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SPACE EXPLORATION

HEARING

BEFORE THE

SUBCOMMITTEE ON SCIENCE, TECHNOLOGY AND SPACE

OF THE

COMMITTEE ON COMMERCE, SCIENCE, AND TRANSPORTATION UNITED STATES SENATE

ONE HUNDRED EIGHTH CONGRESS

FIRST SESSION

JULY 30, 2003

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ONE HUNDRED EIGHTH CONGRESS

FIRST SESSION

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SPACE EXPLORATION

WEDNESDAY, JULY 30, 2003

U.S. SENATE,
SUBCOMMITTEE ON SCIENCE, TECHNOLOGY, AND SPACE,
COMMITTEE ON COMMERCE, SCIENCE, AND TRANSPORTATION,
Washington, DC.

The Subcommittee met, pursuant to notice, at 2:35 p.m. in room SR–253, Russell Senate Office Building, Hon. Sam Brownback, Chairman of the Subcommittee, presiding.

OPENING STATEMENT OF HON. SAM BROWNBACK, U.S. SENATOR FROM KANSAS

Senator BROWNBACK. Well, thank you all for joining us here today. I'm sorry for being late. I was over on the floor and had an amendment on the floor, and so I was unfortunately detained. I do appreciate you being here today, and I look forward to your presentation.

Since becoming Chairman of this Subcommittee, I've been fortunate to meet with people from all across the board in the realm of space science, and I've always been drawn toward the big ideas and the vision of visionaries. And I thought that working with the space exploration field would give me the opportunity to work closely with those big dreamers. In many cases, I have been able to; although, in others, unfortunately, I've been experiencing the opposite.

I hope that today's hearing will bring a little more creativity to the issue of space exploration as we look forward. With activities and responsibilities in space being spread across several governmental agencies, I think it's important that we take a comprehensive look at the totality of U.S. involvement in space exploration.

Throughout meetings with NASA and other space industry representatives, I've become aware of the burden of bureaucracy that, in some cases, plagues American science and space programs. I hope that our witnesses here today will be able to break out of this bureaucracy and will share with us a unifying vision for America in space exploration, utilization, and development.

The U.S. must dominate the Earth-moon orbit for exploration, discovery, scientific research, commercialization, environmental reasons, and national security purposes. This Subcommittee is in the unique position to help shape the American space program and increase the number and purposes of U.S. entrants into space science. But to do that, we need to have an accurate assessment of where we are currently in space exploration and determine where we need to go in the future. NASA is a key agency. But so, too, will be other agencies, such as the Department of Defense, the National Oceanic and Atmospheric Administration, and the Federal Aviation Administration. As we move forward in this Committee with the reauthorization of NASA, I want to involve all governmental agencies involved in space exploration.

Today, we're joined by representatives from DOD, NOAA, and NASA. Each of these agencies are involved in space exploration, and each of these agencies plays a unique role. Federal Aviation Administration's Associate Administrator for Commercial Space Transportation was invited today, but, unfortunately, is unable to join us. That office will be submitting testimony to the record, which I look forward to seeing.

I'm excited to hear from each of you, from a group such as the Marine Corps and what it's currently doing in space exploration. I understand they have some creative potential uses for space travel. Additionally, we are joined by NOAA, who will speak with us about their involvement with weather satellites and other ventures in space exploration. With us, also, is a representative from NASA, who will speak with us about the goal of exploring the solar system and the universe beyond. And we have been joined, as well, by two private sector individuals, Drs. Belton and Lanzerotti, who will share with us some of their findings and research in the space exploration field.

What I hope to get out of this hearing, gentlemen, is really that broad cross-section and the vision of visionaries to steer where the United States and all of its various agencies should be going in the area of space exploration. I don't think it's any secret that right now we're really in the middle of reassessing our involvement and our activity in space exploration—where we should be going, where we should be investing the dollars, in looking forward. When the Gehman Report comes out early fall, I expect that report to really ignite a strong move on the part of Congress to really assess what we should be doing.

The purpose of these hearings, ahead of that time, is to set some of the groundwork and understanding about: Where are we, as a country, and with respect to the various agencies? We've had a hearing, last week, on private-sector involvement in space exploration and where we should be going. And these hearings are all in anticipation of the Gehman Report and the broad and wide-open discussion that I think Congress really needs to have on the future of space exploration by this country.

So that's the nature of the hearing we'll have. I'll look forward to the testimony. Before we get to that, Senator and Astronaut Bill Nelson would be invited to make an opening statement if you'd like.

Bill?

STATEMENT OF HON. BILL NELSON, U.S. SENATOR FROM FLORIDA

Senator NELSON. Thank you, Mr. Chairman.

It's interesting that you made reference to the Gehman Report, because that, of course, is very important. I've been spending some time going through the testimonies on the Gehman Report. I've spent some time with Admiral Gehman and his staff. He has an excellent set of Commission members. They're very dedicated to this task. I think that they are going to come out and point out the mistakes that were made and some of the processes that should be changed. I think, also, they are going to comment on the culture in NASA, the lack of communication, particularly from the bottom up, in NASA; and, as a result of that, hopefully we will be a lot better off.

But it brings us to the subject of this hearing, that NASA can be the most efficiently run, most cost-effective, highly energized and communicating agency, which it has to be, on the cutting edge of what it's doing all the time, all of the exciting work, but unless it has a goal, unless it has a vision, unless it has the next major project, then there's too much of an opportunity for it to drift.

Now, you and I know the importance of the Space Station. We know the importance of getting to and from the Space Station and all of the research that's going on, on the Space Station, but it's time for us to look beyond. It's time for us to dream again. It's time for us to have that can-do spirit reignited that caused this country, in the 1960s, when a President said, "We're going to the moon and safely return in this decade," and all of the excitement in science and mathematics and technology that that caused to explode in our schools, in our colleges, in our universities. And America's space program is uniquely situated to cause that kind of explosion of technology. But you've got to have a dream, and you've got to have a goal.

And so, as you search for it here, Mr. Chairman, thank you very much for calling this hearing.

Senator BROWNBACK. Thank you, Senator Nelson. I look forward to working with you on this.

I think we'll run the clock at 7 minutes, gentlemen. If you don't mind, we will take your full statement into the record as if presented, and you're welcome to read from it or to summarize any way that you would like to.

We'll start with Brigadier General Richard C. Zilmer. He's the U.S. Marine Corps Director of Strategy and Plans Divisions, Plans, Policy, Operation, Headquarters for the Marine Corps. And then we'll go on down the group. And I think what I'll do is, I'll—just before each of you present, I'll introduce you so that people can have an overview of what you do prior to your presentation.

So, General Zilmer, why don't you go ahead, and then we'll proceed on down the panel. You need to get those microphones up pretty close to you, if you would.

STATEMENT OF BRIGADIER GENERAL RICHARD C. ZILMER, DIRECTOR, STRATEGY AND PLANS DIVISION, PLANS, POLICIES, AND OPERATIONS DEPARTMENT, HEADQUARTERS, MARINE CORPS

General ZILMER. Mr. Chairman, Members of the Subcommittee, it's an honor to be with you today. I have submitted a written statement, but I'd like to summarize from that statement, if I may.

It's an honor to represent to you today the Marine Corps' perspective on how NASA's vitality in space exploration capabilities will be critical to the Marine Corps' warfighting strategies of the future. Your sustained interest in and commitment to fully exploiting the opportunities offered by space for commerce, science, and transportation will contribute directly to the preservation of our Marine Corps expeditionary character and our nation's security.

Today, we have significant equities in space for military advantage, global expeditionary reach, strategic surprise, and some that are related directly to space exploration technology. In this regard, an energized NASA will become an important enabler for the Marine Corps.

In 1964, our 23rd Commandant of the Marine Corps, General Wallace Greene, foresaw the use of suborbital space to transport marines. In 2002, Lieutenant General Emil "Buck" Bedard inherited that vision and signed the Small Unit Space Transport and Insertion Universal Needs Statement. Hopefully, within the timeframe of 2025 to 2030, this capability will provide the Marines and joint forces heretofore unimaginable assault support, speed, range, altitude, and strategic surprise. The concept includes a range of strategic capabilities from autonomous weapons payloads to the actual insertion of marines on the ground. The objective, manned capability, reflects our belief that machines and munitions alone cannot replace the value of marines on the ground for many missions.

We do not look to space for its own sake, but, rather, because of the constraints of thick air travel and nonpermissive airspace, these constraints lead to a space-related solution that exploits NASA's space exploration technology and capability road maps. We intend to approach the U.S. Special Operations Command and the Air Force Space Command to refine a joint requirement for this capability.

It is imaginable that the Marine Corps could develop a close operational relationship with the Air Force, executive agencies for space, similar to our traditional Navy bond, in this future global-strike capability. SUSTAIN would truly be joint and reflect the Secretary of Defense's reorganization of national security space.

The SUSTAIN need relates directly to our advocacy for NASA's space exploration activities, as Marine Corps and NASA manned and unmanned technology interests will over overlap significantly. It is this type of synergy that will mitigate the otherwise prohibitive expense of a solo Department of Defense or NASA technology capability development thrust. Accordingly, we are using the early expression of our formal requirements as the mechanism to pull the technologies forward.

We thank you for inviting us to participate in this important forum. The Marine Corps stands ready to work with NASA to meet the national-security challenges of the 21st century on land, at sea, in the air, and through space.

Mr. Chairman, Members of the Subcommittee, at this time I stand ready to answer your questions that you may have.

Thank you.

[The prepared statement of General Zilmer follows:]

PREPARED STATEMENT OF BRIGADIER GENERAL RICHARD C. ZILMER, DIRECTOR, STRATEGY AND PLANS DIVISION, PLANS, POLICIES, AND OPERATIONS DEPARTMENT, HEADQUARTERS, MARINE CORPS

Brigadier General
Richard C. Zilmer
Director, Strategy and Plans

Brigadier General Zilmer was born in Reading, PA. He was commissioned a second lieutenant in the Marine Corps Reserve following his graduation from Kutztown University of Pennsylvania in 1974 where he earned a Bachelor of Science Degree in Secondary Education.

Following completion of The Basic School, Brigadier General Zilmer reported to the 3rd Marine Division in Okinawa and served as a Rifle Platoon Commander with 3rd Battalion, 4th Marines. In June 1976, Brigadier General Zilmer reported to the Marine Corps Recruit Depot at Parris Island, SC, where he served as a Series Commander and Company Executive Officer. He reported to the 2nd Marine Division in 1980 and served as Assistant Operations Officer and Rifle Company Commander with 2nd Battalion, 8th Marines.

In August and September 1982, Brigadier General Zilmer participated in peacekeeping operations in Beirut, Lebanon. Returning to the United States in November 1982, Brigadier General Zilmer was ordered to Marine Barracks, Washington D.C. for assignment and duty with Security Company located at Camp David, MD.

From July 1985 until June 1987, Brigadier General Zilmer was assigned to The Basic School in Quantico, VA, where he served as Tactics Assistant Group Chief and Company Commander of two student companies. In June 1988, Brigadier General Zilmer was assigned to the 1st Marine Division, serving initially as Operations Officer and later as Executive Officer of 1st Battalion, 7th Marines and Operations Officer for Task Force Ripper during Operations Desert Shield/Storm in 1990 and 1991.

Following the Gulf War, Brigadier General Zilmer was reassigned to Headquarters Marine Corps where he served as Section Head of Ground Officer Assignments. In 1993, Brigadier General Zilmer was again assigned to 1st Marine Division as Commanding Officer of 1st Battalion, 1st Marines and subsequently deployed to Okinawa, Japan as the Ground Combat Element of the 31st Marine Expeditionary Unit (Special Operations Capable). Following a brief assignment as Executive Officer of 1st Marines, Brigadier General Zilmer was next assigned in 1995 to the Joint Warfare Staff in Poole, England where he served as the Senior U.S. Marine Exchange Officer to the Royal Marines. In July 1997, Brigadier General Zilmer reported to I Marine Expeditionary Force for duty as Commanding Officer, 15th MEU (SOC). In July 2000, Brigadier General Zilmer was assigned to the United States European Command in Stuttgart, Germany where he served as the Deputy Director of Operations and the Director, Counter Terrorism Joint Planning Group.

In June 2002, Brigadier General Zilmer reported to his present assignment as Director, Strategy and Plans Division, Plans, Policies and Operations Department at Headquarters, U.S. Marine Corps, Washington, D.C.

Brigadier General Zilmer has attended the Infantry Officers Advanced Course at Fort Benning, GA, the Marine Corps Command and Staff Course at Quantico, VA, and the Naval War College at Newport, RI. His personal decorations include the Bronze Star with Combat Distinguishing Device, Meritorious Service Medal, Navy Commendation Medal and the Combat Action Ribbon

Chairman Brownback, Senator Breaux, distinguished members of the Committee; it is my honor to present to you the Marine Corps' perspective on how NASA's space exploration capabilities are critical to the Marine Corps' warfighting strategies of the future. Your sustained interest in and commitment to fully exploiting the opportunities offered by space for commerce, science, and transportation will contribute directly to the preservation of our Marine Corps' expeditionary character, and our Nation's security.

I. Introduction

In recent years many have asked why the terrestrially-oriented Marine Corps takes such an interest in contributing to the roadmaps for national security, commercial, and scientific space in the future. It is true, that unlike National Aeronautics and Space Administration (NASA) and other DOD entities, the Marine

Corps has neither programmatic nor fiscal equities in space. Yet our operational equities in space exploitation for both military advantage and expeditionary reach are at least equal to those of other users. These needs lead us to exploit space-related capabilities. For affordability, we must coordinate and synthesize our technology needs with other DOD and non-military users having similar requirements related to space exploration. Multiple customers having fully coordinated needs and objectives to avoid duplication are critical for the national affordability of any bold space vision today and in the future. We are therefore determined to remain engaged and contribute constructively to the challenges and opportunities of space, to seek out developmental partnerships, and to avoid cost-prohibitive duplications of effort. An energized NASA will be an important enabler for the Marine Corps in realizing such capabilities.

II. Background

With regards to the Marine Corps' role in space exploration and manned space flight, we are proud of the historic role we have played in opening up space as a medium of great practical utility. It is notable that the Honorable John Glenn, a Marine, was the first American in space to orbit the earth. Many Marines have followed in his footsteps, participating as trained astronauts and crewmembers in several manned space programs over the years. For example, just last year Major General Bolden retired after a career that included his participation as an astronaut in the Space Shuttle Program.

From the earliest days of our involvement, we have made both intellectual and inspirational contributions to the Space Program. We have and will continue to help define the critical roles that space will play in national security. Interestingly, it was our 23rd Commandant, General Wallace Greene, who first marked Marines as true space visionaries. In 1964 he accurately foresaw the use of suborbital space to transport Marines at hypersonic speeds for responsive global assault support. Though his vision was technologically ahead of his time, the Marine Corps did take a major step towards realization prior to his passing earlier this year. On 22 July 2002, Lieutenant General Emil (Buck) Bedard signed the Small Unit Space Transport and Insertion (SUSTAIN) need statement, blazing a trail to a new expeditionary assault support capability for the next chapter of Marine Corps history. Its eventual operationalization could fulfill Commandant Greene's vision, enabling speed of response, range, altitude, and strategic surprise unimaginable even by today's expeditionary response standards.

III. USMC Operational Concepts As They Relate to NASA Initiatives

Mission areas related to Space Control and Global Strike are currently being adopted into Marine Corps warfighting concepts and capabilities. As a result, we established a Marine Component in support of United States Strategic Command (USSTRATCOM), namely MARFORSTRAT. As the Marine Corps matures these advanced capabilities in the years and decades ahead, MARFORSTRAT will provide a transformational expeditionary capability that projects the most psychologically effective component of our traditional character, the Marine on the ground.

The SUSTAIN need frames a capability to transport a strategic capability from CONUS to any other point on the globe within two hours of an execution decision. It is important to note that SUSTAIN does not deliberately seek out a space transit capability for its own sake. However, we are also aware that the hypersonic transport speeds requirement, combined with the need to overfly non-permissive airspace en route may necessarily drive the material solution into space.

The SUSTAIN concept includes strategic capabilities options that span the spectrum from autonomous weapons payloads to the landing of Marines on the ground. The range of force application options reflects the validated warfighting assumption that frequently machines and munitions alone will not be able to replace the effectiveness of "situationally curious" soldiers in theater, and the persuasive psychological value of their presence to the mission at hand.

The SUSTAIN capability includes a need to insert, execute, and extract composited modular and relatively self-sufficient and supported larger capability sets without the need to violate any uncooperative or physically non-permissive airspace en route. This challenging requirement is projected for initial operational capability (IOC) between 2025 and 2030. We intend to approach members of United States Special Operations Command (USSOCOM) and Air Force Space Command (AFSPC) to refine a Joint requirement, by means of translating the SUSTAIN need into an Initial Capabilities Document (ICD). This need will heavily leverage on going manned and unmanned Air Force, DARPA, and NASA initiatives and programs, with NASA's manned programs being the key to fulfilling our objective capability. The USMC has also made an effort to make the SUSTAIN need a user-pull foundation piece of the National Aerospace Initiative. While the USMC does not expect to manage a space transport program in the future, our continuing expressions of need will help to steer and integrate the diverse technologies and demonstrations more rapidly and rationally, in the same spirit as Commandant Greene's earlier proposals.

IV. Marine Corps Needs and NASA Technology Leveraging

The SUSTAIN need relates directly to our Service Advocacy for the reinvigoration of NASA's scientific space exploration activities. While the core missions of the Marine Corps and NASA differ fundamentally, the technology sets they will require to accomplish their respective missions share significant commonalities. To the extent that our technology and capability roadmaps overlap with those of NASA and other commercial space transport interests, there exists a tremendous potential developmental synergy that will mitigate the otherwise prohibitive expense of a solo-DOD technology/capability thrust. The Nation can likely only afford one such large, ambitious transformational and/or manned space program at a given time. But that one program can simultaneously serve many customers in commerce, science, and other governmental and civil applications. The key is the early expression of user pull on the technologies and capabilities, combined with their earliest coordination and synthesis. By these means NASA will oversee the development of a large percentage of the military requirements, ensuring successful transition and at relatively lower cost to the other customers. The key again is the earliest validation of the expressed needs.

V. Conclusion

In conclusion, the USMC is pleased with the recent changes to national security space that have provided us a greater voice in space-related warfighting technologies and capabilities, and we thank you for inviting us to participate in this forum. Considering our possible emerging space transportation and warfighting equities, it is important that we coordinate with a reinvigorated NASA as early as possible. Because our needs lean forward ahead of the technology acceleration curve, we desire a NASA that is both energized and unafraid of the space exploration-related science and technology challenges that lie ahead. Whether it is in conjunction with the Air Force Executive Agent for Space or an eventual Space Force or Space Service, the Marine Corps stands ready to work with NASA and others to meet the national security challenges of the 21st Century on land, at sea, in the air, and through space.

Senator BROWNBACK. Well, you posed some very exciting ideas, and I can't wait to get back around to you to ask some of the questions, because that's quite a incredible vision that's quite exciting.

We'll be going on down through the rest of the panelists. So, first, Mr. Orlando Figueroa. Is that correct?

Mr. FIGUEROA. Figueroa.

Senator BROWNBACK. Figueroa. Pardon me. Director of Mars Exploration Program, Office of Space Science of NASA, headquarters here in Washington, D.C.

Delighted to have you here.

STATEMENT OF ORLANDO FIGUEROA, DIRECTOR, MARS EXPLORATION PROGRAM OFFICE, OFFICE OF SPACE SCIENCE, NATIONAL AERONAUTICS AND SPACE ADMINISTRATION HEADQUARTERS

Mr. FIGUEROA. Thank you.

Chairman Brownback, distinguished Members of the Subcommittee, thank you for the opportunity to present elements of a NASA space science perspective on space exploration.

With your permission, I want to take advantage of my position as Director of the Mars Exploration Program to present a unique perspective of the exploration of the solar system and the universe, understanding the origins and evolution of life, and the search for evidence of life elsewhere. All these are elements reflected in the NASA 2003 strategic plan, which you have before you today.

I would like to bring to your attention Goal 5, on page A–11 of the plan, that addresses the theme of today's hearing, space exploration. To this end, Goal 5 states that NASA will, "explore the solar system and the universe beyond, understand the origins and evolution of life, and search for evidence of life elsewhere." Directly tied to this goal are three major objectives, outlined on page A–12 of the plan, that are associated with the Mars Exploration Program. I will present today a unique perspective of this goal and the key objectives that relate to NASA Mars exploration and other future space science missions.

Technological advances and scientific discoveries over the past several years have revolutionized our understanding of the formation of the universe and the origins of life within it. We know now that the basic ingredients of life are common throughout the universe, having been formed within star systems and disseminated through their violent explosions.

Thanks to the incredible advances in life sciences over the past couple of decades, our basic understanding and perspectives of the nature and evolution of terrestrial life has also changed. Life on earth has demonstrated incredible tenacity, resilience, and perseverance in finding ways to take hold and survive even in the most harshest and extreme environments. In fact, we know now that, on Earth, wherever there are basic nutrients, a source of energy, and liquid water, there are life forms.

In addition, recent discoveries indicate that many of these same environments have been present and may still be present today in planets and moons in our solar system. The prospects, thus, of life elsewhere and habitable zones within those, among the trillions and trillions of stars in the universe are quite high, indeed. Now, looking for it and confirming its past or actual presence is a challenge that will take every ounce of human ingenuity, creativity, intuition, and commitment to search and explore to, from, and in space.

Closer to the Mars Exploration Program, you know, Mars, we all know, is a tangible frontier that has captivated humanity's curiosity, scientific imagination, and spirit of exploration for ages. It is the first and most accessible planet in the solar system, therefore, playing a vital role in answering the key questions of how planets evolve into possible habitable worlds and the origins and preservation of life. It is the only planet, other than Earth, that shows strong evidence of liquid water having coursed over its surface. Contributing the current view of Mars as a once wet and warm place are numerous channels that presumably are the product of flowing water on its surface. There are sedimentary deposits similar to those that, on Earth, are indicative of persistent and cyclic water activity. There is subsurface ice and surface features that suggest permafrost and periglacial forms.

Now, although our current understanding of life in the universe may be limited, we believe that the features we see on Mars today are the relics of environments that may have once been habitable and that might have actually harbored some form of life. Mars is the most Earth-like planet in the solar system. It has diurnal and seasonal cycles similar to our own. There are water-based polar caps that wax and wane, vast volcanoes, deep canyons, shifting dunes, and other features that are analogous to those on Earth. And, last but not least, Mars has always beckoned as the first and, in the foreseeable future, perhaps the only planet suitable for human exploration and possible habitation.

We presently have two assets around Mars, Mars Global Surveyor and Odyssey, that continue to rewrite the books about the Red Planet. They will soon be joined by two robotic geologists, so aptly named as "Spirit" and "Opportunity," by Sofi Collis, the 19-year-old winner of the "name the rovers" contest, because they reminded her of the dreams and aspirations, having come to this country from Russia with her adopted mother when she was younger.

In January of next year, Spirit and Opportunity will land on the Martian surface. After the air bags cushion their landing and they're settled on the surface and opened, the rover will roll out to present unprecedented panoramic images of the Martian terrain. These first images will set the stage for scientists to select promising geological targets that will tell part of a story of water in Mars' past and perhaps as a proxy for possible habitable environments.

So over a period of 90 days, the rovers will drive to multiple locations to perform onsite scientific investigations and send to Earth what promise to be breathtaking views and new perspectives of Mars that we will share with the public. As you may be aware, our partners in the European Space Agency and Japan, also have spacecraft on their way to Mars. Clearly, this coming December and January will be quite a busy and exciting time for all of us.

Now, the rovers will complete the first cycle of reconnaissance and validation. It's only the beginning of setting a stage for much higher resolution measurements to search for compelling sites, to be followed by principal investigator-led missions, and concluding this decade with the first *in situ* analytical laboratory since the Viking era. They will be enhanced significantly by long-range mobility, hopefully nuclear power, and far more sensitive and precise instruments as we search for basic building blocks of life.

Now, the next decade of Mars exploration will witness a transition from a search for habitats to the search for definitive measurements of life. And, with the help of the science community, we have, over the past year, developed several alternate pathways of investigations that promise to respond to the unknown and unpredictable discoveries of this decade. All the pathways show common threads—an emphasis on intensive *in situ* analysis and the analysis of Mars samples in Earth laboratories. Now, they will be complemented by other missions from the science community, principal investigator-led.

Now, we expect that this legacy in capability and understanding of a Martian environment will set in motion the transformation for all-robotic to robotic-and-human exploration of Mars.

Now, there is compelling evidence elsewhere in the solar system that there may be water also present in other solar bodies—Europa, Callisto, and Ganymede, the Jupiter moons—and, therefore, the Jupiter Icy Moons Orbiter within the Prometheus Project offers the opportunity for a highly efficient nuclear-electric propulsion system that will visit all three in a single mission. Now, this represents a major step in the understanding of the nature and extent of habitable environments in our solar system.

The large propulsive capability will enable high energy missions that are otherwise impossible, and efficient power supply will allow increased data return, as well as new types of scientific measurements.

But the search for life doesn't end there for us. Over the past few years, and using Earth-based observations, the discovery of Jupiter-sized planets around stars like our own has increased dramatically, to over a hundred today.

Now, operating above Earth's largely opaque atmosphere, with robotic observatories, we will view the heavens in a variety of new wavelengths to understand the nature of the stars, galaxy, and the objects in the universe and what they can tell us about our origins and destiny. So we're developing technologies and techniques, such as space interferometry to enable us to detect and study extra-solar planets, Earth-sized planets within those, and certainly search for telltale chemical signatures of life.

Thank you for the opportunity to address you, and I hope this presents a compelling vision for exploration of the solar system and beyond.

Senator BROWNBACK. It's certainly a very interesting introduction to possible options, quite an interesting set of programs. I look forward to questioning you and discussing more.

Mr. Gregory Withee is the Assistant Administrator for Satellite and Information Services of the National Oceanic and Atmospheric Administration, Department of Commerce.

Mr. Withee, delighted to have you here.

STATEMENT OF GREGORY W. WITHEE, ASSISTANT ADMINISTRATOR FOR SATELLITE AND INFORMATION SERVICES, NATIONAL OCEANIC AND ATMOSPHERIC ADMINISTRATION (NOAA), U.S. DEPARTMENT OF COMMERCE

Mr. WITHEE. Thank you, Mr. Chairman.

Mr. Chairman and Members of the Subcommittee, as Director of the Nation's Civil Operational Environmental Satellite Program, which resides in NOAA, I'm pleased to have the opportunity to testify before you today on NOAA's activities related to U.S. space exploration.

Vice Admiral Conrad Lautenbacher, my boss, has asked me—and administrator of NOAA—has asked me to convey to the Subcommittee his strong support of NOAA's programs that supports space exploration and our contributions to space weather.

My written testimony provides additional details, however, I'd like to highlight some select items.

