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STEVEN W. SQUYRES
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is August 22, 2017. This interview with Dr. Steven Squyres is being conducted by the NASA Headquarters Science Mission Directorate Oral History Project. Dr. Squyres is speaking with us today by telephone from New York. The interviewer is Sandra Johnson. I want to thank you again for agreeing to talk to us, and fitting us into your schedule. I know you are busy.

I wanted to start today by talking about your background and where your interest in, first of all, geology, and your eventual interest in working with NASA came from.

SQUYRES: Sure. I have always been a scientist. I can't remember a time in my life when I didn't consider myself to be a scientist. When I was six years old I wouldn't have told you I want to grow up to be a scientist. I would have told you I was a scientist, I just wasn't a good one yet. So I can't point to the origin of my interest in science because I cannot remember a time when I didn't have it.

I was very fortunate in that both of my parents were scientifically trained. My father got a doctorate in chemical engineering from MIT [Massachusetts Institute of Technology, Cambridge, Massachusetts]; my mother got a bachelor's degree in zoology from Wellesley [College, Massachusetts]. So when I was a kid growing up, for all the physical sciences questions, I went to Dad; for all the life sciences questions, I went to Mom. So I grew up in an environment where science just kind of permeated my thinking. There was never a time when I didn't plan to be a scientist professionally.

The other thing that I loved when I was very young, and still do today, was climbing mountains. I grew up in the flatlands of New Jersey, but my family visited Colorado, the Colorado Rockies [Rocky Mountains], when I was eight years old, and I just absolutely fell in love with the mountains. Being outdoors in nature, being out in the mountains and doing science were two things that I loved from my earliest memories, so it always seemed to me to be a natural thing to want to do geology, because it was a chance to go out and do science out in the mountains.

Now, when I was young, I was interested in all sorts of different kinds of science. But by the time I hit age 18 or so, I had pretty much settled in that geology was something that I wanted to do. I was very fortunate in that the summer that I was 18 years old—the summer between my last year of high school and my first year in college—I was able to participate in a research program, a research expedition, on the Juneau Icefield in southeast Alaska. The National Science Foundation provided fellowships, scholarships, for this sort of thing.

I applied and was selected for the program, and it involved doing an entire summer's worth of geologic fieldwork. Most of my work actually wound up being in northwestern British Columbia, but it was out in the mountains doing science every day. I just fell in love with it. It was just absolutely fantastic. I'm doing scientific research, I'm climbing up and down mountains, it just seemed like everything I wanted to do.

That was the direction I was planning to go when it came time to start my college career. I was an undergraduate student at Cornell University in Ithaca, New York, where I teach today, and became a geology major here. It went well at first, but after a couple years I began to become somewhat dissatisfied. The reason was that what it lacked, to me, was the element of exploration.

One other thing that always—always—captivated me as a child—and this is going back, again, to age 8, 10, 12—was I was always fascinated by exploration. I read and read and read books about early explorers. The Arctic, the Antarctic, underwater exploration—I was captivated by that stuff from a very early age. I grew up in the late ’60s, and this is when Mercury and Gemini and Apollo were going on, and I was transfixed by that stuff. I can remember—gosh, I would have been—when was John [H.] Glenn [Jr.]’s [orbital] flight, ’62?

JOHNSON: Yes, that’s right.

SQUYRES: Yes, ’62. Six years old, I was glued to the television set his entire flight, just captivated. I can picture it, where I was sitting, like it was yesterday. Gemini, Apollo—I was just absolutely fascinated by all that stuff. For me, the notion of being a scientist and the notion of being an explorer were always intertwined.

What happened was I got to Cornell, I spent a couple years studying geology and learning, but it began to feel somewhat dissatisfying for me. Because what I came to realize was that the geologists who have been working on this planet for a couple of centuries have done a pretty good job of figuring a lot of the stuff out.

I don’t want to give the sense that I think of geology as just being filling in the details. There’s all sorts of fascinating work to be done in the geosciences, and there will be for centuries to come. But to me, it didn’t have that element of the unknown. It didn’t have the element of the unexplored, and that was something I very much wanted.

After a few semesters, I began drifting towards doing something on the seafloor. I wanted to do something on the Earth's seafloor, marine tectonics kind of stuff, study of the geophysics and geology of the Earth's seafloor. And then—and oh, I remember this so vividly—there is this wonderful, terrible map—"wonderful" in terms of the work that was done, "terrible" for me—that came out. It was the first really good map of the Earth's seafloor, and it was done by [Bruce C.] Heezen and [Marie] Tharp. It came out in, I want to say, '77, something like that. Everybody's seen it.

It was the first map really put together—it was done, I think, at Columbia University [New York, New York]—showing the physical geography of the entire Earth's seafloor. It was a beautiful piece of work, and the minute that came out it went up on the wall prominently in every geology department in the world. I remember looking at that and saying to myself, "Oh, crap. Now what?" There it was, all mapped out. That map, magnificent piece of work that it was, was the last straw in driving me out of terrestrial geology.

What happened then—I was very fortunate. This would have been my junior year, my third year at Cornell. I had a hole in my schedule, in my course schedule. There was a language requirement at Cornell. I had just barely squeaked by a good enough score on a test, and so there was a semester of Spanish that I thought I was going to have to take but I didn't have to take. So I was looking for an extra course.

My girlfriend was visiting Cornell at the time, and I was showing her around the Cornell campus, and for some reason—I don't remember why—we stopped into the Space Sciences Building, where I teach now. Where I am now as we have this interview. Down on the first floor, there was a little bulletin board, and she spotted it.

On the bulletin board—I remember it was blue—a little three-by-five note card. Just a tiny little sign saying that a professor named Joe [Joseph] Veverka, who was a member of the science team for the Viking mission to Mars—this is 1977, and Viking was flying—that he was going to be teaching a course on the results of the Viking mission. I thought, "Well, that sounds interesting. I'll sign up for that."

I went to the first day of class, and it was a course exclusively for graduate students, he said. He didn't want any undergraduates, and he said, "Are there any undergraduates here?" One hand goes up. It was mine. I thought, "Okay, he is going to throw me out." He said, "Come see me after class"—a very stern voice.

I went to go see him after class, and he was about to throw me out, I can tell. Then what happened was at the critical juncture, a graduate student from the geology department who knew me pretty well saw what was happening, came up, and vouched for my studious nature with the professor. Fortunately, he relented and he said, "Okay, you can take the class." So I was allowed into this class almost by luck.

Because it was a graduate-level course, we were expected to do some kind of piece of original research for our term paper. The whole course grade was based on a single term paper. I had never done that before, so two or three weeks into the semester, I started to think, "Well, boy, I better start thinking about my term paper."

At that time, images from the Viking orbiters were coming down from Mars daily. Of course, no CD-ROMs, no internet, nothing like that. They were printed on big rolls of photographic paper and shipped in big cardboard boxes to the lucky 30 people, or whoever it was, that were part of the Viking science team.

At Cornell, Joe Veverka, the professor, would get these boxes of pictures, and then somebody would slice them up and put them in plastic sleeves and put them into binders. There was this room called the Mars Room in the physics building here where all these pictures were kept. And so I thought, "All right, I'll go and look at some pictures of Mars, and maybe I'll get some ideas for my term paper."

The professor gave me a key to the Mars Room, and I remember this so vividly. I walked in there. It was just metal shelves, and linoleum floor, and harsh fluorescent lights overhead, and boxes of photos, and that sort of thing. I just pulled some of the binders off the shelves and figured I would spend 15 or 20 minutes flipping through the pictures and see if I could come up with an idea for a term paper.

I was in that room for four hours, and I walked out of there knowing exactly what I wanted to do with the rest of my life. It was an absolutely transformational moment for me, because this was the blank canvas I had been looking for. I didn't understand what I was seeing in those pictures, but the beauty of it was that nobody did.

This was stuff that maybe a hundred people on Earth had even seen, and it was an unknown world, and it was fascinating to me. It was just captivating. And that was it. The term paper that I wrote became the first scientific paper I ever published, and that's all I've wanted to do ever since, is explore the planets.

I finished up my undergraduate career at Cornell, and then Carl [E.] Sagan [astronomer and science communicator] invited me to be his graduate student for the Voyager mission, which was what was happening in '79, '80, '81, the timeframe that I was going to be in graduate school.

