NASA at 50 Oral History Project
Oral History Transcript

S. Alan Stern
Interviewed by Steven J. Dick
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This telephone interview with Dr. S. Alan Stern, who served as the Associate Administrator, Science Mission Directorate, was conducted on April 15, 2008, by NASA Chief Historian Dr. Steven J. Dick. Dr. Stern begins by providing his background.

My undergraduate training is in physics and astronomy, and I took a Master's in aerospace engineering and went off and worked in the early 1980s in the aerospace industry. After working for Martin Marietta at the University of Colorado on small scientific payloads, suborbital and orbital, I found myself gravitating more and more towards science. I became project scientist on a number of things, and ultimately an instrument Principal Investigator (PI). After the Challenger accident in 1986 I took some time out to go to graduate school and get a PhD in astrophysics at the University of Colorado. I finished in 1989 and went into a full-time research career, built up a research group ultimately of about 70 people. I became PI on a host of instruments and two NASA missions. Then Mike Griffin offered me the chance to lead SMD in late 2006.

Which two NASA missions were you the PI for?

One is called New Horizons. That's the first mission in the New Frontiers series, and it is the initial reconnaissance of Pluto and the Kuiper Belt. The other is called The Great Escape (TGE). This is a Mars upper atmosphere mission for which I was PI until I came to NASA. I handed that job off but kept New Horizons.

Before you came to NASA, what problems and challenges did you have as a PI for NASA?

Well, I think many of the usual ones. Cost, schedule, technical details. I would say those are the primary.

Then you became the AA for SMD at the beginning of April 2007. What was the scope of your responsibilities as the AA for SMD?

I was responsible for the conduct of the program, and the management of those programs, strategic direction of the directorate in terms of program content, and of course all the things that go with being a boss in terms of personnel, floor space, day-to-day operations, through my leadership staff.

What was the approximate total budget that you had in your portfolio?

The direct budget is $4.44 billion this year.

You've seen the SMD from the outside and the inside over a period of years. Can you summarize how it's changed over time? We're interested in how you see things as having evolved during the time you've been working with NASA.

I've probably been aware of the organization in a concrete way for about 25 years now. But of course as one goes through their career, you gain a more and more mature understanding of what's going on, and so it's hard to really compare my impressions of the 80s to today. I would say I had a much firmer grasp by the 1990s when I was on advisory committees and really interacting in a serious leadership way on missions and instruments and grants, that I had a much firmer understanding. I would characterize the changes in SMD as vast in that period of time. The scope of its programs is much broader. The flight program is much broader in content and depth. I think that the business practices of SMD have improved dramatically. I think there were points in the late 80s and early-mid 90s when many of the processes were almost dysfunctional.

What do you mean when you say business practices?

I mean SMD's interface to the external community, in terms of how it executes its programs, both the research grants and also the flight programs.

How have those changed?

The software systems are much better. I think the quality of the personnel is generally better. The cycle times to get things done are now much quicker, much more responsive. The checks and balances that go with spaceflight are more formal. Really codified in place. I think that the rate of success of SMD missions is a pretty good empirical measure of the fact that things are working pretty well. If you were to compare that to the much lower flight rates but more significant rates of mission failures or significant problems that occurred in the late 90s, for example, I think you'd confirm that empirically.

When you came in as the AA, did you have a strategic vision how you wanted to shape your directorate?

I had a number of big-ticket things that I wanted to accomplish. One was to reinvigorate the flight program. Secondly, I wanted to restructure both the processes and the content of the research and analysis program, and to better connect the research and analysis program to its true motivation. I also had specific goals to enhance our international collaboration portfolio, to make SMD a critical part of the success of the agency with regard to the Vision for Space Exploration, now called the US Space Exploration Policy, and to build better ties throughout Headquarters between the directorate and the other organizations in Headquarters. I also set a goal of opening up human-tended suborbital science using the new generation of manned suborbital spacecraft that are being privately developed.

Let's break some of that down. You mentioned R&A, research and analysis. How does that fit in the scheme of what SMD does?

