# NASA Headquarters Science Mission Directorate
# Oral History Project
# Edited Oral History Transcript

Scott Hubbard
Interviewed by Sandra Johnson
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Johnson: Today is August 22, 2017. This interview with Scott Hubbard is being conducted for the NASA Headquarters Science Mission Directorate Oral History Project. Dr. Hubbard is speaking with us today by telephone from [Stanford University, Stanford] California, and the interviewer is Sandra Johnson. I want to thank you again for joining us and agreeing to talk to us for the project.

Let's start today, if you don't mind, and talk about your background and your education, and how you first became interested in astronomy and physics, since that was your double major.

Hubbard: Well, it began, I remember, when I was probably around nine years old. I am a child of Sputnik [Soviet satellite]. When that first manmade object to orbit the Earth went up in 1957, I would have been almost nine years old.

And that was, of course, a transformative event for the entire world. It was part of the Cold War. Kids that were in school at that time, like I was, were being taught to duck and cover if the Soviet empire launched a nuclear strike, and the whole Space Race that started with Sputnik was part of the Cold War that was going on at the time. I became fascinated with space at that point. My father bought a small telescope, and we looked at Mars together one night out in the backyard.

I should say that I was born in Lexington on the campus of the University of Kentucky where my father was going to school, but grew up in a little town called Elizabethtown, which is about 50 miles south of Louisville. It was the county seat and had all of 5,000 people, which is a huge town for that era in Kentucky. A lot of my relatives lived in towns that were 200 or 300 people.

So I came from a very small-town, rural background. Not a lot of science in Kentucky in the late 1950s. When I was interested in all this there was no internet, so you had to hop to the local library and get books however you could. Television was still in an early phase. But I began to be what some people would call a "rocket boy."

There is a guy, a former NASA guy, named Homer [H.] Hickam [Jr.] that wrote a book called *Rocket Boys*, which became eventually a movie by a different name [*October Sky*]. I would develop rocket fuel in the basement and launch little small rockets in the backyard—much to the dismay of the neighbors—but this was all part of my interest in space, and rocketry, and astronomy, and all of that.

That interest stayed with me and evolved actually into something, looking back on it, that informed much of my future. In addition to the rockets and looking at Mars through a telescope, I became fascinated with the question of "Is there life elsewhere in the universe?" I remember reading not only tons of science fiction, but a lot of science books by people like George Gamow [born Georgiy A. Gamov], and Isaac Asimov, and [Sir] Fred Hoyle, and books on astronomy and cosmology. And that idea, that interest in life in the universe, eventually turned into—many, many years later—astrobiology. When I played the role of setting up the [NASA] Astrobiology Institute, it was sort of that dream or interest when I was nine years old coming to fruition 40 or so years later.

That was a lot of my background. I was in school there in Kentucky. I was the valedictorian of our little high school. I think we had 141 people in the class. Fortunately, there was a program, the National Education Defense Act, that allowed you to go to university. I was able to take advantage of that, and went for undergraduate school to Vanderbilt University in Nashville, Tennessee.

I am moving now years down the road. I was interested in science all through high school, and when I got to Vanderbilt decided to major in a double major in physics and astronomy. I did my senior thesis work on a double star, making the observations myself.

I should say something, though, since this is an oral history, that may be of interest, which is that around the time I was 14, I got—partly because of my father—fascinated by music. His side of the family had a lot of—I guess you'd call them sort of bluegrass or country, folky kind of players. My father played harmonica. I started playing folk music, and then later on got an electric guitar. So going to Nashville, which was even then "Music City, U.S.A.," kind of added a sidelight to my science interest. I kept on playing music, and got pretty good at it. My sophomore roommate—there were groups of rooms, and we were all in the same general area together—was a guy named Gary [E.] Scruggs, who was Earl [E.] Scruggs's oldest son. Earl, of course, was the inventor of the banjo technique that all the bluegrass groups started to adopt, so I got to meet Earl and those folks. Anyway, I kept on playing music.

I can come back to that later, but the science interest was there. And of course I was at Vanderbilt 1966 through 1970, a very turbulent time in the country. Over the years, I have done a lot of interviews of people asking me about where I was when Neil [A.] Armstrong and Buzz Aldrin landed on the Moon, and I'll say, "Well, I was still in undergraduate school on July 20 of 1969. I remember everything stopped when we watched that on TV." But I also hasten to add to the interviewer that you have to realize that simultaneously was the Civil Rights Movement; the war in Vietnam; the Space Race and Apollo [Program], and then to the Moon. And the emergence of Woodstock [music festival], and major rock and roll, and Jimi Hendrix and Janis Joplin [musicians] and so forth, which was all part of the youth movement.

What some people call the hippies—although I don't know anyone that ever considered themselves a hippie who was a teenager at the time. That was a term invented by the media. That was the complex stew in which I was living in Nashville, Tennessee. Really enjoyed the science; really enjoyed the music.

I don't know if anybody bothers to look at the really long, extended, multiple-page version of my bio [biography]. You see at the very beginning there, 1970 to 1973, it says, "Research Engineer, Physics Department," which was true, although somewhere in the footnotes I usually point out that that was actually for about a year or so. I don't remember the exact details, but I served in that function as a research assistant in the Physics/Astronomy Department, and then I got to a point—because I had been doing that during the day and playing music at night—I said, "When will I ever have this opportunity again?" I was 21 or something, and very interested in music, so I actually spent somewhere around a couple of years in Nashville just pursuing the music.

I was, for a while, a full-time musician in Nashville, Tennessee because I wanted to see what it was really like. Discovered that music was great, the music business is not so great, which a lot of people figured out, so I said, "Well, probably rocket science is where my future lies." I was looking at, "Well, how can I make a contribution? What can I do that will make a difference?" I didn't think of myself as a particularly good singer/songwriter. I was pretty good at playing the guitar, and the people in that music business who were really well-known—that was the emergence of Willie Nelson, Waylon Jennings, and those guys. That's when I ultimately, in '73, decided to come to the West Coast and go to graduate school at [University of California] Berkeley, and also started working at Lawrence Berkeley National Laboratory [Berkeley, California].

Let me pause there, since that was, I don't know, 10 minutes of talking, see if we are going in the right direction.