I had applied to, I think, five different graduate schools, all with strong programs in planetary geoscience. Cornell was one of them. I really had no intention of going to Cornell because I had been there as an undergraduate, but I applied for completeness. But Carl was a member of the science team for Voyager, and nobody at any of the other schools that I applied to was, and Voyager was the big thing that was going to be happening when I was in grad [graduate] school.

I accepted his invitation, and then Joe Veverka—the very same professor who had taught that course that I managed to talk my way into—ultimately also became a member of the Voyager science team. So Joe was actually my advisor when I was in grad school in ’79, ’80, ’81.

That’s when Carl was making his *Cosmos* [*A Personal Voyage*] television series, so he wasn’t around much. He was the one who opened the door to Voyager for me, but Joe was kind enough to take me on as his graduate student for Voyager. So I was a grad student on the Voyager project, and that was just an incredible experience.

JOHNSON: How did Carl Sagan become interested in you?

SQUYRES: You know, I never figured that out. I never actually figured that out. By this time, I was spending all my time over in the Space Sciences Building. Probably he just read my application or something. I had a mailbox in the Space Sciences Building, and there was a note in it one day that, “Professor Sagan would like to see you.” Didn’t say why. I had never met the guy. I walked in, and he sits me down and said, “Hey, how’d you like to be my grad student for

Voyager?" My little heart starts going thump, thump, thump, thump, thump. Then I said, "Okay, I will think about it."

I went out of his office, went straight down the hall to Joe Veverka's office, walked into Joe's office, and said, "Joe, Carl has asked me to be his grad student for Voyager." This is before Joe was on the Voyager science team. I said, "Carl has asked me to be his grad student for Voyager. I'd like to do it because I want to work on Voyager, so I want to come to Cornell for grad school, but I want you to be my advisor because I know Carl's not going to be around much." He said, "Okay." I don't know what he was thinking, but he said okay.

That was that. Then Joe eventually got added to the Voyager team too, so both of them were on the team when we actually did Voyager.

JOHNSON: Talk about that for a minute, being a part of that imaging science team and what you were doing.

SQUYRES: My god. That was the most astonishing experience, in so many ways. It was the one mission that was really returning exciting new data when I was at grad school, so it was the place to be. Voyager was a huge step beyond anything that had come before. The Pioneer 10 and 11 spacecraft had flown through the Jupiter system already at that point, but they had very limited payloads, and really not much in the way of camera systems.

There was an enormous amount about the Jupiter system that was unknown, and almost nothing was known about the moons of Jupiter. To me, Voyager is still the greatest planetary mission that's ever flown. The first good look at four big planets and their moons—Jupiter, Saturn, Uranus, Neptune. You only get to do something for the first time once, and that goes to Voyager.

Being able to be there and be part of it was just a life-changing experience for me in so many ways. Scientifically, because my background was geology, my interest was the moons. The solid moons, primarily the big ones—Io, Europa, Ganymede, and Callisto. The Galilean satellites, as they are called, that were orbiting Jupiter. It was an astonishing experience.

These are big objects. Two of them are about the size of Mercury, and the other two are about the size of the Earth's Moon. I mean, these are substantial objects. Never been seen before, knew almost nothing about them, and they went from literally little points of light that you could sort of barely resolve anything—you study them in a telescope but that was it—to entire worlds that you could map and begin to comprehend in a real geoscience sense in the space of 48 hours. It was absolutely incredible.

Of course, I didn't sleep for the whole 48 hours. The entire flyby, I was just riveted by the whole thing. Boy, you talk about the thing that had always driven me was this sense of exploration, and this was just an avalanche of new information about these new worlds. That was an incredible experience in itself.

The other thing was that I had the remarkable good fortunate to get to work with a few supremely talented and supremely generous scientists when I worked on Voyager. Of course, there was Joe and Carl. Larry [Laurence A.] Soderblom—he must have been very young at the time, he must have been in maybe his mid-30s or late 30s—but he was the deputy leader of the Voyager imaging team.

Then somebody who just made a huge difference to me in my career was Gene [Eugene M.] Shoemaker. Gene Shoemaker, U.S. Geological Survey in Flagstaff [Arizona], and also

Caltech [California Institute of Technology, Pasadena, California]. Gene was one of the scientists who was involved in Voyager, and I got to work very closely with Gene, and it was just one of the best experiences.

Let me tell you a Gene Shoemaker story. This is something I will remember for the rest of my life. I'm a first-year graduate student. The Voyager spacecraft—it's probably Voyager 1—is bearing down on Jupiter, '79, so I would have just turned 23 years old. I'm at the [NASA] Jet Propulsion Laboratory [Pasadena, California] for maybe the second time in my life, and things are really starting to heat up. Images are coming down, stuff is happening. We are a few days out from the close flyby.

Now, the Voyager spacecraft didn't carry fancy solid-state data recorders like spacecraft today do. They had tape recorders—physical reels of tape that they recorded data on—and the tape recorder had a limited amount of space on it. A problem came up. This is late at night. The only two people who were around were me and Gene. We are in the imaging area of JPL, and a problem had come up at one of the Deep Space Network tracking stations where you receive data from spacecraft.

Data had come down and it had not been properly received, and so there were images of some of the moons of Jupiter—these are early images at low resolution—that hadn't made it down from Jupiter. They were still recorded on the tape recorder, but they hadn't come down. But we also had a set of commands that were about to go up to the spacecraft to instruct it to take some other pictures that were going to get written onto that portion of the tape recorder. We could decide—do we send those commands up, or do we keep them on the ground?

What it amounted to was we had to choose. We could have the pictures that had been taken already and not get the new ones, or we could have the new ones, which would then overwrite the pictures that we had taken already. But there was no way to get them both. A choice had to be made. That choice was based on science, because these images were taken from different angles, different solar illumination, different rotation of the moons. "Which is more important? Which one had the most science?" Something's got to go.

The engineers want to let the scientists make this decision, so they came in, and there is the great Gene Shoemaker plus this kid. They go and say, "Dr. Shoemaker, we need your guidance on something." They explained the situation, and "We'd like you to come over, and we'll show you exactly what images are at risk, which images you can choose between. Could you please come and give us some guidance on this?"

And Gene, bless his heart, turns to me and said, "Steve, could you come help me out with this? I could use some help on this." And so I went along with him. I had been looking at these images, I had been thinking about them, and if you gave me the chance, I might be able to offer an opinion. He didn't need to do that. It was the first time that a real scientific professional who I deeply appreciated treated me as a peer. I got a lot of that on Voyager, and that just meant so, so much to me.

To this day—gosh, what is it—40 years later now. Forty years later, one of the most satisfying things to me about doing the Mars rover missions, Spirit and Opportunity in particular, is that I get the opportunity to do the same sort of thing for very young students who are interested in this field. I was never, ever able, of course, to repay Gene and all the other Voyager scientists for what they did for me when I was that young, but I feel like now, 40 years later, with the rovers having worked out the way they did and having had so much student involvement in the mission, I finally just feel like I've kind of paid off that debt.

JOHNSON: It’s always nice when—in any field—you are treated as a peer instead of a student. So yes, that’s pretty amazing.

SQUYRES: It meant a lot to me.

JOHNSON: You worked on the Voyager all through your PhD?

SQUYRES: Yes. I worked on Voyager all through my PhD. I wrote my PhD thesis about Ganymede and Callisto, which are the two big, icy moons of Jupiter. Boy, talk about being in the right place at the right time.

Simplemindedly, you can divide the solid bodies in the solar system down into two types. There are the ones that are made of rock and metal, and then there are the ones that are made of ice and rock. Ganymede and Callisto were the first two icy ones anybody had ever seen, ever. I chose those for my PhD thesis, and I got to write the first papers about all of those, coauthored with other prominent scientists, Gene among them. I was just there at exactly the right place at the right time.

Since this is oral history, let me tell you an embarrassing story about myself. I’m really embarrassed by this, but what the heck. We’ll put it out there. This is how I chose my thesis topic. When we got the first data from Voyager—the Voyager 1 and Voyager 2 Jupiter flybys—there were some fabulous discoveries. These volcanoes erupting on Io, these fractures on Europa suggesting that maybe there was an ocean of liquid water underneath the ice, just fantastic stuff.