SMD supports about 3,000 research and analysis grants at any time, and this is really the bread and butter of how we make discoveries in SMD. That is, we pay for human capital, sweat equity if you will, for people to convert the 1s and 0s that come out of spacecraft into discoveries. There's an old saying that mathematicians are machines that turn coffee into theorems. It's something like that. The value of a space science mission in terms of achieving its level one objectives is in my opinion precisely zero if all we do is build it, launch it, fly it, collect the data, reduce it, and put it in a databank, because those 1s and 0s are meaningless until a human being interprets them. It's the actual doing of the science, which must necessarily come last, that the R&A program is about, and also the preparatory work for future missions so that we use our resources properly by understanding where the most important questions are, what precisely we need to measure, and how we need to go about that.

Oftentimes the R&A program has been portrayed as something that provides stability to the scientific community. Now that may be so, but that's not the reason for the R&A program. The reason for the R&A program is to actually produce discoveries, so that the missions have scientific value and that we deliver to the taxpayer what we said we would do. If we said we were going to determine the Hubble Constant to X percent, that doesn't happen until the data's been interpreted and the results published. Unfortunately, the social welfare aspect of the R&A program is one that gained prominence in Washington in the last 20 years, and it's about equivalent to saying that the purpose of your automobile is to provide four seats and a roof over your head. Well, it does that, yes, no doubt, but that's not the purpose of the vehicle. That's a misunderstanding of the purpose of the vehicle. The vehicle is supposed to go from point A to point B, and it just happens to do that with people sitting down and a roof over their head in most cases. The R&A program's purpose is to generate scientific discoveries that either guide us towards the proper content of new missions or that interpret the data from the missions that we're flying.

What part of the budget is R&A?

It's approximately a quarter of SMD's budget.

You mentioned one of the things you wanted to do was more international collaboration. Can you elaborate on that?

One of the precepts of my administration of SMD was the phrase that, "we were going to get more from the budget that we have." What that means is first a recognition that the budget that we have is the budget that we have, it's not expected to go up over time at any substantial pace. So in that zero-sum situation, the way to move the ball faster downfield per unit time is to gain efficiencies in the program by preventing overruns and discarding less useful activities. International collaboration is another example of that, whereby, by doing things with another partner who provides part of the resources, we essentially leverage our resources into flying missions at lower cost or accomplishing missions that we otherwise could not.

Do you want to say anything about export control and ITAR [International Traffic in Arms Regulations], which applies broadly to NASA? I've had people tell me that Cassini, for example, couldn't be done today because of the more stringent rules.

I certainly don't know whether if you ran the experiment, you could do Cassini. I expect you could do almost any mission ultimately. But ITAR has put in place a framework that provides great inefficiencies for apparently little value from the parochial standpoint of the space program. Even from the standpoint of national security, oftentimes it's difficult for those in the program to see how the onerous ITAR regime, when applied to something like an astronomical ultraviolet detector or a dust impact detector made to go to the outer solar system, etc., really boils down to something that's protecting our national security instead of just costing us money and time.

You also mentioned the Vision for Space Exploration. How do you see SMD as fitting within that?

There are a number of threads to that. They're all very important. Let me start by saying in recent years, since the Vision for Space Exploration was first announced in 2004, SMD had undergone some really rough times. The morale of its four constituencies – the Earth, astronomy, planetary, and space physics communities – really was quite low, because SMD had not performed particularly well in terms of advancing those scientific fields, starting new missions, carrying forth on commitments, etc. Because this poor performance was in parallel with the initial development of the Vision, many scientists connected those two dots, as did policymakers, and concluded that the Vision equals bad times for space science, i.e., that one comes at the cost of the other. So one aspect of making SMD help the Vision succeed is to break that equal sign, to show that through improvements in the R&A program and improvements in the flight program, we could in fact have a vigorous and healthy space and Earth science program, a growing space science program, in parallel, in conjunction with the Vision for Space Exploration. Now that's part one, break the equal sign.

Part two is that in order to create a pull for the Vision in the scientific community, of which there was very little 18 months ago, my analysis was that we needed to build a lunar science community. There used to be a vigorous lunar science community in the 1970s, and when the funding went away so did that community. NASA had been very successful in building a vigorous Mars science community in the 1990s by putting in place a program of funded missions and research and analysis budgets. I set it as my task to build such a program for the lunar sciences, because where there are new datasets with sophisticated state-of-the-art sensors and the funding to analyze the data, scientists will come to help make their careers by making discoveries.