JOHNSON: Yes, we are. That's definitely the right direction, and it's very interesting to hear about the background, because not everybody comes to it the same way. So yes, this is the information that we like to get. You're doing great.

HUBBARD: Okay, so just press on?

JOHNSON: Just press on.

HUBBARD: Okay. Coming to the West Coast, I was unsure of exactly how to pursue these ideas that I'd had as a young boy. I had worked with some good astronomers. I was clearly more interested in the space side than the pure physics side at Vanderbilt, but then I went to graduate school. The hot topic then—and this often happens, I think, to young people as they are being advised what to go into.

I remember just stepping back for a minute when I arrived at Vanderbilt, having that interest about life in the universe, and went to the biology department to talk to a person there. They said, "Oh, forget it. There's no future in biology. You won't be able to get a job, don't bother." So I had to wait many more years to pursue my interest in life in the universe and what became the field of astrobiology.

Anyway, the hot topic at the time—I am talking now like '73 or '74 or so—was semiconductor and solid-state physics. This is the invention of the integrated circuit and the processes that created, really, today's electronics. Before that time, everybody was dealing with what are called discrete components. You know, you'd buy a transistor and you'd put it on a board, and buy a resistor and put it on a board, and that's how you made radios and everything else.

So I went to graduate school in semiconductor and solid-state physics, and that dovetailed in part with the work that I was doing—research work—up on the hill at what was called the "Rad Lab" for radiation. It was the Lawrence Radiation Laboratory, then it became the Lawrence Berkeley National Laboratory later on. When I joined the Lab, it was part of the Atomic Energy Commission. These National Labs—whether it was Berkeley, [Lawrence] Livermore [National Laboratory, Livermore, California], Los Alamos [National Laboratory, Los Alamos, New Mexico], and so forth—were thought of as part of the nuclear weapons program.

Lawrence Berkeley, though, had been created in the '30s a little bit before that. But the same man, E. O. [Ernst O.] Lawrence, was a critical piece, a critical individual, in the Manhattan Project. So as I was pursuing my career there, I got to have the opportunity to know some of the characters, some of the people that were involved in the golden age of nuclear physics and the Manhattan Project. People with very famous names, like Emilio [G.] Segrè and Owen Chamberlain.

There was a man named Clyde [E.] Wiegand. Clyde was one of these gentlemen who would see a young person coming in and would sort of decide that they would adopt them. Like, "Hey, come over here. Sit at my lunch table." And Clyde's lunch table up at the Rad Lab up at Lawrence Berkeley were sort of famous conclaves of people. He had worked with all of these Nobel Prize winners and all of the people involved in the Manhattan Project.

Clyde, at the time—I mean, this was 1974 or so, 1975—had a lunchbox that was probably already 20 or 30 years old. In fact, the hinges had worn out, and he used safety pins. A typical scientist engineer—never waste anything, and keep it going forever. It had been repainted blue a couple times. But Clyde was very generous, and he introduced me to these other people there at the Lab. It's really amazing to look up sometime during lunch and there would be four or five Nobel Prize winners sitting around the table.

JOHNSON: What an opportunity.

HUBBARD: Yes, yes. The realization that I got very quickly, though, is that anybody could join the conversation, but if you were going to join the conversation, you better be prepared to hold up your end of the discussion. Because these folks were very, very sharp, obviously. They would leap in with a bunch of questions, and if you knew what you were talking about, and describing whatever science or technology that you were working on, you would get respect and they would talk to you as an equal. They didn't have to check your CV [curriculum vitae] before that.

That was a wonderful experience. I learned a great deal about how research was conducted, and worked on materials and devices for radiation detection. Part of that were new detectors that were used at the still-operating-at-the-time accelerator facilities, in what was called the cyclotron, the Bevatron, the Bevalac, the HILAC [Heavy Ion Linear Accelerator]—all these terms having to do with what kind of an accelerator they were. You needed to have the tools to carry out the research and to detect different particles, and I worked on research for that.

But I also worked on the space side, and worked on an experiment that flew on Apollo-Soyuz [Test Project], high-altitude balloon astrophysics, gamma ray astrophysics. Worked on, at the time, the approach, or the design for a detector of the type ultimately launched years later aboard the [2001] Mars Odyssey mission. The Mars Odyssey mission, which is still orbiting Mars, used a detector like the sort that we were working on at Berkeley to discover that, indeed, there are billions of tons of water ice distributed around Mars, particularly at the poles.

It's an example of how sometimes you work on something that is just a concept, and 30 years later or so you may see that concept employed in a very major discovery. Now, I think today the time between concept and application has gotten shorter, if any of the people making apps [software applications] down in Silicon Valley [southern San Francisco Bay Area, Santa Clara Valley in California] are an example. But this process of fundamental technology to application, that sort of stayed with me as a lesson of how that happened.

Then, in 1980 or so, [Ronald W.] Reagan was elected president, and one of the things that he did that lasted for a while—eventually, this initiative was deleted or the law was repealed because it ended up with people getting in financial trouble—but for a while, they allowed individuals to deduct, against their own personal income, investments in an R&D [research and development] partnership that was conducting research through another entity. This resulted in a whole lot of startup ventures in the Bay Area, and while I was at Berkeley I had developed a number of new technologies, one of which was the process by which you made this ultrapure material that eventually turned into the detectors of the sort that flew on Mars Odyssey years later. A couple of people—one a Czechoslovakian refugee named Jiří Mráz, and a few others—had the idea that this technology might be commercially viable.

We knew of some companies that had done this—General Electric [Company], and one other [ORTEC] in Oak Ridge [California], and one in Europe. So long story short, I got involved in a startup venture in around 1980. This startup venture originally was called Berkeley Germanium [Corp.]—that's the name of the semiconductor—and then later on we were purchased by a company in Connecticut named Canberra Electronics [Canberra Semiconductor, Inc.]. No relation to the city in Australia. I don't know where they got the name, frankly.

We were called Canberra Semiconductors, so then I went from being a sort of bench-level—or maybe with some supervisory responsibilities—researcher, technologist, R&D person into more of the management ranks. I ended up being first Director of Research and Development, and then the General Manager of this whole startup venture.