The nation is accruing substantial benefits from the products and services that NOAA provides to support civilian and military space exploration. These services are critical to enhancing U.S. economic, national, and homeland security. Within NOAA's National Weather Service, the Spaceflight Meteorological Group based in Houston, has provided meteorological support to NASA's human spaceflight program since 1962. This group provides weather forecasts and briefings to NASA personnel that support space shuttle launches and landings and activities at the International Space Station.

NOAA's Space Environment Center is a partnership between NOAA and the U.S. Air Force. The Space Environment Center's space weather warnings and forecasts provide critical support to civilian and military aviation and communications systems on Earth and the NASA astronaut health and safety at the Space Station.

The Space Environment Center implements its mission by continually monitoring the sun's atmosphere using data from NOAA's Polar Orbiting Operational Environmental Satellites—we call those POES—and Geostationary Operational Environmental Satellite, and we call those satellites GOES.

With respect to the contribution from NOAA GOES, through its interagency collaboration with the U.S. Air Force, funded the first—the Air Force funded the first Solar X-ray Imager, which we call SXI, which was build by NASA, and it's now flying in operational mode on GOES–12, as we speak, above the Atlantic and eastern part of the United States. By flying this instrument, the SXI, or Solar X-ray Imager, simultaneously meets civilian and military needs for timely data on solar activity and the locations of solar flares at substantial cost savings to the U.S. taxpayer. The instrument greatly advances NOAA's space weather forecasting and research capabilities and expands the ability of the NOAA GOES satellites to monitor, not only the Earth's environment, but also the sun and space weather disturbances caused by violent solar activity. The instrument's data and derived products are extremely important to satellite operators, electrical power grid operators, astronauts, airline dispatchers, GPS users, radio communicators, and I could go on. On data from NASA's research spacecraft, such as ACE, Image, SOHO—those are various ventures put forward in space by NASA, and the U.S. Defense Department's Defense Meteorological Satellite Program are also used in our forecast and warning services.

We further collaborate with NASA to use unique positions in space, such as the La Grange points, which are balance points between the sun and the Earth, to meet our operational needs.

We are further exploring advanced propulsion technology such as solar sails that will allow us to operate operational platforms in even more useful, but more difficult orbits.

NOAA engages in a number of research and development activities to support our operational requirements, and these partnerships have allowed us to test and integrate new technology and models into our activities that meet our mission requirements to better serve the American public.

In looking at future satellite systems, NOAA has incorporated validated requirements from civilian and military users alike to support the U.S.-based weather and human spaceflight programs into its future satellite systems. NOAA will continue to fly the instrument I already addressed, the solar X-ray imager, on subsequent GOES spacecraft. With respect to our polar orbiting satellites, in collaboration with DOD and NASA we are moving forward with the development of the first National Polar Orbiting Operational Environmental Satellite system—short name is NPOESS—which is a convergence of NOAA and DOD spacecraft scheduled for launch in 2009. Instruments on this spacecraft, NPOESS, will significantly enhance civilian and military space weather activities and better support the U.S. space exploration program.

Now, tomorrow, I will join colleagues on a related topic across the U.S. Government with 34 nations from across the world at the Earth Observing Summit, where Ministerial-level delegates from 30 countries will meet the U.S. Department of State—meet at the U.S. Department of State to seek global agreement to initiate a ten-year program to coordinate our collective space and *in situ* observation programs. I'm pleased to say that Space Weather, which supported space exploration, is a part of these discussions.

The premise of the summit is that by developing a global strategy and partnership using space-based observations with *in situ,* or ground-based, measurements, we will better understand the short-term and long-term trends and natural cycles of the environment. These partnerships will also enhance U.S. space-related activities.

In conclusion, NOAA joins our partners here today to reiterate our full commitment to supporting collaboration among U.S. Federal agencies, the private sector, and academia to support the U.S. space exploration and the critical role of space weather. NOAA appreciates this Subcommittee's interest in our activities and seeks your support to ensure full funding of the President's 2004 budget request. This will allow us to support all aspects of our mission to protect and enhance the U.S. economic, national, and homeland security interests.

So, in conclusion, thank you, Mr. Chairman, Members of the Subcommittee, for this opportunity to testify on this important matter to NOAA and the Nation. I stand ready to answer any questions you may have.

[The prepared statement of Mr. Withee follows:]

PREPARED STATEMENT OF GREGORY W. WITHEE, ASSISTANT ADMINISTRATOR FOR SATELLITE AND INFORMATION SERVICES, NATIONAL OCEANIC AND ATMOSPHERIC ADMINISTRATION (NOAA), U.S. DEPARTMENT OF COMMERCE

Thank you, Mr. Chairman and Members of the Subcommittee, for the opportunity to testify before you regarding the National Oceanic and Atmospheric Administration's (NOAA) activities with other U.S. Government agencies in the area of space exploration. I am Gregory Withee, Assistant Administrator for NOAA's Satellite and Information Services and am responsible for end-to-end management of NOAA's satellite, data and information programs.

Space, and our ability to access, operate in and explore it, continues to capture the imagination. The human concept of space has evolved over the past 60 years from a place to gaze at in wonderment to realizing a valuable real-estate that supports a multi-billion dollar private sector. In the 1940s and 1950s, the U.S. military led space development with its research on rockets and sensors. This provided the foundation for the work that we do at NOAA as the U.S. civilian operational environmental satellite service, as well as the research and development work conducted by the National Aeronautics and Space Agency (NASA). These activities also support a growing communications and remote sensing industry, both within the U.S. and foreign countries.

Today, I will highlight some of the work we undertake at NOAA in space as well as the critical partnerships with NASA, the Department of Defense (DOD), the private sector, and international partnerships that enable us to accomplish our mission in service to the American public.

NOAA's Contribution to Space Exploration

Space, from the Sun to Earth's upper atmosphere, is a strategic and economic frontier. This space environment influences a multitude of human activities and presents numerous scientific challenges. NOAA's Space Environment Center (SEC) has a central role in conducting research to understand the space environment, and performs critical space weather operations for the Nation.

SEC continually monitors and forecasts Earth's space environment. The Center provides accurate, reliable, and useful solar-terrestrial information; conducts and leads research and development programs to understand the environment and to improve services; advises policy makers and planners; plays a leadership role in the space weather community; and fosters the commercial space weather services industry.

While NASA continues its lead role in deep space exploration, space is the arena within which NOAA's Geostationary Operational Environmental Satellites (GOES), Polar-orbiting Operational Environmental Satellites (POES), and the forthcoming National Polar-orbiting Operational Environmental Satellite System (NPOESS) operate. NOAA works very closely with NASA, DOD, and the private sector to build, launch, and operate satellites at various levels in space—GOES at 22,000 miles and POES at 520 miles above the Earth's surface—to support our Earth observing mission. These satellites also have special sensors on-board that contribute to the operational exploration of near Earth space—the Solar X-ray Imager and the Space Environment Monitor—which tell us of operating conditions such as solar flares or ionic storms, and provide information essential to the health and safety of our spacecraft and to human life on Earth.

With the successful recent launch of a Solar X-ray Imager on the GOES spacecraft, SEC has moved the country into a new era of solar observational capabilities and forecasting of solar disturbances. The Solar X-ray Imager (SXI), the first operational solar imager ever flown in space, was launched on GOES–12. Rather than having redundant military and civilian solar X-ray imagers, the first SXI was funded by the U.S. Air Force and built by NASA for flight on NOAA's GOES. SXI greatly advances NOAA's space weather forecasting and research capabilities and expands the ability of the NOAA GOES satellites to monitor not only Earth's environment, but also the Sun and space weather disturbances caused by violent solar activity.

We also receive data from NASA and DOD satellites that are combined with NOAA's satellite data to provide the basis of space weather forecasts. These forecasts are provided to other satellite operators and users on Earth, allowing preventive action to protect vital infrastructure. NOAA provides critical weather and space-based support to NASA and DOD during satellite and Space Shuttle launches and landings, and for operations at the International Space Station (ISS). In fact, without the critical data the SEC provides, NASA astronauts would be unable to safely take spacewalks to work outside of the Space Station. The protection of life and property from space weather is a key requirement that NOAA will continue to support with future GOES and NPOESS systems.

SEC is striving to utilize and enhance the observational capabilities of NOAA for the Nation's benefit. Observations from satellites such as GOES, POES, NPOESS are essential to improve our understanding of the solar-terrestrial system and to provide timely and accurate forecasts. SEC invests significant effort evaluating the need for new data, and participating in other agency, institution, or international programs that hold promise for providing data crucial for improving space weather services.

SEC performs a vital role for the Nation in conducting and coordinating research and its application. As described in the recent National Research Council report—A Decadal Research Strategy in Solar and Space Physics (2003), NOAA should assume full responsibility for space-based solar wind measurements, expand its facilities for integrating data into space weather models, and with NASA should plan to transition research instrumentation into operations. As discussed in the National Space Weather Program Implementation Plan (2000), interagency programs cannot succeed in meeting the Nation's needs without NOAA SEC observations, research, model development, and transition to operations. And, as emphasized in DOD's National Security Space Architect Study (2000), NOAA's current and planned activities are essential to meet Department of Defense's space weather needs.

NOAA's Collaboration with Other Space Operators

The collaboration among space operators is closely coordinated and mutually beneficial. NASA and DOD conduct critical research and development activities, that NOAA assesses and incorporates, as needed, onto its civil operational spacecraft. The space industry provides expertise to assist us in our respective missions. Increasingly, collaboration with private sector and foreign remote sensing operators provides data and information from their platforms that NOAA and other government agencies, such as U.S. Department of Agriculture, U.S. Department of Energy and the U.S. Department of the Interior, use to implement their respective missions.

Collaboration requires a great deal of coordination within the U.S. and internationally. Within the U.S. Government, the Office of Science and Technology Policy provides a mechanism for space policy coordination. Internationally, the Committee on Earth Observing Satellites, and the World Meteorological Organization provide venues for coordinating with other civil space operators. Because building, launching and operating in space is a very expensive undertaking, these coordination mechanisms provide an opportunity for all members to ensure data access and archive from their individual platforms without duplication of effort.

The premise of the Earth Observing Summit that will occur at the U.S. State Department on July 31 among thirty Ministerial level delegates is that by developing a global strategy and partnership using space based observations with *in situ* measurements, we will be able to better understand short-term and long-term trends in natural cycles and the environment.

NOAA's Coordination with NASA and DOD

NOAA works very closely with NASA and DOD in all aspects of our mission. For nearly 60 years, the U.S. Government has been successfully developing and applying space based Earth remote sensing to meet the information needs of Federal and state agencies, and the general public. Today NOAA, NASA, and DOD are planning the next generation environmental operational satellites, and they are working with other Federal and non-Federal users (including the private sector and foreign remote sensing systems partners) to reduce the cost and provide maximum benefit to the U.S.

Historically, NOAA's satellite program has been built and operated primarily to support the needs of NOAA's National Weather Service's forecasting and warning responsibilities. However, NOAA's satellite systems, address numerous climate and global change requirements needed for atmospheric, terrestrial, and oceanic applications. For example, satellite data are an important component in the emerging ocean observing system. In addition, information from NOAA and non-NOAA satellites produced and distributed by NOAA play a vital support role to U.S. economic, homeland, and national security. A recent example of this was the use of NOAA imagery products that supported operations during Operation Enduring Freedom, and Operation Iraqi Freedom, in particular detecting and monitoring dust storm events in Afghanistan and Iraq.

NOAA, NASA and DOD Partnerships in Future Spacecraft Systems

Polar-orbiting Systems

On 5 May 1994, the President directed the convergence of the NOAA's POES program and DOD's DMSP to become NPOESS, designed to satisfy both the civil and national security operational requirements. NASA contributes to this effort through the new remote sensing and spacecraft technologies of its Earth Observing System (EOS) mission. The President also directed the DOD, DOC, and NASA to establish an Integrated Program Office (IPO) to manage this converged system. On May 26, 1995, DOD, DOC, and NASA signed the Memorandum of Understanding that provides the guidelines under which the IPO operates. Under the terms of the MOU, NOAA has overall responsibility for the converged system, as well as satellite operations; DOD has the lead on the acquisition; and NASA has primary responsibility for facilitating the development and incorporation of new technologies into the converged system. NOAA and DOD equally share NPOESS costs, while NASA funds specific technology projects and studies.

NPOESS is a major system acquisition estimated to save approximately $1.6 billion to the taxpayer compared to the cost of operating 2 separate systems. NASA, in cooperation with NOAA, will launch the NPOESS Preparatory Program (NPP) satellite in 2006 as a risk reduction/early delivery program for 4 critical NPOESS sensors, early delivery, test and evaluation of the command and control and data retrieval for NPOESS.

Geostationary Systems

In response to validated user requirements from Federal, state, and local government agencies, private sector, and academia, NOAA is developing its next generation of geostationary and polar-orbiting satellites. The GOES R-Series will continue the NOAA–NASA partnership for geostationary satellites but with the prospect of greater integration of activities. To effectively and efficiently meet the GOES R-Series system requirements, a complete end-to-end (data sensing to information access) approach must be adopted. To facilitate this end-to-end approach, the NOAA–NASA teams are being more closely integrated so that the space and ground systems are developed and acquired as one, not separately, to ensure launch by 2012.

The instruments on the GOES–R will continue to fly the space environment monitoring sensors.

Select Examples of NOAA, NASA, and DOD Cooperation

The 3 agencies collaborate in many efforts that are complementary and mutually beneficial. Highlights of select examples of NOAA, NASA, and DOD cooperation are listed below:

Sensor Development

For many years, NOAA and NASA had a unique relationship developing instruments for Earth observing satellites. NASA funded and conducted the Operational Satellite Improvement Program (OSIP) from 1964 to 1982. The specific purpose of the OSIP was to improve NOAA's operational system by developing, testing, and demonstrating new components of the operational system, or improving the existing components, before NOAA made them integral to the operational system. The program funded "first unit developments", bringing research and development to the level needed for hand-off to NOAA operations. Under this program, NASA funded the first of the series of polar orbiters—Television and Infrared Observation Satellite (TIROS–N) and Improved TIROS Operational System (ITOS–I)—and NOAA funded the follow-ons. NASA also funded the 2 prototype GOES, and the addition of an atmospheric sounding capability to the GOES imager system.

Continuing the sensor development activities discussed above for GOES, POES, and NPOESS, NOAA's research Environmental Technology Laboratory has been involved in studies of various remote sensors such as microwave sounders, lidars and radar systems and has worked with DOD on the properties of ocean surfaces, and their interaction with radio waves. NOAA's ocean laboratories and university partners have developed systems that are used to calibrate ocean measurements from satellites.

Research to Operations

NOAA and NASA have recognized the value of definitive plans for the process of handling research to operations. Working closely with NASA, NOAA is working to develop its own technology infusion roadmaps, as a follow-up to NASA's Earth Science Enterprise Research Strategy and Application Strategy roadmaps. NASA's Moderate-resolution Imaging Spectroradiometer (MODIS) (a multichannel imager flying on NASA's TERRA and AQUA satellites) is the research predecessor for NPOESS' operational Visible/Infrared Imaging Radiometer Suite (VIIRS) instrument. NASA's Atmospheric Infra-red Radiation Sounder (AIRS) is now flying on NASA's AQUA satellite and providing atmospheric profiles of temperature and humidity. Information from the NPOESS Cross-track Infra red Sounder (CrIS) and the GOES Hyperspectral Environmental Suite (HES) will be used by NOAA customers much in the same fashion as AIRS is today.

NOAA and NASA jointly funded a study by the National Academy of Sciences/National Research Council Committee on NASA–NOAA Transition from Research to Operations (CONNTRO). The May 2003 final report is called *"Satellite Observations of the Earth's Environment, Accelerating the Transition from Research to Operations."* Recommendations from that report are being examined and jointly considered by NASA and NOAA. In an effort to maintain cognizance of technologies being undertaken by DOD agencies, NOAA participates in regular meetings with the intelligence community, the U.S. Air Force Space Technology organization (called National Space Architecture), and the U.S. Navy Space Experiments Review Board (SERB).

Collaborative Activities in Ground Systems Support

NOAA operates two Command and Data Acquisition Stations (CDAS), one in Wallops, Virginia (WCDAS), and the other in Fairbanks, Alaska (FCDAS), and a Satellite Operations Control Center in Suitland, Maryland. The primary responsibility of the CDAS is to track, command, and receive telemetry and imagery data from NOAA's geostationary and polar orbiting spacecraft. In addition to supporting the NOAA spacecraft, the CDAS provide a wide range of cooperative support services for NASA and DOD. The FCDAS is a primary site for acquiring data from DMSP on a cost reimbursable basis.

NOAA is also partnering with the U.S. Navy in the acquisition of data from the Windsat/Coriolis mission. Beginning in January 2003, FCDAS began acquiring data from this spacecraft which provides ocean surface wind information and important risk-reduction for similar instruments planned for NPOESS. The FCDAS is also a telecommunications hub for the NASA EOS spacecraft AQUA, flowing data from the NASA antennas at Poker Flats, Alaska to processing centers in the lower 48 states. FCDAS and WCDAS also track and gather space environmental data from the

NASA IMAGE and ACE spacecraft to support NOAA's Space Environment Center's operations in Boulder, Colorado.

NASA and NOAA also are exploring mutual backup agreements for spacecraft data acquisitions at both Fairbanks and Wallops to provide a more robust ground to satellite network.

Products and Services Developed Through Collaborative NOAA, NASA, and DOD Efforts

There are a number NOAA products and services that are developed using data from NOAA, NASA, and DOD. Select examples include:

Search and Rescue Satellite-Aided Tracking (SARSAT) System

SARSAT operates on the NOAA POES and GOES spacecrafts. The purpose of the SARSAT program is to relay distress alert and location information to search and rescue organizations worldwide. In order to coordinate U.S. activities in support of this program, NOAA has established a partnership with the U.S. Air Force, the U.S. Coast Guard, and NASA for the operation of the system. To date, the SARSAT program is credited with rescuing approximately 14,000 people worldwide.

Satellite Data Assimilation

The Joint Center for Satellite Data Assimilation (the Center) is a formal tri-agency program among NOAA, NASA, and DOD to improve utilization of satellite data, and prepare for future instruments in numerical weather prediction models. The goal of the Center is to accelerate and improve the scientific methods for assimilating satellite observations into operational numerical models. Established in FY2002, the Center now has critical mass of scientists from NASA, NOAA, and DOD are collocated in a new joint facility at the World Weather Building in Camp Springs, Maryland. Through explicit coordination and joint funding of research, the agencies have realized several improvements to operational forecasts, for example, by incorporating NASA research satellite data and improving radiative transfer methods.

Human Space Flight Support

The NOAA's National Weather Service has a long history of providing weather support for NASA. In the past, NOAA provided direct weather support to NASA for the Mercury, Gemini, Apollo, and other programs. The Spaceflight Meteorology Group (SMG) of NOAA's National Weather Service provides meteorological support for launches and landings of the Space Shuttle and other programs. SMG provides unique world-class weather support to the U.S. Human Spaceflight effort by providing weather forecasts and briefings to NASA personnel. Space radiation information and forecasts used in the flight operations for both the Space Shuttle and the Space Station, comes directly from NOAA's Space Environment Center to NASA before and during all Shuttle flights.

Space-based Oceanography

NOAA utilizes data from DOD and NASA spacecraft to implement its ocean and coastal mission. Extensive use of Sea-viewing, Wide-Field-of-view Sensor (SeaWiFS) data from a joint NASA-Orbital Imaging mission is used to support biological oceanography. Data from the JASON missions operated by NASA and the European Space Agency, measures sea surface height. These data are used in hurricane forecasting models. Sea surface temperature remains a critical requirement of many agencies to support their respective missions.

NOAA's Coastal Remote Sensing Program (CRS) includes activities such as the Coastal Change Analysis Program and other marine applications of satellite data such as harmful algal blooms, ocean color, and sea surface temperature. Other activities include NOAA's Coastal Change Analysis Program, land cover analysis, benthic habitat mapping, estuarine habitat, coastal water quality, harmful algal bloom forecasts, and topographic change mapping.

In conclusion, Mr. Chairman and members of the Subcommittee, NOAA is pleased to have had the opportunity to provide you an overview of our collaborative activities with NASA and DOD in the area of space exploration and space activities. A key element to our strategy is partnering with other agencies, such as NASA and DOD, the space industry, our international partners, and academia. These partnerships have proved to be wise investments for NOAA and the Nation.

Mr. Chairman and Subcommittee members, this concludes my testimony. I would be happy to answer any questions.

Senator BROWNBACK. Thank you, Mr. Withee, very much.

Dr. Michael J.S. Belton, Ph.D., is the Chair of the Solar System Exploration Survey Committee of the National Research Council in Washington, is also president of Belton Space Exploration Initiatives and emeritus astronomer at the National Optical Astronomy Observatory, in Tucson, Arizona.

Delighted to have you with us. Dr. Belton?

STATEMENT OF MICHAEL J. S. BELTON, PH.D., CHAIRMAN, NRC SOLAR SYSTEM EXPLORATION (DECADAL) SURVEY; EMERITUS ASTRONOMER, NATIONAL OPTICAL ASTRONOMY OBSERVATORY AND PRESIDENT, BELTON SPACE EXPLORATION INITIATIVES, LLC

Dr. BELTON. Thank you very much, Mr. Chairman, Members of the Committee. I'm very happy to be here to discuss the future of solar system exploration with you.

The survey was actually finished—the Decadal Survey on Solar System Exploration was actually finished about a year ago and will finally hit the streets as a published thing in a very short period of time. But, nevertheless, in that one year, what we have seen is, NASA developed the strategic plan, the new three-year plan, and most of the things—in fact, essentially all of the things that the space exploration—well, solar system exploration community put in their report are, in fact, co-opted in the NASA plan. And I have to tell you that we're exceedingly pleased about that. It's a very exciting plan.

The survey, itself, was a grassroots activity, although it was initiated through the NRC by NASA, but basically everybody in the community, one way or another, was involved. We spent a lot of time in various committees funneling down the information through five panels, and then finally to my group that put together the final recommendations.

Since that time, not only has NASA picked up and coopted most of our recommendations, but essentially all of the major groups, official groups, within the community have given strong endorsements to the survey.

Its recommendations are for a very strong and competitive flight program based on a few key scientific questions and a sound infrastructure, research infrastructure, and also including public outreach. We also put great store on a very forward-looking technology program, which we also see reflected in the NASA plans for the next 5 years.

Solar system exploration remains a compelling activity, because within our grasp are the answers to some very, very significant and what we believe are profound questions. So is life—does life exist or did it exist beyond the Earth? Are we alone? That's one of the questions that we believe we can answer with the Mars Exploration Program that Dr. Figueroa's already described to you. Where did we come from? How did life get established on the Earth? We believe that we can contribute to that one, that question, also. And then, finally, what is our destiny? There are aspects of the future of human civilization which depend upon random events occurring from the population of objects in the vicinity of the Earth.

We put our report together around four integrating themes that we believe should guide this endeavor for the next 10 years. The first billion years of solar system history is a critical element of that. We know very little about it, how the planets were put together and the time, in that first billion years, when life finally found its way to the Earth and became established there. We're trying to learn about that. And the program has elements within it to address some of those questions.

Volatiles and organics throughout the solar system, we know they exist in great abundance in the far reaches of the solar system. We want to know how they got to the Earth and the inner solar system. They're the "stuff of life," if you like.

And then we'd want to have some emphasis on the topic of the origin and evolution of habitable worlds. We want to know why Mars and why Venus is so different from the Earth, even though the processes that were involved in their formation were presumably quite similar.

And then, finally, we're very interested in just processes, in trying to understand how the laws of physics apply on these large scales within the solar system and how the things that we see in space can be properly explained in terms of those laws.

So this program of vital spaceflight has to have a mix of small missions, of medium-sized missions to do more complex things, and then just a few very complicated missions. Perhaps once a decade we feel they ought to be flown. We've suggested to NASA that they maintain their Discovery Program, which is a small mission program that basically invites individuals within the community, within the country, to use their cleverness and excitedness about this kind of exploration to invent new missions and not have everything done by committee.

We also want a New Frontiers Program, which is in the President's budget for 2003 and 2004, and we've suggested a system of flight missions that is exceedingly exciting, and prioritized those for them. We want to see an exploration of the Kuiper Belt and its largest member, the Pluto-Charon System. We want to get back to the moon and bring some samples back from near the South Pole, which can tell us a lot about what happened in the Earth-moon vicinity in the first billion years. We want to go to Jupiter. We want to go to Venus, and we want to go to a comet and bring back some of these organics and really take a look at them in the laboratory to understand exactly what they are and how they're made up.

So the future of solar system exploration, to us, looks very, very bright in the community. NASA is taking all of the technological and programmatic steps we think necessary to support future missions that'll provide the answers to a lot of the questions that I posed right at the very beginning.

I think that if we can find it within ourselves in this country to support this program in the way that it's now being laid out for the next 10 years, or 5 years, by NASA, we'll be able to really get to grips with these fundamental questions that I talked about early on.

So thank you very much for inviting me here. I'm very excited about this, and so are my colleagues in the community.

[The prepared statement of Dr. Belton follows:]

PREPARED STATEMENT OF MICHAEL J. S. BELTON, PH.D., CHAIRMAN, NRC SOLAR SYSTEM EXPLORATION (DECADAL) SURVEY; EMERITUS ASTRONOMER, NATIONAL OPTICAL ASTRONOMY OBSERVATORY, AND PRESIDENT, BELTON SPACE EXPLORATION INITIATIVES, LLC

Good afternoon, Mr. Chairman and members of the Committee. My name is Michael Belton, and I served as Chairman of the Solar System Exploration (Decadal) Survey for the Space Studies Board of the National Research Council. The NRC is the operating arm of the National Academy of Sciences, chartered by Congress in 1863 to advise the government on matters of science and technology. I am also an Emeritus Astronomer at the National Optical Astronomy Observatory and President of Belton Space Exploration Initiatives, LLC, in Tucson, Arizona. I have been involved in space exploration for most of my professional life and have been an investigator on several NASA flight missions including Mariner Venus-Mercury, Voyager, Galileo, Contour and Deep Impact.

The Office of Space Science of the National Aeronautics and Space Administration sponsored the SSE Survey to chart a bold strategy for general solar system and Mars exploration over the next decade. The Survey, which reported in July 2002, derived its recommendations and priorities by looking even farther into the future and is based on direct and well-considered inputs from the scientific community and interested public organizations. It has, achieved a broad consensus of opinion in these communities. Its recommendations are for a strong, competitive, flight program based on a few key scientific questions, a sound research infrastructure including public outreach, and a forward looking technology program that I expect will obtain the most innovative and cost effective mission solutions.

A critical element of the charge to the Survey was to formulate a "big picture" of solar exploration—what it is, how it fits into other scientific endeavors, and why it is a compelling goal today. We were also tasked to develop an inventory of top-level scientific questions and provide a prioritized list of the most promising avenues for flight investigations and supporting ground-based activities for the period 2003–2013.

In performing the Survey we took care to trace the relationships between basic motivational questions of interest to the public at large and the scientific objectives, integrating themes, key scientific questions, and prioritized mission list that form the core of our recommendations. Solar system exploration remains a compelling activity because it places *within our grasp* answers to basic questions of profound human interest—Are we alone? Where did we come from? What is our destiny? Mars and icy satellite explorations may soon provide an answer to the first question; exploration of comets, primitive asteroids and Kuiper Belt objects may have much to say about the second; surveys of near-Earth objects will say something about the third.