You get this big team of scientists, and everybody is going to write papers, and you don’t want people stepping on each other’s toes. You want to have some sense of organization—who is doing what, who is writing papers about what topic, and so forth. So Larry Soderblom, who was the leader of the geoscience part of the Voyager mission team, he gets all the geo types together. The high and mighty professors—the Gene Shoemakers and Joe Veverkas—all the way down to the lowly grad students.

He says, "Okay, guys. I'm going to put an envelope on my door, and what I want you to do is take a piece of paper, write your name on it and which moon you want to work on. Pick one moon that you want to work on most, and write your name on it, and put the slip of paper in the envelope. Then, once I've accumulated enough, I'll go through them, and we'll sort of figure out how we are going to divide it up."

This is the embarrassing part—I didn't put my name in initially. I waited and waited until people put their names in. Then one evening, when there was nobody around, I went and I pulled out the pieces of paper. I go through them, and it's like "Io," "Io," "Io," "Europa," "Europa," "Io," "Europa," "Europa," "Europa," "Io," "Io," "Europa," "Europa," and then "Ganymede, Gene Shoemaker." I realized if I picked Ganymede for my thesis topic, maybe I can work with Gene. That was how I picked it.

I did, and I picked Ganymede as the focus for my PhD thesis. I figure all the really sexy, jazzy science is the Europa and Io stuff. There are a lot of people who want to do that. Gene is really fascinated by Ganymede. "If I work on Ganymede, I can work with Gene." And I did. I spent a summer in Flagstaff, Arizona, working with Gene and writing papers together, and it was just a fantastic experience. But yes, that's how I picked my thesis topic.

JOHNSON: Actually, it sounds pretty smart to me.

SQUYRES: Yes. Well, anyway, a little bit underhanded, but it worked out well in the end.

What happened next was also very tightly tied to Voyager as well. Of course, the big discovery on the moons of Jupiter from the Voyager 1 flyby was the volcanoes on Io. Io—nobody had a clue what was going on. Io turned out to be the most volcanically active body in the solar system, with six or seven volcanoes spewing sulfur compounds a couple of hundred kilometers out into space as we were flying by the moon. It was incredible.

The story there, there was a very interesting story. There was a group of scientists—one of them at UC [University of California] Santa Barbara named Stan [Stanton J.] Peale, and then Ray [T.] Reynolds and Pat [Patrick] Cassen at NASA Ames [Research Center, Moffett Field, California]—all of them theoreticians. Some years before, they had developed a theory, a mathematical formulation, of how tidal heating works. The idea that when tides affect a solid body and sort of flex it back and forth, it can be heated.

They had developed all the mathematical theory behind tidal heating, and done all the calculations. They were interested in how much tidal heating there would be on the Moon, on the Earth's Moon, which is in a somewhat elliptical orbit, and so it experiences tides. They did all the math, and they did all the physics, and they worked it all out. And it turns out it was, like, 5 degrees or something. They wrote up the paper and nobody paid much attention to it, and that was that.

Then some years later, Voyager is bearing down on Jupiter, and it's going to take pictures of the moons of Jupiter. One of them—I'm not sure who it was—one of the three of them got the idea. "Just for the heck of it, let's just dust off our old tidal heating equations and try applying them to the moons of Jupiter, and just see if anything interesting pops out before Voyager gets there."

They ran the calculation for Io, and they were astonished. The calculation said that the thing should basically melt. That the tidal heating would be so intense and so violent that this should be the most volcanically active body in the solar system. "Okay, we got it wrong. We have got to go back and check our calculations." They go back and they check it, and they check it, and they check it, and that's what the numbers say.

They dashed off a paper to *Science* [academic journal]. They wrote it in, I think, just a few days, blasted it off to *Science*, got it reviewed. It was published in *Science* one week before the flyby, all right? This is pre-internet, so word doesn't travel all that fast. Unless you go to your library or you go to your mailbox and you pick up the latest issue of *Science*, you are not going to see this paper.

Meanwhile, the entire Voyager science team, we are so focused on getting ready for the flyby nobody is reading journal articles. We fly by Io, there is the whole story of how the volcanoes were discovered; it was fantastic. And sure enough, Io is the most volcanically active body in the solar system.

Then somehow we heard about this paper, and we went and read it. I was just blown away. It was just such a gutsy thing to do to trust your calculation to make this outlandish prediction when you have got a very good chance of being proven wrong in a week. I was just blown away by that.

Now, one thing that I learned—one of the things that I learned through my association with Gene Shoemaker, with Larry Soderblom, with Carl Sagan, with Joe Veverka on Voyager—was that when you surround yourself with really smart people, good things can happen to you. I was so impressed by these guys that I decided, okay, post-doc [doctoral fellowship], I want to go work with these guys.

And so my post-doc was at NASA Ames, and I had Joe Veverka write me a letter of recommendation. In fact, I don't think it was even a letter. I think he actually called up Ray Reynolds or Pat Cassen and talked to them, and that's how I ended up working for NASA. Those guys just impressed me so much, I just realized that, hey, if I go and work with those guys, good things are going to happen, and they did.

After I finished my PhD in 1981, I went out to NASA Ames. I was a post-doc there for two years, and then got hired on, and I was civil service for three years.

JOHNSON: What were you working on during that time?

SQUYRES: Everything under the sun. It was great. When you are in grad school, your job is to get out of grad school, and the path out of grad school, it goes through the thesis. You have got to get the thesis done, and the thesis can't be just a complete hodgepodge of different topics, no matter how entertaining they might be. You need to bear down and get a substantive piece of work on one topic done. That's what I did when I was in grad school, and I just focused on Ganymede and Callisto, and that was it.

I got to Ames, and the shackles were off. I could do whatever I wanted. So I was doing Mars, I was doing moons of Jupiter, I was doing moons of Saturn, I was doing comets, I was doing studies for future planetary missions, I was doing theoretical calculations, geologic mapping—I was doing just everything all at once. I just exploded in a whole bunch of different directions scientifically, and it was great.

When you first get out of grad school, those early first few years—you finished your PhD, so you have your entry ticket. You are now a real PhD scientist. But it's before people start asking you to serve on committees and people start asking you to review papers, and people start asking you to do this, that, and the other thing. It's a time of enormous potential and freedom, and I recognized that. I realized it at the time that this was a chance for me to really get a lot of stuff done. And I did, and it was a very exciting time. I loved being at Ames. I just loved it there, it was fantastic.

JOHNSON: But you didn't stay.

SQUYRES: Well, yes. Here is what happened there. I was very happy at Ames. I really liked being at Ames, I liked working for NASA. The only downside to it, really, was that it's right in the heart of Silicon Valley [Santa Clara Valley, California], and trying to live in that real-estate market on a government salary was tough. I was married, didn't have any kids yet, but the kids were definitely on the horizon.

I wanted to stay at Ames, I liked it very much. I sort of had this vague long-term plan that I'd stay at Ames maybe 10 years, and then, I don't know, convince the University of Colorado that it couldn't live without me. Because they are in Boulder, Colorado, and there's lots of mountains, right? That was kind of this vague, long-term concept.

What happened was one day I got a call from Cornell, and they had a faculty job open and wanted to know if I was interested. I said no. I had been at Cornell as an undergraduate, I had been there as a graduate student. I had only been away for about four or five years at this point, and, I don't know, going back just didn't seem like the right thing to do from a career perspective. Things were going great at Ames. I really didn't want to leave, so I said, "No, I am not interested in the job. Thanks for contacting me."

I went home that evening and told my wife. My wife was born and raised in Ithaca, New York. Her family is there. She said, “You did what?” So the next morning, I called back. I called Cornell again and said, “I think I might be interested.” I went and visited, and it went well, and they offered me the job. I sort of oscillated over it for a while. I wasn’t sure really whether I wanted to do it or not, because there were a lot of pluses to staying at Ames.

But in the end, the appeal of Ithaca as a place to raise a family—small town, inexpensive real estate, terrific schools, low crime rate. Buy a nice house out in the countryside on five acres of land—you can’t do that in Silicon Valley. And so that’s what we did. Came back and moved back to Ithaca in 1986, and I’ve been here ever since. Raised two daughters, and they both loved it. In hindsight, as much as I enjoyed Ames, and as much as I still miss Ames, it was the right decision.

