DISCOVERY 30TH ANNIVERSARY ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

DAVID E. SMITH
INTERVIEWED BY ERIK M. CONWAY
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CONWAY: I'm Erik Conway. I'm interviewing now retired from Goddard Space Flight Center scientist David Smith, and my first question, David, is please give me a quick biographical sketch of yourself, your educational background and interests.

SMITH: Well, my educational background is nominally mathematics, England, University of Durham. I then did a master's and a Ph.D. at the University of London. The master's was on wave propagation in plasmas, and the Ph.D. was on planetary gravity, topography and things of that kind.

Then I came to the States in '68, joined NASA about a year or so later, and ran a program at NASA Goddard [Space Flight Center, Greenbelt, Maryland] on Earth science, really it was, for a good many years, then moved to planetary science at Goddard in about 1985. Left Goddard in 2008. When I left, I was a division director there at the time, what is now called a division director, I should say, at Goddard, and up until that time when I was also working on getting GRAIL [Gravity Recovery And Interior Laboratory].

Then I left Goddard and joined MIT [Massachusetts Institute of Technology, Cambridge]. I joined MIT in early 2009, and that's where I am right now as a research scientist and doing the things I like to do, only.

CONWAY: Only, instead of the usual management problems of staff, yes.

SMITH: Yes.

CONWAY: Yes, I understand. So as I said before, I've interviewed you about your role in Mars Observer and then Mars Global Surveyor, so I have a little bit of your background in other planetary missions. So tell me how you got on to the GRAIL mission.

SMITH: Oh, well, how I got on to it, well, bottom line is in about, I'm guessing it's about 2004, December, when Gregg Vane at JPL [Jet Propulsion Laboratory, Pasadena, California], who was then running the new business office, and I knew Gregg going back years because I'd done more work at JPL than I have actually at Goddard in terms of missions, and so we met for lunch one day and he said, "I've got a proposition for you and Maria [T. Zuber]. We would really like to propose a Discovery mission to do the gravity field of the Moon. We have a technique of how that could be done very well." Maria and I were familiar with the technique. In fact, the technique goes back a long, long way, but this was a very special approach with these two low satellites with very, very high accuracy.

Also I think it was in that year, 2004, Maria and I had both been accepted as the PI [principal investigator] and deputy PI of an instrument on the Lunar Reconnaissance Orbiter, LRO, and that's going now. So I'm the PI, Maria's the deputy PI. It provides topography of the Moon, great stuff. It's the marker, in some sense, for a while to come because it's a great instrument and some very exceptional measurements.

The thing that one needs to do when one's interested in these structural planets, as both Maria and I were, mine from the more geodetic side and Maria from the more internal structure side, was both gravity and topography. So when Gregg Vane said, "We'd like to give you an offer to lead a Discovery mission to get the gravity field of the Moon," it was kind of a dream come true. We'd done something very similar on MGS [Mars Global Surveyor]. We'd run the altimeter and we led the gravity experiment. Well, we didn't lead the gravity experiment; that was led by somebody else. But I was a member of that particular team, a co-I, you might call it, of that team, or team member.

So we did the gravity and topography of Mars, and that was great. Frankly, we did the same thing actually at Mercury just a few years later, but we did the gravity and topography. So getting a chance to do the Moon, which was pristine because of its nature and its age and its uncorrupted—if I might put it that way—surface, if you know what I mean, wow, what an opportunity.

But as soon as Gregg asked us, there was kind of a pause, as I remember it, and I had to say, "I can't do that. I'm now the PI of the laser altimeter on LRO." I said, "Much as I'd like to, but I couldn't do that." There was another aspect to it as well, that I was the division chief at Goddard Space Flight Center, and really the mission out at JPL would have crossed a few barriers that scientists don't care about, but institutions do, very much so. But my comment was, "Look, if Maria will take this on, I'll be her backup and number two, but I can't lead it, okay?"