Although the scientific goals of NASA's Solar System Exploration program have been quite stable, in recent years the emphasis has increased in two areas—the search for the existence of life, either past or extant, beyond Earth, and the development of detailed knowledge of the near-Earth environment in order to understand what potential hazards to the Earth may exist. The field of astrobiology has become an important element in solar system exploration and there is an increasing interest in learning more about objects that could collide with the Earth at some future time.

The Survey developed four integrating themes to guide solar system exploration in the coming decade:

- *The First Billion Years of Solar System History.*—This formative period propelled the evolution of Earth and the other planets, including the emergence of life on Earth, yet this epoch in our Solar System's history is poorly known.
- *Volatiles and Organics; The Stuff of Life.*—Life requires organic materials and volatiles, notably liquid water, originally condensed from the solar nebula and later delivered to the planets by organic-rich cometary and asteroidal debris.
- *The Origin and Evolution of Habitable Worlds.*—Our concept of the "habitable zone" is being expanded by recent discoveries on Earth and elsewhere in the Solar System. Understanding our planetary neighborhood will help to trace the evolutionary paths of the planets and the fate of our own.
- *Processes; How Planets Work.*—Understanding the operation of fundamental processes is the firm foundation of planetary science, providing insight to the evolution of worlds within our Solar System, and planets around other stars.

With these four themes agreed to, the Survey was able to prioritize among the literally hundreds of scientific questions of interest to the community. The resulting set of twelve key questions with high scientific merit should guide the selection of flight missions over the next decade. We measure the scientific merit of a question by asking whether its answer has the possibility of creating or changing a paradigm, whether the new knowledge might have a pivotal effect on the direction of future research, and to what degree the knowledge that might be gained would substantially strengthen the factual basis of our understanding.

The twelve key questions, grouped within the four themes, are:

The First Billion Years of Solar System History.

1. What processes marked the initial stages of planet and satellite formation?
2. How long did it take the gas giant Jupiter to form, and how was the formation of the ice giants different from that of the gas giants?
3. What was the rate of decrease in the impactor flux throughout the solar system, and how did it affect the timing of the emergence of life?

Volatiles and Organics; The Stuff of Life.

4. What is the history of volatile material; especially water, in our Solar System?
5. What is the nature and history of organic material in our Solar System?
6. What planetary processes affect the evolution of volatiles on planetary bodies?

The Origin and Evolution of Habitable Worlds.

7. Where are the habitable zones for life in our Solar System, and what are the planetary processes responsible for producing and sustaining habitable worlds?
8. Does (or did) life exist beyond the Earth?
9. Why did the terrestrial planets diverge so dramatically in their evolution?
10. What hazards do Solar System objects present to Earth's biosphere?

Processes; How Planets Work.

11. How do the processes that shape the contemporary character of planetary bodies operate and interact?
12. What does our solar system tell us about other solar systems, and vice versa?

To advance the subject these scientific themes and key questions must be addressed by a series of spaceflights of different sizes and complexities. Also, as resources are finite, these proposed new flight missions must be prioritized.

It is important at this juncture to understand that the foundation on which the Survey's priorities rest must also be maintained and secured. The top-level *programmatic* priorities that are required to provide the foundation for productivity and continued excellence in solar system exploration are:

- Continue approved Solar System Exploration programs, such as the Cassini-Huyens mission to Saturn and Titan, those in the Mars Exploration Program, the Discovery Program of low-cost missions, and ensure a level of funding that is adequate for both the successful operations and the analysis of the data and publication of the results of these missions.
- Assure adequate funding for fundamental research programs, follow-on data analysis programs and technology development programs that support these missions.
- Continue to support and upgrade the technical expertise and infrastructure in implementing organizations that provide vital services to enable and support Solar System exploration missions.
- Continue to encourage, facilitate and support international cooperation in its Solar System exploration flight programs.

Maintaining a mix of mission size is also important. For example, many aspects of the key science questions can be met through *Discovery* class missions (<$325 M), while other high-priority science issues will require larger, more expensive projects. Particularly critical in our strategy is the *New Frontiers* line of missions ($325–650 M), which are Principal-Investigator (PI) led, medium class, competed missions. This line was proposed in the President's FY 2003 budget submission before the Survey was completed. The Survey strongly supported the proposal to establish a New Frontiers line of competitively procured flight missions with a total mission cost of approximately twice the Discovery cap.

Experience has also shown that large missions that enable extended and scientifically multi-faceted experimentation are an essential element of the mission mix. The Survey recommended that the development and implementation of Flagship (>$650M) missions, comparable to Viking, Voyager, Galileo, and Cassini-Huygens, be at a rate of about one per decade to provide for the comprehensive exploration of science targets of extraordinarily high priority.

Within this structure the Survey recommended the following prioritized flight program of missions in general solar system exploration in the period 2003–2013. It must be emphasized that, at NASA's request, the prioritization was done within cost classes and not over the entire list. Also by NASA's request, the priorities for the Mars Exploration Program were kept separate from the priorities for the Solar System Exploration Division.

Small Class

1. Discovery missions (at a frequency of approximately 1 every 18 months)
2. Cassini Extended Mission

Medium Class

1. Kuiper Belt/Pluto
2. South Pole Aitkin Basin Sample Return
3. Jupiter Polar Orbiter with Probes
4. Venus In-situ Explorer
5. Comet Surface Sample Return

Large Class (at a frequency of approximately 1 every decade)

1. Europa Geophysical Explorer

For the Mars exploration program the Survey recommended that in the coming decade the flight program should focus on missions that get down onto the surface of the planet with the ultimate goal of implementing Mars Sample Return missions in the period immediately following the current decade. It is believed that such samples are necessary to settle the question of the presence of life. The Survey recommended the following flight mission priorities for Mars exploration in the period 2006—2013:

Small Class

1. Mars Scout line
2. Mars Upper Atmosphere Orbiter

Medium Class

1. Mars Smart Lander
2. Mars Long-lived Lander Network

Large Class

1. Mars Sample Return (Preparation for flight missions in the next decade)

In addition the Survey committee counseled that NASA should seek to engage international partners at an early stage in the planning and implementation of Mars Sample Return; that the Mars Smart Lander (MSL) while addressing high priority science goals, should take advantage of the opportunity to validate technologies required for sample return, and that the Scout program should be structured like the Discovery program, with PI leadership and competitive missions. The Survey advocated that a Scout mission should be flown at every other Mars launch opportunity.

This future program for solar system exploration laid out above clearly requires a mix of Medium and Large class missions to adequately challenge current scientific paradigms. It also requires that small missions whether Discovery class, Mars Scout, or mission extensions, provide focused ways of responding quickly to discoveries made or provide vehicles for entrepreneurial creativity and new scientific ideas. Our proposed Kuiper Belt-Pluto mission may well be the last great reconnaissance mission within solar system exploration and, with it completed, we can expect that the program will rapidly enter a phase of large and medium class missions operating on the surfaces of planets or within their atmospheres and plasma environments. These missions will utilize technologies, yet to be practically developed, that will enable long sojourns, power advanced instrumentation, and return samples to the Earth. The inclusion of Project Prometheus and the optical communications initiative in the President's FY 2004 budget submission are two excellent examples of the type of technology development that is needed to move solar system exploration forward.

The Survey recognized that a significant investment in advanced technology development is needed in order for both the recommended flight missions to succeed and to provide a basis for increased science return from future missions. The following list of future possible missions (unprioritized) with high science value was noted by the Survey and gives some idea of the technical challenges that lie ahead:

Terrestrial Planet Geophysical Network	Trojan/Centaur Reconnaissance Flyby
Asteroid Rover/Sample Return	Io Observer
Ganymede Observer	Europa Lander
Titan Explorer	Neptune Orbiter with Probes
Neptune Orbiter/Triton Explorer	Uranus Orbiter with Probes
Saturn Ring Observer	Venus Sample Return
Mercury Sample Return	Comet Cryogenic Sample Return

The Survey identified the following areas in which we believe that technology development is appropriate:

Power:	Advanced RTGs and in-space nuclear fission reactor power source
Propulsion:	Nuclear electric propulsion, advanced ion engines, aerocapture
Communication:	Ka band, large antenna arrays, and optical communication
Architecture:	Autonomy, adaptability, lower mass, lower power
Avionics:	Advanced packaging and miniaturization, standard operating system
Instrumentation:	Miniaturization, environmental (Temperature, Pressure, radiation) tolerance
Entry to Landing:	Autonomous entry, hazard avoidance, precision landing
In-Situ Ops:	Sample gathering, handling and analysis, drilling, instrumentation
Mobility:	Surface, aerial, subsurface, autonomy, hard-to-reach access
Contamination:	Forward contamination avoidance
Earth Return:	Ascent vehicles, in-space rendezvous and Earth return systems

These technology areas were not prioritized by the Survey. Nevertheless, I note that in-flight power and nuclear electric propulsion initiatives were included in the 2003 budget request and appear again in the 2004 request as Project Prometheus. Also, there are other elements of the above list that are, I believe, being actively considered for inclusion in a future mission in NASA's New Millennium program.

The road that leads to the future of any endeavor is usually well defined only at its start. And quickly, the future becomes obscured by latent uncertainties: the possibility of new discoveries, of changing paradigms, changes in national policy, blind alleys, and funding pleasures and disappointments. Solar system exploration is no exception and in the time since the Survey was completed and published I have felt great excitement and considerable pleasure as important elements of our strategic plan have been proposed to Congress and move, hopefully, towards reality. The New Horizons mission, which I believe can fulfill our goals at the Kuiper belt and Pluto, is seeing strong support; the proposed Jupiter Icy Moons Mission will more than fulfill our goal of a flagship mission to further explore the subsurface oceans on Europa while simultaneously applying the new technologies that the Survey advocates as a basis for much of the future program. The most important of these new technologies—in-flight power and nuclear electric propulsion—are adequately covered in the proposed Project Prometheus. The New Frontiers program is going ahead and we await details of how NASA intends to implement this program to include the flight priorities that we have advocated. Finally, the research infrastructure, which underlies the flight program, also appears to be drawing adequate support.

The tragic *Columbia* accident will no doubt have effects on this program in ways that I cannot anticipate. Whether these effects will be positive or negative remains to be seen. However, I note the old proverb "much good can often come out of adversity." Since the end of the Apollo Program, the human spaceflight program has served to enable a number of robotic missions (the Shuttle has been needed to launch important spacecraft such as the Ulysses, Magellan, and Galileo probes, and the Hubble Space Telescope), but has not played a direct role in the exploration of other solar system bodies. In the distant future I expect that this may change in some elements of the program. Human exploration of Mars is a long spoken of goal but faces major technical challenges. A second area is the protection of the Earth from a potentially hazardous near-Earth object on a collision course. The role of humans, if any, in such an endeavor has not yet been satisfactorily worked out and, in my opinion, deserves attention.

In conclusion, the future of solar system exploration appears to be very bright. NASA is taking the technological and programmatic steps necessary to support future missions that will explore our solar system in astounding detail. Supported by the strategy laid out in the Survey, future solar system exploration will enable us to answer three fundamental human questions: Are we alone? Where did we come from? What is our destiny?

Senator BROWNBACK. Thank you very much for that excellent presentation, Dr. Belton, and I look forward to some questions and dialogue with you a little bit later.

Our final panelist is Dr. Louis Lanzerotti, Ph.D. He's a distinguished member of the technical staff, Bell Laboratories, Lucent Technologies, out of New Jersey.

Delighted to have you here. And you also have other experiences in your background that will help you testify here today for us. Thanks for joining us.

STATEMENT OF LOUIS J. LANZEROTTI, DISTINGUISHED RESEARCH PROFESSOR, NEW JERSEY INSTITUTE OF TECHNOLOGY, CONSULTING PHYSICIST, BELL LABORATORIES, LUCENT TECHNOLOGIES, AND CHAIRMAN, SOLAR AND SPACE PHYSICS SURVEY COMMITTEE, NATIONAL RESEARCH COUNCIL

Dr. LANZEROTTI. Well, thank you, Mr. Chairman, and it's a pleasure to be here. And, Senator Nelson, glad to be here. I remember appearing before one of your Committees, Senator Nelson, when you were in the House many years ago, and it's good to see you again.

I'm here to talk about solar and space physics research in the Nation. Solar and space physics is basically a study of the sun, and predominantly the environment, space environment, between the sun and the planets and around the Earth. And it has been and is a exceptionally vibrant and important field of research since the discovery of sunspots by Galileo in the 17th century, and certainly since the discovery of the Van Allen radiation belts around the Earth, in 1958, 40 years ago, at the dawn of the space age.

But not only is a study of the sun and the Earth of tremendous scientific interest and continuing interest, but this research also has important relevance for the increasing number of modern technologies that fly in space that can be affected by the solar and space environment. And Mr. Withee addressed those quite well in his statement.

And, in fact, this environment affects human spaceflight and the humans both in low-Earth orbit and ultimately in high-Earth orbit and as we go back to the moon and to Mars and as well as airliners that fly over the polar regions at the time of solar disturbances, make use of—airlines make use of the warnings that are provided by NOAA.

So late in the year 2000 or so, NASA, NOAA, the National Science Foundation, Office of Naval Research, and Air Force Office of Scientific Research all joined together and asked the National Research Council to conduct a comprehensive study of the current status and the future directions of U.S. ground-based and space-based research programs in solar and space physics because of its scientific importance, because of its relevance to society, and growing relevance to society. And so this Decadal Survey was carried out in parallel with the Solar System Planetary Exploration Survey that Dr. Belton just spoke about.

We really don't understand the underlying driving physical processes for all the temporal and cyclical changes that we see in the sun and that the sun produces at Earth and all those environmental changes that we see in the upper atmosphere, from the Aurora to changes in the Van Allen radiation belts.

So this Decadal Survey sets out five broad challenges that define where—taking what we know now, what we have learned from such incredible missions as the NASA SOHO mission, for example, and several other NASA spacecraft missions in the Earth's magnetosphere—to say where we should be going in the next decade, and establish a specific integrated program prioritized both on scientific impacts, as well as societal relevance, and that apply to these agencies that sponsor this study—NOAA, NASA, NSF, and the DOD, the Office of Naval Research, and the Air Force Office of Scientific Research.

And I want to emphasize that the prioritized recommendations fit within realistic budget guidelines, as well, and they are in this report that you have in front of you. These guidelines, we received from the several sponsoring agencies just to make sure that we weren't going off in crazy ways, in terms of budgetary implications, but, nevertheless, making sure that we were really addressing the key scientific and societal issues.

As I said, we've had some tremendous understandings of the sun. We can now see the interior of the sun—infer the interior of the sun, its oscillations with time, but we still don't understand some of the fundamentals, certainly don't understand some of the fundamental underlying drivers of the sun.

And so one of the five challenges that we identified was to significantly advanced our understanding of the sun's structure. And it will be addressed by a NASA solar probe mission that will fly closer to the sun than any spacecraft to date, make measurements at the very source region of the solar wind which flows out from the sun, fills the whole space environment of the solar system, and impacts Earth and Earth's magnetosphere.

This challenge is also being addressed by a National Science Foundation initiative called the Frequency Agile Solar Radio Telescope, which is an array of radio telescopes on Earth, to understand in detail some of the phenomenology and details of solar activity at very small spatial scales, together with optical measurements that are in progress and are ongoing.

Since the Van Allen belts, we have had tremendous understanding of the Earth space environment, but there are many challenges in the temporal changes. And the challenge for making further large understandings of the behavior of the Earth's environment will be addressed by a number of sequential NASA missions, of which the first priority is called the Magnetospheric Multiscale Mission. This is four spacecraft flying, in a coordinated fashion, to try to understand better and get a handle on the basic physics underlying the energy transfer from the sun, the solar wind, into the Earth's magnetosphere.

And, in parallel, there's an NSF initiative called the Advanced Modular Incoherent Scatter Radar, AMISR. I'm sure all of you have heard about that. This radar will acquire critical new data from the upper atmosphere at the very high latitudes on the Earth. These are regions that affect radio communications and radar transmissions and those types of things.

Finally, a third of the five challenges—I can't address all of them this afternoon in my oral testimony—is an especially important one. It's identified in a report that involved space weather. And, particularly, it's a development of a real-time predictive capability for anticipating and perhaps eventually being able to mitigate the impacts of solar disturbances on Earth. Space weather. Mr. Withee spoke about this. This is part of the National Space Weather Program, which is a joint initiative by DOD, NASA, NOAA, and NSF. And a central recommendation of our survey study is that NOAA, through its Space Environment Center, assume the responsibility for a new, and continuing then, spacecraft to monitor solar emissions before they reach Earth. At the present time, these studies are being carried out by a more scientifically-oriented spacecraft. They're doing a very good job, but their time in space is running out. They're old, and we need a new and continuing spacecraft called the Upstream Solar Wind Monitor. The NOAA Space Environment Center is, even now—as Mr. Withee pointed out, but I will say, as a civilian—is even now our central national resource for information on space weather, and NOAA, taking the responsibility for a upstream monitor for the solar wind in the future, will make a tremendous impact on the U.S.'s capability for understanding space weather.

In summary, our survey report provides the directions for the next decade for this important research field. Priorities are established, resource requirements are realistic. Exciting new understandings of the Earth and the sun will result, as will very important practical applications for society.

Thank you very much for being able to be here.

[The prepared statement of Dr. Lanzerotti follows:]

PREPARED STATEMENT OF LOUIS J. LANZEROTTI, DISTINGUISHED RESEARCH PROFESSOR, NEW JERSEY INSTITUTE OF TECHNOLOGY, CONSULTING PHYSICIST, BELL LABORATORIES, LUCENT TECHNOLOGIES, AND CHAIRMAN, SOLAR AND SPACE PHYSICS SURVEY COMMITTEE, NATIONAL RESEARCH COUNCIL

Good afternoon, Mr. Chairman and members of the Committee. My name is Louis Lanzerotti, and I served as Chairperson of the Solar and Space Physics Decadal Survey for the Space Studies Board of the National Research Council. The NRC is the operating arm of the National Academies, initially chartered by Congress in 1863 to advise the government on matters of science and technology. I am also Distinguished Research Professor at the New Jersey Institute of Technology and a consulting physicist at Bell Laboratories, Lucent Technologies.

I am here today to provide an overview of the future of solar and space physics during the corning decade. I would like to begin by giving you some context for this area of science.

The Sun is a variable, magnetic star. Solar and space physics research focuses on understanding the activity of our Sun and its effects on the Earth and the other planets. It also seeks to understand the physical processes that take place in the area in space around planets, including Earth. These planetary space environments are regions of ionized gas (or plasma) whose motions are subject to the influence of magnetic and electric fields. Solar and space physics seeks finally to explore and understand the interaction of the Sun with our galactic environment; that is, with the gas and dust between our solar system and near-by stars. Within this interstellar cloud, the solar wind, a continuous supersonic outflow of magnetized plasma from the Sun, not only interacts with the Earth and planets, but also inflates an enormous bubble, the heliosphere, whose boundaries lie far beyond the orbit of Pluto and have yet to be explored. It is the entire heliosphere that is the domain of solar and space physics.

The knowledge that space physicists gain through their study of the Sun and solar system plasmas are very often applicable to the study of distant stars and galaxies and are related to laboratory plasma research. And, very importantly, in the particular case of the interactions of solar emissions with the Earth, this research has considerable practical importance for technological systems and for humans in space.

The explosive release of energy from the Sun—solar storms—produces a variety of disturbances in the Earth's space environment. These disturbances, known as "space weather," can adversely affect critical space-based and ground-based technologies and pose potential health hazards to astronauts and to the crews and passengers of aircraft flying polar routes. Understanding solar activity and its effect on the Earth's space environment is key to developing the means of understanding and ultimately mitigating the adverse effects of space weather. Recognition of the importance of achieving this understanding led to the establishment during the past decade of NASA's Living with a Star Program and the NSF-led interagency National Space Weather Program.

Another area in which solar and space physics makes important contributions of practical value is the study of global climate change. Knowledge of both long-and short term variations in the Sun's activity and output is critical to distinguish between natural variability in the Earth's climate and changes that result from human activity.

That, in brief, is the scope and content of the field of solar and space physics. Since the space age began over 40 years ago, we have learned much about the workings of the Sun and the space environments of Earth and the other planets. But there are many questions still to be answered. In late 2000 the National Aeronautics and Space Administration (NASA), the National Science Foundation (NSF), the National Oceanic and Atmospheric Administration (NOAA), the Office of Naval Research, and the Air Force Office of Scientific Research asked the NRC to conduct a comprehensive study of the current status and future directions of U.S. ground- and space-based solar and space physics research programs. To carry out this task, a Survey Committee and five specialized study panels were established. The findings of the study panels were presented to the Survey Committee, which prepared a summary report based on the recommendations of the panels as well as on its own deliberations. Throughout the study process, the study panels and Survey Committee actively sought a broad community consensus with input from the wider solar and space physics community.

The Survey Committee's report, *The Sun to the Earth—and Beyond: A Decadal Research Strategy in Solar and Space Physics,* identifies five broad scientific challenges that define the focus and thrust of solar and space physics research in the decade 2003 through 2013. Further, the report develops specific program priorities that will be needed for the four sponsoring Federal agencies, NASA, NSF, NOAA, and DOD, to meet these challenges. *The Sun to the Earth—and Beyond* also identifies key technologies that must be developed to meet the immediate and projected requirements of solar and space physics research and presents policy recommendations designed to strengthen the solar and space physics research enterprise. Throughout its deliberations, the Survey Committee paid particular attention to the applied aspects of solar and space physics—to the important role that these fields play in a society whose increasing dependence on space-based technologies renders it ever more vulnerable to space weather.

To address the five scientific challenges set forth in *The Sun to the Earth—and Beyond,* the Survey Committee devised an integrated and prioritized set of research initiatives to be implemented in the 2003–2013 time frame. Nearly all of these initiatives are either planned or have been recommended in previous NASA and NSF planning efforts. The recommended initiatives fall within four categories: small programs (<$250 million); moderate programs ($250-$400 million); one large program costing (>$400 million); and "vitality" programs that focus on the infrastructure for solar and space physics research. To arrive at the final recommended set of initiatives, the Committee relied on two criteria—scientific importance and societal benefit. Based on these criteria, the Committee assigned priorities to the recommended initiatives. A complete listing of the Survey Committee's prioritized recommendations, along with a thumbnail description of each program, is given in Table ES.l of the Executive Summary of the report, which is attached to this testimony. Instead of going through the entire list with you, it would be more instructive, I think, for me to outline the five science challenges identified by the Committee and to indicate the role that the four or five highest-priority initiatives will play in addressing those challenges during the coming decade.

Challenge 1: Understanding the Structure and Dynamics of the Sun. During the past decade, thanks to several space missions and new ground-based observations, we have achieved notable advances in our knowledge and understanding of the structure and workings of the Sun's interior and the structure and dynamics of the million-degree solar atmosphere, the corona. However, answers to certain fundamental questions continue to elude us. Why, for example, is the Sun's corona several hundred times hotter than the Sun's surface? How is the solar wind, which expands from the corona, accelerated to the supersonic velocity that is measured in the solar system? How is the very intense magnetic energy that is stored in the Sun released both gradually and explosively? What is origin of the variability ("turbulence") observed in the solar wind and that affects Earth? To answer these questions, the Committee strongly recommends implementation of a NASA *Solar Probe* mission to undertake the first exploration of the regions very near the Sun, which is the birthplace of the heliosphere itself. Measurements made close to the Sun by a Solar Probe will revolutionize our basic understanding of the solar wind. In addition, the Committee gave strong endorsement to the development of an advanced ground-based radio telescope (funded by NSF), the *Frequency-Agile Solar Radiotelescope,* that will provide a revolutionary new tool to study explosive energy release, three-dimensional structure, and magnetic fields in the corona.

Challenge 2: Understanding Heliospheric Structure a11d the Interaction of the Solar Wind with the Local Interstellar Medium. We have acquired a great deal of new knowledge during the last ten years about the inner heliosphere (within the distance of Jupiter's orbit) and its changes over the course of a solar cycle—most of our data has come from the joint NASA/European Space Agency Ulysses mission, which has provided single-point measurements over the poles of the Sun, *i.e.,* out of the plane of the planets. The Survey Committee now recommends the implementation of a *Multispacecraft Heliospheric Mission* that would place four or more spacecraft in orbit about the Sun at different distances and solar longitudes to monitor changes across its entire globe. This mission will provide insight into the connections between solar activity, heliospheric disturbances, and the effects of the solar wind on Earth. This mission will thus represent an important addition to our national space weather effort.

As I noted earlier in my statement, the solar wind inflates a giant bubble known as the heliosphere within the local interstellar medium. The outer reaches of the heliosphere and its boundary with the interstellar medium are among he last unexplored regions of the solar system. An *Interstellar Probe* that could directly sample these regions and move beyond the heliosphere to measure the material in the Sun's galactic environment has long been a dream of the space science community and would be one of the grand scientific enterprises of the early 21st century. Implementing such a mission exceeds our present technological capacity, however, particularly with respect to propulsion and power. The development of nuclear power capabilities in the next decade, as is presently planned by NASA, or the development of solar sails, would greatly facilitate an interstellar probe mission in the future.

Challenge 3: Understanding the behavior of the space environments of Earth and other solar system bodies. Earth's space environment draws energy from its interaction with the supersonic solar wind. This interaction drives the flow of plasma within the magnetosphere—the volume of space controlled by Earth's magnetic field—and leads to the storage and subsequent explosive release of magnetic energy in disturbances known as geomagnetic storms. (The northern and southern auroras are dramatic manifestations of this convulsive energy release.) The transfer of energy from the solar wind to the magnetosphere results from episodic merging of Earth's geomagnetic field with the portion of the Sun's magnetic field that is swept along with the solar wind. This process is known as magnetic reconnection. While the general role of this energy transfer in affecting the Earth's space environment has long been recognized, there are numerous unanswered fundamental questions. Therefore the Survey Committee endorsed as its highest priority in the moderate program category the NASA *Magnetospheric Multiscale (MMS)* mission, a four-spacecraft Solar Terrestrial Probe mission that is designed to study magnetic reconnection inside the magnetosphere and at its boundaries.

Some of the energy extracted from the solar wind is deposited in Earth's high latitude upper atmosphere, thus creating the aurora. To study the effects of magnetosphere disturbances on the structure and dynamics of the upper atmosphere, the Committee has assigned high-priority in the small program category to the NSF's *Advanced Modular Incoherent Scatter Radar (AMISR).* AMISR's ground-based observations at high latitudes will provide essential contextual information for in situ, orbital "snapshot" measurements by spacecraft missions such as the NASA *Geospace Electrodynamics Connections (GEC)* mission, a Solar Terrestrial Probe mission also recommended by the Committee.