So the first step was to break the equal sign and to effectively neutralize opposition to the Vision within the scientific community. The second was to create a constituency in the scientific community that actually wanted to pull the Vision forward. I think we were successful at both of those. When humans return to the Moon and are able to do field science on another world for the first time in almost 50 years, we need to be in a position as the Science Mission Directorate to exploit the capabilities that will be there. So we initiated a program of seven flight missions, initiated the NASA Lunar Science Institute, and a started variety of different grant and technology development programs, which when wrapped together provided a pretty comprehensive, even though small, lunar program within SMD that comprised a few percent of our planetary budget, but which I think had very highly leveraged that few percent of our budget into large gains for the Vision in terms of its support.

The idea of a Lunar Science Institute is an interesting one. How much did that follow in the footsteps of the Astrobiology Institute?

100%.

That's located out at NASA Ames now?

Yes. That's where we put the central node. Later this year NASA will solicit and select its first wave of research nodes around the country. Their budgets and program management will be run out of that central node at Ames, just like the parallel case of the Astrobiology Institute.

How important do you think astrobiology is for NASA's mission?

I think it's very important for a number of reasons. First of all probably the most fundamental question that the average human being wants to know about the universe is whether it's inhabited, and particularly whether it's inhabited with other intelligent civilizations. So the search for extraterrestrial life, which is the basis of astrobiology, is a very important connection between the people who pay for the space program and the space program itself, those who carry it out. Furthermore, I don't think it would be easy to list very many discoveries that would be more fundamental to our understanding of the universe in which we live than to discover extant examples or even extinct examples of life having arisen independently of planet Earth, and then to go from zero examples to one or more, and ultimately to put in a framework some understanding of the general conditions and propensity for life to evolve in the universe. As I say, I can't think of very many other things that are equally profound in terms of scientific endeavor for a space agency.

Would you say the search for life is one of the main drivers of the Mars program, for example?

It is, absolutely. If Mars didn't have an astrobiological potential, if it were just another Venus, I don't think that our emphasis on Mars would be nearly as intense.

What is your feeling about funding resources for the Earth sciences?

My feeling at the time I began to architect this program, before I came in, was that we needed to move out more quickly on the Earth Science Decadal Survey than NASA had been planning. As a part of the FY09 budget request, we put together an Earth science initiative that added about $600 million to the Earth science program over the five years of the budget, and allowed a number of new starts to accelerate that program. In fact, I thought that was so important that when our request for new funds was denied, I argued for and succeeded in asking Office of Management and Budget to allow us to take those funds from the space sciences so that the Earth science program could accelerate regardless.

Does that mean that you take the problem of global warming and climate change as something real? Of course it's been a subject of political controversy.

Absolutely. From all the data that I've seen, I think it's incontrovertible that the global environment has been warming over the last century and a half at an accelerating pace.

How about robotic and human spaceflight? There's been a controversy about human spaceflight taking away funding from space science. What are your feelings about robotic and humans in the context of human spaceflight?

It's not a choice of either-or. Rather it's as if someone were to ask you whether over the next year you'd rather eat or breathe. It is clear you need to do both. Further, I think that in the 21st century we will see a great blurring of this human and robotic dichotomy. In the Apollo program humans did exploration of other bodies in the solar system, and I'm quite convinced that that will be the case again in this century, beginning with the Moon and probably extending next to asteroids, Mars, and then on to other bodies in the solar system.

Moreover, in SMD in the last year we put in place a human program for suborbital science to parallel our robotic suborbital science program that's been there for 50 years. So whether you use humans or robots, you're just asking which kind of tool you're using to advance the science. Both tools have their strengths and weaknesses. Unfortunately, in the space science program, with the exception of Spacelab missions and some small amount of use of the International Space Station, humans really have not been involved in the scientific enterprise on orbit or in space since Apollo. As I say, I think that's changing, and it is changing across the globe, and it will be forever changed—for the better. I don't think it's likely to reverse itself again.

What would you consider the most challenging aspect of your time as an AA?