This was just the right opportunity for Maria anyway. She'd been my deputy on a number of things and she led the near laser-ranging measurement to go to Eros 433, an asteroid, back in the mid-90s. So a lot of thought went on. Well, yes, okay, the bottom line of that lunchtime agreement with Gregg was, "Okay, we have a shot at it."

From then on, it got more complicated and we got much more involved, of course, but Maria did lay down some conditions which I thought were a little tough at the time for an institution like JPL, and it certainly would have been for Goddard as well, that she, Maria Zuber, has to run the mission, period, not just a piece if it, not just the science, not be the figurehead; she has to run the mission. She wants to control everything, okay, from the beginning. Now, she may not describe it that way, but that's exactly how she said it, and she told Charles, Charles Elachi, as well.

One of the things became very evident when we accepted this offer. Charles Elachi, who we both knew quite well, we'd been competitors and companions of Charles over the years, and he just said to us, "Maria and Dave, we, JPL, need this Discovery mission." I don't know the circumstances on that. I didn't remember JPL being in trouble or anything, and a Discovery mission wouldn't help them out particularly that much, if you know what I mean, but, nevertheless, he said, "We really need this one."

But Maria came back and said, "Well, yeah, but there are some conditions. I have to be able to run this mission."

He said, "Fine." He said, "Fine. I'm going to get you to come and talk to one of my Monday morning meetings with what I call the directors of JPL." It was about 9:00 o'clock on Monday morning and everybody gets together in the executive conference room or Charles' conference room.

So he had us in there, and Maria and I were sitting on the side of Charles, and he gave an announcement that basically we had accepted to do this proposal and he wanted to get full support from the JPL management. And Charles said to Maria, "What do you want from JPL?"

She was very explicit about she wanted to basically control the budget. She and I had an argument about whether that should be at the 25K level or the 10K level, which is what she wanted. And she didn't want any flak from the institutional component, particularly the—what will I call it? The networks [Deep Space Network] was an issue, which control things. "Well, we'll do this." And she said, "No, I want the support to do it. Otherwise, we don't do it." She went through the directors in terms of what she needed and so on.

And Charles said, "Okay. You heard that. I expect you to support Maria and Dave in the way she has requested." So this set us off on a really good footing with the Lab. She also wanted to choose all the managers, including the project manager, and, again, this actually went better at the executive level than it did at the project level. Projects don't like being told who they can have as managers, if you know what I mean. And I think I'm right in saying before David [H.] Lehman, there was somebody else that was there temporarily.

CONWAY: Leon Alkalai? You mean Leon?

SMITH: Okay, yes, we're stepping back a bit now. Leon was given the job of being the proposal manager. So we worked with Leon for all the years until the proposal was, in fact, accepted, and that must have been 2007, because we launched in 2011. So, yes, so we got on very well with Leon, although I don't think he's any longer at JPL, but Leon was at the time at JPL and very, very capable, and he understood exactly what we wanted. He didn't have any problems with Maria pulling the strings, as appropriately. And she did.

Then we got down to formulating how it should be done and what part JPL could play, and the important part was this ranging system between the two spacecraft. JPL was already working on that, and that was fortunate, because there's no way that could have been developed in the two or three years from the time of the proposal being accepted, but it had been going on for a few years before, and I'm guessing principally on in-house R&D [research and development] money, but I don't know. They might have got money from NASA to start working on it.

Anyway, that was coming on well. Again, we knew the people involved because we'd had a pretty long association with the Lab, so many of the individuals who were involved were colleagues in one way or another on other missions or something.

So, we started to draft a proposal, and Maria was in charge, very much so, but she didn't get in the way, as far as I'm concerned. On the contrary, what she said to me, she said, "The one thing I need you to do right up front is determine what the requirements for the gravity field will be. Tell me what we need to do the structure of the body." And this was exactly what I liked to do or would do. My Ph.D. was involved in the gravity field of the Earth, and so I knew about gravity in fair detail, what we would need to do. And Maria, from the beginning, said, "I really want to get as much fine structure as I can."