The Committee also emphasizes the scientific importance of investigating the complex space environments of other planets. Such investigations serve as rigorous tests of the ideas developed from the study of Earth's own environment while extending our knowledge base to other solar system bodies. Therefore the Committee strongly recommends a NASA *Jupiter Polar Mission (JPM),* which will study energy transfer in a magnetosphere that, unlike Earth's, is powered principally by planetary rotation instead of by the solar wind. All previous missions to Jupiter have flown in or near the equatorial plane, leaving the energetically important polar regions unexplored.

Challenge 4: Understanding the basic physical principles of solar and space plasma physics. The heliosphere is a natural laboratory for the study of plasma physics, and a number of the initiatives proposed by the Committee will lead to advances in understanding fundamental plasma physical processes. For example, as noted above, MMS is specifically designed to study magnetic reconnection, a physical process of fundamental importance in all astrophysical systems, from the Earth to the solar system to our galaxy and beyond. To complement the observational study of such fundamental processes in naturally occurring solar system plasmas, the Survey Committee recommends vigorous support of existing NASA and NSF theory and modeling programs as well as support for new initiatives such as the Coupling Complexity Research Initiative, a joint NASA/NSF theory and modeling program.

Challenge 5: Developing a near-real time predictive capability for the impact of space weather on human activities. Most technologies that fly in space and some that are on Earth's surface are affected severely by the geomagnetic storms whose origins can be traced to the Sun. These events produce subsidiary space weather phenomena, such as the blackouts of high frequency communications and disturbances of satellite transmissions, including those from spacecraft such as the global positioning system. The high energy solar particles can severely disrupt spacecraft operations and present serious radiation hazards to astronauts and to the crews and passengers of aircraft flying on polar routes. In addition to interfering with communications and navigation systems, strong geomagnetic storms often disturb spacecraft orbits because of increased drag in the high altitude atmosphere, and they even have caused electric utility blackouts over wide areas.

Both our understanding of the basic physics of space weather and our appreciation of its importance for human activity has increased considerably during recent years. Much remains to be learned, however, about processes—such as changes in the Earth's radiation—that affect the environment in which many satellites operate; about the variations in the properties of the highest regions of the atmosphere that can adversely affect GPS navigation systems and high frequency radio propagation; and, finally, about the changes that occur on the Sun that ultimately cause the detrimental effects of space weather. The Survey Committee has therefore ranked as its second highest priority in the moderate-program category the *Geospace Missions* of NASA's Living with a Star program. These missions consist of two pairs of spacecraft that will be instrumented to study, respectively, changes in the upper atmosphere and the behavior of the Earth's radiation belts during geomagnetic storms.

Of critical importance both for our efforts to understand and predict space weather and for basic solar and space physics research is information about solar wind conditions prior to their reaching Earth. Such information is currently being provided by the NASA Advanced Composition Explorer (ACE) spacecraft and the NASA Wind satellite. However, both spacecraft are now operating beyond their design lifetimes. The Survey Committee considers it of paramount importance to ensure uninterrupted monitoring of the solar wind and therefore assigned high priority to the implementation of an *Upstream Solar Wind Monitor* as a replacement for ACE and Wind. Given the operational importance of the measurements that such a monitor would provide, the Committee recommends that responsibility for its implementation be transferred from NASA to NOAA. The importance of space weather and of this challenge to national needs is also reflected in the high prioritization that the Committee assigned to the multi-agency National Space Weather Program.

In addition to specific research initiatives to address the five science challenges, the Survey Committee gave careful consideration to the "infrastructural" requirements for a robust solar and space physics research program during the coming decade. *The Sun to the Earth—and Beyond* thus offers a number of recommendations in the following areas: technology development, solar and space physics education, and space research policy and program management, including space weather policy. All of the recommendations in these areas are given in the Executive Summary attached to my statement, so I will summarize only a few of the key ones here.

High-priority areas of *technology development* identified by the Committee include advanced propulsion and power, highly miniaturized spacecraft, advanced spacecraft subsystems, and highly miniaturized sensors of charged and neutral particles and photons. A number of initiatives in these areas are already under way within NASA such as the New Millennium Program, the Sun-Earth Connection and Living With a Star instrument development programs, and the In-Space Propulsion program, and the Committee strongly endorses these initiatives.

The Survey Committee's consideration of issues related to *education* was driven by two main concerns: how to provide a sufficient number of trained scientists to carry out the research program set forth in *The Sun to the Earth—and Beyond* and how solar and space physics can contribute to the development of a scientifically and technologically literate public. Here I will mention only one of the Survey Committee's recommendations—namely, that NSF and NASA jointly establish a program to provide partial salary, start-up funding, and research support for four new faculty members a year for five years in the field of solar and space physics. I was pleased to learn recently that the NSF has already taken significant steps in this direction. Such a program will augment the number of university faculty in solar and space physics and is essential for a strong national solar and space physics research program during the coming decade.

As I noted earlier, in my comments on the space weather challenge, the Survey Committee strongly recommends that NOAA assume responsibility for the implementation of an upstream solar wind monitor. Other Survey Committee recommendations regarding *space weather policy* address measures to facilitate the transition from research to operations, the acquisition and availability of data on solar activity and the geospace environment, and the roles of the public and private sectors in space weather applications. NOAA and the DOD, as the two operational agencies, are primarily responsible for implementing most of the Survey Committee's recommendations in this area.

Finally, the Survey Committee developed a number of *policy recommendations for strengthening the national solar and space physics research program.* For example, a vital space research program depends on cost-effective, reliable, and readily available access to space that meets the requirements of a broad spectrum of missions. The Survey Committee therefore recommends revitalization of NASA's Suborbital Program, the development by NASA of a range of low-cost launch vehicles, and the establishment of procedures of "ride shares" on DOD (and possibly foreign) launch vehicles. The Committee also addressed the impact of export controls on solar and space physics research, which inevitably involves international collaboration, and recommended that the relevant Federal agencies implement procedures to expedite international collaborations involving exchanges of scientific data or information on instrument characteristics.

Let me now conclude my comments with a quote from Marcel Proust: "The real voyage of discovery consists not in seeking new landscapes, but in having new eyes." The solar and space physics research program envisioned by the Survey Committee for the corning decade offers both: visits to new solar system landscapes—the unexplored near-Sun region, Jupiter's polar magnetosphere—and the "new eyes" of observational initiatives such as MMS, FASR, and AMISR and of advanced theoretical and computational initiatives such as the Coupling Complexity Research Initiative, which will enable us to "see" the fundamental connections underlying the complex phenomena captured in our observational data.

ATTACHMENT

EXECUTIVE SUMMARY

The Sun to the Earth—and Beyond

A Decadal Research Strategy in Solar and Space Physics

Solar and Space Physics Survey Committee, Space Studies Board, Division on Engineering and Physical Sciences, National Research Council of The National Academies, The National Academies Press, Washington, D.C., *www.nap.edu*

Science Challenges

The Sun is the source of energy for life on Earth and is the strongest modulator of the human physical environment. In fact, the Sun's influence extends throughout the solar system, both through photons, which provide heat, light, and ionization, and through the continuous outflow of a magnetized, supersonic ionized gas known as the solar wind. The realm of the solar wind, which includes the entire solar system, is called the heliosphere. In the broadest sense, the heliosphere is a vast interconnected system of fast-moving structures, streams, and shock waves that encounter a great variety of planetary and small-body surfaces, atmospheres, and magnetic fields. Somewhere far beyond the orbit of Pluto, the solar wind is finally stopped by its interaction with the interstellar medium, which produces a termination shock wave and, finally, the outer boundary of the heliosphere. This distant region is the final frontier of solar and space physics.

During the 1990s, space physicists peered inside the Sun with Doppler imaging techniques to obtain the first glimpses of mechanisms responsible for the solar magnetic dynamo. Further, they imaged the solar atmosphere from visible to x-ray wavelengths to expose dramatically the complex interaction between the ionized gas and the magnetic field, which drives both the solar wind and energetic solar events such as flares and coronal mass ejections that strongly affect Earth. An 8-year tour of Jupiter's magnetosphere, combined with imaging from the Hubble Space Telescope, has revealed completely new phenomena resident in a regime dominated by planetary rotation, volcanic sources of charged particles, mysteriously pulsating x-ray auroras, and even an embedded satellite magnetosphere.

The response of Earth's magnetosphere to variations in the solar wind was clearly revealed by an international flotilla of more than a dozen spacecraft and by the first neutral-atom and extreme-ultraviolet imaging of energetic particles and cold plasma. At the same time, computer models of the global dynamics of the magnetosphere and of the local microphysics of magnetic reconnection have reached a level of sophistication high enough to enable verifiable predictions.

While the accomplishments of the past decades have answered important questions about the physics of the Sun, the interplanetary medium, and the space environments of Earth and other solar system bodies, they have also highlighted other questions, some of which are long-standing and fundamental. This report organizes these questions in terms of five challenges that are expected to be the focus of scientific investigations during the coming decade and beyond:

- *Challenge 1: Understanding the structure and dynamics of the Sun's interior, the generation of solar magnetic fields, the origin of the solar cycle, the causes of solar activity, and the structure and dynamics of the corona.* Why does solar activity vary in a regular 11-year cycle? Why is the solar corona several hundred times hotter than its underlying visible surface, and how is the supersonic solar wind produced?
- *Challenge 2: Understanding heliospheric structure, the distribution of magnetic fields and matter throughout the solar system, and the interaction of the solar atmosphere with the local interstellar medium.* What is the nature of the interstellar medium, and how does the heliosphere interact with it? How do energetic solar events propagate through the heliosphere?
- *Challenge 3: Understanding the space environments of Earth and other solar system bodies and their dynamical response to external and internal influences.* How does Earth's global space environment respond to solar variations? What are the roles of planetary ionospheres, planetary rotation, and internal plasma sources in the transfer of energy among planetary ionospheres and magnetospheres and the solar wind?
- *Challenge 4: Understanding the basic physical principles manifest in processes observed in solar and space plasmas.* How is magnetic field energy converted to heat and particle kinetic energy in magnetic reconnection events?

- *Challenge 5: Developing near-real-time predictive capability for understanding and quantifying the impact on human activities of dynamical processes at the Sun, in the interplanetary medium, and in Earth's magnetosphere and ionosphere.* What is the probability that specific types of space weather phenomena will occur over periods from hours to days?

An effective response to these challenges will require a carefully crafted program of space- and ground-based observations combined with, and guided by, comprehensive theory and modeling efforts. Success in this endeavor will depend on the ability to perform high-resolution imaging and *in situ* measurements of critical regions of the solar system. In addition to advanced scientific instrumentation, it will be necessary to have affordable constellations of spacecraft, advanced spacecraft power and propulsion systems, and advanced computational resources and techniques.

This report summarizes the state of knowledge about the total heliospheric system, poses key scientific questions for further research, and lays out an integrated research strategy, with prioritized initiatives, for the next decade. The recommended strategy embraces both basic research programs and targeted basic research activities that will enhance knowledge and prediction of space weather effects on Earth. The report emphasizes the importance of understanding the Sun, the heliosphere, and planetary magnetospheres and ionospheres as astrophysical objects and as laboratories for the investigation of fundamental plasma physics phenomena. The recommendations presented in the main report are listed also in this Executive Summary.

An Integrated Research Strategy for Solar and Space Physics

The integrated research strategy proposed by the Solar and Space Physics Survey Committee is based on recommendations from four technical study panels regarding research initiatives in the following subject areas: solar and heliospheric physics, solar wind-magnetosphere interactions, atmosphere-ionosphere-magnetosphere interactions, and theory, computation, and data exploration. Because it was charged with recommending a program that will be feasible and responsible within a realistic resource envelope, the Committee could not adopt all of the panels' recommendations. The committee's final set of recommended initiatives thus represents a prioritized selection from a larger set of initiatives recommended by the study panels. (All of the panel recommendations can be found in the second volume of this report, *The Sun to the Earth—and Beyond: Panel Reports,* in preparation.)

The committee organized the initiatives that it considered into four categories: large programs, moderate programs, small programs, and vitality programs. Moderate and small programs comprise both space missions and ground-based facilities and are defined according to cost, with moderate programs falling in the range from $250 million to $400 million and small programs costing less than $250 million. The Committee considered one large (>$400 million) program, a Solar Probe mission, and gave it high priority for implementation in the decade 2003–2013. The programs in the vitality category are those that relate to the infrastructure for solar and space physics research; they are regarded by the Committee as essential for the health and vigor of the field. The cost estimates used by the Committee for all four categories are based either on the total mission cost or, for level-of-effort programs, on the total cost for the decade 2003–2013. FY 2002 costs are used in each case.

In arriving at a final recommended set of initiatives, the Committee prioritized the selected initiatives according to two criteria—scientific importance and societal benefit. The ranked initiatives are listed and described briefly in Table ES.l. As discussed in Chapter 2, the rankings in Table ES.l, cost estimates, and judgments of technical readiness were then used to arrive at an overall program that could be conducted in the next decade while remaining within a reasonable budget. The committee's recommendations for sequencing of specific missions and initiatives for NASA and NSF are presented in Figures ES.l and ES.2, and Figure ES.3, respectively. Nearly all of the recommended missions and facilities either are already planned or were recommended in previous strategic planning exercises conducted by the National Aeronautics and Space Administration (NASA) and the National Science Foundation (NSF). While the Committee did not find a need to create completely new mission or facility concepts, some existing programs are recommended for revitalization and will require stepwise or ramped funding increases. These programs include NASA's Suborbital Program, its Supporting Research and Technology (SR&T) Program, and the University-Class Explorer (UNEX) Program, as well as guest investigator initiatives for national facilities in the NSF. In the vitality category, new theory and modeling initiatives, notably the Coupling Complexity initiative (discussed in the report of the Panel on Theory, Modeling, and Data Exploration) and the Virtual Sun initiative (discussed in the report of the Panel on the Sun and Heliospheric Physics), are recommended.

Recommendation: The committee recommends the approval and funding of the prioritized programs listed in Table ES.l.

The committee developed its national strategy based on a systems approach to understanding the physics of the coupled solar-heliospheric environment. The existence of ongoing NSF programs and facilities in solar and space physics, of two complementary mission lines in the NASA Sun-Earth Connection program-the Solar Terrestrial Probes (STP) for basic research and Living With a Star (LWS) for targeted basic research—and of applications and operations activities in the National Oceanic and Atmospheric Administration (NOAA) and the Department of Defense (DOD) facilitates such an approach.

As a key first element of its systems-oriented strategy, the Committee endorsed three approved NASA missions: Solar-Band the Solar Terrestrial Relations Observatory (STEREO), both part ofSTP, and the Solar Dynamics Observatory (SDO), part of LWS. Together with ongoing NSF-supported solar physics programs and facilities as well as the start of the Advanced Technology Solar Telescope (ATST), these missions constitute a synergistic approach to the study of the inner heliosphere that will involve coordinated observations of the solar interior and atmosphere and the formation, release, evolution, and propagation of coronal mass ejections toward Earth. Later in the decade covered by the survey, overlapping investigations by the SDO (LWS), the ATST, and Magnetospheric Multiscale (MMS) (part of STP), together with the start of the Frequency-Agile Solar Radio (FASR) telescope, will form the intellectual basis for a comprehensive study of magnetic reconnection in the dense plasma of the solar atmosphere and the tenuous plasmas of geospace.

The committee's ranking of the Geospace Electrodynamic Connections (GEC) (STP) and Geospace Network (LWS) missions acknowledges the importance of studying Earth's ionosphere and inner magnetosphere as a coupled system. Together with a ramping up of the launch opportunities in the Suborbital Program and the implementation of both the Advanced Modular Incoherent Scatter Radar (AMISR) and the Small Instrument Distributed Ground-Based Network, these missions will provide a unique opportunity to study the local electrodynamics of the ionosphere down to altitudes where energy is transferred between the magnetosphere and the atmosphere, while simultaneously investigating the global dynamics of the ionosphere and radiation belts. The implementation of the Ll Monitor (NOAA) and of the vitality programs will be essential to the success of this systems approach to basic and targeted basic research. Later on in the Committee's recommended program, concurrent operations of a Multi-Spacecraft Heliospheric mission (LWS), Stereo Magnetospheric Imager (SMI) (STP), and Magnetosphere Constellation (MagCon) (STP) will provide opportunities for a coordinated approach to understanding the large-scale dynamics of the inner heliosphere and Earth's magnetosphere (again with strong contributions from the ongoing and new NSF initiatives).

To understand the genesis of the heliospheric system it is necessary to determine the mechanisms by which the solar corona is heated and the solar wind is accelerated and to understand how the solar wind evolves in the innermost heliosphere. These objectives will be addressed by a Solar Probe mission. Because of the importance of these objectives for the overall understanding of the solar-heliosphere system, as well as of other stellar systems, a Solar Probe mission[1] should be imple-

[1] The Solar Probe mission recommended by the Committee is a generic mission to study the heating and acceleration of the solar wind through measurements as close to the surface of the Sun as possible. The previously announced Solar Probe mission was cancelled for budgetary reasons. A new concept study for a Solar Probe was begun in January 2002 and is currently under way. This new study builds on the earlier science definition team report to NASA and is examining, among other issues, the power and communications technologies (including radioisotope thermal generators needed to enable such a mission within a realistic cost cap). The measurement capabilities being considered in the study comprise both instrumentation for the *in situ* measurement of plasmas, magnetic fields, and waves and a remote-sensing package, including magnetograph, Doppler, EUV, and coronal imaging instruments. The committee notes that the Panel on the Sun and Heliospheric Physics recommends as its highest-priority new initiative a Solar Probe mission whose primary objective is to make *in situ* measurements of the innermost heliosphere. The panel does not consider remote sensing "a top priority on a first mission to the near-Sun region," although it does allow as a possible secondary objective remote sensing of the photospheric magnetic field in the polar regions. (See the Solar Probe discussion in the report of the Panel on Sun and Heliospheric Physics, which is published in *The Sun to the Earth-and Beyond: Panel Reports,* in preparation.) While accepting the panel's assessment of the critical importance of the *in situ* measurements for understanding coronal heating and solar wind acceleration, the Committee does not wish to rule out the possibility that some additional remote-sensing capabilities, beyond the remote-sensing experiment to measure the polar photospheric magnetic field envisioned by the panel, can be accommodated on a Solar Probe within the cost cap set by the Committee.

mented as soon as possible within the coming decade. The Solar Probe measurements will be complemented by correlative observations from such initiatives as Solar Orbiter, SDO, ATST, and FASR.

Similarly, because of the importance of comparative magnetospheric studies for advancing the understanding basic magnetospheric processes, the Committee has assigned high priority to a Jupiter Polar Mission (JPM), a space physics mission to study high-latitude electrodynamic coupling at Jupiter. Such a mission will provide both a means of testing and refining theoretical concepts developed largely in studies of the terrestrial magnetosphere and a means of studying *in situ* the electromagnetic redistribution of angular momentum in a rapidly rotating system, with results relevant to such astrophysical questions as the formation of protostars.

Technology Development

Technology development is required in several critical areas if a number of the future science objectives of solar and space physics are to be accomplished.

Traveling to the planets and beyond. New propulsion technologies are needed to rapidly propel spacecraft to the outer fringes of the solar system and into the local interstellar medium. Also needed are power systems to support future deep space missions.

Recommendation: NASA should assign high priority to the development of advanced propulsion and power technologies required for the exploration of the outer planets, the inner and outer heliosphere, and the local interstellar medium.

Advanced spacecraft systems. Highly miniaturized spacecraft and advanced spacecraft subsystems will be critical for a number of high-priority future missions and programs in solar and space physics.

Recommendation: NASA should continue to give high priority to the development and testing of advanced spacecraft technologies through such programs as the New Millennium Program and its advanced technology program.

Advanced science instrumentation. Highly miniaturized sensors of charged and neutral particles and photons will be essential elements of instruments for new solar and space physics missions.

Recommendation: NASA should continue to assign high priority, through its recently established new instrument development programs, to supporting the development of advanced instrumentation for solar and space physics missions and programs.

Gathering and assimilating data from multiple platforms. Future flight missions include multipoint measurements to resolve spatial and temporal scales that dominate the physical processes that operate in solar system plasmas.

Recommendation: NASA should accelerate the development of command-and-control and data acquisition technologies for constellation missions.

Modeling the space environment. Primarily because of the lack of a sufficient number of measurements, it has not been necessary until quite recently for the solar and space physics community to address data assimilation issues. However, it is anticipated that within 10 years vast arrays of data sets will be available for assimilation into models.

Recommendation: Existing NOAA and DOD facilities should be expanded to accommodate the large-scale integration of space-based and ground-based data sets into physics-based models of the geospace environment.

Observing geospace from Earth. The severe terrestrial environments of temperature, moisture, and wildly varying solar insolation have posed serious reliability problems for arrays of ground-based sensor systems that are critical for solar and space physics studies.

Recommendation: The relevant program offices in the NSF should support comprehensive new approaches to the design and maintenance of ground-based, distributed instrument networks, with proper regard for the severe environments in which they must operate.

Observing the Sun at high spatial resolution. Recent breakthroughs in adaptive optics have eliminated the major technical impediments to making solar observations with sufficient resolution to measure the pressure scale height, the photon mean free path, and the fundamental magnetic structure size.

Recommendation: The National Science Foundation should continue to fund the technology development program for the Advanced Technology Solar Telescope.

Connections Between Solar and Space Physics and Other Disciplines

The fully or partially ionized plasmas that are the central focus of solar and space physics are related on a fundamental level to laboratory plasma physics, which directly investigates basic plasma physical processes, and to astrophysics, a discipline that relies heavily on understanding the physics unique to the plasma state. Moreover, there are numerous points of contact between space physics and atmospheric science, particularly in the area of aeronomy. Knowledge of the properties of atoms and molecules is critical for understanding a number of magnetospheric, ionospheric, solar, and heliospheric processes. Understanding developed in one of these fields is thus in principle applicable to the others, and productive cross-fertilization between disciplines has occurred in a number of instances.

Recommendation: In collaboration with other interested agencies, NSF and NASA should take the lead in initiating a program in laboratory plasma science that can provide new understanding of fundamental processes important to solar and space physics. The establishment of such a laboratory initiative was previously recommended in the 1995 NRC report ***Plasma Science.***

Recommendation: NSF and NASA should take the lead and other interested agencies should collaborate in supporting, via the proposal and funding processes, increased interactions between solar and space physics research and allied fields such as atomic and molecular physics, laboratory fusion physics, atmospheric science, and astrophysics.

Solar and Space Environment Effects on Technology and Society

The space environment of the Sun-Earth system can have deleterious effects on numerous technologies that are used by modem-day society. Understanding this environment is essential for the successful design, implementation, and operation ofthese technologies.

National Space Weather Program. A number of activities are under way in the United States to better understand and mitigate the effects of solar activity and the space environment on important technological systems. The mid-1990s saw the creation of the National Space Weather Program (NSWP), an interagency program whose goal is "to achieve, within a ten year period, an active, synergistic, interagency system to provide timely, accurate, and reliable space environment observations, specifications, and forecasts." In 1999, NASA initiated an important complementary program, Living With a Star (LWS), which over the next decade and beyond will carry out targeted basic research on space weather. Crucial components of the national space weather effort continue to be provided by the operational programs of the Department of Defense and NOAA. Moreover, in addition to governmental activities, a number of private companies have, over the last decade, become involved in developing and providing space weather products.

Monitoring the solar-terrestrial environment. Numerous research instruments and observations are required to provide the basis for modeling interactions between the solar-terrestrial environment and technical systems and for making sound technical design decisions that take such interactions into account Transitioning of programs and/or their acquisition platforms or instruments into operational use requires strong and effective coordination efforts among agencies. Imaging of the Sun and of geospace will play central roles in operational space forecasting in the future.

Recommendation: The involved agencies, in consultation with the research community, should jointly assess instrument facilities that contribute key data to space weather models and operational programs, both public and private, and determine a strategy to maintain them or should work to establish facilities necessary for operational use. NOAA and DOD should lead this assessment and should report on it publicly.

Recommendation: NOAA should assume responsibility for the continuance of space-based measurements such as solar wind data from the Ll location as well as near Earth and for distribution of the data for operational use.[2]

Recommendation: NASA and NOAA should initiate the necessary planning to transition solar and geospace imaging instrumentation into operational programs for the public and private sectors.

[2] For example, a NOAA-Air Force program is producing operational solar X-ray data. The Geostationary Operational Environmental Satellite (GOES) Solar X-ray Imager (SXI), first deployed on GOES–M, took its first image on September 7, 200I. The SXI instrument is designed to obtain a continuous sequence of coronal X-ray images at a 1-minute cadence. These images are being used by NOAA's Space Environment Center and the broader community to monitor solar activity for its effects on Earth's upper atmosphere and the near-space environment.

Transition from research to operations. Means must be established for transitioning new knowledge into those arenas where it is needed for design and operational purposes. Creative and cutting-edge research in modeling the solar-terrestrial environment is under way. Under the auspices of the NSWP, models that are thought to be potentially useful for space weather applications can be submitted to the Community Coordinated Modeling Center (CCMC, currently located at the NASA Goddard Space Flight Center) for testing and validation. Following validation, the models can be turned over to either the U.S. Air Force or the NOAA Rapid Prototyping Center (RPC), where the models are used for the objectives of the individual agencies. In many instances, the validation of research products and models is different in the private and public sectors, with publicly funded research models and system-impact products usually being placed in an operational setting with only limited validation.

Recommendation: The relevant Federal agencies should establish an overall verification and validation program for all publicly funded models and system-impact products before they become operational.

Recommendation: The operational Federal agencies, NOAA and DOD, should establish procedures to identify and prioritize operational needs, and these needs should determine which model types are selected for transitioning by the Community Coordinated Modeling Center and the Rapid Prototyping Centers. After the needs have been prioritized, procedures should be established to determine which of the competing models, public or private, is best suited for a particular operational requirement.

Data acquisition and availability. The transfer functions that relate a given solar observation to the effects on a specific technological system are largely unknown. During the coming decade, gigabytes of data could be available every day for incorporation into physics-based data assimilation models of the solar-terrestrial environment and into system-impact codes for space weather forecasting and mitigation purposes. DOD generally uses data that it owns and only recently has begun to use data from other agencies and institutions, so that not many data sets are available for use by the publicly funded or commercial vendors who design products for DOD. Engineers typically are interested in space climate, not space weather. Needed are long-term averages, the uncertainties in these averages, and values for the extremes in key space weather parameters. The engineering goal is to design systems that are as resistant as possible to the effects of space weather.

Recommendation: DOD and NOAA should be the lead agencies in acquiring all the data sets needed for accurate specification and forecast modeling, including data from the international community. Because it is extremely important to have real-time data, both space-and ground based, for predictive purposes, NOAA and DOD should invest in new ways to acquire real-time data from all of the ground-and space-based sources available to them. All data acquired should contain error estimates, which are required by data assimilation models.

Recommendation: A new, centralized database of extreme space weather conditions should be created that covers as many of the relevant space weather parameters as possible.