I learned more than I intended to ever learn about not only R&D, but plumbing and heating, and the three-day rule when you're paying employees' withholdings to the Internal Revenue Service, and so forth. It was a quick, jumping-into-the-deep-end, practical school in management of a research organization. And those lessons, what I learned on the fly there, stayed with me rest of my career, and particularly when I started moving up the ranks in NASA.

This was quite a venture. It was, I think you would say, modestly successful, at least at the beginning, and we did get bought out. But it was nowhere near the scale of the buyouts that are occurring now. A couple of grad [graduate] students from my department founded something they called Skybox [Imaging] that is an orbiting remote sensing venture, and I think Google [LLC] bought the company for $500 million.

JOHNSON: Oh, goodness.

HUBBARD: Yes, so that's what all the millenials are looking for. Anyway, this was a little-bitty buyout. Although it helped form the basis, later on, for me to be able to put a down payment on a house in California.

The company lasted for about five years. It was based on the assumption that nuclear power was going to be a really big deal—a mainstay in the world of power generation. For a while, that certainly looked to be true. Then people began to have all kinds of misgivings about, "Well, where do you store the waste?" and other power generation approaches were surging ahead. This had a lot to do with the price of oil at the time.

The company decided—the current company in Connecticut—that they were going to consolidate everything back in Connecticut, and they wanted to move all the equipment there, and I declined to go. I wasn't interested in leaving California, I really enjoy it out here. So that's when that ended, in '85.

I stayed in the San Francisco Bay Area, and moving on now to the—I'm just kind of walking my way through my CV, but it's a good reminder of what happened. I did a number of things, consulting work. I had done a little consulting work on the side when I was at Lawrence Berkeley—they allowed you to do that as long as there was no conflict of interest—and for a year or so was moved from the consulting realm to actually being an employee of what was known as the Stanford Research Institute.

Then, just like IBM [International Business Machines Corp.], it just became "SRI" [International]. I think the letters don't officially mean anything anymore. Worked on some projects that were largely classified at the time, and have now been declassified.

Now I'm going to talk about something—since this is an oral history for the future—that I don't generally talk about, but it might be of interest to people who are following other threads of research. Part of this program was the now-declassified government program sponsored by the Army, the CIA [Central Intelligence Agency], DIA [Defense Intelligence Agency], and others to see if a thing that people call "psychic functioning" actually exists. If you Google [internet search] a new book called *Phenomena* [*The Secret History of the U.S. Government's Investigations into Extrasensory Perception and Psychokinesis*], that has all the characters and the stories of all these different projects. It was a deeply classified program, and there's all kinds of different levels of classifications. If you have hung around JSC [Johnson Space Center, Houston, Texas], you probably know a little bit about that. But I got a chance to be part of something for a while there that was really quite interesting. Not something you find in your average space business.

JOHNSON: No, and I actually saw that. Somewhere I read about it and I thought, "Well, that's an interesting little tidbit. It would be interesting to talk about." So this is good that we're having a chance to do that.

HUBBARD: Yes, a whole sidebar on that. As I said, because of the giggle factor, I don't ordinarily talk about it much, because to really talk about what you did there, and what kind of processes and procedures and experimental protocols were put into place, it takes more than your typical sound bite, or elevator pitch, or 25-words-or-less. It's the kind of thing where you have to sit down and have an extended discussion to talk about the effort that was put in to see if any of this stuff is real.

Then, in ’87, I got the opportunity to join NASA Ames Research Center [Moffett Field, California]. Although I had been working, as I said, on some space applications of technology and things that were a part of Apollo-Soyuz and eventually other missions, this was where I really began my space career. When I was interviewed, I was already, I think you would say, mid-career. In ’87—I was born in ’48, so I would have been 39 years old. That would put me on a mid-career level. So as I was brought in, one of the things they said that they wanted me to do was to be a rainmaker. They didn’t use those words, but to create new projects and programs.

The group at NASA Ames was small but very high quality. Ames is primarily a technology center. It was created in 1939 as the Ames Aeronautical Laboratory on the eve of World War II. The then-NACA—and you may have, in your interviews, come across that, the National Advisory Committee for Aeronautics—it founded Langley Research Center [Hampton, Virginia] in 1915, and then founded Ames in December of ’39, and then Lewis [Research Center]—now Glenn [Research Center, Cleveland, Ohio]—in [June] 1940, with a set of wind tunnels that were aimed at doing the research to support military—and industrial—but military aircraft for what everybody saw on the horizon, which was another war.

Then when Sputnik flew and the Space Act of 1958 was passed, it became NASA Ames Research Center. “Ames,” by the way, was Joseph Sweetman Ames. He was the [fifth] chair of the NACA. He never came to California.

This group that I was joining, the Space Projects Division, was responsible for the Pioneer series [satellite probes]. When NASA was created, they gave space assignments to all of the labs, all the facilities. The two assignments that Ames got were space biology, which became—a sort of synchronicity or serendipity—the assignment in the area that, through a lot of changes and permutations, became astrobiology. Then the other assignment was heliophysics. In other words, orbiting the Sun and making measurements of particles and fields.

Ames then took over the Pioneer series at about Pioneer 6. I think the early Pioneers may have been done by JPL or one of the other—maybe it was a military group [U.S. Air Force], I'm not positive. So I was coming into this group that had done Pioneer starting in the early '60s—Pioneer 6, 7, 8, 9, 10, 11, Pioneer Venus—and then got the job for the Galileo probe that was part of the Galileo mission to Jupiter. But that was winding down, and they didn't have anything on the horizon. The group there—the head of that was named Joel Sperans—were very concerned about going out of business. It was something for which there was a lot of skill, and knowledge, and capability of managing, developing, and operating a certain class of smaller, medium-sized space missions.

The big groups within NASA for space science are, of course, Goddard [Space Flight Center, Greenbelt, Maryland] and JPL [Jet Propulsion Laboratory, Pasadena, California]. But there was this other, smaller mission capability that existed at Ames. That was my charter, was to come in and find new things to do that fit with the capabilities of the Center.