I said, "Okay, let me see what we can do." And there were some limits on what we could do, provided by, essentially, how well we could keep the spacecraft at the right altitude above the Moon, and we knew that from other missions that had been flown, a 50-kilometer orbit was probably an okay orbit to stay in for a while without too much adjustment to the orbit, because the gravitational perturbations would slowly bring it down or make it eccentric, and eventually it will crash. We really needed an orbit that would be pretty much the same throughout, at least for a period of time, like three or four months or thereabouts, to get the data we needed. It didn't need to be a long mission, by the way. And we did.

We chose an architecture, in essence, of how we would do it. It would be just the two spacecraft, there would be no other instrumentation, and I got to write the requirements down literally in wavelengths of the planet's gravity field and in the sizes of blocks in principle that we should be able to see on the Moon. I worked with Maria because I was working with her the whole time, though she was at MIT, but we were in constant contact almost day and night, anyway.

I agreed to travel to JPL during that period of the formation of the mission, if not every week, every other week, I suppose, and when possible. Maria went in between in the other weeks, so there was always one of us present there. Slight exaggeration, but we did the best as we could to be there all the time for all the meetings and everything, and when we couldn't, we would get on the—I can't remember using Webex or Zoom in those days.

CONWAY: There was a thing called ViTS, do you remember? NASA had this Video Teleconference System called ViTS.

SMITH: I don't remember using videoconferencing, actually, at all. I don't remember a telephone link at the time, but it worked.

So, you know, we got down to writing that proposal. Maria was very good. I mean, Maria's strength in many ways was she always knew what was important and what was not important. She didn't have a difficulty in making a decision. And that came about also, frankly, when we had some influence at the lab from a senior scientist, who was actually more trouble than he was worth, and Maria insisted at one stage when he, in front of a [NASA] Headquarters review board, made some statements and comments that afterwards Maria said to the Lab, "He's just got to go. He's got to go. Don't care. Got to go."

And David Lehman was very nervous about that. He was a very important person in the system at JPL, at least scientifically important, technically important, and that would only cause trouble. Maria said, "No, he's got to go. We'll manage without him, no problem at all," and we did. It wasn't an issue at all. On the contrary, things went more smoothly because the chemistry was better. The chemistry was a problem, wasn't for Maria and I, but mainly within JPL. He was interfering with the engineers who knew what they were doing, the instrument engineers, and it was in that context that he just, in a sense, overstepped his mark, and I agree. So Maria discussed it with Headquarters and discussed it with JPL, and just said, "As far as I'm concerned, this has got to happen." So that happened.

There were issues of that kind that came up every so often, as they do in all projects. But we more than survived them. We survived very much and healthily, and the writing of the proposal went well. We managed to get selected in the first round, then got the second round. That was a little awkward for me because I think the two other instruments missions being proposed in the second round were both from Goddard. One was OSIRIS-REx [Origins, Spectral Interpretation, Resource Identification, and Security – Regolith Explorer], which is now ORISIS, which now went to OSIRIS-REx. It was, frankly, just too expensive anyway. It couldn't make it at Goddard. Wow!

CONWAY: Discovery cost box.

SMITH: And review boards do have a habit of seeing through that stuff, if you know what I mean. But, yes, so it was a bit of luck in getting missions accepted. Things have to be just right for you, and they were in this case as well, and in the second time around, in the step two, we provided the additional details and then fleshed out the proposal even more and made a very credible effort.

Under Maria's tutelage, in a sense, she insisted that we not bridge the cap or not even get close to the cap, and so the question came up—I can't remember how it was resolved. Maria said, "Look, by the way we've done this and the way we've planned it, there will be $50 million left over." And the classical question at the center is, "Well, let's bid the whole thing, higher than 20 million, whatever it is. Never mind. I have the 50 million in spare down there packed in various places, you know."