Public and private sectors in space weather applications. To date, the largest efforts to understand the solar-terrestrial environment and apply the knowledge for practical purposes have been mostly publicly funded through government research organizations, universities, and some industries. Recently some private companies both large and small have been devoting their own resources to the development and sale of specialized products that address the design and operation of certain technical systems that can be affected by the solar-terrestrial environment. The private efforts often use publicly supported assets (such as spacecraft data) as well as proprietary instrumentation and models. A number of the private efforts use proprietary system knowledge to guide their choices of research directions. Policies on such matters as data rights, inteilectual property rights and responsibilities, and benchmarking criteria can be quite different for private efforts and publicly supported ones, including those of universities. Thus, transitioning knowledge and models from one sector to another can be fraught with complications and requires continued attention and discussion by ail interested entities.

Recommendation: Clear policies describing government and industry roles, rights, and responsibilities should be developed and published by all agencies and interested commercial enterprises involved in space weather activities in order to optimize the benefits of the national investments, public and private, that are being made.

Education and Public Outreach

The committee's consideration of issues related to education and outreach was focused in two areas:

- How to ensure a sufficient number of future scientists in solar and space physics; and
- How the solar and space physics community can contribute to national initiatives in science and technology education.

Solar and space physics in colleges and universities. Because of its relatively short history, solar and space physics (SSP) appears only adventitiously in formal instructional programs, and an appreciation of its importance is often lacking in current undergraduate curricula. If SSP is to have a healthy presence in academia, additional faculty members would be needed to guide student research (both undergraduate and graduate), to teach SSP graduate programs, and to integrate topics in SSP into basic physics and astronomy classes.

Recommendation: The NSF and NASA should jointly establish a program of "bridged positions" that provides (through a competitive process) partial salary, start-up funding, and research support for four new faculty members every year for 5 years.

Distance education. Education in SSP during the academic year could be considerably enhanced if the latest advances in information technology are exploited to provide distance learning for both graduate students and postdoctoral researchers. This would substantially increase the educational value of the expertise that currently resides at a limited number of institutions.

Recommendation: The NSF and NASA should jointly support an initiative that provides increased opportunities for distance education in solar and space physics.

Undergraduate research opportunities and undergraduate instruction. NSF support for the Research Experiences for Undergraduates (REU) program has been valuable for encouraging undergraduates in the solar and space physics research area.

Recommendation: NASA should institute a specific program for the support of undergraduate research in solar and space physics at colleges and universities. The program should have the flexibility to support such research as either a supplement to existing grants or a stand-alone grant program.

Recommendation: Over the next decade NASA and the NSF should fund several resource development groups to develop solar and space physics educational resources (especially at the undergraduate level), to disseminate those resources, and to provide training for educators and scientists in the effective use of such resources.

Strengthening the Solar and Space Physics Research Enterprise

Advances in understanding in solar and space physics will require strengthening a number ofthe infrastructural aspects of the Nation's solar and space physics program. The committee has identified several that depend on effective program management and policy actions for their success: (1) development of a stronger research community, (2) cost-effective use of existing resources, (3) ensuring cost-effective and reliable access to space, (4) improving interagency cooperation and coordination, and (5) facilitating international partnerships.

Strengthening the solar and space physics research community. A diverse and high-quality community of research institutions has contributed to solar and space physics research over the years. The central role of the universities as research sites requires enhancement, strengthening, and stability.

Recommendation: NASA should undertake an independent outside review of its existing policies and approaches regarding the support of solar and space physics research in academic institutions, with the objective of enabling the Nation's colleges and universities to be stronger contributors to this research field.

Recommendation: NSF-funded national facilities for solar and space physics research should have resources allocated so that the facilities can be widely available to outside users.

Cost-effective use of existing resources. Optimal return in solar and space physics is obtained not only through the judicious funding and management of new assets, but also through the maintenance and upgrading, funding, and management of existing facilities.

Recommendation: The NSF and NASA should give all possible consideration to capitalizing on existing ground-and space-based assets as the goals of new research programs are defined.

Access to space. The continuing vitality of the Nation's space research program is strongly dependent on having cost-effective, reliable, and readily available access to space that meets the requirements of a broad spectrum of diverse missions. The solar and space physics research community is especially dependent on the availability of a wide range of suborbital and orbital flight capabilities to carry out cutting-edge science programs, to validate new instruments, and to train new scientists. Suborbital flight opportunities are very important for advancing many key aspects of future solar and space physics research objectives and for enabling the contributions that such opportunities make to education.

Recommendation: NASA should revitalize the Suborbital Program to bring flight opportunities back to previous levels.

Low-cost launch vehicles with a wide spectrum of capabilities are critically important for the next generation of solar and space physics research, as delineated in this report.

Recommendations:

1. *NASA should aggressively support the engineering research and development of a range oflow-cost vehicles capable oflaunching payloads for scientific research.*
2. *NASA should develop a memorandum of understanding with DOD that would delineate a formal procedure for identifying in advance opportunities for piggybacking civilian spacecraft on certain Air Force missions.*
3. *NASA should explore the feasibility of piggybacking on appropriate foreign scientific launches.*

The comparative study ofplanetary ionospheres and magnetospheres is a central theme of solar and space physics research.

Recommendation: The scientific objectives of the NASA Discovery Program should be expanded to include those frontier space plasma physics research subjects that cannot be accommodated by other spacecraft opportunities.

The principal investigator (PI) model that has been used for numerous Explorer missions has been highly successful. Strategic missions such as those under consideration for the STP and LWS programs can benefit from emulating some of the management approach and structure of the Explorer missions. The solar and space physics field is especially appropriate for placing many of its major science objectives in charge of a PI.

Recommendation: NASA should (1) place as much responsibility as possible in the hands of the principal investigator, (2) define the mission rules clearly at the beginning, and (3) establish levels of responsibility and mission rules within NASA that are tailored to the particular mission and to its scope and complexity.

Recommendation: The NASA official who is designated as the program manager for a given project should be the sole NASA contact for the principal investigator. One important task of the NASA official would be to make sure that rules applicable to large-scale, complex programs are not being inappropriately applied, thereby producing cost growth for small programs.

Interagency cooperation and coordination. Interagency coordination over the years has yielded greater science returns than could be expected from single-agency activities. In the future, a research initiative at one agency could trigger a window of opportunity for a research initiative at another agency. Such an eventuality would leverage the resources contributed by each agency.

Recommendation: The principal agencies involved in solar and space physics research-NASA, NSF, NOAA, and DOD—should devise and implement a management process that will ensure a high level of coordination in the field and that will disseminate the results of such a coordinated effort—including data, research opportunities, and related matters—widely and frequently to the research community.

Recommendation: For space-weather-related applications, increased attention should be devoted to coordinating NASA, NOAA, NSF, and DOD research findings, models, and instrumentation so that new developments can quickly be incorporated into the operational and applications programs of NOAA and DOD.

International partnerships. The geophysical sciences—in particular, solar and space physics—address questions of global scope and inevitably require international participation for their success. Collaborative research with other nations allows the United States to obtain data from other geographical regions that are necessary to determine the global distributions of space processes. Studies in space weather cannot be successful without strong participation from colleagues in other countries and their research capabilities and assets, in space and on the ground.

Recommendation: To expedite international collaborations that involve exchanges of scientific data or information on instrument characteristics, the Federal Government, especially the State Department and NASA, should implement clearly defined procedures that recognize that all major scientific space missions have components that include participants from universities, private companies, and nonprofit organizations.

Table ES.I—Priority Order and Brief Descriptions of the Recommended Programs in Solar and Space Physics

Type of Program	Rank	Program	Description
Large	1	Solar Probe	Spacecraft to study the heating and acceleration of the solar wind through *in situ* measurements and some remote-sensing observations during one or more passes through the innermost region of the heliosphere (from 0.3 AU to as close as 3 solar radii above the Sun's surface).
Moderate	1	Magnetospheric Multiscale	Four-spacecraft cluster to investigate magnetic reconnection, particle acceleration, and turbulence in magnetospheric boundary regions.
	2	Geospace Network	Two radiation-belt mapping spacecraft and two ionospheric mapping spacecraft to determine the global response of geospace to solar storms.
	3	Jupiter Polar Mission	Polar-orbiting spacecraft to image the aurora, determine the electrodynamic properties of the lo flux tube, and identify magnetosphere-ionosphere coupling processes.
	4	Multispacecraft Heliospheric Mission	Four or more spacecraft with large separations in the ecliptic plane to determine the spatial structure and temporal evolution of CMEs and other solar-wind disturbances in the inner heliosphere.
	5	Geospace Electrodynamic Connections	Three to four spacecraft with propulsion for low-altitude excursions to investigate the coupling among the magnetosphere, the ionosphere, and the upper atmosphere.
	6	Suborbital Program	Sounding rockets, balloons, and aircraft to perform targeted studies of solar and space physics phenomena with advanced instrumentation.
	7	Magnetospheric Constellation	Fifty to a hundred nanosatellites to create dynamic images of magnetic fields and charged particles in the near magnetic tail of Earth.
	8	Solar Wind Sentinels	Three spacecraft with solar sails positioned at 0.98 AU to provide earlier warning than Ll monitors and to measure the spatial and temporal structure of CMEs, shocks, and solar wind streams.
	9	Stereo Magnetospheric Imager	Two spacecraft providing stereo imaging of the plasmasphere, ring current, and radiation belts, along with multispectral imaging of the aurora.
Small	1	Frequency-Agile Solar Radio Telescope	Wide frequency-range (0.3–30 GHz) radio telescope for imaging of solar features from a few hundred kilometers above the visible surface to high in the corona.
	2	Advanced Modular Incoherent Scatter Radar	Movable incoherent scatter radar with supporting optical and other ground-based instruments for continuous measurements of magnetosphere-ionosphere interactions.
	3	L1 Monitor	Continuation of solar-wind and interplanetary magnetic field monitoring for support of Earth-orbiting space physics missions. Recommended for implementation by NOAA.
	4	Solar Orbiter	U.S. instrument contributions to ESA spacecraft that . periodically corotates with the Sun at 45 solar radii to investigate the magnetic structure and evolution of the solar corona.
	5	Small Instrument Distributed Ground-Based Network	NSF program to provide global-scale ionospheric and upper atmospheric measurements for input to global physics-based models.
	6	UNEX	Revitalization of University-Class Explorer program for more frequent access to space for focused research projects.
Vitality	1	NASA Supporting Research and Technology	NASA research and analysis program.

Table ES.I—Priority Order and Brief Descriptions of the Recommended Programs in Solar and Space Physics—Continued

Type of Program	Rank	Program	Description
	2	National Space Weather Program	Multiagency program led by the NSF to support focused activities that will improve scientific understanding of geospace in order to provide better specifications and predictions.
	3	Coupling Complexity	NASA/NSF theory and modeling program to address multiprocess coupling, nonlinearity, and multiscale and multiregional feedback.
	4	Solar and Space Physics Information System	Multiagency program for integration of multiple data sets and models in a system accessible by the entire solar and space physics community.
	5	Guest Investigator Program	NASA program for broadening the participation of solar and space physicists in space missions.
	6	Sun-Earth Connection Theory and LWS Data Analysis, Theory, and Modeling Programs	NASA programs to provide long-term support to critical-mass groups involved in specific areas of basic and targeted basic research.
	7	Virtual Sun	Multiagency program to provide a systems-oriented approach to theory, modeling, and simulation that will ultimately provide continuous models from the solar interior to the outer helios here.

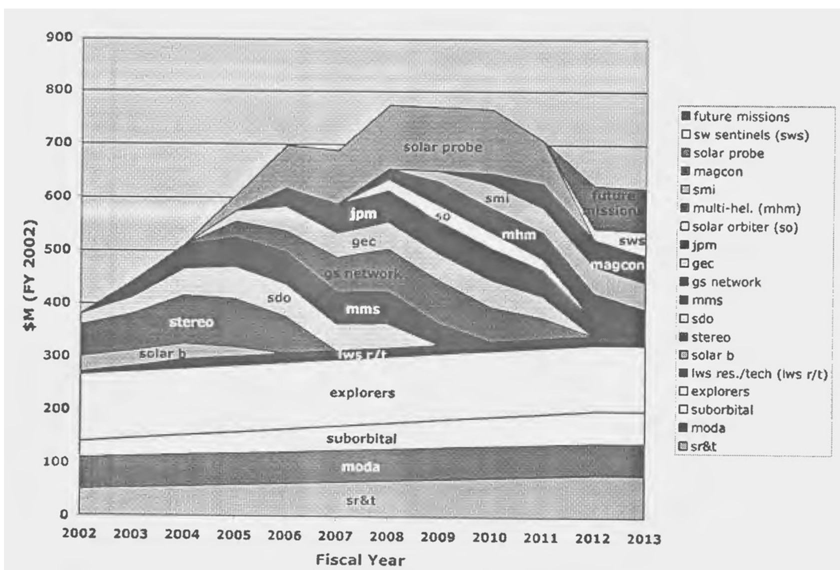

FIGURE ES.1 Recommended phasing of the highest-priority NASA missions, assuming an early implementation of a Solar Probe mission. Solar Probe was the Survey Committee's highest priority in the large mission category, and the Committee recommends its implementation as soon as possible. However, the projected cost of Solar Probe is too high to fit within plausible budget and mission profiles for NASA's Sun-Earth Connection (SEC) Division. Thus, as shown in this figure, an early start for Solar Probe would require funding above the currently estimated SEC budget of $650 million per year for Fiscal Years 2006 and beyond. Note that MO&DA costs for all missions are included in the MO&DA budget wedge.

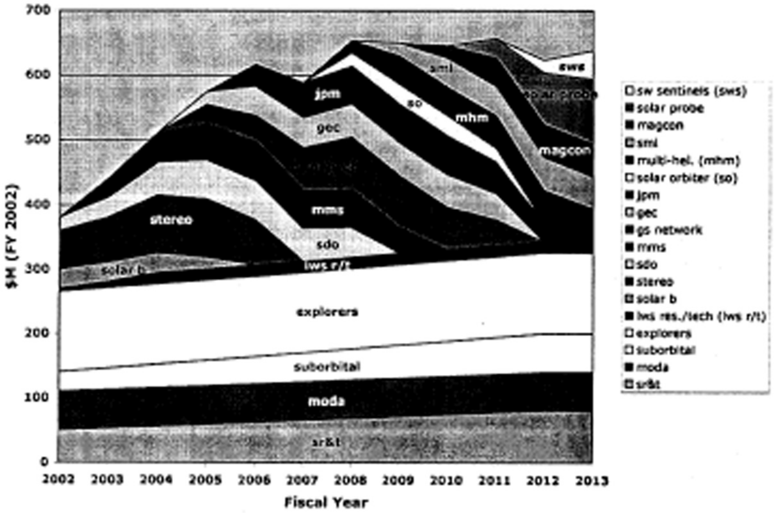

FIGURE ES.2 Recommended phasing of the highest-priority NASA missions if budget augmentation for Solar Probe is not obtained. MO&DA costs for all missions are included in the MO&DA budget wedge.

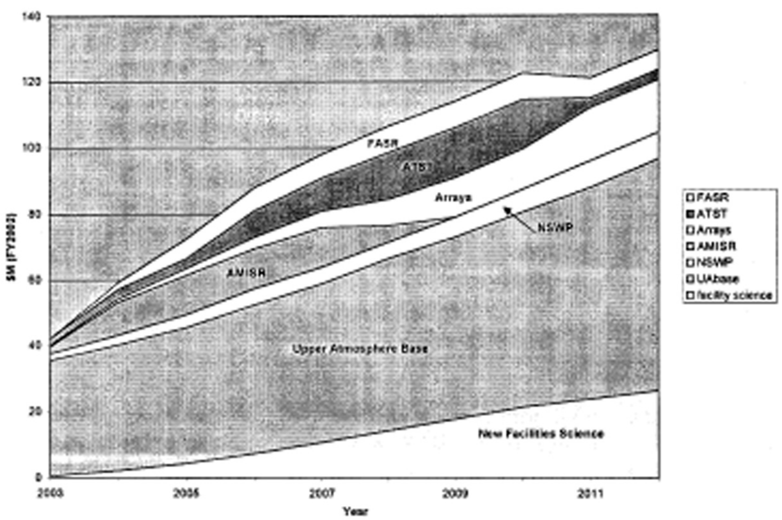

FIGURE ES.3 Recommended phasing of major new and enhanced NSF initiatives. The budget wedge for “New Facilities Science” refers to support for “guest investigator” and related programs that will maximize the science return of new ground facilities to the scientific community. Funding for New Facilities Science is budgeted at approximately I 0 percent of the aggregate cost for new NSF facilities.

Senator BROWNBACK. Thank you. And that was an exciting presentation by all the panel members.

I think what we’ll do is run a clock here at 7 minutes, back and forth. Senator Nelson, you had another engagement that you need to get to?

Senator NELSON. Go ahead.

Senator BROWNBACK. Do you want to go first with your questions? Would that let you get to your engagement then? I'll let you do that, if you'd like.

Senator NELSON. Well, that is very kind of you, Mr. Chairman.

I'm curious, Dr. Belton, do you think, from a mathematical probability, that there is life out in the universe?

Dr. BELTON. My conviction is that life exists elsewhere than the Earth, yes.

Senator NELSON. I agree.

Do you think in our lifetime we will see some evidence of that?

Dr. BELTON. Yes. In fact, this is one of the things that really excited us in doing this study. With the Mars-sample return now on the horizon—we don't think it'll happen in this coming decade, but we're going to be getting ready for it—when that sample comes back, providing it's the right sample, Orlando—when that sample comes back, I think we'll learn an awful lot.

Senator NELSON. Well, if there's life there and there's life here, what is our destiny?

Dr. BELTON. Well, that's a much more complex question, and it has to do with limitations to the length of time that species exist on the Earth. And as you—I'm not a paleontologist, but I can read this material—and as you know, species live and die on this Earth, and it seems that one of the reasons why that might happen—not proven, but might happen—is the result of cosmic impacts, an impact with the Earth by some sizable object.

And so if you kind of look at all the things that could happen to human civilization and human species, that's probably the one that has the shortest time scale associated with it. Therefore, I think it should be taken seriously. But that was not something that I wanted to talk about in my remarks.

Senator NELSON. Given what you've stated—and I share most of that—if you were designing a goal, a mission, for NASA, would you want to go back to the moon with a lunar colony? I'm talking about the manned, human kind of mission. Would you want to do the bold strike to Mars? Give us some of your thoughts.

Dr. BELTON. Well, it so happens that I've just written a letter, maybe 2 months ago, to Dr. Martin, who is the space architect at NASA, on this very subject about my own personal views. These are not community views, by any means. But, basically, I feel that, in spite of the very sad events that occurred, these sad events also probably lead to an opportunity to reassess and set a new goal.

And so in my letter to Dr. Martin, which was signed by some 20 others of my colleagues, we tried to—made the case that human spaceflight should move out beyond low-Earth orbit into the near-Earth space and then, as an intermediate goal, toward Mars exploration, which certainly is something that the community, in general, is interested in as an intermediate goal; that they should look toward the near-Earth objects that come very close to the Earth all the time, and with the thought in mind that we should learn, over some protracted period, how to manage this problem of collisions that will happen sooner or later.

Senator NELSON. For any of you, if we venture to Mars, in order to be not fried by solar flares there has got to be some kind of shielding. Can any of you comment on that? Can we create our own magnetic field around a spacecraft?

Dr. Lanzerotti?

Dr. LANZEROTTI. That would be very difficult to do, the size of the magnetic field that you might need for the size of the spacecraft that you have.

The solar flare problem is a really tricky one. As you well remember and know, the solar event that occurred in August 1972, between the last two Apollo flights, would have been fatal for any astronauts on the moon at the time, or in transit to the moon. It just happened that Apollo 16 and Apollo 17 bracketed that August 1972 event.

So we need to know—and we did not know very much about predicting solar events at that time. We know a little bit more now, but we don't know a lot more now. That's why our committee devoted an entire chapter in our Decadal Report to this whole issue of applications and the understanding of the sun and the Earth's space environment, in order to get a much better predictive capability.

The new X-ray imager on the NOAA GOES spacecraft is a step in that direction, but it's not the final word, by any means. The solar probe, the advanced modular—or the Frequency Agile Solar Radio Telescope and other things that we have going on will provide a lot more data, then we can do more models and begin to understand the sun a lot better than we know now. But we're a long way from very accurate prediction.

But it may be able to progress in parallel with a human effort toward Mars in the future. Perhaps, if I might step in Mike's arena here for a moment, I guess personally I think perhaps going back to the moon and gaining some understanding of that might be a better way, particularly in lieu of needing to understand the space environment and the solar activity at the same time.

Mr. WITHEE. Senator, if I may add? Your question highlights the need for 24/7 operational space-weather forecast and warning service, because the events such as has been described there need to be not looked at just during the 40-hour work-week, but 7 days a week and 24 hours a day. NOAA has such a service. We're trying to get better at it. We're working with the Air Force to try to provide these kind of services for our own Earth, but also we have extended those services out into space to support missions such as you're talking about.

Thank you.

Senator NELSON. For any of you, do you think that there was life, or still is, on Mars?

Mr. FIGUEROA. There is evidence that the conditions in—or habitable environments might have existed in the past and may exist today. Whether there is life on any of those, it remains an open question, but certainly the conditions for life to have emerged—and knowing how tenacious and persistent it can be on Earth, as we have learned over the last couple of decades—the probabilities are in the favor of that being a positive answer, but we do not know, and that is what the program is designed to do. A difficult question to answer directly, but we're on a path that we hope gets us there.

Senator NELSON. Thank you.

Senator BROWNBACK. I enjoyed the exchange. This is very informative from all sources.

Senator NELSON. Well, as you can see, I get excited about this, and——

Senator BROWNBACK. I do, too.

[Laughter.]

Senator NELSON. By the way, I was quite intrigued by Dr. Belton's question, "Where did we come from?" And you know how people get all tangled up in their knickers over that. I've never seen a conflict between the first chapter of Genesis and the creation of the universe, as we've seen it, because I don't happen to define that the Lord would define a day as 24 hours. So I'm very intrigued with your question, Dr. Belton.

Senator BROWNBACK. I've had an eminent theologian say to me that Genesis was written—this was God describing how He created; not man describing how God did it. So describe it in different terms in different ways.

I've got a number of questions for different panel members, but I want to followup, Dr. Lanzerotti, with your comment, because at the heart of what we're trying to strike at here is—we seem to be stuck mentally in low-space orbit—mentally—that we just kind of—where are we going with the space program? And where should we be going, and why? And then would the public support this? You seemed to articulate a point of view about whether we should go back to the moon to understand space better. Would you go ahead and finish that thought?

Dr. LANZEROTTI. Mr. Chair, I'm not really prepared to expand further on a comment that I'm about to make. About 12 years ago, there was a major study, which was chaired by Norman Augustine. It resulted in an Augustine Report which talked about the future of some of our national space endeavor, and delved at quite some length into the directions of human spaceflight, as well as the robotic spaceflight, which I principally addressed in my testimony here.

And the Augustine Report made some very cogent recommendations and statements and had some discussions of the need for defining our directions of human spaceflight, and discussed, therein, the need for defining the longer term vision for humans in space, both humans in space and in the context of a robotic program. And I'm, unfortunately, not prepared to discuss that in any depth.

My personal opinion is the one that I stated. I think that going back to the moon would be a very beneficial enterprise for the Nation. And that's discussed at some extent in the Augustine Panel, and I would recommend that to your Committee, Subcommittee, and to the staff, to review some of the discussions in there.

Going back to the moon would be not just an opportunity to understand better humans on another body, but would also possibly lead to a base of some scientific measurement capabilities, both in the Earth's magnetosphere, as well as perhaps some astronomical measurements made from the moon—radio astronomy, optical measurements. And people in those communities are investigating those kinds of opportunities.

It seems to me like that might be a beneficial and profitable direction as we go out into the solar system further.

Senator BROWNBACK. Could we discover more scientific exploration and useful information from going back to the moon than investing in continued flights into low-space orbit—or low-Earth orbit?

Dr. LANZEROTTI. I think I'm—I certainly have personal views, but I think I'm beginning to get beyond my area of expertise, and I would like to not answer that directly, if I might.

Senator BROWNBACK. Would anybody care to respond to that?

[No response.]

Senator BROWNBACK. Your silence is deafening.

[Laughter.]

Senator BROWNBACK. Dr. Belton, I want to explore with you a little bit further. You support the notion of near-Earth objects. Near-Earth space travel is where you think that we should be going, I believe was the term that you used in the—answered to the question. What do you define as near-Earth space travel?

Dr. BELTON. Basically, from here to the moon. I don't have any disagreement with what Dr. Lanzerotti said about going back to the moon with the idea of setting up telescopes there or making measurements of particles and fields, so forth. I think that's been well studied in the past, and there are many positive aspects to that.

But it seems to me, also, that this problem of collisions, even though they're very, very rare, is something that we do have to take seriously. Somebody has to start this business, within human society, of taking care of this problem. We don't know whether it's going to happen tomorrow or whether it's going to happen a thousand years or ten-thousand years from now. It's a totally random process. We've started to look. NASA has a very strong program, a Spaceguard Program, which was mandated by Congress a few years ago, looking for the very large objects that could cause global catastrophes.

But there are also lots and lots—in fact, thousands of times more—smaller objects that we don't know where they are that have something like a one-percent chance in the lifetime of the population of this country of hitting this country with a—it could release the amount of energy equivalent to a ten-megaton bomb, for example. This is a 50-meter object, like the one that collided over Russia in 1908, destroyed 2,000-square kilometers of forest. Thank goodness it was forest. Those things, again, are rare. One percent in a hundred years, roughly. One-percent chance in a hundred years.

But it's going to take the order of 50 to 100 years just to learn how to do something about these things, and it may well be that in learning how to do something about them, we may have to, in fact, employ human participation in space. It's not sure. It's not been studied. It needs studying. Nobody's studying it right now. All we're doing is looking for the large objects coming in.

So I think—I agree with Dr. Lanzerotti, the moon is one place to go. But I also think this other problem is one that faces humankind, and somehow we have to get ourselves in a position to decide what to do about it.

Senator BROWNBACK. So do you think we could discover more information, more exploration data, more research that's useful and that's—really, even a changing of the human mind and the human spirit—by going back to the moon rather than focusing most of our efforts in low-space orbit, low-Earth orbit?

Dr. BELTON. Well, again, there are so many things that happen in low-Earth orbit, that I would feel a little uncomfortable just talking about the things that I do know about that I'd like to see happen, and being negative about—without researching it—about what happens in low-Earth orbit. So it's a simple question you ask, but it's a very difficult one to answer, and I would think that we would have to take care in how we answer that question.

Senator BROWNBACK. Well, and that's why we're asking it of people that are very knowledgeable, because it comes down to a resource allocation, then, as well.

Dr. BELTON. From a science point of view, doing solar system exploration, low-Earth orbit really is—it's been important, in the sense that the Shuttle is being used to launch major missions, so human participation in solar system exploration has been very significant.

Senator BROWNBACK. It's been very what?

Dr. BELTON. Significant. For example, getting Galileo launched on its way to Jupiter, almost a decade ago now, was very, very much dependent upon what human spaceflight could do at that time with the Shuttle——

Dr. LANZEROTTI. But it was designed——

Dr. BELTON.—and was——

Dr. LANZEROTTI.—but it was designed for that. I mean, it could have been designed for a unmanned rocket.