Just a year or two after I arrived was the [President] George H. W. Bush announcement of the Human Exploration Initiative that became the Space Exploration Initiative. That was the first Bush speech about "We shall go to the Moon and on to Mars." This is the first real interesting, I think, tale of me in NASA. It's seeming like there's a long string of presidential speeches about space exploration, and then they don't go anywhere. The only one that went anywhere was [President John F.] Kennedy's speech [May 25, 1961, announcing the goal of "landing a man on the Moon and returning him safely to the Earth"]. And that was because of

[Vice President] Lyndon [B.] Johnson being able to maneuver that, get the funding for it, and pushing it through Congress. And it was a creature of the Cold War.

So here was the Space Exploration Initiative, and as part of that were supposed to be a number of precursor missions that, say, before you sent humans back to the Moon or on to Mars, there were supposed to be robotic science missions that went to gather data, demonstrate landing techniques, other technologies, and so forth. It looked like an interesting, important area to be involved in, and my boss, the division chief at the time, agreed. Said, "Yes, let's take a look at this. Maybe we can do something."

I started going to the various meetings, and the proposals put forward by JPL fit the JPL mold, which is big missions. They have always had a major so-called strategic or flagship mission going, and they proposed something—I don't know what it would be in today's dollars, but it was clearly in the multiple-billion-dollar class. Two launches, two orbiters, landers, you name it.

The gentleman at NASA Headquarters [Washington, DC] who was in charge of advanced programs for planetary science, a gentleman named Dudley [G.] McConnell—who worked for the division chief at Headquarters, whose name was Geoff [Geoffrey A.] Briggs, who later on left Headquarters and came to Ames—got in touch with me.

I'm not quite sure how my name made it to them, but the essence of Dudley McConnell's comment was, "Look, we have got these mega missions being proposed by JPL and they're not going to fly. They are simply too expensive. Is there anything we can do for way less than $1 billion?"

So I put a little team together. We started thinking about this, and the first thing we did—and part of this may be in my book [*Exploring Mars: Chronicles from a Decade of Discovery*].

I'm not sure, but I'll tell you the brief version of the story. We said, "The launch vehicle determines everything in terms of the amount of mass and therefore the amount of expense, so let's use a much smaller launch vehicle. That will constrain it, and therefore help limit the cost."

We picked something called the Delta II, which had never been used at the time for a planetary mission. I had McDonnell Douglas [Corporation], that then made the vehicle, check and be sure that it had a reasonable enough launch energy to get a payload to Mars, and they sent me the numbers.

Then we started looking at other innovations. What we needed was a robust, inexpensive way to get to the surface of Mars and make some measurements. This was at the time when airbags began to be featured as certainly optional—and in one or two cases, I think Volvo, standard equipment—on automobiles.

When you go through the atmosphere of Mars, it's the worst possible case. If you are going to the Moon, the surface of the Moon, there is no atmosphere whatsoever, right? So it's all retrorockets, and you can plan everything around a single type of technology. If you're reentering the Earth, or you're going to a place like Titan or Venus, you've got a very, very thick atmosphere. Using a heatshield and parachute, you can do just about everything you need to do to get safely to the surface.

Mars's atmosphere is thin. It's enough to slow you down, but not completely slow you down, so you have to use a variety of techniques, all in sequence, to get to the surface of Mars. Now, the Viking mission that had landed in 1976 used a so-called terminal descent. That's the last few hundred meters of landing with retrorockets. But those are big and expensive—a lot of engineering, a lot of hardware development.

So I said to my group one day—of about four or five planners, engineers, advanced-concept people—"Well, let's assume that we have the heatshield to enter the atmosphere, and then we use the Viking-style supersonic parachute," which we thought would be available. "Then, though, when we get to the very end, we just inflate airbags and let it bounce, and use the airbag as a way to take—." What we were looking for is the way to take the shock of landing out of that last piece so that it never spiked above about 40 g's [gravity]—40 times the Earth's gravitational pull. That was what we thought the electronics could tolerate and not be super expensive. Each one of these statements had a lot of engineering and analysis behind it somewhere. And had a very good team, mixture of young and experienced folks.

We started looking at that, and I was summoned to NASA Headquarters. I got the charter to do this in late '89, maybe very early 1990. So around April of 1990, I was summoned to NASA Headquarters to present the results of our study. The concept not only included the much smaller launch vehicle, it had the idea of direct entry to Mars.

So it didn't go into orbit like Viking did. We were just going to fly one shot, and go through the atmosphere and land. Because again, having an orbital vehicle as a carrier for the lander would add another system and more expense. What we were trying to do was to take out the expensive items at every juncture.

In fact, as far as getting there went, I remember sitting in my office there in Building—I think it was 244—looking out the window one day and thinking, "How could we carry this there?" I thought of the phrase "cruise stage." In other words, something whose only purpose was guidance and a little bit of propulsion, and a little bit of solar power. Just enough to keep the landing alive and make the little small corrections you make when you're going from Earth to Mars, assuming you are on a good trajectory to start with.

So April of 1990, I was summoned to NASA Headquarters by Geoff Briggs and Dudley McConnell to present where we were. At the time, some of the straw-man science objectives had been put together by another person who arrived at Ames about the same time I did, an astronomer named David Morrison. He presented some science objectives, and then I got into "How are you going to do this?" and "What do you think it's going to cost?" Science objectives were generally agreed upon. Those don't change radically over the years. It's the implementation where everybody focuses on your cost and believability, and engineering realism, and whatnot.

Geoff Briggs had a kitchen cabinet of people he relied on for advanced concepts, and one of those was a very well-known person in the industry at the time named Jim [James S.] Martin [Jr.]. Jim, who is now deceased quite a few years, was the project manager for the Viking mission, and Viking was—still is, probably—the most expensive planetary mission ever.

I was just doing this calculation—it would be about $7 billion in today's dollars, which dwarfs anything but the James Webb Space Telescope. And that's Astrophysics, not Planetary. Jim was one of these larger-than-life people, about six [feet], four [inches], 280 [pounds] or so. Big guy, big voice, brush-cut military-style haircut. It was either Jim's way or the highway, you know? He left no doubt about who was in charge.

When I was giving this presentation, Jim started peppering me with questions. "Why do you think that'll work?", "Tell me about—", "This is—", "Are you sure about that?" I would walk through the logic, or the engineering, or the analysis, and in the back of my mind I was thinking, "Oh my god, I'm blowing this whole thing. I'm probably telling him more than I even know." Finished the presentation. Jim gets up and comes over to me, and sort of pokes his finger at me and he says, "You know, this might actually work."