JOHNSON: Did you know when you went back that you were going to continue to have this association with NASA through all these years?

SQUYRES: Actually, that’s a really good question. When you make the choice to leave NASA—I was as civil servant working for NASA. I had a NASA badge and all that. When you make that decision, you sever some of your ties. I was no longer going to be a NASA employee. I still wanted to work on NASA missions, but what that meant was that if I wanted to be associated with NASA missions going forward, I was going to have to write proposals, and I was going to have to, through competitive peer review, get associated with these various missions.

Now, I had, at that point, already had a little success with that. There was a mission called Mars Observer—a very ill-fated mission—that I guess the science team for Mars Observer was selected, I think, in 1985. This is when I was at Ames. I had written a proposal to become a member of the science team for the gamma ray spectrometer on that mission. The proposal was selected, and so I became a member of the science team for that.

So I was already having a little bit of success getting associated with NASA missions, and when I moved from Ames to Cornell, my affiliation with the Mars Observer missions moved with me. I remained a part of that science team, of course, so it was just a different way of getting myself associated with NASA.

The other thing that happened around this time, though—this was when the notion of the Mars rovers was first born. I wouldn’t say it was prompted by the move to Cornell, but it coincided with it pretty closely. In the 10 or so years between when I first walked into the Mars Room and decided I wanted to be a planetary scientist to when I finished my PhD and moved to Cornell and was a professor, I had done a lot of science that was theoretical in nature. A lot of geophysical calculations, that sort of thing, and I did a lot of science that was analysis of data, primarily imaging data from spacecraft. And I was beginning to find it frustrating, I was beginning to find it unsatisfying.

I was doing all this research on Mars, for example. Mars was the place that most captivated me. I had been sort of sidetracked to the moons of Jupiter because that was what was happening when I was in grad school, but it was always Mars that kind of did it for me. And the reason was that Mars, of all the planets, looked like it was the one that had the greatest potential to have had habitable conditions at the surface. I just found that fascinating, and always had.

I remember I would do all this science with the Viking images and theoretical calculations of conditions on the Martian surface, and the problem was it was so hard to get to anything like a definitive answer. You would do these theoretical calculations and you’d come up with an answer, but then you'd make a few reasonable tweaks to some of your parameters and you'd get a different answer. You would look at these pictures of things from orbit, and you would look at it and you would say, "Well, it could be this. But it could be this. Or it could be this."

The thing was—remember I had years of training going back to the Juneau Icefield when I was 18 years old of doing field geology. Boots, a hammer, working with the rocks close up. I'm looking at these things from orbit and I'm trying to figure out what it means, and I knew—I just knew—that if you could just give me five minutes with my boots and a rock hammer on the ground on Mars, I could answer the question. You get down and you look at the rocks up close, and there are things that you can see—if you know what you are looking for—that will give you definitive answers that you just can't get from orbit.

That frustration with working with orbital data, working with remote sensing data, and my desire to really, really, really dig into this issue of the habitability of Mars, those two things together were what led me to start thinking about Mars rovers. I first really began the quest to get some serious geological instrumentation onto the surface of Mars in 1987, which was like a year after I showed up at Cornell.

JOHNSON: Did you have any involvement with [Mars] Pathfinder?

SQUYRES: No, I didn't. I did not. I tried, but I failed. We are getting into the stuff that I talk about in my book now [*Roving Mars: Spirit, Opportunity, and the Exploration of the Red Planet*], but yes, the Mars Pathfinder mission, they were looking for a camera on that lander. That was the first Mars surface science proposal I submitted, the first of four.

After I had been at Cornell for a few years, I took my first sabbatic leave at an aerospace company. I left Cornell for a year and went out to Boulder, Colorado, and I worked at Ball Aerospace [& Technologies Corporation]. I was there for quite some time, and worked to develop a camera system, which we proposed for Mars Pathfinder.

The proposal was unsuccessful. We made an egregious error in writing our proposal. The camera that we proposed had to fit into a specific three-dimensional volume on the spacecraft. There was a diagram that NASA provided that showed what the volume was, and we looked at that volume, and we designed a camera that would fit nicely into it. What they had done was they had printed the diagram sideways on the page—they had rotated it 90 degrees from the actual orientation on the spacecraft—and we misinterpreted the diagram.

Yes, we misinterpreted the diagram. They wanted a camera that was short and fat, and we proposed one to them that was tall and thin. Because we misread that diagram, our proposal was basically rejected out of hand. That was a learning experience, boy. So I was not involved in Mars Pathfinder. In fact, the day that Pathfinder landed, I was in a cabin in the Rocky Mountains working hard on the proposal that eventually became [Mars Exploration Rovers] Spirit and Opportunity.

JOHNSON: That camera system, just as an aside, did it ever get used on anything else?

SQUYRES: The only thing that actually survived was the name. It was a true panoramic camera. It had a 3,000-pixel detector, just a line array that you would rotate very smoothly and precisely to build up this big, big, big panoramic image all in one sweeping motion. It was a beautiful camera, and because it was a panoramic camera, we called it Pancam.

The Pancam cameras that we flew on Spirit and Opportunity wound up, in the end, using a completely different design because of a whole bunch of engineering and financial constraints that we faced, but sort of to honor the team that I worked with to develop that first Pancam camera for Pathfinder, we kept the name. But the name was all that survived. It was a beautiful camera.

JOHNSON: We can talk about the Mars work too, but you were also busy doing other proposals, or working as investigator on other things, during the late ’80s.

SQUYRES: Yes, yes. The thing that I really wanted to focus on, of course, was Mars and Mars rovers. I spent 10 years, and there were four proposals before we finally got selected on the fourth try.

But there were other things going on in that same timeframe too, and those proposals were all successful. I proposed to be on the science team for the Magellan mission, on the radar team for the Magellan mission to Venus, and was successful as part of that. I was on the gamma ray and x-ray spectrometer team for the Near Earth Asteroid Rendezvous mission, so that was a big thing for me.

And then even though Mars Observer was unsuccessful, the gamma ray spectrometer that I was part of eventually did fly on the Mars Odyssey mission, and so I was involved in that as well. There were a number of missions that were happening in that timeframe, in the early to mid-’90s, that I had proposed to be part of the team. I proposed to be on and I was selected for the imaging team for the Cassini mission to Saturn. So there were a bunch of things that I was working in the same general timeframe.

JOHNSON: Do you want to talk about or discuss any of those, and what your impressions were, or maybe some of your favorite memories in some of those?

SQUYRES: Sure, sure. Magellan, for example, that was a fantastic experience. Magellan, of course, was a radar mapper. There had been a few missions to Venus previously. The U.S. mission, Pioneer Venus, had had an ice altimeter, and it produced kind of a low-resolution topographic map of the planet.

Then there had been Soviet missions which had done some radar mapping on portions of the planet, and put—successfully—several landers down on the planet's surface. But the objective for Magellan was to produce a high-resolution imaging radar map of the entire planet. It's a big planet, it's the size of Earth.

The spacecraft got there, and the way Magellan worked was it would lay down these little spaghetti-like strips of radar coverage. It would come in over the pole, it would lay down a strip—I'm probably going to get the numbers wrong, but it was about 25 kilometers long, I think, and thousands and thousands and thousands of kilometers long from the north pole down towards the south pole. You would get this little narrow strip.

Then the spacecraft was in an elliptical orbit, so the orbit would carry it farther away from the planet. It would turn, it would send that radar image—this little noodle of data—down to Earth. Meanwhile, the planet's rotating. Venus rotates very slowly. The planet's rotating underneath the spacecraft. The spacecraft comes in, and the orbit was sized and timed just so that when the spacecraft came in over the pole again, the planet would have rotated 20-some odd kilometers, and so you would lay down another strip right next to it. And then another, and then another, and then another.

It was fascinating to me because it was like the exact opposite of Voyager. Voyager, you would get this distant view, and then, wham, you would get the whole moon of Jupiter just all at once. Whereas with Magellan, it was this very slow reveal. It was like a crack in the door opening very slowly, and this radar strip would get slowly—every day—wider, and wider, and wider, and wider, until a whole planet eventually came into view. That was fantastic.