Dr. BELTON. That's right. That's right. It could have been.

So it's a difficult question that you ask, and I would not want to be too negative about activities in low-Earth before I had thought about it a little bit more.

Senator BROWNBACK. Mr. Figueroa, from NASA's perspective, would you like to jump into this conversation and make any comments?

Mr. FIGUEROA. Well, the comments that I may make will be somewhat limited, but I will say that, you know, from the point of view of human exploration, research and exploration around Earth's orbit is important, but it need not stop there, because human exploration expands beyond just the near-Earth vicinity. Whether it is the Earth or an intermediate point before we can venture into going to Mars, or a place like Mars, are things that are under study for the space architect in NASA, and one that I, you know, asked that be considered for a future report.

I would also add that the predictive capabilities around Earth's orbit are important and essential, but not sufficient. I think the investments in technologies that allow us to protect humans outside of the shelter of a Earth's magnetic field are also key. And we recognize, in NASA, those challenges and are trying to take steps that lead us in that direction.

Now, whether it's moon or intermediate points as which one is the higher priority, I'm not prepared to answer, but we are, as part of our studies, looking across the board at all those questions.

Senator BROWNBACK. I would just note to you and to all the panelists, there are a lot of questions regarding the Space Shuttle and the safety of this program overall, and growing unease amongst a number of people about the—certainly the safety, the efficacy, the cost efficiency, the level of scientific knowledge that we're gaining from going to and from the Space Station. These are constantly nagging questions. They're being repeated in the media often. And I think, like—I don't quite remember quite who it was; maybe it was Dr. Belton that said that this—we are at a tragic point; we're also at a very opportunistic—there's a great opportunity at this point for us to rethink what it is that we're doing and where is it that we're going.

And so I really welcome the dialogue and the discussion, but it's going to come to a fine point fairly soon here when the Gehman Report comes out and when people start questioning, you know, just clearly about the safety and the efficacy of the Space Shuttle Program, the age of this technology, what are we learning from the continued—the cost of this program on each Space Shuttle launch. I forget what the number is now of cost-per-launch of the Space Shuttle, but it's a factor of ten higher than what was predicted when we first started into this program, so it's—now, that's not unusual in government programs. I want to recognize that, that that happens to us a lot. But we've got a lot of big questions coming here all at the same time, and they're going to come to a point pretty quick—I think, this fall—and then you're going to see Congress and the Administration wrestling with the point, OK, now, where do we go with the future of the space program? Do we stay in the low-Earth orbit, where we are now, by and large—although we have a number of missions going to different places, unmanned missions—or is it time for us to try to establish a different vision and fund that and move off of the Space Shuttle or, complete the Space Station, but move on forward? So we will need your expertise and your thoughts, and we need them rather quickly.

Anything from NASA on that point?

Mr. FIGUEROA. I'm afraid, Mr. Chairman, I will be stepping into a territory that I'm not qualified to comment on, and I would just like to note for the agency to have the opportunity to address those in the not-too-distant future.

Senator BROWNBACK. Well, I hope the agency's thinking a lot about them.

Mr. FIGUEROA. Yes. We are.

Senator BROWNBACK. Because we're going to need to have some answers here.

General Zilmer, I can't help but ask you a hypothetical from what you described. Let's say that sometime in the future, when this technology is developed to be able to move people in an out traveling through space, that we're involved or want to—going to be involved in a conflict somewhere in Central Africa in a time when this technology's pulled forward by your investment in funding. Describe how this would work and your vision of what you're trying to pull this forward in using space in the Marine Corps.

General ZILMER. Thank you, Senator.

Senator BROWNBACK. Get that microphone up to you, if you would.

General ZILMER. Thank you, Senator.

Let me begin by saying, first, the Marine Corps is not infatuated with space travel for the sake of space travel. But as we look at the enduring battlefield advantages of speed, standoff, lethality, and now stealth, and we look to the technologies that are already very, very promising—the DARPA HyperSoar Program, NASA's X–43 system. These are technologies that are, as I said, very, very promising.

And our quest to reduce our ability to react to strategic events around the globe really drives this needs statement that we articulated last year, which is to do point-to-point travel on any point on the globe in 2 hours or less.

Senator BROWNBACK. Point-to-point——

General ZILMER. Point-to-point——

Senator BROWNBACK.—anywhere on the globe——

General ZILMER.—anywhere on the globe——

Senator BROWNBACK.—in 2 hours.

General ZILMER.—in 2 hours.

Senator BROWNBACK. Wow.

General ZILMER. And, again, it's the technologies that are emerging out there that allow us to look at that.

We understand that the bar, that bar, is set very, very high. The issue of fuels, the issue—the physiology of manned flight at those sorts of speeds, the technology, where it's going. We don't mean to undermine or underplay the importance of that technology. The vision, for that matter, is very, very easy to have, but it's that ability to be able to react to strategic events that really drives the capability that we're looking for.

So, as I said, it's not the infatuation with space travel, and nor do we think we will ever see a craft that says "United States Marine Corps" on it, but it's that capability to respond quickly to events that would unfold in Central Africa someplace, the ability to respond to a WMD event, the ability to respond to some consequence-management event, the ability to respond with a surgical capability that arrives with some sweep of capabilities, perhaps autonomous weapons systems, that's the vision that we're looking for in the future, and that's why we looked at things like 25 to 30 years in the future to be able to do that.

Senator BROWNBACK. But so you would be projecting, though, that an event occurs or is getting ready to happen, and you would literally launch marines with their equipment from some point into low-Earth orbit to be able to land in this position. You've got to land in a ground-based capacity or on some sort of runway, I would guess.

General ZILMER. Senator, yes, when we looked at the—when we developed the needs statement, we looked at, VSTOL—Vertical/Short Takeoff and Landing—capability, the ability to loiter on station, to insert whatever that payload happens to be, whether it's marines or whether it's special forces in the future, a joint force of the future. But, yes, it was designed or conceptually looked at to have that ability to respond and then return from that location at the completion of the mission.

Senator BROWNBACK. Return in a low-space orbit, then, as well?

General ZILMER. Possibly. It could be low-space. It could be return via the same means. But there may be some ability to look at how we operationally conceptualize that. It may be returning to some other intermediate staging base along the return route, where time is not quite as critical as it was to get us to the site of the incident to begin with.

Senator BROWNBACK. That's impressive. And are you funding the initial phases of that technology? Are you doing—is that something that you're seeking funding from——

General ZILMER. Sir, we are not funding anything along these lines right now. And this gets back to—I think if there's an optimism to be expressed here at this hearing, is that the technology, we believe, is going to go there eventually, whether it's 25, 30 years from now. The development of those technologies are going to provide perhaps other spinoff capabilities that'll be important for military application. We want to be part of that development of that technology.

What we contribute to this is the intellectual capacity to operationalize these ideas. That's what we give to this right now, and that's why we're so interested in some of the technologies that are out there that may potentially support that in the future.

Senator BROWNBACK. You might be interested to know we had a hearing just last week of commercial sector space travel, and two people testified regarding subspace travel, and the other one, orbital space travel on a commercial basis. Two of them were looking at it as a space tourism, much like aviation started out as just people flying around the country and saying, "Hey, you want a ride?" "I'll give you five dollars," and, "Hop in and we're going to take a real quick tour." They aren't suggesting five dollars for these trips. They were suggesting 50,000 for doing it. But they were also suggesting that this a way that the business takes off, that it moves forward in the development of this technology. Also, they cited, as others have, that U.S. Government military needs and demands may pull this on forward much more rapidly.

So while what you're describing as 25 years, you're saying, down the road; the gentlemen last week were testifying about 5 to 10 years. Now, we'll see if they're able to pull that along quite that fast or not. It does make an interesting and exciting capacity.

NASA is looking—you've cited in your testimony about nuclear-powered engines by the end of the decade. Is that correct, Mr. Figueroa, and that you feel like that this is a very important part of being able to move forward?

Mr. FIGUEROA. A key element of the Prometheus Project and the JIMO mission is the availability of nuclear electrical-power engines. And, yes, there are some in development now that will be available to support such a mission.

Senator BROWNBACK. By when?

Mr. FIGUEROA. By the end of the decade. It will be available for a JIMO mission at the turn of the next decade.

Senator BROWNBACK. These must be quite small, then, nuclear-powered—nuclear-power plants, then.

Mr. FIGUEROA. No, I beg your pardon, there's a fission reactor and then the engine that takes advantage of that nuclear energy and turns it into electrical power, a nuclear-electrical propulsion system.

Senator BROWNBACK. But your power plant can't be very big to do this. What size are you——

Mr. FIGUEROA. In the order of——

Senator BROWNBACK.—designing that to be?

Mr. FIGUEROA.—kilowatts of energy.

Senator BROWNBACK. But what physical size would it be? You're going to put this on——

Mr. FIGUEROA. Oh, these reactors are of the size of, I would say, a small refrigerator or, you know, half a desk, if you will.

Senator BROWNBACK. But you'll be able to have that technology available to use by the end of the decade?

Mr. FIGUEROA. That is our expectation. And so the plans on the JIMO mission on the Prometheus Program is to put us on that track.

Senator BROWNBACK. Good. Good.

Gentlemen, thank you very much for putting forward some of the thoughts and the visions. I articulated to you, you know, what I see as our struggle coming up, and our opportunity as where we need to be going with the space programs. We don't have unlimited budgets, so it isn't that we can do everything that everybody would desire to do, but that we want to be focused and strategic in doing the things that we really need to do. And there is a yearning and a sense that we don't have the vision, the unifying vision, to date and that we need that to really pull us on forward.

The final question I'd like to pose, probably to you, Dr. Belton, if I could, and maybe there would be others that should answer this: Have we failed to articulate that unifying vision? You've all talked about various programs that are being funded and the work that we're doing. Some people feel like we've really lost our edge in space. There are a lot of things that are taking place. Have we lost that edge, or is it just that now, instead of one goal, to the moon, we articulated in the 1960s, that we're in many areas and it's actually moving forward pretty nicely, U.S. space work?

Dr. BELTON. Well, what I would say is that, from the point of view of robotic exploration, we certainly haven't lost our edge. We're doing remarkable things, and the plan for the next 5 years and the plan that we have for the next 10 years, the kind of things, technological things, that Dr. Figueroa was talking about, these are very, very exciting. They're right at the edge. They're something that we can all, in this country, be immensely proud of.

Now, in terms of the Shuttle and the ISS, International Space Station, I feel, as a private citizen, that, yes, it seems to us that because of the problems that the program has been facing for the last 15 years or so, that it has somehow lost its way. It's not clear what ISS is all about or what it's for, what the grand plan is. I don't see that. I don't see a grand plan that involves all of the things and capabilities that we've developed coming together. But, as a taxpayer, I don't see that we can abandon the Space Station at all. We've got a tremendous investment in there.

We know that the most expensive part of space travel is getting off the ground and that first couple of hundred kilometers and so forth. And so whatever happens to the Shuttle or whatever its replacement might be, hopefully a less expensive replacement, it seems to me that the International Space Station is part of the future.

I agree with you that the future is certainly not clear. But moving out into near-Earth space and these things that Dr. Lanzerotti has talked about—measuring systems, observing systems on the moon, or the kind of thing that I've talked about, with the near-Earth asteroids—are only part of the picture.

So it seems to me that you're right, we need to look at it a little more closely. But my feeling is that ISS has got a big role to play in this.

Senator BROWNBACK. So, if I understand what you're saying, in the robotics, non-human area, we're doing very nicely. In the human spaceflight area, we're really——

Dr. BELTON. That's my——

Senator BROWNBACK.—stalling.

Dr. BELTON. That's my impression, yes.

Senator BROWNBACK. Dr. Lanzerotti, do you have some thought on that?

Dr. LANZEROTTI. I agree with Dr. Belton. The United States has no peer in robotic exploration of the solar system and the universe. The Decadal Strategies established by the astronomers, and now by the planetary exploration and by solar and space physics that Dr. Belton and I talked about, lay out visions that will keep the United States preeminent and will provide incredible new understandings and concepts for our place in the universe and in our solar system and on Earth.

But, indeed, our vision for human exploration is sorely lacking, as I would say from my vantage point as a taxpayer. I have testified on numerous occasions in the past related to this, and I don't see that things have changed in the last decade, decade and a half, when I have been asked more specifically about these things in those kinds of context.

I was a member of the Augustine panel. I was a member of a couple of the redesign of the Space Station panels and was never happy with some of the directions that were talked about at those times.

And I think our vision for humans in space needs some really hard thinking. I think the Augustine panel provided an opportunity a decade-plus ago, and that might want to be followed up at some point to both see what was done there and to see whether that couldn't be expanded upon and looked at for the future, in terms of humans in space.

Senator BROWNBACK. Dr. Belton, is it time to shelve the Shuttle?

Dr. BELTON. No. We need a way to get to the Space Station and take large payloads up into space. And I think the original idea of the Space Station was as a way station of moving these things into space, from the surface into the space. I think whether you call it the Shuttle or whatever else you call it, you're going to need a very large booster to carry substantial payloads from the surface up into low-Earth orbit, at first. I think those kind of things will be needed.

For example, if we want to go to the moon and build a telescope—I'm sure the radio astronomers could invent one for us—it's going to take a great deal of material and structures and so forth, the kind of things that they've been—working with on the Space Station itself; only, taking that to the moon. I don't see them doing that directly from the Earth's surface. Maybe other people have better ideas. But it would seem to me that the Space Station would be an essential element of getting out into space with large structures.

The kind of things that I'm interested in with these asteroids—we don't know what it'll take to mitigate a collision. We know we have to either deflect or disrupt one of these objects that are coming in. And the system that would do that, it's not clear exactly what it would be, whether it even could be entirely robotic. It might involve a considerable degree of human participation. These things need to be looked at and studied. They're not being looked at right now.

Senator BROWNBACK. Very good.

Gentlemen, thank you very much. It's been an excellent panel and a very good discussion, and I'll look forward to further engaging you at a later date.

The hearing's adjourned.

[Whereupon, at 3:55 p.m., the hearing was adjourned.]

A P P E N D I X

PREPARED STATEMENT OF HON. FRANK R. LAUTENBERG,
U.S. SENATOR FROM NEW JERSEY

Mr. Chairman,

Thank you for holding this hearing on the space-related activities of Federal agencies other than the National Aeronautics and Space Administration (NASA). It's going to take some work for NASA to "right itself" in the wake of the Space Shuttle *Columbia* disaster and Congress will have to redouble its oversight. But that's a topic for another day.

Today, we are going to hear testimony about what the National Oceanic and Atmospheric Administration (NOAA) is doing in space to learn more about *climate change,* weather forecasting, and coastal and ocean monitoring. We'll also hear about space exploration missions designed to help us learn more about the origins and evolution of the Earth, other planets, our Sun, and our Solar System. The potential benefits of such research probably can't be calculated.

We'll also hear about what the Department of Defense (DOD) is up to in space. DoD's space budget is actually 33 percent bigger than NASA's! DOD is developing the capacity to detect the launch of an enemy's missiles so early that we will be able to use ground and sea weapons to destroy the missile while it is still in the boost phase. We also need to reduce our vulnerability *in space.* As dependent as we have become on satellites for a broad array of military and civilian purposes, I'll be interested to hear about what progress we are making in protecting the assets we deploy in space from enemy attack.

Mr. Chairman, I would close by saying that I think all of what we're about to hear this afternoon is a pretty good example of what the Federal Government does—and does well—without attracting much attention from the general public. I hope we *continue* to do it. It's imperative that we continue to do it. But that takes money. When taxes are cut too much, and revenue streams dry up, and budget deficits spiral out of control, the Government's ability to undertake the programs and research we're reviewing here comes into question. We can't be for more tax cuts *and* these important space programs. Thank you, Mr. Chairman.

BELTON SPACE EXPLORATION INITIATIVES, LLC
Tucson, AZ, April 4, 2003

Mr. GARY L. MARTIN,
NASA Space Architect,
Washington, DC.

Dear Mr. Martin,

The *Columbia* tragedy has triggered a public discussion of the future of the space station, space station science, and the utilization of humans in space. The outcome that we expect from this activity is an endorsement of a program of human spaceflight at NASA—perhaps returning to the goal enunciated by President Reagan in 1988: "To expand human presence and activity beyond Earth-orbit into the solar system"—accompanied by a prolonged and, possibly, divisive debate on the utility of the space station for science. As space scientists, we believe the latter can be avoided by adding a new, exciting, and affordable goal for human spaceflight and the use of the space station. This is the inclusion of "mitigation" or "NEO deflection studies" (*i.e.,* how to *prepare* for a comet or asteroid that is found on an Earth-threatening path), as one of NASA's primary goals. This goal, which we believe can combine the best of robotic and human space capabilities, can also be thought of as a precursor to another future endeavor (*e.g.,* see the discussion in *Scientific Requirements for Human Exploration, Space Studies Board, 1993*)—that of a manned mission to explore Mars. Also, such a goal can be thought of as logical extension of the congressionally mandated survey, currently being conducted in the Office of Space

Science, to find any potentially hazardous near-Earth objects (NEOs) larger than one kilometer.

In a recent workshop for NASA's Office of Space Science, we developed a roadmap for attaining the "Scientific Requirements for Mitigation of Hazardous Comets and Asteroids" (*www.noao.edu/meetings/mitigation/report.html*). This roadmap shows that to gain the basic knowledge needed for some future mitigation technology, a new NASA program is needed consisting of many novel robotic missions to acquire detailed geophysical information on the physical diversity, the subsurface, and the deep interiors of a variety of near-Earth objects. In addition, NASA and DoD will need to work together to "learn" how to apply deflection technologies including the application of low thrust devices, the application of novel in-space power sources, and/or the rapid application of large amounts of energy on small solar system bodies. We expect that a mix of both human and robotic missions to objects in near-Earth space and new uses for the space station will be required to test these technologies. The Space Science Board has already noted that there is a need for an optimal mix of human and robotic activities in such endeavors in their *Scientific Opportunities in the Human Exploration of Space* (Space Studies Board, 1993).

All of this leads us to propose a new goal for human and robotic space flight: *Show how humans and robots can work together on small objects in near-Earth interplanetary space to: (1) accomplish new fundamental science on planetary objects; (2) aspire to previously unimaginable technical achievements on objects in interplanetary space; and, (3) protect the Earth from the future possibility of a catastrophic collision with a hazardous object from space.* Since these activities would allow human spaceflight to cross the threshold into interplanetary space, they could also be thought of as a precursor activity to provide the essential technical and medical experience for that more distant, but even more challenging, goal—a *human* exploratory mission to Mars.

We also note that among the recent NRC Solar System Exploration "Decadal" Survey recommendations is one that exhorts NASA ". . . to make significant new investments in advanced technology in order that future high priority flight missions can succeed." Particular stress was put on in-space power and propulsion systems such as advanced RTG's, in-space fission reactor power sources, nuclear electric propulsion (NEP) and advanced ion engines. In the President's 2004 budget proposal, NEP figures strongly in connection with a future mission to the icy satellites of Jupiter as part of the goal to understand the origins and extent of life in the solar system. "Mitigation," or even the gathering of the specific knowledge that will be needed as a prerequisite for such an activity, was not dealt with in the Survey, since it is a technical goal and not an exploration or scientific goal. But it is now clear, as a result of the mitigation workshop, that low thrust propulsion and the application of in-space power systems to collision avoidance may now be the best way to proceed. It is a small leap to imagine *an experiment to deflect a small near-Earth asteroid though the application of thrust from a NEP system (or an advanced SEP) fueled by an advanced power source.* Moreover it is an objective that resonates with your agency's newly stated objective of ". . . Protecting the Home Planet . . . As only NASA can!" In short, we see an important coupling between the requirements for the long-term future of solar system scientific exploration, as expressed by the Decadal survey, the needs of planetary protection, and a worthwhile program that utilizes humans, the space station, and robots in near-Earth interplanetary space.

In public discussions of the President's in-space nuclear power and propulsion system initiative, the issue of environmental safety can be expected to arise even though extensive past experience has shown that such systems are extremely safe. Nuclear safety is a matter of great public concern that we share. However, we would also like to point out that the likely application of these kinds of technologies to a future NEO deflection system will also mitigate against the possibility of a much greater environmental hazard: that of a NEO impact itself. Thus, from an environmental perspective, there may be much to be gained in the application of these systems to the NEO collision problem.

A cogent new goal is needed for human spaceflight and significant investments and experimentation are required to develop in-flight power and propulsion systems for future solar system exploration. In addition, a new program needs to be started at NASA to create an adequate scientific basis for a future mitigation system and, simultaneously, to learn how to apply future collision mitigation technologies. There is a nexus between these goals and objectives that we believe should become the basis of a new thrust for NASA as it emerges from the analysis and public discussion surrounding the *Columbia* tragedy. We advocate, and strongly believe, that by adopting this goal the United States can go forward with human spaceflight utilizing the space station with productive, well-supported and meaningful objectives.

We are, sincerely yours,

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Setting Priorities in U.S. Space Science

Joseph K. Alexander, Space Studies Board, National Research Council, Washington, DC

Abstract

Two new long-range space science strategy studies are notable not only for what the new reports say and do for their respective discipline areas but also for what they demonstrate in terms of shared conclusions and in terms of the feasibility of forging consensus on community priorities. Both studies engaged a broad segment of the research community to survey their respective fields, recommend top priority scientific goals and directions for the next decade, provide recommendations for programmatic directions and explicit priorities for government investment in research facilities, and address issues of advanced technology, infrastructure, interagency coordination, education, and international cooperation. The two studies demonstrate that cross-program priorities can be established when a community sees the effort as being beneficial to the long-term health of the field.

Introduction

Can scientists reach consensus on priorities across a whole disciplinary area or is such an endeavor impossibly contentious, especially when there will be losers as well as winners? Can a scientific community come together behind a set of priorities or will the very attempt to do so tear the community apart? Is the effort required to set community-wide priorities so difficult as to make the process too lengthy to yield results that are timely and actionable?

The preparation of long-range scientific strategies and recommendations for the scientific directions of a field of research has been a traditional role of study committees convened by the National Academies. What has been rare, however, is the development of consensus strategies that span the full range of the interests of a discipline and that set out explicit programmatic priority recommendations for the field. Astronomers in the United States first undertook this task in the 1960s[i] and that community has revisited the effort every decade thereafter.[ii] In 2002 the National Research Council (NRC) Space Studies Board oversaw the completion of two new reports that expanded the creation of decadal scale consensus strategies into two other research fields-solar system exploration[iii] and solar and space physics.[iv] This milestone is notable not only for what the new reports say and do for their respective discipline areas but also for what they demonstrate in terms of shared conclusions and in terms of the feasibility of forging consensus on community priorities.

In this article we describe common features that make the new decadal-scale surveys particularly important, summarize some of their notable recurring themes and conclusions, and draw some general retrospective conclusions about the process of developing discipline-wide, long-range, science strategies.[v] To be sure, there are also significant distinctions between the different reports, and we do not mean to minimize them or to suggest that the differences are insignificant. Rather, the two new surveys illustrate at least two important points. First, the process of crafting these decadal science strategic surveys has produced carefully articulated community priorities, not unconstrained wish lists. Second, there are many aspects of the different surveys where the authoring committees arrived at similar, even identical, conclusions, and those similarities are notable as a consequence. Finally, while both of the two new surveys, as well as the earlier astronomy and astrophysics models, focused on priorities for programs in the U.S., one might expect that much about the processes may be more broadly generalizable.

Commonalities

The two new studies have a number of important attributes in common. First, like their predecessors in astronomy and astrophysics, they were derived from broad community input. Both surveys utilized websites, "town-hall meetings" at professional society conferences, expert panels, and other outreach vehicles to solicit the views of and participation by a large cross section of the relevant scientific communities. Both the solar system exploration and the solar and space physics surveys were organized around a group of topical panels comprised of 6–12 disciplinary experts and a steering committee that was charged with integrating the work of the panels and other inputs to create a single set of recommendations. In both studies, the panels and steering committee drew on formal participation from several scores of individuals, and several hundred more researchers participated in the town-hall style meetings that the Committees organized. In the solar system exploration survey several hundred more scientists prepared a collection of topical white papers for use by the Committee and panels. External peer review of the draft survey reports, conducted under the auspices of the NRC, added another 15 participants to the preparation of the solar system exploration report and another 40 participants to the solar and space physics survey.

Other significant common attributes of the new surveys include the fact that they both:

- take a long-term look at their respective fields and recommend top priority scientific goals and directions for the next decade (See boxes.);
- direct recommendations to all of the principal agencies that support facilities and research in the relevant fields;
- provide recommendations for programmatic directions and explicit priorities for government investment in research facilities, including space flight missions; and
- address issues of advanced technology, infrastructure, interagency coordination, education, international cooperation.

Box 1

Crosscutting Themes and Key Scientific Questions in Solar System Exploration[vi]

- First Billion Years of Solar System History (planet formation/emergence of life)
    1. What processes marked the initial stages of planet and satellite formation?
    2. What was the nature of Jupiter's formation, and how different from that of Neptune, Uranus, and Saturn?
    3. How did the impactor flux decay in the early solar system, and how did this affect the timing of life emergence on Earth?
- Volatiles and Organics: The Stuff of Life (organic materials, water, etc.)
    4. What is the history of volatile compounds, esp. water, in the solar system?
    5. What is the nature of organic material in the solar system and how has this evolved?
    6. What global mechanisms affect the evolution of volatiles on planets?
- The Origin and Evolution of Habitable Worlds
    7. What planetary processes generate and sustain habitable worlds, and where are the habitable zones in the solar system?
    8. Does (or did) life exist beyond Earth?
    9. Why have the terrestrial planets differed so dramatically in their evolutions?
    10. What hazards do solar system objects present to Earth's biosphere?
- Processes: How Planets Work
    11. How do processes that shape the character of planets operate and interact?
    12. What does the solar system tell us about the development of extrasolar planetary systems and vice versa?

Box 2

Science Challenges in Solar and Space Physics[vii]

1. *Understanding the structure and dynamics of the Sun's interior, the generation of solar magnetic fields, the origin of the solar cycle, the causes of solar activity, and the structure and dynamics of the corona.* Why does solar activity vary in a regular 11-year cycle? Why is the solar corona several thousand times hotter than its underlying visible surface, and how is the supersonic solar wind produced?

2. *Understanding heliospheric structure, the distribution of magnetic fields and matter throughout the solar system, and the interaction of the solar atmosphere with the local interstellar medium.* What is the nature of the interstellar medium, and how does the heliosphere interact with it? How do energetic solar events propagate through the heliosphere?

3. *Understanding the space environments of Earth and other solar system bodies and their dynamical response to external and internal influences.* How does Earth's global space environment respond to solar variations? What are the roles of planetary ionospheres, planetary rotation, and internal plasma sources in the transfer of energy among planetary ionospheres and magnetospheres and the solar wind?

4. *Understanding the basic physical principles manifest in processes observed in solar and space plasmas.* How is magnetic field energy converted to heat and particle kinetic energy in magnetic reconnection events?

5. *Developing near-real-time predictive capability for understanding and quantifying the impact on human activities of dynamical processes at the Sun, in the interplanetary medium, and in Earth's magnetosphere.* What is the probability of occurrence of specific types of space weather phenomena over periods from hours to days?