And if I remember correctly, Maria said, "No, I want to go in for less than that." I don't know how—if it was 50 million less. But the net result was in the end of it all, there was a 50-million-dollar savings, and the good part about that was that it funded completely an extended mission. So we could do an extended mission, and any extra money, JPL already had the funds, if you know what I mean, hadn't got it in their hands, but it had all been basically approved. So I'm guessing from what I'm saying now is we must have gone in not with $50 million under the cap, but probably gone in with a smaller number, much smaller number, and then still managed to save the equivalent of a total of about 50 million, of which the remaining—I think at the end there was about $20 million actually hanging back after the mission was all over and everything. So, yeah, we did very well.

Now, I've gone on talking about this mission for a while and I haven't asked if you have any specific questions.

CONWAY: You touched on a bunch of them that I want to go back to and I hope I don't miss. So, first off, a little bit more about you, and that is what interested you about GRAIL specifically? We talked a little bit about your background, but what about GRAIL specifically? What did you see it as accomplishing that you thought would be fantastic?

SMITH: Oh, understanding the interior structure of the Moon just like we've done at Mars and Mercury and so on, and we'd also been on the Clementine mission as well, to do something very similar. So the gravity and topography were the two things that we need, and LOLA [Lunar Orbiter Laser Altimeter] on LRO, the laser altimeter, was going to provide one piece of that, and the other piece we really needed was gravity. There wasn't much chance from LRO. We could do okay, but we could not get the far side properly because you can't track it. So you have to have a two-spacecraft system that can track itself on the far side. And this mission and this approach gave us that opportunity, so it was kind of a slam-dunk in terms of something that we wanted to do. It was an opportunity, so for me, yes, it was a chance to really do the highest-quality gravity and topography that has ever been done anywhere, basically, and what we could learn from it.

Turned out that there was at least as much as we had hoped to learn from it, and a lot of it came in the extended mission, but we couldn't have done the extended mission, which was a lower altitude, without having done the first one to understand just how bad or how dangerous it could be at the Moon in low altitude. And, of course, at the same time we had LRO out there. It went up in 2009. So the two spacecraft, we both had basically—I was the PI of one and she was the PI of the other, and I was deputy of her, etc. We had a great arrangement in which the two datasets we wanted were coming together at the same time.

So it was just a tremendous opportunity to get the best that was likely to be possible for the foreseeable future, and I think it'll be a while before, if ever in some ways, we get a better global gravity field of the Moon, and the topography will be tough to beat as well, at least—and I say topography will be tough to beat, tough to beat in a sense of being geodetically controlled. You can't do geodetic control as well with imaging and so on. You can do topography, but the altimeter really controls the shape of the body, and that's what it was doing.

So we got this very high-resolution topography field coming out with resolutions of 5 meters, 10 meters on the surface, thereabouts, maybe a little more, a little less, depending. And the gravity field was coming in at not meters, but eventually we got down to about 3 to 5 kilometers resolution on the surface, perfect resolution to compare the two, because the gravity information, to get the best out of the gravity information, you have to remove the topography of the planet. You don't see inside the planet until you get rid of the rough stuff on the outside, and the topography, we had that. What we do was to work out how to get the density out of it, and that actually comes out also from the gravity and topography together, and you work out what the minimum variance is and so on. The net result is this works for 2550, not as good for 2650, kilograms per cubic meter, this kind of stuff.

We put together a team. We had an initial team, not very big, a dozen, maybe, fifteen, first-rate team. Between Maria and I, we knew all the key players and we were aware of the gravity and topography specialists in the nation and elsewhere. So, yeah, we had really a first-rate team, and it was a great period from 2011 to—it was all over by 2012, although the team continued until 2014 or '15, I think it was, meeting and doing science two or three times a year.

CONWAY: Do you think the simultaneity of LRO helped you sell GRAIL? I mean, was that part of your argument, that because you had one of these datasets, you needed the other at the same time? There's always this conflict in a Discovery Program about there's other great science from the other proposals too.

SMITH: Yes and no. That's how Maria and I felt. However, in 2004, '05, and '06, when we were proposing GRAIL, LRO had not launched, and the worst thing you could do is tie one mission to another, if something goes wrong or an instrument fails, for example. So, no, we couldn't. We were just very aware of that, and we pointed out that if that mission goes and gets there, it will be the perfect combination.