That was a wonderful experience also. By that time, I was a PhD scientist. I was on the faculty at Cornell, so it wasn't a grad student kind of experience. But still, I was working for the most part with scientists who were more senior than me, who knew more about Venus than me, so it was a chance to learn a lot. It was a chance to work with radar data, which is in many respects very different from camera data, imaging data. I learned a great deal, and that was a terrific experience.

The Near Earth Asteroid Rendezvous mission – that was another great one. That was more gamma ray and x-ray spectroscopy, like I had done at Mars. This was on the asteroid Eros, and that was a whole new thing. That was the first time anybody had ever orbited an object like that, and so we had to learn how to fly a spacecraft around that kind of object. How to do mapping when you have got this irregular, complicated-shaped spinning object underneath you.

Each one of them was a new challenge, and each one of them, you are seeing things that nobody had ever seen before. That combination of technical challenge and geographic exploration, seeing stuff that nobody had ever seen before, just appealed to me greatly. I don't know, that impulse to get there first has always been something that has driven me. In some ways, it's a very selfish motive, right? "I want to see it first."

But that's always been a big, big part of what I've loved about what NASA does, what I've loved about being involved in these missions, is the chance to get a first look at something that no human eyes have ever seen before. I have always found that very compelling.

JOHNSON: Has it ever been compelling enough for you to want to see it first yourself, as an astronaut?

SQUYRES: Yes. The problem is that in the timeframe when I could have flown as an astronaut, it was all low-Earth orbit. There was no chance to actually go there, and we weren't going to the Moon—I was too late for the Moon, too early for Mars. In order to do the kind of exploration that I wanted to do, I had to give up on the notion of going myself, and focus instead on building robotic systems that would serve as a proxy.

What I have done to soften that blow is, throughout the years that I have been doing this ever since grad school and before—once I made the decision to conduct my career with robotic exploration—is to do my exploring sitting at a desk in front of a computer. I did also at the same time make a decision to continue to be engaged very much, whenever I get the chance, in real boots-on-the-ground exploration on Earth.

I have been going on scientific expeditions, doing field research, of many, many sorts—most of it NASA-related in one fashion or another—for many years. In the Arctic, in the Antarctic, deep ocean. I've done a whole bunch of that just so it provides me a way of scratching that itch. In fact, as we are speaking now, four days ago, I just got back from a week of NASA-funded fieldwork in some caves in Idaho.

So, yes. I've done, like I said, Arctic, Antarctic, a lot of underwater stuff, caves now. It provides me with a way of satisfying that selfish need to go do it myself.

JOHNSON: I do want to talk about the NEEMO [NASA Extreme Environment Mission Operations] experience and some of those other experiences.

SQUYRES: Oh, yes, yes. NEEMO is a great example.

JOHNSON: Yes. We can get to those, but just out of curiosity, what were you looking for in caves in Idaho?

SQUYRES: Okay, so this is an interesting problem. It has to do with the issue of the habitability of Mars. Mars has a very, very challenging environment at its surface. At the surface of Mars, there is a very intense radiation environment. Not just ultraviolet, but also cosmic rays that can penetrate significant depths into soil and rock. Very damaging for life. Enormous daily temperature swings. The temperature difference between daytime and nighttime is 100 degrees Celsius.

So the Martian surface environment is very dangerous and unstable, whereas down in a cave—thermally stable, completely shielded from cosmic rays. It's a much, much more favorable environment, in some respects, than the surface environment. Now, why would you expect caves on Mars? Mars is covered with basaltic volcanism, and basaltic volcanism produces what are called lava tubes. We know that there are these lava tubes on Mars, and then the lava tubes are caves.

We are working in a place called Craters of the Moon [National Monument and Preserve] in Idaho where there are large basaltic lava fields that were laid down 2,000, 3,000, 4,000, 5,000, 10,000 years ago, and there are lots and lots of these lava tubes, these lava caves. On Mars in lava caves today, it's very, very cold, and probably not all that suitable an environment for life today.

However, if you go far enough back in time, to go back to Mars 3 billion, 3.2 billion, 3.5 billion years ago, those lava caves could have been some of the best environments for Martian life to be able to persist and survive. Because it's shielded from the cosmic rays, it's more thermally stable, and so forth.

Now, the question then becomes what do you do? What do you go and look for? If you want to look for evidence of former life in Martian caves, what do you look for? It turns out in these caves in Idaho, there are some fascinating mineral deposits. These are minerals like sulfates that are precipitated from very small amounts of liquid water.

A little bit of liquid water will percolate through the ground, and then it will evaporate onto the walls of these caves or on the floor of these caves, and it'll form these dense white mineral deposits. It turns out there are microbes that are living in the caves, and they become trapped in these mineral deposits like bugs in amber. Trapped inside of them.

The team that I was working with, what we were doing is using sterile procedures—so we are in caves and wearing hard hats and kneepads, but we've also got surgical gloves and surgical masks and sterile tools—and what we are doing was getting samples of these mineral deposits. Then the scientists take those back and extract DNA [deoxyribonucleic acid], amplify it using techniques like polymerase chain reactions (PCR), sequence it, do genomic determinations of what microbial communities exist in these things, and help you to understand the ways in which what you might call biosignatures—the evidence for past life—can be preserved in those kinds of mineral deposits.

So that if you go to caves on Mars, what kind of mineral deposits might you want to look for? What kind of stuff might you want to look for preserved in those minerals that could provide evidence that something had lived in a cave on Mars billions of years ago? It's kind of laying that groundwork.

It's not very different conceptually from work that I did in Antarctica back in the mid-'80s, when I knew that someday I wanted to try to send a rover to Mars and look for what kinds of processes might have gone on in Martian lakes. So we were studying Antarctic lakes as a way of helping us to prepare for the kinds of tools that we might want to send to ancient dry lakebeds on Mars someday.

It can be valuable to go to analogous terrestrial environments to learn about what you might find in the Martian environment, to prepare you for the exploration that you want to do maybe a few decades down the road.

JOHNSON: Do you think that exploration, as far as getting in these lava tubes on Mars, would that be robotic or human?

SQUYRES: It's going to be tough either way. I'll tell you one thing for sure, it's not going to be solar-powered rovers. It's dark.

My guess is that initially, it would probably be robotic systems. For a place like Mars, you usually want to send robots first, and then humans later. That's what we're doing now. I don't know, I don't know. The first thing you have got to ask is "What are we looking for?", "What are the scientific questions we are going after?" And then you say, "Okay, what's the best way to do it?" We are really just getting started on this stuff. You could do it either way, or both.

But it was fun. I'm not doing the science, really. I'm not extracting the DNA, I'm not doing the PCR. I'm a field assistant. I'm out there with scientists who are in their 20s and 30s, like I was when I first started doing this stuff, and I am a field assistant. I have got decades of experience doing fieldwork in all kinds of settings. I carry rock samples, I help navigate us to the mouth of the cave. I go in and I'll scribble down notes that are dictated to me by the real scientists doing the real work.

It's wonderful fun for me. I'm 61 years old, I'm still physically fit enough that I can do this kind of stuff and enjoy it. I don't know how much longer that's going to last, but I can now. It's just wonderful fun working with these just talented, energetic 30, 35-years-younger-than-me scientists, all full of ideas, and just go out there and help. Just go out there as their field assistant. I get to stretch my legs and have fun, and they get some data. I love it.

JOHNSON: Just curious, when you started working and doing research for NASA, it was an interesting time. There was a lot going on as far as near-Earth orbit, as far as the [Space] Shuttle, and then working on ISS [International Space Station]. But there were also all these other missions in your field, going on to other planets. I would imagine there was a lot of excitement because, as you said, Voyager, all these things were just beginning. Do you see that excitement now in these younger people that you are working with?

SQUYRES: Oh, more so. More so. Much more today than back then. We look back on the early ’80s, and let’s talk about planetary missions. Name them, name the planetary missions that flew in the 1980s. It was almost nothing. There were no Mars missions going, Viking was over. Voyager kept flying, but it was launched in ’77.

Magellan didn’t come along until the end of the decade. Galileo was supposed to happen, but then it went through launch delay after launch delay, and then it took forever to get to Jupiter because of the trajectory that they had to fly. It was actually a time of very, very little planetary [science] activity.

Now, as you say, there were lots of other exciting things that were going on at the time. The Shuttle was really a going concern, the Hubble Space Telescope was getting designed, and then eventually launched. There were plenty of exciting things happening at NASA at that timeframe, but it was a time of a lull in some respects in the planetary program.