Shared Mission and Facility Priorities

Each survey was carried out in the context of its own disciplinary framework and each survey arrived at a unique set of project priorities for the particular field. Consequently, it is not particularly meaningful to compare the specific program or project priorities in the two reports. However there are notable commonalities in the criteria that the two studies employed to arrive at priorities. Both studies first translated the broad scientific goals into a series of more detailed questions, which were then mapped into a series of

programmatic initiatives. To winnow the potential initiatives into a realistic number and put them into an explicit priority order the survey committees used similar criteria.

For the solar system exploration survey the criteria were as follows:

1. Scientific merit
    - Will answering the scientific question have the possibility of creating or changing a scientific paradigm?
    - Might the new knowledge have a pivotal effect on the direction of future research?
    - Will the new knowledge to be gained substantially strengthen the fact base of understanding?
2. Opportunity—Do budgetary situations, planetary orbital configurations, developments in other scientific fields, or concurrent program developments make timing propitious?
3. Technology readiness—Is the initiative technologic feasible and affordable, and does it have an important technological relationship to other priority initiatives?

The solar and space physics survey used criteria that were similar in many respects, but which included one key difference that related to societal relevance of initiatives. The criteria were as follows:

1. Scientific merit—What is the potential scientific impact on the field as a whole?
2. Societal relevance—What is the potential for improving understanding, quantifying the impact, reducing uncertainties, and creating predictive capability regarding the effects of space weather?
3. Timing—What is the optimum affordable sequence of programs, what programs need to be simultaneous, what is the state of technological readiness of competing programs, and which programs are most urgent in the event of unforeseen limitations?

Using their respective criteria, both surveys produced a set of recommended missions, ground-based facilities, and research initiatives. In each survey the recommended programs were sorted into several broad costs categories so that major facility investments requiring hundreds of millions of dollars were not pitted against relatively small augmentations, or vice versa. Each study committee sought to recommend an overall program whose total cost might realistically be expected to be affordable by the relevant agencies over the coming decade. As we will note below, this aspect of the surveys remains to be one of the most substantial challenges for scientific committees to handle.

Common Recommendations on Infrastructure, Coordination, and Cooperation

Research and Analysis Grants Programs

Both new survey reports agree, as did the astronomy and astrophysics report in 2000, that research and data analysis grants programs (usually referred to as "R&A" in NASA's program) are often under-funded and in need of support. They all suggest financially bolstering R&A programs and/or creating new ones. The solar system exploration report argues that R&A programs convert flight mission data into new understanding, create "the knowledge necessary to plan the scientific scope of future missions," furnish "the context in which the results from missions can be correctly interpreted," and provide "a prime breeding ground for . . . team members of forthcoming flight missions." The report concludes, "Healthy R&A programs are of paramount importance and a necessary precondition for effective missions . . ." The SSE survey recommends "an increase . . . in the funding for fundamental (R&A) programs . . . that is consistent with the augmented number of missions, amount of data, and diversity of objects studied." The solar and space physics report stresses that "the underlying vitality of the . . . discipline depends heavily on the robustness" of NASA's and NSF's research grant programs, and it recommends priorities for a number of "existing and new activities that stabilize and enhance the connective fabric of the solar and space physics program."

The reports all also make recommendations regarding research databases, data analysis, computational studies, and theory. The solar system exploration report calls particular attention to problems with data analysis and archival programs and concludes, "In order to get the maximum value out of the scientific data returned from . . . missions, it is essential (first) . . . to ensure that the archiving entity . . . has the necessary resources for the job and is treated as an important component of each mission from the outset" and (second) "to dramatically improve the data analysis programs." The solar and space physics report stresses support for theory, computation and data analysis by recommending a joint NASA–NSF effort that integrates computational tools, fundamental theoretical analysis, and state-of-the-art data analysis under a single umbrella program.

Advanced technology

Both reports cite the need for investments in new space instrument technologies, and both specifically endorse the development of advanced power, propulsion, and space communication technologies. They both also support improvement and miniaturization of research instrumentation. Technology recommendations from the reports are summarized in Table 1. Although the scientific objectives of space missions in the two fields may be quite distinctive, the priority areas for technological advances to support future missions are remarkably similar.

Table 1.—Recommended Advanced Technology Areas

Solar System Exploration	Solar and Space Physics
Space-based technologies	*Space-based technologies*
• Advanced nuclear power and nuclear electric propulsion	• Advanced propulsion and power technologies
• Advanced optical and/or radio communications	• Advanced spacecraft technology • Advanced instrumentation
• Advanced architectures for spacecraft autonomy and adaptability	• Command, control, and data acquisition technologies
• Planetary science instrument capability and environmental tolerance to achieve less mass and power	*Ground-based technologies:* • Expansion of facilities for large-scale integration of space and ground-based data sets into physics-based models
• Planetary landing systems, *in situ* exploration systems, and Earth-return technology	• Support for new approaches to design and maintenance of ground-based distributed instrument networks with regard to the severe environments in which they operate
• Advanced autonomy for mobile mechanisms (rovers)	• Technology for the Advanced Technology Solar Telescope

Inter-agency coordination and cooperation

In agreement with earlier reports dealing with astronomy and astrophysics, both new reports note that contemporary scientific questions are growing ever larger in scope. The increase in measurement complexity and the ambitiousness of scientific goals are said to demand that all relevant Federal agencies work together.

The solar system exploration report recommends that NASA collaborate with the NSF on a large ground-based telescope. The solar and space physics report notes that "Interagency coordination over the years often has yielded greater science returns than could be expected from any set of single agency activities . . . In the future, it is possible that a research initiative within one agency could trigger a window of opportunity for a research initiative in another agency." The report then recommends that "The principal agencies involved in solar and space physics research—NASA, NSF, NOAA, and DOD—should implement a management process that will ensure a high level of coordination in this field, and that will disseminate the results of such a coordinated effort widely and frequently to the research community. . . . Increased attention should be devoted to the coordination among NASA, NSF, NOAA, and DOD of research findings, models, and instrumentation so that new developments in each of the areas can quickly be incorporated into operational and applications programs of NOAA and DOD."

International cooperation

All the reports agree that international collaboration is vital for furthering scientific knowledge in the areas of space science, astronomy, and physics, and all reports cite or imply two reasons to continue pursuing international partnerships:

1. Missions and projects can be accomplished that the U.S. would not otherwise be able to support financially by itself.
2. The exchange of resources, scientists, and ideas across international boundaries will enhance scientific return.

The solar system exploration report recommends "that NASA encourage and continue to pursue cooperative programs with other nations." Similar views appear in the solar and space physics report, which describes how international collaboration is especially important in solar and space physics. The committee finds, "The United States has strongly benefited from international collaborations and cooperative research in solar and space physics . . . Sharing the financial burden has allowed the space physics community to execute an ambitious and effective program in a cost-effective manner. The benefits of these international activities have permitted the acquisition of data and understanding that are essential for the advancement of science and of applications."

Education and Public Outreach

The surveys express concerns about the decreasing number of undergraduates pursuing degrees in the physical sciences. They suggest more effort at collaborations between educators and researchers to create and improve K–12 programs. The reports note that these programs must spark the interest of the younger population in the areas of astronomy, physics, and space science. They also make explicit references to the NASA Office of Space Science education and public outreach program, and they endorse better communication between the science and education communities and point to shortfalls in funding. The solar and space physics report goes on to recommend that solar and space physics be integrated into physical science curricula at both the undergraduate and graduate levels.

General Observations, Conclusions, and Lessons Learned About Decadal Strategy Surveys

In addition to the common themes in the survey reports themselves, there are notable conclusions from an assessment of how successful were the processes of conducting the surveys. The following reflect a sampling of the perspectives of survey committee chairs and members and other participants in the studies.

Perhaps the most important broad conclusion to be drawn is that *cross-program priorities can be established when a community sees the effort as being beneficial to the long-term health of the field.* This had been demonstrated amply for astronomy and astrophysics, but whether other discipline communities could manage such an ambitious task had once been uncertain, even doubtful. The new surveys that were completed in 2002 serve to illustrate that debating and setting specific, consensus priorities for a whole field is feasible.

One critical success factor is the extent to which the members of the research community have opportunities to participate in the process to have their views considered. *Broad community involvement appears to have been essential to establish ownership and acceptance and to sustain consensus across the discipline.*

The astronomy and astrophysics surveys have a 40-year history on which they can be judged, and the record is one of largely successful impacts in terms of the staying power and actual implementation of the consensus recommendations. The other surveys are still new, and so they remain to be assessed for impact.[viii] Nevertheless, there are a few key attributes that do appear to be critical for success. First, *translating explicit scientific priorities into clear program priorities makes the strategies more useful and more powerful.* This step can put a clear sense of realism and commitment in the strategies and provide clear guidance for decision makers about the views of the scientific community.

Second, *there is a delicate balance between setting firm priorities and leaving flexibility for agency managers to deal with the vagaries of the Federal budget process and new developments in a field.* Because the survey reports are advisory and not binding, agency officials always do have such flexibility, but the more a report appears to tie an official's hands or move from scientific advice to implementation direction, the more delicate aspects of the process become. A second important challenge that appears to confront all the surveys here is the process of making reliable, quantitative, program cost estimates by which to categorize recommended initiatives. Survey committees are not especially well equipped to perform substantive cost analyses, particularly without having to rely on either the agencies they are advising or program advocates with agendas of their own. However the failure to be realistic about cost assessments can ultimately undermine the credibility of the overall recommendations.

Finally, to repeat an important point with which this article began, *distinctions and discipline-unique findings and recommendations in each of the reports are equally important and should not be overlooked.* While the different surveys often do reinforce one-another and do share important common themes, they always need to be accepted as unique treatments of their respective fields for which there are unique conclusions and recommended strategic actions that merit attention.

Footnote References

[i] *Ground-based Astronomy: A Ten-Year* Program, NRC, 1964

[ii] The most recent astronomy and astrophysics survey was *Astronomy and Astrophysics in the New Millennium,* NRC, 2000.

[iii] *New Frontiers in the Solar System: An Integrated Exploration Strategy,* NRC, 2002

[iv] *The Sun to the Earth—and Beyond: A Decadal Research Strategy in Solar and Space Physics,* NRC, 2002

[v] While the opinions expressed in this article are solely the author's, the thoughtful suggestions and perspectives from NRC and Space Studies Board colleagues, especially Michael Belton, Radford Byerly, Louis Lanzerotti, John McElroy, George Paulikas, Lara Pierpoint, Donald Shapero, and David Smith are acknowledged with pleasure.

[vi] From *New Frontiers in the Solar System: An Integrated Exploration Strategy,* NRC, 2002

[vii] From *The Sun to the Earth—and Beyond: A Decadal Research Strategy in Solar and Space Physics,* NRC, 2002

[viii] In fact a significant number of the initiatives recommended in the solar system exploration report are included in the Administration's NASA budget proposal for Fiscal Year 2004.

WRITTEN QUESTION SUBMITTED BY HON. FRANK R. LAUTENBERG TO RICHARD C. ZILMER

Question. General Zilmer, the Senate has discussed the use of satellites for missile defense at length over the past few years. Is there a future for this technology?

WRITTEN QUESTION SUBMITTED BY HON. FRANK R. LAUTENBERG TO GREGORY W. WITHEE

*Question.*Mr. Withee, I think we all would like to have more reliable weather forecasts for ourselves and certainly for our troops, but we're just not there yet. Could you discuss the level of investment you feel would be necessary to dramatically improve the quality of our weather forecasting? And how long would this take to implement?

RESPONSE TO WRITTEN QUESTION SUBMITTED BY HON. FRANK R. LAUTENBERG TO MICHAEL J. S. BELTON, PH.D.

Question. Mr. Belton, the quest for pure knowledge has driven the academic world for much of our history. Knowledge for knowledge's sake undeniably has great value. But as there is so much that we do not know about space, are you able to foresee discoveries that could lead to practical improvements in the lives of everyday people? If you had to justify increased investment in space exploration, what would be your overarching reason?

Answer.

Dear Senator Lautenberg:

Thank you for your question, I will do my best to provide an answer below.

First, in agreement with the language in your question, the quest for new knowledge, *i.e.,* scientific research and exploration, is, in my opinion, the fundamental basis of our modern way of life. Space exploration is only a part of this and it effects the way we live and conduct our daily business in only indirect ways. Its effects are mainly in the way we think of and perceive the future. In the longer term future, the benefits of space exploration may not be so indirect. For example, it is a fact that sometime in the future we, *i.e.,* all of us, will face the prospect of the collision of an asteroid or comet with the Earth. Even the smallest, most probable, of these will release the energy of 1—2 hydrogen bombs at a random location.If we are unlucky, it could be 10 or 100 times worse. Providing that we will have advance knowledge of this event—and we (*i.e.,* NASA) are looking now with increasing capability—all that we have learned from the current and past exploration program of small bodies in the solar system will be brought into play. This is part of the value of having a continuing robotic space exploration program.

I have attached a copy of a paper that I have written on the subject that shows that just to prepare for such an emergency will take the order of 25 years and $5B of U.S. treasure.

Another part of your question asks about "..are you able to foresee discoveries.." The short answer is no, by definition of the word "discovery"! However, perhaps a more satisfactory response is to answer that we can be sure that such discoveries will, in fact, be made. This is easily demonstrated in the history of robotic space exploration where the view of the solar system, its origins, and evolution has been transformed in the last 30 years. The school books have been rewritten several times on this subject; the minds and outlooks of our children have been affected in profound ways that will only be fully understood in a generation or so.

Finally, if I had to justify an increased investment in robotic space exploration my overarching reason would be that we have in the last 30 years taken the current space technology as far as it can go and to ensure that we get the greatest return for our continuing investment we need a new advanced technology in space. To advance our knowledge further we need a new approach to in-space power, in-space propulsion, advance communications systems, autonomous avionics, microtechnology, etc. All of these things are in the proposed Project Prometheus that the administration has put before the congress, including new, exciting, and potentially enormously productive science missions that will employ this advanced technology.

In the above I have mainly addressed robotic exploration, but there is another (expensive and worrisome) aspect—*i.e.,* human spaceflight. Here I believe that we urgently need a NEW goal to make it worthwhile. I have recently written a letter to Dr. Gary Martin, NASA's Space Architect on this subject. My advice is to move human spaceflight to new challenges beyond LEO activities into near-Earth space. I believe that this activity should be coupled with something that will ultimately be useful to all mankind, *i.e.,* learning how to mitigate the prospect of mitigating an impending collision of a sizable asteroid or comet with the Earth. If you have time to read my paper you will see that this is a non-negligible challenge that will need time, money, and all of the ingenuity that the human race can muster.

I hope this response helps you answer some of the big questions that are facing you this year in congress.

Yours sincerely,

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ATTACHMENT

TOWARDS A NATIONAL PROGRAM TO REMOVE THE THREAT OF HAZARDOUS NEOS

Michael J.S. Belton—*Belton Space Exploration Initiatives, LLC*

I consider issues associated with the establishment of a national program in the United States to prevent asteroidal collisions with the Earth. I take the position that costs associated with future damage to social infrastructure rather than potential loss of life will stimulate public representatives to begin work on a system to mitigate the possibility of an asteroidal collision. With some uncertainty, there is a 0.3 percent chance of a 50-meter, or larger, sized asteroid impacting United States territory in the lifetime of its current population (~100 years). I show how a probable lack of concern for this small probability might be offset by the cost of the damage that could be caused by the large energy release (>10 Megatons of TNT) on impact.

I outline four conditions, focused on the interests of United States citizens, that I believe will need to be met before the start of a national mitigation program is viable. These reflect issues of public concern, feasibility, cost, timing, and security. Establishment of a public consensus on how well these conditions have been met and some modestly detailed preplanning are probably prerequisites for the initiation of a national program. I outline a planning roadmap that indicates what a national program might look like up to the point where work on a practical mitigation project directed at a specific target could begin. I also indicate how responsibilities for the task might be divided up between different government agencies. Rough estimates of the time to complete these preliminary activities (~25 yr), and a rough estimate of the cost (~$5B) are given.

Introduction

It is a demonstrable fact that asteroids of all sizes and less frequently cometary nuclei suffer collisions with the Earth's surface. The impact hazard, which is defined in Morrison *et al.* (2002) as ". . .the probability for an individual of premature death as a consequence of impact," has undergone considerable analysis with the conclusion that the greatest risk is from the very rare collisions of relatively large asteroids that can create a global scale catastrophe in the biosphere (Chapman and Morrison, 1994). In the last decade, the question of how to deal with the hazard has lead to considerable activity and advocacy on the part of the interested scientific community, and activity at government level has been stimulated in the United States, Europe and Japan (a detailed overview is given by Morrison *et al.,* 2002): There are now survey programs to search for objects that could be potentially hazardous; there are high-level calls for increased observational efforts to characterize the physical and compositional nature of near Earth objects (*e.g.,* The UK NEO Task Force report, Atkinson, 2000); an impact hazard scale has been invented to provide the public with an assessment of the magnitude of the hazard from a particular object; there have been considerable advances in the accuracy of orbit determination and impact probability.

Nevertheless, it seems that the question of how governments should go about preparing to mitigate the hazard needs some further attention. It has been advocated, as reflected in the review of Morrison *et al.* (2002), that because of long warning times (decades to hundreds of years have been suggested) we should simply wait until an actual impactor is identified to develop a mitigation system for asteroidal collisions. In the mean time, or so it is presumed, surveys to reach ever-smaller objects, scientific research and exploration characterizing these objects, basic research, etc., would continue to be supported by government agencies much as they are today. Such presumptions are, in my opinion, dangerous and, unfortunately, a high priority for these activities relative to other future scientific endeavors cannot always be guaranteed. Productive programs that enjoy adequate support today may face dwindling support in the future simply because of changing national priorities and interests. In addition, waiting an indeterminate amount of time for an impactor to be found invites, at least in my opinion, neglect; particularly at the level of government.

To resolve these problems in the United States an affordable and justifiable national plan is needed, which incorporates the above scientific research and exploration and that is focused on the technical goal of mitigating the most probable kind of the impact that can cause serious damage to the social infrastructure in the lifetime of the current population. Such an approach requires redefining the hazard in terms of cost rather than deaths together with a demonstration that the expected cost of the plan is commensurate with the losses that would most likely be incurred in the impact. This approach also builds into a mitigation program the notion of scientific requirements. An operational mitigation system or device can still wait until an impactor is identified, but meeting the scientific requirements for that system is something that ought and, I believe, should proceed now. There are other benefits to this approach: (1) by defining this program as a technical imperative rather than a scientific one the element of direct competition with established science goals is removed—even while significant elements of the program remain scientifically productive. (2) By focusing on the most probable impacts, *i.e.,* smaller asteroids, a process of learning and gaining experience is implied that might, unless fate and statistics defeat us, allow us to more effectively deal with the larger and less probable objects further into the future.

Goals

The probability of impact appears to be random and the average impact rates of the dominant component—the near-Earth asteroids—are reasonably well known. In this chapter I will consistently use impact rates estimated by a power law distribution in Morrison *et al.,* (2002). In other recent, but unpublished, work it is pointed out that the observed rates for objects near 50 m in size may be even less by a factor as large as 2 (Harris, 2002). If these new rates are substantiated it should be a straightforward task to adjust the relevant numbers given in this paper with little change to the argument.

Asteroids larger than 50 meters across, roughly the minimum size that could cause calamitous effects at the surface, collide with the Earth on average once every

600 years. This is equivalent to roughly a 0.3 percent chance that United States territory could be hit in the lifetime of its population (~100 years). With a typical relative velocity near 20 km/sec (Morrison *et al.,* 2002) the impact will almost instantaneously release an energy of 10^{16}–10^{17} Joules into the local environment, *i.e.,* roughly the equivalent of a 10 Megaton bomb or about half the energy that the United States Geological Survey estimates was released in the Mount St. Helens volcanic event. I have chosen to deal with objects of this size because they are the most likely impactors that present day American public officials may have to deal with. Also the effects of such natural disasters are close to the realm of contemporary public experience, *e.g.,* the effects of the 1908 Tunguska meteor explosion over the Siberian wilderness where the blast severely affected an area of 2000 km^2 of forestland are widely known (Vasilyev, 1998). Impacts by much larger objects, *i.e.,* larger than about 1 km that can cause global scale catastrophes, will, by definition, also affect U.S. territories whatever the location of the impact (Chapman, 2001). But these less frequent collisions occur at a global rate of about 1 per 500,000 yrs, which translates into a 0.02 percent chance during the lifetime of the current population of the United States. While I include these kinds of impacts in the argument below, it does not depend upon them. At the present time no government agency in the United States has been given the responsibility to deal with these potentially hazardous collisions. NASA exercises a mandate from the U.S. Congress to locate 90 percent of the objects greater than 1 km that exist in near-Earth space by 2008 but has no existing authority to act if an object on a collision trajectory is found (Weiler, 2002). Given the above collection of facts, it would seem that the primary issues that confront society with respect to mitigation are: When is the best time to invest in the research and development that would make it practical to mitigate the effects of such hazardous collisions in the future? Who should be responsible? And, what is the best way to go about it?

One can anticipate that achieving resolution on such issues will be a controversial task and each of the above questions could stimulate wide discussion. In this chapter I will simply assume that, if the justifications outlined below hold up, most United States citizens will want their government representatives to support the development of a system that could prevent the impact of a dangerous asteroid (*i.e.,* one greater than 50 meters in size) found on a collision course with United States territory, or a ~1 km asteroid found on a collision course with the planet at large, particularly if it were to occur during their lives. The prevention of such collisions I take to be the goal of the national mitigation program.

Justifications

There is a set of conditions that I expect would have to be satisfied in order to justify the expenditure of U.S. national treasure on an asteroid mitigation system. These conditions reflect the kinds of questions that I believe any reasonable citizen might ask before agreeing to proceed, *e.g.,* why are such a low probability events worth worrying about? Is today's technology up to the job? Will the result of this effort be useful to us even in the absence of a collision in our lifetimes? Will this effort to protect our lives and property create collateral problems we don't need? I have tried to capture the essence of these questions in the following statements:

1. The public would need to view the prospect of an impact by a 50 m asteroid within the territorial boundaries of the United States, or 1 km object impacting anywhere on Earth, as a serious concern.
2. Our technical ability to create a reliable mitigation system would need to be reasonably assured, and it should be possible to build it in time to give a fair chance that the next hazardous object to threaten the territories of the United States could be dealt with.
3. The net cost of creating a reliable mitigation system should be no more than typical losses that might be incurred if an impact of a 50 m object were to happen within the territorial boundaries of the United States.
4. The implementation of a mitigation system must not create more dangers than already exist.

It seems self evident that the first step towards a national program would be a high-level, government-sponsored, study of such issues. This would be followed, if warranted, by the assignment of responsibility and the establishment of a funded program perhaps along the lines of existing community recommendations. (*e.g.,* those in the report of Belton *et al.,* 2003).

The first condition involves the perception and assessment of risk by the public. This is apparently a topic with few experts (*cf.* Chapman 2001) and maybe impossible to quantify. In my view, it is essentially a political issue and any assessment is almost certainly made best by politicians currently in office, *e.g.,* by relevant congressional committees or in the administration itself. I have already noted that the impact rate for 50-meter and larger objects give about a 0.3 percent chance of an asteroid collision on U.S. territory during the lifetime of the population. The chances that any particular location in the U.S. would be directly affected are approximately 5000 times less. These chances have to be modified for coastal cities (where much of the population resides) since they could be seriously inundated by a tidal wave, say 5m high or greater, caused by asteroids that impact in the ocean. Ward and Asphaug (2000) have considered such impacts, but their impact rates for the most efficient impactors for this process are about six times too high relative to those in Morrison *et al.,* (2002). Correcting for this I find the respective chances of this happening are about 0.07, 0.03, and 0.1 percent for San Francisco, New York City, and Hilo in a 100-year period. To make it clear that these are small probabilities, I note that the chance that the population will not experience the effects from a collision in its lifetime is about 99.6 percent. Such small chances are, I believe, unlikely to raise much public concern even though the threat is real. It is only when palpable knowledge of the level of destruction that a random 10 megaton explosion could cause on a particular area, *e.g.,* the combined energy released by more than 770 Hiroshima bombs, or roughly half the energy of the Mt. St. Helens disaster, or roughly 10 times the energy radiated by the largest earthquake ever recorded in the US, is pointed out to the public that notice might be taken. When knowledge of this level of destruction is combined with an awareness that a reliable defense could be built for a relatively modest cost, and that some significant fraction of the costs could themselves be mitigated through productive applications to science and space exploration, then I believe there is a chance that the need for a mitigation effort now could become justified in the public mind.

It is interesting to speculate on how typical individuals in the population might view these risks and trades. I would imagine that such persons would quickly conclude that an impact would be very unlikely to have any direct affect on them, their family, or their livelihood. I would expect that they would quickly lose interest and presume that if something should be done about such rare and terrible events then "someone" in government would be taking care of it. They might be surprised to learn that the "someone" in government they assumed to be taking care of things doesn't exist and that, in fact, no one in government presently has any responsibility to do anything about it. Certainly, in the aftermath of a random 10 Megaton explosion somewhere in the United States, or a 5-meter tsunami wave inundating a coastal city, they would be both pleased at the performance of disaster relief and tsunami warning organizations but sorely perplexed by the lack of preparedness in government organizations that might have prevented the disaster.

The second condition addresses whether the construction of a reliable mitigation system can be assured and whether it would be timely. There appear to be four essential elements in such a system. First, there must be an assured ability to locate and determine the orbit of the impactor with sufficient accuracy and warning time; second, it must be possible to reliably deduce the general physical properties of the impactor so that planning for a mitigation system can achieve a reliable result; third, we must have the ability to intercept it before the collision takes place; and fourth, we must have the ability to deflect or disrupt the impactor.

Most objects hazardous to the earth are on near-Earth orbits (Chesley & Spahr, chapter 2). To reach most of the 50 m sized objects in 10 years, telescopic surveys would have to operate at around V = 25 magnitude (this is based on an extrapolation of data in Morrison *et al.,* 2002). By comparison, the surveys that are operating today have a limiting magnitude near 19.7 mag, *i.e.,* more than a factor of 100 brighter. These rough figures simply mean that *at present* telescopic technology is very far from what would be required to meet the goal of the national mitigation program. However, plans are already afoot that will push the present survey capability to a limiting magnitude of V=24 where most 200 meter objects could be found in a 10 year period. The proposed Large-aperture Synoptic Survey Telescope (LSST) facility could do this if the requirement is built into the design. The implementation of such a telescope, which is at the edge of present engineering technology, has already been advocated in the reports of two independent committees backed by the National Research Council (Space Studies Board 2001, 2000a). To reach 90 percent completeness at V=25 in a reasonable amount of time new technological limits would need to be achieved on the ground or space based systems will be required (*e.g.,* Jedicke *et al.,* 2002; Leipold *et al.,* 2002). As put succinctly by Jewitt (2000) if these, or similar, facilities are not made available: ". . . we will have to face the asteroidal impact hazard with our eyes wide shut."