I thought, "Oh, I didn't blow it after all!" This then lit the fuse on the idea of a small mission going to the surface of Mars, doing science. Later on, the idea of adding a small rover came into the picture as a concept, and we looked at it. This was originally part of a larger constellation, or a larger group of landers that was going to be called the Mars Environmental Survey, or MESUR for the acronym. This would be the first test case, the technology-test demonstration. It would be a pathfinder, which is kind of a term of art in the space business—something you do to show how you get from here to there.

Then, as I was developing this concept, the politics began to come into it. Because this meant that Ames was possibly finding a new mission that it could execute that was in the right cost. I think at the time it was $180 million or something. It would probably be in what's called the Discovery mission category these days, so you multiply that by three or something to get to the current Discovery "cost cap". But clearly, a mission that would be executable within the Ames capabilities. Ames did the entry heatshield work for everybody else in the Agency—it still does—and the other things would not be that challenging, we didn't think. I mean, the airbag and the cruise stage, I described the aspects to you.

There was a great hue and cry by the folks down at JPL that Ames was getting into their backyard. There was just an amazing piece of inter-center politics. The guy who was the head of Science at the time—and this would have been about 1990, '91—a fellow named Len [Lennard A.] Fisk visited Ames for the first and only time ever to see if Ames wanted to take on the role of developing this small mission to Mars as a part of a way of reducing the cost of doing science. I had already presented the overall concept to Fisk in—it must have been June of 1990, something like that. I presented to the first level of people in April, so yes, I think it was June of

1990. I have since gotten to know Len, and we were just on a review committee together. It's sort of fascinating to think back all those years.

But this was where there was a Science Definition Team created, and the chairman of the Science Definition Team was Steve [Steven W.] Squyres. Steve actually began his career at Ames. We may have crossed paths a little bit. I think he was finishing his post-doc [doctoral fellowship] about the same time I arrived. But this is where we really were working together in a significant way.

Fisk was really taken with this idea, particularly of doing good science at a much lower cost than what JPL was proposing. He came out to Ames and asked the Center Director at the time, a fellow named Dale [L.] Compton, "Do you folks want to do this?" Unfortunately, Dale was not what you would regard as the most assertive, confident, or aggressive person, at least not on the space side. I think he was fundamentally an aeronautics guy. He was always concerned about the Center taking on more than it could deliver, and he was hesitant and uncertain. He didn't give, unfortunately, Len Fisk a warm feeling that Center management was really behind this idea.

Net result was Fisk wrote a letter to Ames—to the Center Director, but copied me—saying, "Thank you very much for this great idea. We are going to assign it to JPL." Because they had gone in and made a full-court press to say, "Hey okay, great idea. We can do this. We can keep the costs down, and blah, blah, blah, and Ames can't do it." A lot of inter-center politics, which, now that it's 27 years ago, now the story can be told. Most of these people are retired or gone.

But to JPL's credit, they assigned Tony [Anthony J.] Spear, who had the right concept, and he put together the right team. And they did in fact deliver what was renamed Mars

Pathfinder, and for a very modest budget, and it all worked, and it deployed the little rover. That—I didn't realize it at the time—set me on the course years later to become NASA's first "Mars Czar" [Director for Mars Exploration], but that was 10-plus years later. The concept that became Mars Pathfinder was my initial foray into Mars work.

What happened in my career at NASA is that I went back and forth between line management—that is to say being in charge of a branch, or a division, or ultimately the entire Center—on the one hand, and being in charge of projects on the other hand. So I had a role in Mars Pathfinder—even though the whole project went to JPL, they still needed Ames support. Things like the heatshield, and wind tunnel tests, and various types of technology that Ames was expert in—and I made sure that they knew that.

I was in charge of that sub element of the overall project for a couple of years, and then also began to move up through the line management ranks. And I also—so now, moving to the first job as Branch Chief—called on my expertise that I had developed at Lawrence Berkeley in the detector realm. Those were detectors for ionizing radiation. The expertise of part of Ames was in infrared detectors, but the technologies were very similar. So this branch that I helped create did a number of things, including advanced concepts like I had just done for Pathfinder and infrared detectors. I stayed with that for several years.

Then I was asked by the Center to take on the role of working on the Ames's major [International] Space Station project. Ames had a longstanding role—as I said, from the very beginning when NASA was created, Ames was assigned two roles. One that became the Pioneer series for heliophysics, but the other was space biology. So Ames had a long, long history of developing life science payloads.

This was where Ames and JSC would get into it, "Who is in charge of life sciences?" Well, eventually, they reached a détente by saying, "Okay, JSC will handle all the human space flight stuff," so astronaut healthcare and feeding. Non-human—that is to say rats and mice, and bugs and worms, and plants and such—that can be a focus of Ames. That's how they more or less kept the peace over many decades.

Ames was assigned the job of developing something called the Centrifuge [Accommodations Module], which would be a major life science facility [the Space Station Biological Research Project] onboard the International Space Station, and they needed somebody to be in charge of the systems engineering of all of this. I don't remember how many pieces of hardware there were. There was a habitat for the animals, there was a lab bench. There was this centrifuge itself, with controls. Some animals would be at one g, some would be free-floating in space.

I stayed with that for a year or so, and then Joel Sperans, who was the head of the Space Projects Division—his deputy retired, and he asked me to come up and help him run the division, and also to see if I couldn't continue in this role of developing new things for Ames to do in the space realm. The Galileo probe was the last space project of Ames, and it was winding down. It was at the final integration and test stage just before launch, so the future was empty for other things.

Now, about this time, the Mars Pathfinder project and another project from a place called Applied Physics Lab [Laurel, Maryland]—it's part of Johns Hopkins University [Baltimore, Maryland]—it proposed something called NEAR, the Near-Earth Asteroid Rendezvous. But these two smaller-scale projects—done much more efficiently with minimized technology and a real eye on keeping the costs low, but still doing good science—was instrumental in NASA

Headquarters—and particularly a gentleman named Wes [Wesley T.] Huntress [Jr.]—creating the Discovery Program.