Cost control. Part of getting more out of the budget that we have is the desire to reduce the rate and the magnitude of cost overruns on projects, to make them rare instead of routine. In fact, analysis that I had done early in my tenure at SMD showed that, on average, SMD had been spending about $1.2 billion out of its roughly $3 billion flight program on mission costs that had not been anticipated at the beginning or initiation of flight programs. Now sometimes these are the agency's fault, sometimes these are the contractor or center's fault, sometimes they're everybody's fault, and sometimes they're nobody's fault. (I literally mean that last bit, too, because sometimes an act of God happens, it hails on a Space Shuttle.) So one will never reduce the rate of cost increases to zero, and in fact we're in a business where we build ones of things, not many, and sometimes twos of things, and at the edge of technology. So I am not naive.

I do not expect, did not expect, cost overruns to go away. But in many cases they are controllable. Part of it is a psychology that simply allows cost increases, that we're always going to write the check in the end, because of the sunk cost argument. Getting control over that is probably the single most important thing that SMD could do to improve its future posture, because the return on that change in behavior is so great monetarily that it dwarfs any realistic expectation of budget increases. So it's essentially an issue of running the engine of SMD at higher efficiency so that you get more bang per buck.

What do you consider your biggest accomplishment during your time as AA?

I would put two things on equal footing. It's very difficult, having just left the agency, to really judge, because I have not yet really gained a properly distant perspective. But there are quite a number of things that I think we really turned heads doing. I think the two which have had the biggest immediate impact are reinvigorating the research and analysis program and reinvigorating the flight program. We started ten new missions in a year, three of which were already in play, seven of which are new initiatives in the FY09 budget, including the new lunar program, outer planet flagship, dark energy mission, astrophysics, a program of Earth science missions for the Decadal Survey, solar probe, etc. That coupled with the strong reinvigoration of the research and analysis program I think is the highlight.

What do you think, looking down the road a few years, will be the most difficult challenges SMD will face?

The first would be cost control. My analysis is that it really is a cultural change that has to take place, and that kind of thing doesn't happen in a year or even two years, but probably takes several years to set in place something meaningful.

What do you mean by cultural change?

We have to break the psychology that NASA will simply tolerate cost increases on its programs as a matter of course.

What would you see as NASA's most difficult challenges in the coming years?

I think there are two. One is cost control across the agency. This may be even more important for the human spaceflight program than it is for SMD. Secondly, to become more relevant and responsive to American society. I think that there is generally a perception that NASA is a nice thing to have and that it does good work, but that it's not necessary. I think that perception in the public's mind is incorrect, but that the case for that has not been well made. So while the public perceives NASA as icing and not cake in terms of the success of American society, I think it is in fact cake, and that needs to be better illustrated to the American people so that we can be successful in the 21st century as we were in the 20th century. The “we” in that is the American nation. .

What you think is NASA's most important role for the nation?

I don't think there's a very good answer to that. I think that there are three or four equally important things that NASA does, and I wouldn't place any one above the other. Other people might choose to do that, but I think inspiration, both in terms of psychology of the nation and inspiration of young people to go into technological and scientific careers, is a fundamental benefit of NASA. So is the value we provide in terms of understanding our home planet and the ways that it's changing, the ways that it behaves, everything from the weather to global change to land use and oceanography.

The quest for scientific knowledge, to understand our place in the universe, the evolution of our home solar system, the universe itself, how it originated, the question of extraterrestrial life, I would place that on an equal footing with the other two benefits that I just spoke to. Then there's the technological benefits that come from the space program. There I don't mean the spin-offs that people typically talk about, like Teflon or sports bras or what have you. I'm talking about the meta-spin-offs that come from the space program, such as the miniaturization of electronics that came in the 1960s, the communications revolution that came about as a result of having geosynchronous satellites, the great change in the way that we plan our lives because we have weather satellites, and on and on down the list, in terms of the really big scope, big picture changes, in which I would include ecological awareness, which really was very much intertwined with the first views of the Earth as an actual planet in space against the hostile environment on its own. Those pictures of the Earth as a globe made primarily by Apollo spacecraft really heightened awareness. This is a major benefit of the space program.

Are there any lessons learned based on your experience at NASA, or based on what you know about its history?

I learned a great deal about the inner workings of NASA and its relationship with external stakeholders in the administration and the Hill. Those are personal lessons learned in terms of the machinery and processes, how things work so to speak.

How about NASA's future? Do you think it's a bright future, or indeterminate, or a bleak future?