Now, counterbalancing that is the fact that the missions that were flying were absolutely groundbreaking. Voyager, as I said, was the first good look at four complete planetary systems. Magellan was the first really good, comprehensive look at Venus. That’s not to denigrate the Venus missions that had come before, but Magellan was a huge leap over anything that happened previously. The missions that were happening were really groundbreaking, and so you look back on them and they—and rightly so—are considered historic.

But it was a time of limited activity, whereas today, there are so many missions flying. You go to a lunar and planetary science conference and it’s just swarming with young scientists—way, way, way more than there were back when I was coming up. I think today is a tremendously exciting time, and in some respects more exciting than it was back then.

JOHNSON: Maybe because we have seen some of the results of those earlier missions now.

SQUYRES: Yes. And you build on them. I think the other thing is that NASA just has a more vibrant planetary program now. Back in those days, we would fly a very small number of very big missions, the so-called flagship missions, and there weren't the smaller missions. You didn't have Discovery missions, you didn't have New Frontiers missions. Smaller, PI [principal investigator]-led things just simply didn't exist. There is a more steady stream of new discoveries and new data today than there was back then.

JOHNSON: It seems like for humans, there has always been this interest in Mars, and for a lot of reasons. I am sure scientific reasons, and some you mentioned. Each [presidential] administration wants to put their stamp on NASA and science—some more than others. The first President [George H. W.] Bush made his statement about going back to the Moon and onto Mars, as his son [President George W. Bush] did later. Is Mars one of the things that drives that interest, do you think, especially with the generation now?

SQUYRES: Sure, of course. Yes, yes. Yes, absolutely. The thing about Mars is Mars has two characteristics that make it unique. One is that it's one of the few places in the solar system where habitable environments might exist today, and did exist in the past. There aren't a lot of those.

The other is that it is one of the very few places—really, the only place—where humans can go to those potentially habitable environments, where humans can go to environments that were once habitable. That issue of accessibility for humans and habitability for potential former life forms, those uniquely converge at Mars.

Europa's fantastic, fabulous. There could be an ocean on there, on Europa, but you are not going to put humans down into that ocean anytime soon. Same with Enceladus. Whereas Mars, accessible. That combination of habitability and accessibility makes Mars unique.

JOHNSON: As you have been working through the years on proposals, in those early times—and as I mentioned, the Presidential administrations come and go, and the budget also comes and goes, especially for exploration. Maybe just talk for a minute about competing for NASA dollars.

SQUYRES: Oh, boy. Yes, yes. The competitive process—incredibly valuable, incredibly necessary, incredibly unpleasant. It has to happen. The ratio of pretty good ideas to actual flight opportunities is enormous.

There are so many pretty good ideas out there and so few opportunities for flight that you need some process by which the people with the ideas can demonstrate convincingly that their idea is worth hundreds of millions of taxpayer dollars, and is better than this idea, and that idea, and the other idea that somebody else came up with. So how do you make that decision? The way that it's done is through competitive peer review, and it's a very Darwinian process. A lot of failures, not too many successes. Far more unsuccessful proposals than successful proposals.

The things that make it unpleasant—first of all, you usually lose. Most proposals are unsuccessful, so you put years of your life into these things, and most of the time you come up with nothing. It pits friend against friend, colleague against colleague. Now, we're professionals. We can get along and compete with one another. But that issue of competing for your professional success against your friends and your peers, it's not an entirely comfortable experience. You all want to be friends once the competition is over, but it kind of sucks some of the fun out of it when you are trying to beat your pals.

But it forces you to do good work. I am sitting in my office in Ithaca, and I have a stack here of some of the past proposals that I wrote for doing rover stuff on Mars that were unsuccessful. I spent 10 years writing unsuccessful proposals for NASA before we were finally selected on the fourth try.

It was no fun, but I can look at those failed proposals today and I see the flaws. They were the best that I could do at the time. I put together the best team that I could, we'd work as hard as we could, we would do the best work that we could, but I can see the flaws, and I can see why, yes, they probably didn't deserve to be selected, no matter how I might have felt about them at the time.

That competitive pressure forces you to make tough decisions. It forces you to get better at what you do. It forces you to sharpen your thinking and improve your ideas, and improve your designs, and come forward with something that actually has some reasonable probability of working.

It would be incorrect for anyone to assert that the best proposals always win, because the proposal evaluation process—the process of reading all of these proposals and deciding among them—is as imperfect as the proposal writing process. It's done by humans, people make mistakes, people make misjudgments.

So the whole thing—proposal writing, proposal reviewing, proposal selection—every aspect of it is imperfect, but it's good enough that if you look at the missions that have flown, it's worked out pretty darn well.

JOHNSON: Well, as you mentioned, you did four before one got accepted.

SQUYRES: I'll tell you, after the first three I was ready to quit. I was just fed up. I was very close to a career change after that third proposal.

JOHNSON: Talk about some of the proposals that didn't succeed, and what the difference is. I know you learned from the ones that don't succeed, but what made the fourth one successful?

SQUYRES: Yes, okay. One of them we've already talked about. One of them was the proposal—a camera for Mars Pathfinder, and that was just a really stupid mistake. Now, even if we hadn't made that stupid mistake, it's not clear that we would have won, because there were a couple of really solid competitors. But that mistake was disqualifying.

The next proposal was an entire scientific payload for the 1998 Mars lander mission. It was a very nice payload. It included many of the instruments that ultimately flew on Spirit and Opportunity. We had three different teams competing against us. The team that won was selected to do a mission that became known as Mars Polar Lander. When I got the news that we had not been selected for the '98 lander mission, that was a devastating loss because I really thought we had a good proposal. I really thought it was strong. I wanted to win it very, very badly.

I remember I got the call from NASA Headquarters [Washington, DC]. I was in Paris [France] at the time. After getting that phone call, I couldn't sleep. I spent the whole night walking around through the streets of Paris in the rain just feeling depressed. That mission became Mars Polar Lander, which of course was a mission that ultimately failed because the lander crashed on the Martian surface. As bitter a disappointment as that loss was, it may have been one of the best things that ever happened to me.

The next proposal was a Discovery mission proposal, so this is a PI-led mission to send a small rover about the size of—well, no, not as big as one of the MER [Mars Exploration] Rovers, but a comparable set of science objectives. That was not selected either. Then what did happen was they sort of took that basic concept that we had proposed and turned it into an opportunity for a rover mission to be launched in 2001. We proposed a payload for that rover, and that was the one where we ultimately got selected.

What then happened was the rover got kicked off of that mission and it became a lander mission, and then the lander mission got canceled after the '98 lander failed. Then from the ashes of that arose Spirit and Opportunity. That's a whole long, complicated story that I tell in great detail in my book.

JOHNSON: Yes. I'm sure it is complicated.

SQUYRES: Really complicated.

JOHNSON: Yes, but it is kind of interesting, the whole background of that, and I was talking with [G.] Scott Hubbard this morning.

SQUYRES: Oh, okay. Well, Scott knows that story well. He was right in the middle of it all.

JOHNSON: Right. He has also written a book about Mars, so there is a lot of documented stuff out there. But it's just interesting, that whole human element, especially trying to get these things going. Like you said, you had thought about those rovers early on—

SQUYRES: Oh, yes.

JOHNSON: —and if you couldn't get your own boots on the ground, then you could get these rovers up there. Was it 2000 that it was decided that they could go ahead with a rover?

SQUYRES: Yes, it was 2000. Summer of 2000.

JOHNSON: And then, of course, the story goes that [NASA Administrator] Dan [Daniel S.] Goldin said, "Well, why not two rovers?"

SQUYRES: Oh my god, I think—to this day—that would have to be the most astonishing phone call of my life.

JOHNSON: Well, just talk about that for a bit, if you don't mind.

SQUYRES: Okay. There were two ’98 missions. There was [Mars] Polar Lander and [Mars] Climate Orbiter, and they both failed. After the failure of both of those missions, Ed [Edward J.] Weiler, who was now the [NASA] Associate Administrator for Space Science, kind of wiped the slate clean in the Mars program and says, “Okay, look. Let’s just stop, figure out what we did wrong, and then do something different.”