The Discovery Program was a peer-reviewed proposal, principal investigator-driven set of missions. The idea that Wes sold to the [presidential] administration—and got funded, and got into a line item in the congressional budget—was to hold a competition. They would start this line by just sticking in Pathfinder and NEAR to say, "Well, this is the kind of mission we are looking for." I asked Wes at the time about Ames participating, and he said, "Well, I'm never going to assign another mission to Ames. If I assign something"—a so-called strategic mission—"it will be either to Goddard or to JPL."

"But," I said, "what if we win one?"

He said, "If you win one that's open to everybody, that's good on you if you can do that."

I set about getting a really solid proposal process in place, and started working with the scientists that were in the Space Science Division—there are two parallel divisions, Space Projects and Space Science; I was over in Space Projects—working with the scientists and trying to find ideas that would fit within this realm of a highly focused, PI [principal investigator]-driven, lower-cost space mission that would follow certain constraints.

I think we ended up with four or five concepts, some involving Ames scientists, some involving scientists elsewhere. And as part of that, I was approached by Lockheed Martin [Corporation] next door, Lockheed Sunnyvale [California], which did a lot of the Fleet Ballistic Missile and other types of work.

I was approached by them saying, "You know, we have seen this announcement for these small, low-cost missions. We may have an idea." So I got together with the principals there. A lunar scientist named Alan [B.] Binder, and the head of this division over at Lockheed, who had been at Ames—that was the connection. His name was Angelo Guastaferro, otherwise known as "Gus." Gus Guastaferro.

We put together a proposal for a mission that would be managed through NASA Ames, executed and implemented by Lockheed Sunnyvale. This was part of the paradigm that they were trying to do, was to get the science, NASA Center, and industry all working together so that you would have—typically, these missions involved some kind of a triad like that, some kind of everybody bringing to the table what they know how to do. This would be NASA Ames and Lockheed Sunnyvale.

We put in this proposal, called Lunar Prospector, and we were selected as the first competitively-selected Discovery mission. I think it probably still holds the record as being the least-expensive Discovery mission ever. At the time, including launch vehicle, it was $63 million. Which, if you have been doing these interviews for a while, you know in space terms that's really small.

JOHNSON: It definitely is.

HUBBARD: Yes. Usually it's at least hundreds of millions, if not billions.

So I was, in the proposal, the NASA mission manager. The principal scientist was over at Lockheed, the project manager for implementation was over at Lockheed. We had enough funding for about a year-and-a-half-or-so study to further define the characteristics beyond what was in the proposal, and then three years of doing the development, and then launching in1998.

This was quite, again, a cutting-edge experience in not only proposing something, but now actually carrying it out. And it did a bunch of things. It demonstrated how cheaply you can build a mission, provided you scale the requirements appropriately. You don't try to do much, and you have the people with the right philosophy. It also kept Ames in the space business for another four or five years while other missions were being developed.

And it was a big success. It achieved all of its science objectives, it got an extended mission. Found strong evidence of water ice at the poles of the Moon, which had only been hinted at very indirectly. This was the first direct detection of what now has been followed up on by the Lunar Reconnaissance Orbiter, years later. And it really established the Discovery Program as something that could be done, could be led in a cost-effective way, and could really establish this smaller-scale set of missions as part of the Planetary Science Division portfolio.

And looking at my CV here, I realize I got a little bit out of synch. Let me jump into about 1994 or so, when, unfortunately, Joel Sperans, who was a great guy, had a stroke and then passed away. I was, for a couple of years there, the acting chief of the Space Projects Division, and I had to worry about not only the programs but also the personnel and all the other stuff that comes with being a division chief.

We had a major—Ames still has responsibility for a very major flying observatory called SOFIA, the Stratospheric Observatory for Infrared Astronomy. It's an eight-foot telescope in a heavily-modified Boeing [Company] 747, and it was the last strategic mission assigned by the Science Directorate at Headquarters, by the Astrophysics Division. Having been successful in getting the Discovery Program going and writing the proposal for what became Lunar Prospector that was selected in '95, I began to encourage other people and actively support—using little tiny bits of discretionary funding—principal investigators, scientists, engineers, technologists who had an idea that might turn into another Discovery mission.

There's a gentleman I met at Ames in 1988 named Bill [William J.] Borucki, who had this clever but technologically immature idea for detecting planets around other stars by looking at how that planet would eclipse—occult—its parent star and cause the light to go down ever so slightly. If you could measure that, you could know about the mass and the orbit period and so forth of the planet.

That mission went through four proposals—my recollection—and eventually won a slot in the Discovery Program. It became Kepler, which now, of course, is credited with having discovered thousands of planets around other stars. That was a major success by another person at Ames, where it took support over a period of many years to get it mature enough to make it actually happen.

JOHNSON: These projects were coming on, and you were, as you said, more in that management position. How did you help provide the support for them working through that process in that position that you were in?

HUBBARD: Well, two things were happening simultaneously. When Lunar Prospector got selected in '95, I was the acting chief of the Space Projects Division. So I had both jobs for a while, being the mission manager and the projects division acting chief. The Center had, at the top, the Director's discretionary fund, and projects that were interesting could get small amounts of seed money from the Center. But to get that, what you needed was a plausible idea, a team. You needed support of the other managers in the area.

When I identified Borucki as having an interesting idea but being technologically immature in '88, one of the things that I did as the Chief of the Projects Division was to help him write the proposal to the Center's Director's discretionary fund. I think you could get $20,000 or $40,000, or some small amount of money. Because back then, the civil servants were still funded through a separate account, their salaries were.

The other thing that I did was to designate some of the civil servants under my responsibility, under my authority, to work with Bill Borucki in developing the idea. I would say, "Okay, Larry"—or George, or whoever—"I'd like you to spend 20 or 30 percent of your time helping Borucki develop this idea. If he needs an analysis, or he needs a concept, or he needs something, you help him out." Those were the two ways in which I could encourage projects that I was not directly a leader, a manager, for.

JOHNSON: Yes, that clears that up for me, because I was wondering how you were able to do that.

