United States with meaningful hands-on experience with space instrumentation and space systems, which is jeopardizing the ability of the nation to maintain a vigorous presence in space into the future, regardless of whether we are in space for reasons of commerce, exploration, national defense, or scientific research. This deficit leads not only to a loss of capability, but also to escalating costs of many of the space systems vital to the nation’s security and industrial competitiveness.

The scientists and engineers who learned their trades during the first decades of the space age have reached or are nearing retirement. These were exciting years for a young person to enter space research, and space attracted many of the best young scientists and engineers. These years were marked by frequent launches of smaller missions many of which were led by university-based teams that included graduate students. These students got plenty of hands-on experience, and learned first hand the difficulties of designing and constructing an experiment or engineering system that would operate reliably in space. Many students also learned from designing and building experiments for smaller, suborbital flights on rockets or balloons, or by observing with an airborne telescope.



The chart shows that the number of these opportunities peaked in 1968, at the height of the Apollo program. Since then the number of student opportunities provided by spacecraft missions, rocket and balloon fights and airborne observatory sorties has diminished from over 250 per year to consistently less than 50 per year. Most graduate students now never have an opportunity to do hands-on science. Instead, the vast majority of science PhD students analyze data obtained from instruments they have never seen and thus have only a vague idea of how they work or how they might malfunction. They certainly don't learn the important skills needed to conceive of, and to help design and construct a space experiment.

The chart hides another phenomenon. As space missions have, necessarily, become more complex, they also take longer to design and construct. The increasing complexity means that fewer universities have the resources and capabilities to manage the complexity, so increasingly missions are being run by non-academic laboratories and research centers. The mission timescale is now significantly longer than a typical graduate student remains in school. Both of these effects significantly decrease the likelihood of graduate student involvement, exacerbating the problem.