Detection of near-Earth objects is only a part of the equation. Also essential is the capability for rapid determination of accurate orbits to yield long warning times and accurate calculation of impact location and probability. These are not minor requirements and demand extended post discovery follow-up observations (Chesley and Spahr, chapter 2), advances in astronomical radar systems (Ostro and Giorgini, chapter 3), and in computing technology (Milani *et al.,* 2003). While the above discussion indicates that a large increase above today's capability is called for and a considerable amount of telescope building and observational and interpretive work over an extended period of time are implied, there appear, at least in my opinion, to be no fundamental showstoppers to this aspect of a mitigation system. Time and money are the limiting factors.

Detailed knowledge of the general physical properties (mass, spin state, shape, moments of inertia, state of fracture, and a range of surface properties) will be needed for any hazardous asteroid that becomes a target (Gritzner and Kahle, chapter 9). Just the choice of a particular mitigation technology and its operating parameters will obviously be sensitive to the physical and compositional nature of the target. Experience shows that only a few of these parameters can be deduced with any precision from Earth based observations and *in situ* space missions will need to be flown to determine these parameters. Since this would at least take the time needed to build, launch and to intercept a hazardous target, typically 4 or 5 years, it is possible that there will not be enough warning time to accomplish this. In such a case the mitigation system itself may have to determine some of the critical properties (*e.g.,* shape, mass, moments of inertia, internal state of fracture . . .) when it arrives at the target *while other properties would have to be inferred from a database of properties that has been built up as part of a more general exploration and research program.* The latter will also play a crucial role in developing several new and essential measurement techniques, *e.g.,* radio tomography (Kofman and Safaeinili, chapter 10) and seismic assessment (Walker and Huebner, chapter 11; also Ball *et al.,* chapter 12) of the interior structure of small asteroids, and new ways to measure the composition and porosity of surface materials. It seems clear that an aggressive *near-Earth asteroid* space exploration program will need to be integrated within the mitigation program.

The requirement for robotic spacecraft to intercept and to land on a small asteroid is easily within current capability and has already been demonstrated by the NEAR mission at the asteroid Eros (Veverka *et al.,* 2001). Mitigation techniques may require more advanced capability for operations around these small, very low mass, objects as discussed by Scheeres (chapter 14), but, again no serious impediments that could derail a future mitigation project are anticipated.

Our ability to disrupt, or adequately deflect, a rogue asteroid of a particular size headed towards Earth is completely hypothetical at the present time. There are many ideas (for a summary see Gritzner and Kahle, chapter 9) on what should be done and there are clearly many serious uncertainties in the application of nuclear devices (Holsapple, chapter 6). Similar uncertainties are also latent in the application of a solar concentrator (Gritzner and Kahle, chapter 9). From a purely theoretical point of view it should be possible to find technical solutions these problems. However, it is clear that early *in situ* interaction experiments need to be done on small objects before we can be sure where the problems are and which techniques are viable. The B612 Foundation (*www.b612foundation.org*) has been formed to address the challenge of demonstrating that significant alterations to the orbit of an asteroid can be made in a controlled manner by 2015. Success with this endeavor would also be a major landmark in any mitigation program.

In summary, it would seem that we already have experience with many of the elements needed for mitigation, but that significant development, new capability, and time will be required for success. The lack of a demonstrated technique for deflection or disruption is a particular cause for concern. There are also other serious uncertainties, the chief being whether or not human activities in space (*e.g.,* for the assembly of parts of the system in low Earth orbit, or at the target asteroid) would need to be included. This could strongly affect the ultimate cost of a practical mitigation system and therefore its viability. But overall, though there are many technical areas that need considerable investment in time and money to achieve success, there appear to be no fundamental reasons why a mitigation system could not succeed.

The third condition has to do with the cost of a mitigation system. For costs to be acceptable the mitigation program costs should be comparable (hopefully less) than estimates of the cost of the damage caused by the most probable kind collision, *i.e.,* that of a 50 m asteroid, on the territory of the United States in the lifetime of the current population. The advantage of estimating costs this way is that we can deal with real examples of costs incurred as a result of damage to infrastructure that are provided by historical events.

The United States is a well-developed country and has many large metropolitan areas and valuable, if modestly populated, rural areas. Even its under populated desert areas often have valuable resources embedded in them. The economic losses, mainly timber, civil works and agricultural losses associated with the 1980 Mt. St Helens event in rural Washington State (approximate energy release: 24 megatons) were estimated at $1.1 billion in a congressionally supported study by the International Trade Commission. In a metropolitan area near Los Angeles, the 1994 Northridge earthquake caused economic loss that was officially estimated at $15 billion with most of the damage within 16 km of the epicentral area, and here the energy release was far less than that which could be released by the kind of impact that we are considering. I believe that these two examples are near the extremes of the economic losses that might be incurred as a result of a localized 10-megaton event occurring at a random place within the United States. On this basis I would argue that a $10 billion cost cap to a mitigation program would not be out of line. In the planning roadmap developed below an investment of approximately $5 billion should cover the costs of the initial preparatory phase of a mitigation program with the expenditures extending over 25 years, *i.e.,* an average funding level of $200 million/year. This is not far from the typical levels invested in major program lines at NASA today, and so the amount is not unusually large. This leaves a further $5 billion that would be available for the implementation of mitigation mission to a specific target. Providing human spaceflight participation is not needed, this is within the expected costs of other extremely large robotic missions that have been flown or proposed. My conclusion is that condition on cost can be met and that the annual budget for a mitigation program will not be too different from costs experienced in existing robotic space programs. If human spaceflight is shown to be an essential element in a mitigation system, then the cost argument made here will need to be substantially modified,

The fourth and final condition has to do with environmental and civil security. Mitigation concepts that depend on even a modest proliferation of explosive nuclear devices in space or on the ground will, in my opinion, be non-starters if this condition is to be met.

Mitigation Programmatics

Mounting a defense against a sizable incoming object from space will be a complex task. There are national and international issues that need to be resolved; there are issues involving the delegation of responsibility between civil and military authorities; there are science issues; there are political issues involving goal setting, mission scope, and cost containment; and, finally, there are environmental and civil security issues.

Here I advocate a three-phase process to establish a mitigation capability that roughly separates out strategic, preparatory, and implementation functions. It is probably prudent if these are accomplished sequentially since changes in one can be expected to have large consequences for the phases that follow.

The purpose of the first, or *strategic,* phase is to clarify the overall goal of the program, set up its scope, identify funding, and the assign responsibilities. Because of the significance of the mitigation program to the entire population, It should be initiated by a responsible entity within the Federal Government, either in the administration or the congress, with, presumably, expert advice from individuals and grass roots organizations.

The second, or *preparatory,* phase includes all that needs to be done to achieve the scientific and engineering requirements on which the design of a reliable and effective mitigation system will depend. This phase begins once an assignment of responsibility is made and funds are available to proceed. It should ideally be completed before a target on a collision course is identified, but in case we are not this fortunate, it should also include an "amelioration" element that takes care of what to do if an unexpected collision occurs.

The last, or *implementation,* phase can only be pursued efficiently after the preparatory phase is completed and a hazardous target has been identified. In this phase all of the specific requirements of a particular target are addressed and the construction, test and implementation of an actual mitigation device is carried out. To my knowledge no one has advocated beginning work on this phase at this time. It is probably the most expensive part of the work and may involve elements of human spaceflight.

The Strategic Phase

I have already advocated that the goal of a national program would be to design and implement a system to negate the most probable collision threat to United States territories in the next 100 years: a 50-meter or larger near-Earth asteroid.

The prime task in the strategic phase, which might take 3–5 years to accomplish, would be to assess this goal in competition with alternative program concepts and make a definitive selection. Identification of an approximate timeline, suitable programmatic arrangements, and an adequate budget profile, *i.e.,* a roadmap, would follow. Institutional responsibility would need to be assigned. Expert preliminary technical evaluation in the strategic phase is necessary to ensure that the goal is achievable and to obtain a better basis for cost estimation. There are many sources of advice including existing expertise within government agencies, their advisory committees, and committees of the National Academies.

I have placed considerable stress on the idea that the program should start out as a national program rather than one that is international in scope. This is a matter of pragmatism rather than xenophobia. Fostering program growth from existing expertise within the national space program should be more effective and less costly than initiating a brand new top-down international effort. The program may also involve discussion and use of military assets that could be a sensitive issue if placed in an international context. Finally, it is well known that national policies and priorities change on short timescales tied to political cycles, while stable funding and a sustained effort over two or three decades is needed for a mitigation program. I believe that such stability is best obtained in the context of a national program. Cost can also be expected to be an issue in an international program. While it would be beneficial to share development costs, I would expect the total program costs to be enlarged over that of a national program in order to immediately encompass a mitigation system capable of addressing the more difficult goal of combating large near-Earth asteroids that can do global damage. With this said, it is important to recognize that the collision threat is worldwide and much expertise lies beyond national boundaries. International cooperative projects that contribute to a national program are obviously to be encouraged. For an indication of the level of international interest and direction the reader is referred to the conclusions reached in the Final Report of the Workshop on Near Earth Objects: Risks, Policies and Actions sponsored by the Global Science Forum (OECD 2003) that suggest actions that could be taken at governmental level.

It should also be understood at the outset that the mitigation program advocated here is aimed at a specific technical goal and is not a scientific or space exploration program. To be sure, the program will have remarkable scientific and exploratory spin-offs, but these are not in any sense the primary goal. This is important because closely allied scientific and exploratory endeavors already have well thought out priorities and widely supported goals that should not be perturbed by the establishment of a mitigation program. This is particularly so in astronomy and astrophysics, in solar system exploration, and in space physics where goals are focused on understanding origins—particularly of life, physical and chemical evolution, and the processes that explain what we experience in space (Space Studies Board, 2001, 2002a, 2002b). It would, in my opinion, be disruptive to try and embed a national mitigation program within one of these scientific endeavors. For mitigation, a separate program with a clear technical goal is required.

The Preparatory Phase

This phase should include at least the following five elements: hazard identification, amelioration, basic research, physical characterization of targets, and, what I call, interaction system technology.

Hazard Identification. The operational goal of this element would be to locate and determine the orbit of the next 50-meter, or larger, near-Earth object that will, if mitigation measures are not taken, collide with the Earth. This goal must be accomplished with sufficient accuracy to determine if the object will also collide on United States territory. It should also provide a sufficiently long warning time. Initially I propose to set the goal for this warning time as at least 10 years, which is the minimum time that I expect it would take to implement a robotic mitigation system that might be capable of deflecting a 50 meter object. Astronomical survey systems are expected to yield much longer warning times (~100 yr) for collisions with the Earth itself. But these warning times shrink when the impact error ellipse must fit within the area of United States territories (D. Yeomans, private communication).

This is a distinctly different kind of goal from that associated with the Spaceguard survey and clearly goes far beyond it. Yet it is, in my opinion, a necessary goal if a national mitigation program is to be justified to the public. To pursue this goal, this element should contain the following components: (1) Completion of the Spaceguard survey. (2) Implementation of the Large-aperture Synoptic Survey Telescope project, along the lines recommended in the recent Solar System Exploration Survey (Space Studies Board, 2003), and a parallel development of the USAF/Hawaii PanStarrs telescope system (*http://pan-starrs.ifa.hawaii.edu*) to pursue a modified Spaceguard goal which will lead to the detection and orbital properties of 90 percent of near-Earth objects down to a size of 200-meters within about 10 years from the start of the survey. (3) Design and implementation of a technologically advanced survey system, or possibly a satellite project to take the Spaceguard goal down to the 50-meter size range. (4) A ground-based radar component developed from the capabilities that already exist at Goldstone and Arecibo in conjunction with other facilities (Ostro and Giorgini, Chapter 3) to provide improved orbits for potentially hazardous objects and to lengthen collision-warning times. (5) The final component is a suitably fast computing, data reduction, orbit determination, and archival capability. This capability could be part of the arrangements of one or more of the above telescope projects. To scope the size of the problem there are an estimated one million near-Earth space objects down to 50-meters in size and, using the results in Bottke *et al.,* (chapter 1), only about 250 of these may be hazardous to the Earth at the present time. However, there are some 210,000 objects in this population that, while not currently Earth impactors, could, through the effects of planetary perturbations, become hazardous to Earth in the relatively short term future (D. Yeomans, Private communication).

In the roadmap (Figure 1) I show these projects with some overlap stretched out over a period of 25 years. It is envisioned that these telescope systems (and others available to the astronomical community) would provide follow-up observations for each other and, where possible, make physical observations.

The goal of the *Amelioration* element is to mitigate the effects of unavoidable impacts. There are many community organizations that could fulfill this function throughout the United States and on a national level the new Department of Homeland Security would obviously be involved However, none of these organizations have, to my knowledge, been tasked on how to respond to an unanticipated impact. As the mitigation program progresses accurate warnings and alerts should become available and the newly invented Torino scale (Binzel 2000) will be used to communicate the level of danger to the public. Resources in the event of an actual disaster would presumably be allocated as is done today to provide relief from the effects of tsunamis, earthquakes, fires, and other natural disasters and not charged to the mitigation program itself.

Basic Research. There is a need for a small basic research program within the umbrella of the mitigation effort that is unfettered from well-focused goals of the other components. Here a research scientist or engineer would be able to obtain funds to support the investigation of novel theoretical ideas or laboratory investigations that are related, but not necessarily tied, to established mitigation goals. Examples are investigations into the causes of the low bulk densities that are being found for many asteroids (Merline *et al.,* 2002; Britt *et al.,* 2002; Hilton, 2002), or the details of how shocks propagate in macroscopically porous materials are a couple of areas of current interest. There are already a number of individuals, many at academic institutions or private research facilities, undertaking such investigations in the United States who could form the core of this effort.

Target Characterization. The goals of this element are twofold: (1) To obtain the information needed so that observations of a hazardous target can be confidently interpreted in terms of the surface and interior properties that are of most interest to mitigation; (2) To develop and gain experience with measurement techniques that allow characterization of the state of the interior of a small asteroid and the materials within a few tens of meters of its surface to the level of detail required for mitigation.

To meet these goals the program should provide opportunities to try out novel types of instrumentation and perform detailed characterizations of the physical, compositional and dynamical properties of a wide sample of the primary asteroidal types with the purpose of creating an archive of such properties. This kind of research, of course, already has a substantial history with considerable advances in understanding spin properties (Pravec *et al.,* 2002), multiplicity and bulk density (Merline *et al.,* 2002; Britt *et al.,* 2002; Hilton, 2002) for asteroids as a group and the distribution of taxonomic groups within the NEOs (*e.g.,* Dandy *et al.,* 2003). Nevertheless, studies of the physical and compositional properties of these NEOs are being outstripped by their discovery rate. There are three elements that should run in parallel: (1) an Earth-based observational program focused on physical and compositional characterization, including radar studies, that can reach large numbers of objects and sample their diversity. Diagnostic spectral features over a broad frequency range should be sought to better characterize the nature of each object. (2) A reconnaissance program of low-cost multiple fly-by missions, similar to that advocated by the UK NEO Task force (Atkinson, 2000), to sample a wide diversity of objects and to respond quickly to particular hazardous objects so that a first order characterization of their properties can be accomplished. (3) A program of medium sized rendezvous missions that can sample their interiors, and get down onto their surfaces to do seismic investigations. I have included four of these relatively costly missions that would include ion drive propulsion and visit at least two targets each.

The final component is a strong, coherent, data analysis and interpretation program. This should cut across all missions and include Earth based work. Participation beyond the membership of the scientific flight teams would be strongly encouraged. The goal here is to integrate the net experience of the entire suite of investigations and produce the most complete database available on the properties of near-Earth asteroids, a database that can be confidently used to diagnose the properties of a potential Earth impactor.

Interaction System technology. This element is the most technically oriented part of the preparation phase. Here the goal is to learn how to operate spacecraft and instruments in the close vicinity of the surfaces of very small asteroids, emplace and attach devices to their surfaces, learn their response to the application of various forms of energy and momentum, *etc.* All of these techniques must be *learned* (see, for example, the advice of Naka *et al.,* 1997). Experience must be gained over the full range of surface environments that the various types of asteroids present. Experiments to test the ability and efficiency of candidate techniques to deflect and, possibly, disrupt very small, *i.e.,* otherwise harmless, near-Earth asteroids should be done as part of this element. The history of space flight tells us that when the time comes to implement a particular mitigation device we should not trust the first time application to deliver on its promise. Much can go awry and practice will be needed. It is in this element of the plan that the necessary practice should be acquired.

It is also in this element where it will become clear what, if any, role human spaceflight might play in a mitigation system. A completely robotic approach would presumably be much cheaper if, in fact, such an approach were feasible. But it is possible that human participation may be essential for the effectiveness and reliability of a mitigation system.

The Implementation Phase

The goal of this phase is to safely deviate, disrupt, or otherwise render harmless a 50 meter or larger object found to be on a collision course with United States territory in the most reliable manner and at the lowest cost. This goal can be extended to the entire Earth if the hazardous object is found to be above the size that can cause global scale havoc. If the object is smaller than this critical size and not threatening U.S. territory, the United States may still be involved in the implementation of a mitigation device, but jointly with those nations whose territory is threatened. While this goal is clearly stated, addressing it will have some subtle difficulties due to errors latent in locating the precise impact point. Locating the latter within United States territory is much more difficult than determining that the Earth will undergo a collision. It may be that the implementation phase may have to start before it is determined for sure that United States territory is at risk (I thank D. Yeomans for this insight).

It will not be possible to outline a detailed plan for this phase until the preparation phase is largely complete. Nevertheless, a few essential attributes seem self-evident: (1) It would only begin when a collision threat is confidently identified. (2) It would normally, *i.e.,* if there were enough warning time, involve many of the same components found in the preparatory phase, but with their focus entirely oriented towards the target object itself. (3) It would include the design, construction, and application of the chosen mitigation system.

A Planning Roadmap

Figure 1 lays out a crude timeline for the preparatory phase that shows how the different activities that have been described interlace with one another. Estimated dollar costs, without allowance for inflation, are simply based on personal experience in NASA flight programs. The timeline for the preparatory phase is presented over a 25-year period. This time span is somewhat arbitrary and could have been made shorter by increasing the parallelism of the components. However, there are practical limits to such parallelism. These include the availability of facilities and qualified manpower, as well as acceptable limits on average and peak annual dollar costs. In my experience, average costs of $200–250M/yr with a peak of $300–400 in any one year are not untypical. The profile for this plan gives an average cost of $200M/year with a peak of $610M in year fifteen. This, relatively large peak is due to the confluence of work on six flight missions in a single year. Expert consideration of this plan with more focus on costs could presumably relieve the magnitude of this peak.

Hazard identification includes the remainder of the Spaceguard program, half of the LSST, and PanStarrs programs, and, towards the end of the phase, a space based asteroid survey mission (SBAS) for the smaller objects and objects in orbits that are difficult to observe from the ground. In the case of the Spaceguard program, which is underway at the present time, I have assumed that this program would continue until the LSST and PanStarrs survey are well underway. The National Science Foundation (NSF) would presumably support the LSST and part of the PanStarrs program. Also included in this component are provisions for an underlying and continuing research and analysis program. One provision (HIR&A, or Hazard Identification Research and Analysis) is focused on providing search software, archiving, orbital analysis, and related tasks; the other is support for an ongoing program of radar observations related to high precision orbital determination. I have assumed that the SBAS (Space Based Asteroid Survey) mission would be pursued on the scale of a NASA Discovery program.

For the Amelioration component I have assumed that elements of the Department of Homeland Security would undertake this task for a modest cost of $1.5M per year. This includes approximately $1M/yr for research into such issues as risk control, management, disaster preparation, etc. In the unlikely event that a collision occurs during the preparation period, special disaster relief funds would need to be appropriated as is usually done for unanticipated natural disasters on a case-by-case basis.

The Basic Research component is shown as equally divided between theoretical and laboratory investigations. The correct balance between these lines would have to be judged on the basis of proposal pressure. The program scope is at the modest level of $2M/y, which should adequately support some 20 independent investigations.

Target Characterization is broken down into four groupings: (1) A Reconnaissance mission line, which is conceived of a series of low-cost multiple flyby, impact, or multiple rendezvous missions similar to those recommended by the UK NEO Task Force (Atkinson, 2000). Its purpose is to provide basic physical and compositional data on the wide variety of NEOs that are known to exist. Based on experience with planning proposals, three targets per mission seems feasible with a new start every four years, *i.e.,* six missions seems plausible. To lower costs, I also assume that the basic fight system will be similar in each mission with an average cost of $175M per mission. (2) An Interiors mission line consisting of three moderately complex missions with the goal of making a detailed survey of the state of the interior and subsurface of six different types of asteroids including, if possible, a candidate cometary nucleus. These multiple rendezvous mission missions are conceived of as focusing on either radio tomography or seismic investigations and would address at least two targets each. They are expected to fall near the low end of the cost range of the NASA New Frontiers mission line. (3) A data analysis line. Here the object is to encourage the larger science community (*i.e.,* beyond the scientific flight teams) to get involved in the interpretation of the return from these missions and ensure that the data from all of the missions are looked at in an integrated way. (4) A Characterization (R&A) line which is to primarily to support Earth-based telescopic investigations, including radar, of NEOs and potentially hazardous objects from the point of view of understanding their global physical and compositional properties.

The Interaction system technology component is, at present, the most poorly defined part of the preparation phase. The necessity and scope of this component is based on the discussion of Naka *et al.* (1997) and in the roadmap I have broken the tasks down into two broad elements: (1) Interaction experiments, and (2) Intercept technology. It is clear that this element has goals of significant complexity and will need a considerable amount of detailed pre-planning. The lead responsibility for carrying out these missions should lie with the Department of Defense, although some sharing of responsibility with NASA may be required. I have imagined that the tasks in this element could be carried out within the scope of five relatively complex missions with costs similar to those of the Interior line.

Major milestones

In programs of this size it is helpful to identify major accomplishments towards the underlying goal through a series of milestones. In Table 1 I list some candidate milestones showing the relative year in which they might be accomplished and the agency that would presumably be responsible.

Milestone	Responsibility	Year
Start of strategic phase	Congress or Administration	1
Assignment of authority and responsibility	Administration	2
Congressional approval for a new program line	Congress	4

Milestone	Responsibility	Year
Start of preparatory phase	NASA, DOD, DHS	5
Start of reconnaissance line missions	NASA	5
Beginning of LSST survey (objects down to 200 m)	NSF	8
Start of Interiors line missions	NASA	9
Beginning of SBAS survey (objects down to 50 m)	NASA	20
First demonstration of a deflection technique	DOD	21
Determination of need for human participation in space	DOD	21
Conclusion of preparatory phase	NASA, NSF, DHS, DOD	30

Summary

I have presented what I believe is a practical approach to a national program to mitigate the threat from asteroidal collisions. It is based on a goal that addresses the most probable threat from an extraterrestrial object to the United States during the lifetime of the current population, *i.e.,* the impact at of a 50-meter or larger near-Earth object within the territorial boundaries of the United States during the next hundred years. I propose four conditions that would need to be met before the start of a program could proceed. In essence these conditions try to balance a presumed public disinterest due to the low probability of an impact and the relatively large cost of a program to deal with it, against the typical cost of damage to the social infrastructure that might occur and the bonus in scientific knowledge that the program would produce.

The program itself is constructed from three components that would be pursued sequentially. A strategic phase, which lays the political and programmatic basis; a preparatory phase, which creates the necessary scientific and technical knowledge that is needed to provide a secure foundation for the design and implementation of a mitigation system; and an implementation phase, in which a mitigation system is built and flown with the goal of preventing a collision.

A plan is outlined that accomplishes the strategic and preparatory phases within three decades at a modest annual budgetary level for a total cost of approximately $5 billion. The final implementation phase needs to be accomplished within a cost cap of $5 billion in order for the above argument to hold. It is expected that this can be achieved with a purely a robotic system. If, however, it is determined during the preparatory phase that human presence in space is needed as part of the system, the implementation costs can be expected to be larger than are allowed by the above arguments.

In developing this program, I largely downplay three important issues often associated with mitigation: an impact by comet nucleus, an asteroidal collision by an object that is sufficiently large to cause a civilization-wrecking global catastrophe, and the large number of deaths that could caused by such events. This is done simply because of the rarity of such events, and the lack of any palpable public experience of the destructive force of such an incredible events on the Earth and, finally, what I perceive as a necessity: we must *learn* how to deal with small asteroids before we can expect much success in mitigating a collision involving a large one. Asteroidal collisions will continue to happen and, as our society grows, will have increasingly costly consequences. I would hope that the program that I have sketched out here might be considered as a first step towards the realization of an operational mitigation system in the United States.

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Figure 1: Roadmap for the Implementation Phase of a National Mitigaion Program

The elements of the preparatory phase of a national mitigation plan are listed in the left hand column and the estimated costs to completion in the right hand columns. This phase would be preceded by a strategic phase and followed by an implementation phase. The goal of the preparatory phase is to accomplish all that needs to be done to lay the scientific and engineering basis for the design of a reliable and effective mitigation system. The approximate phasing of each element within the timeline is shown by a line of x's. The reasons for the choice of a 25-year timeline are discussed in the text.

Figure 1: Roadmap for the Implementation Phase of a National Mitigation Program

Year	1				5					10					15					20					25	Cost($M)	Subtotals
Hazard Indentification (NASA/NSF/DOD)																											600
Spaceguard	x	x	x	x	x	x	x	x	x	x	-	-	-	-	-											20	
LSST				x	x	x	x	x	x	x	x	x	x	x	x	x	x	-	-	-	-	-	-	-	-	100	
PanStarrs	x	x	x	x	x	x	x	x	x	x	x	x	-	-	-	-	-	-	-							80	
Radar	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	50	
HIR&A	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	50	
SBAS															x	x	x	x	x	x	x	x	x	x	x	300	
Amelioration (DHS)																											36
Indentification of needs	x	x																								1	
Training and operations			x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	12	
Risk and Disaster Management Research			x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	23	
Basic Research (NASA/NSF)																											50
Theory	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	25	
Laboratory	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	25	
Target Characterization (NASA)																											2345
Reconnaissance line																											1050
1	x	x	x	x	x	x	x																			175	
2					x	x	x	x	x	x	x															175	
3									x	x	x	x	x	x	x											175	
4													x	x	x	x	x	x	x							175	
5																	x	x	x	x	x	x	x			175	
6																					x	x	x	x	x	175	
Interiors line																											1200
1					x	x	x	x	x	x	x	x	x	x	x											400	
2								x	x	x	x	x	x	x	x	x	x	x								400	
3											x	x	x	x	x	x	x	x	x	x	x					400	
Data analysis				x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	80	
Characterization (R&A)	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	15	
Interception System Technology (DOD)																											2000
Interaction Experiments																											
Mission 1				x	x	x	x	x	x	x	x															400	
Mission 2													x	x	x	x	x	x	x	x						400	
Intercept Technology																											
Mission 1						x	x	x	x	x	x	x	x													400	
Mission 2										x	x	x	x	x	x	x	x	x								400	
Mission 3																x	x	x	x	x	x	x	x	x		400	
Total																										**5031**	