At one point, there were just dozens of different possibilities for what might fly in 2003. After a very, very chaotic process and winnowing of them down, it came down to two. It came down to two missions. One was what eventually became Mars Exploration Rover project, and the other was what eventually became the Mars [Pathfinder] rover. There was a surface rover delivered using an airbag system, versus a big orbiter with lots of instruments hung on.

There was this head-to-head shootout, two days long, at NASA Headquarters summer of 2000. Each team had basically a day to present their science and their engineering, and then there was this panel—I think it was 12 people, something like that—that was to go off and make a recommendation.

Ultimately, the decision was going to be made by Dan Goldin, the NASA Administrator. This committee would make a recommendation to Ed Weiler, Ed would come to his conclusion and would make a recommendation to Goldin, and Goldin would make a decision. It was going to be one or the other. It was going to be either the rover mission—this one rover—or it was going to be the orbiter.

We knew the day that it was going up to Goldin. By the time it went up to Dan, I had already heard from Scott Hubbard that the recommendation was going to be for the rover mission. That was the decision that Weiler had decided on, and so he was going to recommend that to Dan.

I was sitting exactly where I am right now, staring at the same phone—I had the same phone, right here—waiting for the phone to ring. I was sitting here and waiting for the phone to ring, and the phone rang, and I picked it up. I was expecting it to be one of the guys—Scott Hubbard maybe, or Carl [B.] Pilcher, one of the Mars people from NASA Headquarters—with the news.

Instead, it's the entire Mars office. Five, six, seven—I don't remember—a half a dozen people all on a speakerphone. They said to me, "Steve, we have to ask you a question."

I said, "Okay, what?"

They said, "Can you build two?" And as God as my witness, the next two words out of my mouth were, "Two what?"

They said, "Two payloads."

I said, "Why would you want two payloads?" It still wasn't sinking in.

They said, "For two rovers." I just about fell off my chair.

It was brilliant. It was brilliant, because there were some real problems with what we had proposed. One is it's risky, and we all knew it. Landing on Mars is risky. A lot of Mars lander attempts have failed. This was a dangerous thing to try, and you really improved your chances if you can fly two.

The other thing was at the time, we knew so little from the missions we had flown—this is before MRO [Mars Reconnaissance Orbiter], this is before an enormous amount of data that we now have was in hand—you didn't really know the right kind of landing site to send the rover to. If you have got to pick just one landing site, you could guess wrong and you come away with nothing. Whereas if you can send two rovers, send them to two very different sites, you double your chances of making a good decision about the kind of site to go to.

It was a brilliant strategic stroke, and he caught us all completely off guard. Not one of us had even thought about what it would take to do this. Now, I knew where my payload stood—I knew what the status was on my instruments, I knew how mature they were—so my response to the guys at Headquarters was, "Sure I can build two. Just send money."

Then off they went. It took a few days to kind of round up the money to pay for the second rover and all of that, but in the end we flew two. But I don't think I have ever received a more surprising phone call in my life. And it was all Dan, it was all Dan Goldin.

JOHNSON: Yes. A lot of things then were "all Dan Goldin."

SQUYRES: Yes. Oh, yes.

JOHNSON: I know you have covered a lot of things in the book, I am sure, but since—when was the book? 2005, 2006?

SQUYRES: Something like that, yes.

JOHNSON: A lot's happened since then, including these rovers had a long life. Talk about that just for a minute, and the experience of one of them still going strong.

SQUYRES: Yes. Anybody tells you they thought the [Opportunity] rover was going to last this long, they are lying. I can actually prove to you that we didn't think they were going to last more than a couple years at most.

Each rover carries what's called an X-band transponder. It's the radio, basically, and it's the electronics unit within the vehicle that carries out the communications with Earth. The frequency on which these transponders receive and transmit is locked in. You can't change the channel, the frequency. It's got a single frequency.

We built four of these. We built two flight transponders, and then we built two flight spares that were identical to the flight units in case something went wrong. So we had four of them. The flight units were both fine—we flew them on Spirit and Opportunity—so now we have these two transponders sitting on the ground after we launched.

Not long after we launched, the Mars Reconnaissance Orbiter project, which was the mission that had been selected to follows ours and was going to be an orbiter, came to us and said, "Hey, guys, we need an X-band transponder. You are not using your flight spares. These things cost $1 million apiece. Can we have one of your flight spare transponders?"

We said, "Sure. We are going to be dead by the time you guys get to Mars, so go ahead, take one."

The result was that the Spirit rover and the MRO orbiter for a number of years communicated to Earth on the same frequency. Operationally, that was a pain in the neck. We learned how to work around it, but we never would have given our flight spare transponder to another Mars mission if we had thought we were going to still be alive 26 months later when that mission arrived. So I can prove to you that none of us thought it was going to last 26 months.

JOHNSON: It's definitely a lot longer than I'm sure any of you had planned.

SQUYRES: Yes. It was, and has been, and continues to be. It has caused a lot of problems, much more money spent on flight operations than anyone, either us or NASA Headquarters, had ever anticipated. From many of us who had plans to move onto other things in our careers, it has kept us working on this one mission for a very long time.

I wouldn't have it any other way. I'm so thrilled that the rovers continue to operate. I love it. I love flight operations. I was doing flight operations for Opportunity yesterday, I'll be doing it again tomorrow. Today is a day off in-between, but I do it on a regular basis, and I absolutely love it.

There is an interesting thing about that. Actually, there are several interesting things about that. One of them is that when we first did this and we first proposed it, I seriously underestimated Mars. I always had this comforting notion that if we managed to launch these rovers and get them to Mars and operate them, that after a while we would be able to say, "We did it." We would be able to sit back with some satisfaction and say, "We have learned all that we can learn about these two places on Mars with these two vehicles. Done."

Never happened, never will. You won't. Mars has turned out to be so much more complicated, so much more geologically diverse, so much more interesting than I ever imagined. What I've come to realize is no matter when these things die—I mean, Opportunity could die tomorrow, it could die 10 years from now—but I guarantee you that when it dies, there will be some tantalizing thing just beyond our reach that we're excited about and didn't get to. That's what happened with Spirit.

Spirit went on for six years, and it was this wonderful place that was the next thing, and we didn't make it. We could have discovered more. The same thing with Opportunity. When Opportunity dies, there is going to be stuff that we could still do with it. It's a good thing, right?

You keep discovering new stuff. But it's always going to be a source of frustration at the end of the mission.

The other thing is that the open-ended nature of the mission—not knowing when it's going to end—has always made the decision-making process challenging. Every day, we need to sit down, look at where we are on Mars—the broad strategic picture, the questions we are seeking to answer. Look at the tactical situation—the rocks that are in front of us right now, the rocks that are just down the road, whichever direction we think we want to go.

And we have to decide, "Do we stay or do we go?" Do we stay here at this place where the science is kind of interesting and do it thoroughly and carefully, and then move on later? Or do we drive away right now to this place that's far away where we think the science is really good? We face that decision on a daily basis, and we have been doing that for 13 and a half years.

If you could tell me the day on which the rover will die, and the way in which it will die, then I could plan. If we knew how long it was going to last, we could balance the science that we know we can get here against the science that's farther away, because we know how far we can drive before the thing dies. Not knowing when it's going to die, you always have to make that decision on the basis of inadequate information.

We designed for what was supposed to be a 90-day mission. Now, none of us thought the wheels were going to fall off when the sun came up on the 91st day. I always thought that we were going to get probably six months out of them, maybe nine months, maybe-maybe-maybe even a year.

But there were decisions that we made early on in the mission where we drove away from potential discoveries because we were worried the mission might be ending in a few months and we wanted to get moving and find new stuff. If I had it to do over again, I would do it differently.

One of the things that I learned is that if you follow that line of reasoning, it will make you crazy. It does you no good whatsoever. You get to a point where you have to make a decision. You make the decision on the basis of all the information that you have in front of you at the time, and you move on from it.

Down the road, you learn something new. Down the road, you realize you have got a longer-lived vehicle than you thought you had? Well okay, use that information now. You can't cry about the fact that you didn't have that information back then. But that kind of "When is this going to end?" element of the mission has been an interesting aspect of the experience.

JOHNSON: Why have they lasted? Spirit obviously lasted a lot longer than expected, and then why is Opportunity still going? Why is it so much resilient than you had expected?