HUBBARD: Yes. For the civil servant Centers, it's a little different than it is for JPL. JPL—people there don't have the kind of quasi-permanent jobs that the civil servants do, but they have a lot more flexibility in allocating some of their overhead money and some of the Caltech [California Institute of Technology, Pasadena, California] money to supporting new concepts.

The way that we did it at Ames was a little bit of this small amount of actual dollars that you could use to buy the time of a contract employee or a post-doc, but also you could just—at least in those days—assign a civil servant to help out. Running then, nearly concurrently, was the Pathfinder project that was moving along, and the Lunar Prospector mission.

As I look back on all this now, I think, "How did I have the time to do all of this?" But I had a very supportive wife, who passed away a number of years ago, but I just remarried in April. This trip we're taking September 1 is really our honeymoon.

JOHNSON: Well, congratulations.

HUBBARD: Yes, thank you.

Somehow, working around the clock, I was able to stay on top of both Lunar Prospector and the Ames roles in Mars Pathfinder that were offset by, oh, maybe two or three years. So they were in different phases. Then I got a promotion up to the directorate level as an associate director. I continued working on the things I just described, and also took on responsibilities for the projects that were in the directorate, and helping the guy who was in charge of the directorate—his name was Joe [Joseph C.] Sharp—with that responsibility.

As that wound down, as we get to the later '90s, I got another promotion or request to come to yet another level, which was the Deputy [Director] in the Space Directorate. The Space Directorate at the time had about 600 people in it, and it was responsible for all the Earth, Life, and Space Sciences, and the projects that were within them.

If I remember correctly, I think that was when I went from being a General Schedule employee to the Senior Executive Service [SES]. I'm sure you've run across that.

JOHNSON: Yes.

HUBBARD: That's a big deal when you get promoted to SES. I was in the Space Directorate at the time, and this is then when—I gather that I had already unknowingly attracted the attention of one of the most, I don't know, interesting—and some people would say "unusual"—Administrators NASA's ever had, which is Dan [Daniel S.] Goldin. If you have interviewed anybody from the era of the '90s, you have probably run across Dan's name.

JOHNSON: Yes, many times. And we've actually interviewed him, too.

HUBBARD: Yes, yes. I have seen him within the last few years, and he is much calmer now than he used to be. He was full of ideas, but very volatile, very mercurial. You never were quite sure what he was going to say, or what he was going to do, and you weren't sure if he was doing it for effect or if he had just lost his temper for a while. This personality characteristic was really difficult to deal with. Fortunately, there were people at Headquarters, Jack [John R.] Dailey among them—and I think Jack is still the Director of the [Smithsonian National] Air and Space Museum [Washington, DC]. But he was, at the time, a recently-retired, I think, four-star general. He had the ability to corral Dan from not firing everybody in the building twice a day.

It was at this point that I was asked by Goldin—who had been forced to come up with some new assignments for Ames. There is a back story there that I'll tell you in a minute. One of those was intelligent systems—advanced computer science, information systems, that sort of thing. Not running your desktop services, but really advanced supercomputing, and more in the artificial intelligence realm.

But the other one that a group of us at Ames and at Headquarters helped create was this field of astrobiology. Finally, many years after being told by the Vanderbilt chair of biology that there was no future in it—that would have been, let's see, '67, so 30 years later—I helped create this field of the study of life in the universe.

Then Goldin specifically said, "I want to create an institute around this, and I am not going to give you a nickel for bricks and mortar." Those were his exact words. "So you have to create a new interdisciplinary field, and you've got to not spend any money other than the bare minimum, and certainly not"—he told me, Goldin told me—"not going to pay for building a new building, because that's what you guys at the Centers always want. And then I've got to support it for the next 30 years, so I'm not going to do that."

So this became NASA's first virtual institute, the first institute devoted to this new discipline. He said, "Hubbard, go set this thing up. Make it happen." Then he said, "And after you get it all set up and it's all working and everything, then, to put it on the map, I need a"—and these were his exact words—"I need a King Kong biologist." I said, "Oh, okay. Yes, sir. We'll see what we can do."

I set about creating the template for a virtual institute that dealt with a multidisciplinary, interdisciplinary field. It was quite a challenge. A lot of working back and forth across lines of Space Science and Biological Science. At the time—of course, DNA [deoxyribonucleic acid] had been known for a long time—but the field of genomics and understanding of origins of life, and what are called extremophiles—all of that was just emerging, and we wanted to take advantage of it.

The person who was the head [Associate Administrator] of Space Science [Enterprise] at Headquarters at the time, Ed [Edward J.] Weiler, was really very interested in what [Carl E.] Sagan called "the pale blue dot"—finding other worlds that would be habitats for life, as well as seeing whether life ever emerged on Mars. We were just getting the Europa results then.

I had to come up with mechanisms to incentivize proposers—this would all be done through the peer review proposal process—to work across lines. We made it a requirement that you had to demonstrate you were working at least beyond your own department in a proposal—preferably several departments, or even several institutions—and show how those interdisciplinary research efforts were going to be carried out.

There was all the backroom stuff of how you were going to actually logistically make this work. I instituted the first ever—as far as I know—program of virtual communications capability. I mean, we deployed what was at the time—'98 was when I set this up—the best videoconferencing capability. It still was not as reliable as you would have liked, but it served the purpose of showing us whether or not you could use these kinds of videoconferencing tools to foster communications across multiple organizations.

The thing that I am really pleased by is that this template of a virtual research organization has now been successfully cloned at least twice more that I am aware of. There is the so-called SSERVI Institute—Solar System Exploration [Research] Virtual Institute—and then there is a NASA Aeronautics Institute that uses the same template. So the things that I created, or I had the lead in creating, in about 1998 have been used now again several times—successfully, I gather—over the last 20-plus years.

Now, it so happened that there was, I think, a visiting scholar at Stanford—who happened to be a Nobel Prize winner in physiology and medicine—named "Barry," Baruch [S.] Blumberg. He turned out to be a guy who was very curious about this whole field of origins of life, and life elsewhere. The Center Director at the time, named Harry [Henry] McDonald, and I went over to Stanford to visit Barry, and we pitched him the idea. My job was to create this Institute and make it happen, and make it work and so forth. Then Goldin's idea, which was not a bad one, was if you are really going to get respect for this, you need to have a very visible person at the helm. So we convinced Barry to take this on. I think it was up until the gentleman who is from Goddard who won the Nobel Prize for the COBE [Cosmic Background Explorer] mission, [John C. Mather]—I think this was the very first Nobel Prize winner who had ever actually been employed by NASA. So this was a big deal, and it helped put astrobiology on the map. And the Institute, of course.