I think it's indeterminate, but NASA certainly has the seeds of a very bright future. It really depends upon how NASA's value is communicated to American society, and how well NASA executes its programs. I do think that the future that NASA offers is undervalued and probably underinvested in by the nation. I think that we could have a much larger positive impact with a somewhat greater budget.

Would you encourage someone to choose to work at NASA?

Absolutely. I've worked at NASA as a graduate student, working as a summer intern, and as an associate administrator. Those are two of the most valuable experiences in my life. Almost everyone I know that works at NASA feels special about their job and their contribution to society, and I know very few people who regret the time they spend there. So I would certainly encourage people to come and spend part or all their careers at the agency.

You're the PI for New Horizons. Can you share your thoughts on what your hopes are for the New Horizons mission that gets to Pluto in 2015?

Narrowly speaking, my hopes are that we are good stewards of the spacecraft, it performs well, and that we have a successful encounter at the Pluto system and then on into the Kuiper Belt. Speaking more broadly, this is the first mission to a new planet and a new kind of planetary body since the late 1980s. When New Horizons launched in 2006, it was the first launch of a spacecraft going to a new kind of place, a new planet, since Voyager launched in 1977, 30 years before. When New Horizons arrives at the Pluto System in 2015, most Americans then will not have been alive or able to remember the first era of planetary reconnaissance. Yet with this mission, this is the capstone of planetary reconnaissance, where we complete the basic inventory of the solar system.

One of the great lessons of planetary science is that every time we go to a new kind of place – first giant planet, first icy satellite, Venus, Mars, Mercury – we typically find out that we have to rewrite the textbooks, that our ideas were very naive, and that nature is much richer than our data led us to believe from an Earth-based vantage point. So I really expect that the first reconnaissance of the Kuiper Belt and the Pluto system, this new kind of world and these dwarf planets that are the most populous class of planets in the solar system will revolutionize our knowledge of our home solar system.

Can you be more specific about what kinds of scientific questions you hope to have answered?

It is a reconnaissance mission and so our objectives are codified in terms of things like mapping Pluto and its satellites at such-and-such a resolution with such-and-such a signal-to-noise; mapping surface compositions with various technical specifications; assay the temperatures and pressures in the atmosphere to such a level, those kinds of things. What we're trying to do in reconnaissance, as is always the case, is to gain a basic first order understanding of the characteristics and evolutionary history of the body or bodies that we're exploring. In the case of the Pluto system, it's Pluto and its three satellites, and any satellites we might discover that haven't yet been found.

We know that Pluto and its cohort population of dwarf planets in the Kuiper Belt are objects that were growing towards much larger sizes but which were arrested at the smaller size in their formation. So from a technical standpoint in computer models of solar system formation, Pluto and its cohort are the developmental equivalent of an embryo whose development was arrested in the mid stages of gestation. So we have an opportunity in studying these worlds to see something we have never seen before and to understand something new about planetary formation, by seeing not the finished end product or the initial building blocks -- that is large planets like the giant planets and the terrestrial planets, or building blocks like comets and asteroids -- but an object that was in the process of growing piece by piece from comet after comet impact, but not yet to a large-scale planet. So this mid stage of planetary gestation is something that's only been seen in computers, and here we will have an opportunity to really study this kind of body and I think open up a whole new chapter in terms of our understanding of planetary formation.

I know you also have some thoughts on Pluto's status as a planet, with the whole International Astronomical Union (IAU) controversy over the last couple years on this.

I could talk to you about this for a long time! There are many aspects to it. Simply put -- and I have had this discussion many times with colleagues -- there is no characteristic that one can identify in the Earth or other bodies which the IAU's definition of a planet sanctions, that Pluto and its cohort do not have. Atmospheres, moons, geological activity, a core, a crust, all those things are there. So there is no fundamental distinction between the dwarf planets and larger planets in terms of their characteristics, only an arbitrary distinction, which is just a distinction of size. In the same way that a Chihuahua is still a dog because it shares certain deep characteristics with the other things we call dogs, Pluto and these worlds and even still smaller ones are still planets, they're simply dwarf planets.