How the words were in the proposal, I'm not sure, but it wasn't stretched to make people think, "Oh, this is going to rely on it. Otherwise, you can't do anything." And that's not true. I didn't write the requirements in a way that absolutely had to have the topography. Now, there's some things you can do without—a lot of things you can do without topography in terms of the gradients, the gravity field, and things of that kind, that can tell you an awful lot, but it's much better with the topography that you can remove and then you get this clean gravity field without the shell on the top.

But, yes, it turned out the combination of the two, in Maria's, I think, almost her first paper coming out of this probably in '10 or '11, or maybe '13 in the end, she actually showed in her paper that the correlation between the gravity field and the topography field, shape and so on, was 1:1. Well, I should say was 98 percent correlation for any crater bigger than 20 kilometers will be seen in the gravity field, and it would be correlated with the gravity field down to craters the size, I think, of about—well, depending—200 kilometers or something like that. On the far side, it's even better. The near side is not quite so good because of the mascons [mass concentrations] and all that jazz. But yes, that came out, and wow! So we got this correlation. This tells us a number of things about the gravity field, where the gravity field signal is coming from.

Yes, it was serendipitous in a sense. We think something would happen, but the history of doing Mars and doing previous missions to the Moon and anywhere else had never had the resolution that you needed to do this with either gravity or topography, and so the correlation kind of trailed off after a little while, didn't provide much information. But this one was kind of flat line right up there, 98 percent or something. Wow! That was an immediate kind of discovery that will play an important part in the overall understanding of the Moon's gravity field and its interior.

CONWAY: Talk about the process of choosing the science team. How did you decide who to pursue and who not to, etc.?

SMITH: As I said before, we knew the key players, in our minds, that were in this field, and we'd worked with many of them before. They were nearly all—I think they all were university people. I don't think there was anybody from Goddard or JPL, other than project people like Mike [Michael M.] Watkins and Sami [W.] Asmar and so forth. Goddard, we had data processing people as well in there, but I don't think any of them were actually co-Is. However, they did become major players within the meetings that we had afterwards. We didn't restrict the meetings to the twelve or fifteen [co-I's]. We used to bring in the other people who were processing the data at JPL and had any work at Goddard as well.

And one of the advantages we had in selling the mission was that Goddard and JPL were two, if you like, gravity centers, if you know what I mean, for analysis of the gravity field, and one of the points we made, we were going to go for a size gravity field, which was at least degree and order of 180x180, a 30-kilometer block on the surface. We did ten times better than that in the end. But to do that very large field analysis, we really should have two groups that would have to check each other, and that's exactly what happened and it worked out well, and nobody now—some of us who analyze data, which I'm doing right now, I don't know sometimes whether I'm using a gravity field developed at JPL or whether it was developed by Goddard, etc. They're equally good for all the reasonable scales that you're interested in. So, yeah, that was another plus.

But selecting the team, we knew key individuals at various universities that had worked in this field for years, on the gravity, knew about the interior structure, and we asked them if they would be prepared to join us quite early on. Maria was the primary leader on that. This was her mission and she needed to be comfortable with who was being selected, and she pretty much knew. She's not the kind that dithers about people in some ways or anything. "No, no, not him, not him. Yes, yes, we want so-and-so. Good. We want someone on interior structure but from a broad scale. Yes, Roger [J.] Phillips. Okay." These were people from various places in the U.S. The only one from outside the U.S. was Mark [A.] Wieczorek, if I remember correctly, in Nice, University of Southern France, I think it is, or something like that in Nice. But he was a U.S. citizen, but he came essentially free of charge to us.

But money was not the major issue in terms of the size. On the contrary, one of the good things about GRAIL was although Maria kept a tight hand on the money, we didn't really feel as if we had to scrimp and save all the time and nickel and dime it here. On the contrary, we didn't. Right at the very beginning before we'd even worked out a budget, Maria decided that every co-I should get a million dollars right from the beginning. And that was a large sum in those days. No strings attached, you'd get a million dollars. Basically, you could spend it how you like on your own salary or you can bring in students and so on. And most of these co-Is were professors, many, if not most, covered by the university itself, so they could spend the money on students, and they did. GRAIL produced an incredible crop of really first-rate postdocs and students, etc., who are now throughout the community and some of them are now at JPL and around and so on, various places that remember GRAIL very well and wanted to remember GRAIL and MGS as well. They were on both missions, or connected to them, both of them.