Shortly after that—I don't know what they call it, maybe it's the "Peter principle" or something. I got pulled up to the Center level as the Associate Center Director for Astrobiology and Space [Research]. Harry McDonald and his immediate folks there really wanted to take advantage and highlight what the Center was doing in astrobiology. And given what I had done, you know, I helped be one of the public faces.

Of course, we had Barry Blumberg at the Institute, but this was a broader portfolio about all the different things we were doing under the umbrella of Astrobiology, as well as conducting the reviews of the flight programs. McDonald was a computer scientist by training, and he felt he needed somebody else to actually review these projects and see if they were on schedule and on budget and that sort of thing, as well as to help stimulate the new ones. It was at this point, at the Center level, that I began to be able to have some direct control over this Director's discretionary fund for helping to support new missions.

We've got another few minutes. Maybe this is a place to sort of wind up today's discussion.

JOHNSON: Yes. Yes, that would be good.

HUBBARD: Of course, this next one being the NASA Headquarters Mars Program Director, the Mars Czar. I don't know if you have ever heard of a reporter in the space field named Leonard David?

JOHNSON: Yes, the name is familiar.

HUBBARD: Yes. He works for *Space News* [publication], and he's worked for various of these institutions. But he was the one who nicknamed me the "Mars Czar." So that stuck.

Anyhow, two missions that had been launched as part of Dan Goldin's "faster, better, cheaper" philosophy—he told JPL and Lockheed Martin—by then, Lockheed and Martin Marietta had merged. In fact, when I was the manager of the Lunar Prospector mission, it was during that period that two things happened. One is Newt [Newton L.] Gingrich shut down the government, if you remember that one.

JOHNSON: Right. I do.

HUBBARD: And Lockheed and Martin Marietta merged. So lots of interesting stuff going on in the environment.

Given that Pathfinder was so successful at a low cost, Goldin told JPL and Lockheed Martin that he wanted now two missions for the price of one Mars Pathfinder. That resulted in Mars Polar Lander and Mars Climate Orbiter, and the people that were in charge of those took all kinds of foolish risks in order to save money and meet the schedule. And they were not given any relief on schedule, budget, or requirements. So both of those failed.

One probably went in too low in the atmosphere because of an incorrect conversion of English units to metric units—newtons to foot-pounds. That was Mars Climate Orbiter. The other failed because of a missing line of code in the lander computer. It didn't bother to check whether it was on the ground before it turned the rockets off. This is all documented in the book.

As a result of those two failures, there was a major failure review board conducted [Mars Program Independent Assessment Team], chaired by another icon of aerospace named Tom [A. Thomas] Young. Tom came to the conclusion that the Mars program had a whole bunch of flaws in it—lack of systems engineering, and not enough rigorous oversight, and "oh, by the way," nobody was in charge of the whole program.

There was a science piece, and there was an engineering piece, and there was a programmatic/budget piece. One of their recommendations was there had to be somebody at NASA Headquarters in charge of all of the Mars missions and the entire program, which was a line item in the budget.

I got tapped by them, by NASA Headquarters, to do that. It was one of these typical Dan Goldin meetings where you have a phone call with him, and he said, "I'm going to be in Huntington Beach [California] on Saturday. Be there, I need to talk to you." End of conversation. So you show up—and fortunately, I had enough friends in high places to have some idea of what was going to go on, and I was able to make a few notes to myself about what to remember to ask about.

He said, "I want you to be the first ever Mars program leader"—we ended up calling it Mars Program Director—"and you'll be in charge of all this stuff. You did a good job on the Astrobiology Institute, now make this work." I think his almost exact words were, "Fix the mess."

There was a lot to work on. There were not good communications, people didn't trust each other. There were also a lot of technical flaws, the systems engineering had not been followed rigorously, people weren't putting enough effort up front into what's called the formulation period now. And just a whole long list of things that had to be addressed. Also, we needed to create a new mission queue.

So it wasn't just fixing the pieces that were broken. We had to come up with a whole new program, which we did, and that queue of missions lasted for 15 years. The last mission that I feel any paternity for is Mars Science Lab, Curiosity. There are other missions now that are moving ahead—Mars 2020, and ultimately the Mars Sample Return.

But that experience—I mean, it was both a challenge and an opportunity, to be able to take a nearly clean sheet of paper—with a terrific team of folks at Headquarters, and in industry, and academia, and at JPL and the other Centers—and fashion something that has stood the test of time.

JOHNSON: Yes, it definitely has.

HUBBARD: The one encounter with JSC on that—of course, JSC has always been interested in the future of human exploration, that's the main job of the Center—was I got called to have a phone call, and then a visit with one of your most famous people, George [W. S.] Abbey. Have you encountered George or heard about George?

JOHNSON: Well actually, interesting side note—George Abbey is the one that started the Oral History Project. It's how we started in 1996.

HUBBARD: Oh, cool.

JOHNSON: It was his idea. So yes, we have had encounters with George Abbey, who is another interesting personality.

HUBBARD: Yes. No, he is. Never in doubt about what he wants to do, or where he wants to go. Some people would call him “Darth Abbey.”

JOHNSON: Yes. Much like Dan Goldin, when he started our project he had a meeting, and at the end of the meeting he told a couple of people to stay for a minute. He told them, “I want to do this. Now, go do it.” You know, “No money really, but go do it.”

HUBBARD: Right. “You’ve got the hunting license, okay? What more do you need?”

JOHNSON: That’s right.

HUBBARD: Why don’t we wrap up there?

JOHNSON: I think it’s a good place. You defined it as a challenge and an opportunity to start with a blank sheet of paper, so maybe talk about how you go about building something from a blank sheet of paper the next time we talk. Then we can go on from there.

HUBBARD: Sounds great. I look forward to it, and enjoy speaking with you. It's sort of fun to think back to all of these things, and to, as I said, wonder "How in the world do I do all of that?" Somehow, it got done.

[End of interview]