Now the second aspect of this difficulty I have with the IAU's end result is that their definition of a planet depends upon where the planet is in its solar system. As one very simple example, the Earth is admitted as a planet in our solar system, but if you were to have a collection of identical Earths at farther and farther distances from the Sun, by the time you reached Pluto's orbit those identical Earths would no longer be planets. Now I think this is patently absurd, and it goes against the grain of every other classification system in astronomy. There is no requirement for example that a star is only a star if it's within a certain distance of another star or controls the space around the star or whatever. The same goes for pulsars and black holes and galaxies and everything else in astronomy.

The IAU has essentially arranged by a small number of people an arbitrary definition whose goal was to keep the number of planets low so that people could remember the names of all of them. This is absurd. If that were the case, we would never have more than a countable number of rivers on Earth or mountains or many other things, maybe only have ten species for example. It's just not the way science is done. We don't figure out the answer we want and then arrange the algorithm to give us the answer. That's dogma.

So you would argue that Pluto is a dwarf planet?

I coined the term dwarf planet in 1990 in the academic journal *Icarus.* It is a dwarf planet. But that's a kind of planet, just like dwarf human beings are still human beings, they're just smaller.

Where the IAU went wrong was saying that a dwarf planet is not a planet, which doesn't make much sense to anybody.

Right. Another place where the IAU went wrong – and I'm just going to finish my thought because it rounds out the three primary arguments – first no fundamental characteristic that's different, second the argument that I just espoused, and then finally the third error of the IAU was in voting. Science is not done by voting. If we collected the most eminent scientists of the world into a room and voted the sky was green, it would not make it so. We didn't vote on relativity or continental drift or on recombinant DNA, because voting is a process that works very well in certain aspects of human culture, but it does not work for science, because the vote doesn't change the way things work.

I think that that picture of scientists voting and making it so was the greatest moment of pedagogical damage to science in many decades, if not the past century. It has undermined the scientific method and people who have to deal with whether global warming, for example, is a belief or a fact, whether evolution is a fact established by data or a belief, and on and on through other issues, were all undermined by this view, this widely publicized view, that scientists vote to make things so. By voting that certain objects are or are not planets does not change their characteristics.

So where do you think this issue is going? Is it going to be reversed? Or what do you think is going to happen?

Well, I hope we don't vote again, that's for sure. Science comes to consensus because eventually the facts constrain us to a model that explains all of them, and that scientists simply adopt because there's no sense in working in a framework that doesn't actually produce correct results. What I see is there was a lot of backlash after the IAU's vote, petitions by astronomers that said, "I just won't use that," textbook authors that said, "Well that's horse puckey," so forth. But now I see an increasing number of conferences, like the AAAS this year in Boston, the European Geophysical Union last year in France, later this year the meeting that's taking place at Johns Hopkins, on planet definition, that are empirical evidence that the issue is not well settled, or else there wouldn't be a need for such meetings.

I think that on a timescale of five or 15 years or 20 years this will settle out about where it started with astronomy recognizing that there is a wide variety of planets of all sizes just like stars and other objects in astronomy, and then maybe the IAU will catch up someday. But I really think that the current IAU definition is almost universally accepted as so deeply flawed that it's not workable.

And it doesn't even encompass the exoplanets.

This is part of the arbitrary nature of that definition, because again the people in that room wanted to achieve a specific objective: small numbers of planets. So they had to do something very antiscientific besides just voting. They had to construct the definition so narrowly that it only applied in our solar system. Of course, science is a reductionist enterprise in which we try to generalize over a large number of facts and reduce them to a small number of concepts. So this goes against the grain of the way that science works. And it's ultimately doomed, but I can't tell you the timescale, that's my opinion. Maybe at the 100th anniversary of NASA, when these words are read by somebody, they'll get a nice smile and they'll know which year or decade in which the tide changed. I think it's already changing. I suspect by the time we get New Horizons to Pluto, this will be a question from a Trivial Pursuit game at best.

New Horizons won't care whether it's a planet or not.

No, it doesn't know to care.

Do you want to say anything in conclusion?

I'm very grateful and honored to have had the opportunity to work for NASA. I think SMD really shined in the way it performed. There was a lot of innovation and a lot of positive forward movement in both the science program and its connections to the Vision that I think are widely appreciated. I've been very touched by hundreds upon hundreds of emails from people in the scientific community, in the government, within NASA about the progress that SMD made in the last year. I hope that that progress will continue.