SQUYRES: You asked that question very politely. Some people just say, "You were really sandbagging us with that 90-day thing, weren't you?" There always is this temptation to under-promise and over-perform. But it comes down to three basic reasons. Number one, we built good hardware. If you want to accuse us of over-engineering these things, I will plead guilty as charged. Number one.

Number two, the thing that we thought was most likely to kill the vehicles was going to be buildup of dust on the solar arrays. The Mars Pathfinder mission, for example, was the first solar-powered lander on the surface of Mars, and it saw a steady monotonic accumulation of dust throughout the mission. More dust, more dust, more dust—that was it. That was all we knew.

So we thought that an accumulation of dust on the solar arrays was going to be the end. What we encountered instead was that there would be periodic gusts of wind—we refer to them as cleaning events—that have cleaned off our solar arrays.

It's been very different for the two rovers. For Spirit, we would have long stretches—many, many months—of accumulation of dust. And then, pow, one huge cleaning event that it was like taking the vehicle to the carwash, and it would all of a sudden be very, very clean again. Then it would go on for months and months and months, and then you'd have another one. We had a handful of those—not very many—but it completely gave the vehicle a new lease on life.

For Opportunity, we have never really gotten that rover really dirty. The wind regime at the Opportunity site is lots and lots and lots of little wind gusts that are continuously sort of cleaning the vehicle. Nobody anticipated those. Nobody expected that that was going to happen, and that has been, in large measure, responsible for the extended life.

The third thing is we figured out a trick. These rovers were designed to last for 90 days. They were sent to places on Mars that are—especially Spirit—in the southern hemisphere. We always knew that in the wintertime, when the Sun goes low in the northern sky, the conditions for the rovers were going to get very challenging. Now, what we don't have—what we sort of wish we had—is actuators, motors and gearboxes—that could take the solar arrays and tilt them.

The solar arrays are flat, like the deck of the rover, and they are just always on the same orientation as the plane of the rover deck. What you'd like to have, is you'd like to have some motors that you could use to tilt these things, point them towards the Sun, point them towards the north when the Sun gets low in the northern sky, and boost the power output. We don't have that.

But what we can do is we can drive onto north-facing slopes. While the missions were designed from the outset to operate on flat terrain, they've lasted so long and we've been able to drive them so far that we have managed to find access to mountains and craters and hillsides, and places where we can operate the vehicle on substantial topography for which they were never designed or intended.

So what we have done is in the wintertime, we drive them onto north-facing slopes. We are doing that right now with Opportunity. As we speak, Opportunity is in the depths of Martian winter. The Sun is low in the northern sky, and what we do is we drive from one place where there is a steep northward slope to another place where there is a steep northward slope. And we keep the rover always—especially when it's at rest—oriented such that the solar arrays are tilted towards the Sun.

We make what we call "lily pad" maps. What a lily pad map is, we will use our stereo cameras to generate a topographic map of the terrain around us. We will calculate which areas have high northward-facing slopes and are therefore safe for the rover. We then color-code those maps, so we have a special color that indicates this is a safe spot. And then maybe 20 meters away, there might be another safe spot, and then we will drive from one of these safe spots to the next, to the next. Like a frog hopping from one lily pad to the next, to the next on a pond. That's why we call them lily pad maps.

The combination of lily pad driving plus gusts of wind, plus the engineers at JPL built some kick-ass hardware, that's why we're still going.

JOHNSON: That's pretty amazing.

SQUYRES: It is.

JOHNSON: You mentioned that it did create some problems just because the funding had to continue. Did anyone ever propose the idea, because of funding and budget cuts, that you just turn them off?

SQUYRES: At one point, because of some overruns on the Mars Science Laboratory Project, there was some talk at NASA Headquarters about shutting off one of the two Mars Exploration Rovers to pay for part of that overrun. At one point there was some talk about doing that, but it never happened.

Now, what does happen is every two years, we go through a review process in which the ongoing planetary missions, MER among them, all have to get up in front of a very senior review panel, describe the accomplishments of the past two years, describe the plans for the next two years, and undergo a fairly rigorous peer review. The results of which NASA uses to decide do we keep operating these missions, or are we going to shut something down.

So yes, every two years I have to get up in front of my peers and justify continued funding of the mission, which, that’s a perfectly reasonable thing to have to do. We are spending about 12 million bucks [dollars] a year operating Opportunity, and that’s a big pile of money. It’s a razor-thin budget for us. We are operating the vehicles very, very efficiently, but you could do a lot of good in the world with $12 million. You could do a lot of good at NASA with $12 million. So yes, we have to get up and justify our continued operations, and we take that real seriously.

JOHNSON: One of the other things that you do and have done throughout this—and especially after the successful landing—is you have been the face of the rovers on television. Being interviewed and communicating with the public. As you mentioned before we started, you have different people in your audience, and being able to explain scientific things so that the general population can understand and get excited about what's going on is important.

One of your mentors, Carl Sagan, was probably the first to really be good at that, or to take that on as a mission. Talk about that for a minute, and what influence he had as far as you being able to do that, and just where that came from in you to be able to do that.

SQUYRES: That is tremendously important. It's one of the most important things that we do. I learned many things from Carl Sagan, but that one has to go very close to the top of the list. Carl understood before almost anybody else in the space science world how critically important it was to convey to the public the significance and excitement of what we do with these planetary missions.

NASA's not giving our team a billion dollars so we can go off and have fun, and write our scientific papers and advance our careers. They are giving us a billion dollars because they have made the judgment that the discoveries that we will make and the work that we will do will be of sufficient importance to the people who pay for it that it's worthwhile. You do not reap that benefit—you do not prove your worth—unless you communicate to the people who pay for it what they are getting for their billion dollars.

So I have always taken that very, very, very seriously. Carl was the pioneer, and we are all following in his footsteps when it comes to explaining, especially planetary, science to the public. He was the master. But not only was he masterful at doing it, he understood its importance before almost anybody else did.

So yes, I have always taken that very, very seriously for a couple of reasons. One of them is, as I said, you are trying to help people to understand the science, and the significance of discoveries and what they mean, in comprehensible, straightforward language. Not a lot of scientific mumbo-jumbo, but "Here is what it means, and this is why I am excited about it, and why I hope you are excited about it too."

The other thing, though—and I almost feel that this second item is the most important—I grew up, as many of us working on MER did, in the '60s and early '70s watching Mercury and Gemini and Apollo on TV, and dreaming of sending spaceships to Mars someday. We got to do it. Those pioneers, those were our inspiration.

To me, I really believe that one of the most important things—one of the most significant legacies of missions like MER and others—is going to be that there is some kid somewhere who is watching the landing, and watching the first discoveries, and watching us jumping up and down like we just won the Super Bowl or whatever. Looking at that and saying to themselves, "That's really cool, but I bet I could do better."

You look at young people in the world today, with all of the flood of information that they are dealing with from television, and social media, and all these sources of information flooding at them. They are making decisions about what to do with their lives. They are making decisions today that'll affect the direction that they could go with their lives later, when they are in high school, when they are in college, when they are out in the job market.

If you can give young people the sense that science and engineering are cool, and exciting, and fun, and awesome—that's a great thing to do. They are not all going to go work for

NASA. They are not all going to become astronauts or build Mars rovers. But they are going to follow pathways that are going to make it possible for them to develop new technologies, new consumer products, new pharmaceuticals, new things that this society desperately needs.

To the extent that by conveying to the public, especially the young people, what we are doing and the excitement and fun of it, that's one of the absolute most important things that we do. Yes, I'm very, very proud of the discoveries that we have made on Mars and the way that we have conveyed them both in the scientific literature and to the public. But I almost feel like the most important legacy of some of these—especially a Mars rover mission which is the one that, frankly, received disproportionate visibility with the public.

When you receive that much visibility, it gives you a special opportunity—but I think also a special responsibility—to try to use it in ways that benefit the public, that benefit the country. And we tried really hard to do that.

JOHNSON: I think you succeeded.

SQUYRES: Oh, yes. Keep going, the rover's still driving. We are going to discover something big tomorrow.

JOHNSON: That's right. We have been going almost a couple of hours, so it'd probably be a good place to stop for now. But I appreciate you talking to me today, and finding time in your schedule.

SQUYRES: Sure. Glad to do it.

[End of interview]