So yes, we did it that way, by people we knew were topnotch people, and the only thing that happened after that was decide how to bring in participating scientists, and those are chosen essentially by Headquarters. But Headquarters admitted they needed the input of at least the PI on who should be selected, because they didn't want to duplicate capabilities. They wanted people that could fit in between or could work areas that were not clearly laid out already.

CONWAY: Interdisciplinary people.

SMITH: Yes, exactly. I mean, they certainly became interdisciplinary at the end if they didn't start out. But yes. So nobody else had input into it except Maria, because it is Headquarters' prerogative. But they were funded by Headquarters separately, actually, I think, although we had to put that money in the budget, didn't we? Yeah, that's right.

Anyway, so that's how we built the team up in the end. It eventually ended up with a few of our meetings, very few of our meetings were less than about thirty people, I suppose, in the end, from the science team, the interdisciplinary, all the participating scientists, and then the working colleagues from JPL and Goddard, etc. So that's how it went.

CONWAY: You mentioned something that I latched onto, which was you're essentially dual gravity modeling teams at Goddard and JPL. I've seen that kind of dual or sometimes multiple analysis cases a few times in some Earth missions, for example, but is it common to do that? It doesn't seem to me that it used to be.

SMITH: No, and I don't think it's happened since in the field of what I would call planetary geophysics, to my knowledge. Now, you will find, for example, we can take Europa for example, the lead gravity investigator is Erwan Mazarico from Goddard, but the team consists of many people from all over. I don't know how many there are, probably fifteen or something like that, which includes another person from Goddard and I think there's a couple of people from JPL. I'm trying to think of the people now.

I'm guessing what will happen is that there will be an analysis done at Goddard and there will be one done at JPL. It's logical there would be. On the other hand, we, in part, sold GRAIL on the need for that capability, you know, the dual checking.

It's likely that Europa, for example, in the icy moons, they don't have that resolution, they won't be that good, so there won't be the challenge to do the very large fields. So although we had to sell a mission that would get a 30-kilometer block size like this, which is what we call degree and order 180, JPL managed to go out to 1,800, which is 10 times linearly more resolution. So it's 10,000 times more resolution, so you get down to about a few kilometers, let's put it that way, and now getting down to the noise, but that's what we did. Goddard groups, I know they have 1,200. They might have gone to 1,600. I'm not quite sure. But JPL, I think they used a computer at Ames [Research Center, Moffett Field, California] to actually do that calculation. It's such an enormous calculation, they had to use a climate machine. So the people at Goddard used a climate machine at Goddard because the calculation was so big. And it worked. But I don't think there's any other example where a mission was sold on that basis.

And I would say the other thing, come to think of it, that was special about GRAIL, and it might be true of others, but this mission was built around and designed based on the capability of the technology that was being put together, in the sense that the people doing the Ka band ranging between the two spacecraft were telling us they can get down to a certain level of quality, fractions of a micron per second over two seconds or five seconds or whatever, which is a kind of dimension that people like me, at least, work with and can translate and use.

So I was able to go in and use that in terms of setting the requirements. If they had come back with a different number, say ten times worse, I would have had to have modified that understanding a little bit in the objectives and the requirements. But this time we really set it, we all came together at the right time, and we were really pushing the limits of—we normally work in, say, millimeters per second velocity over a second or two. We're now talking about microns per second over a few seconds. And I think the guys in the end got down to about .03 microns per second over five seconds or something, which is—you don't need to go down to one second, but the spacecraft moves enough and it's high enough up that there's only so much resolution you can get out of it.

But yes, we did factor into the design, or into the requirements, the capability of the design, and I remember using that at various stages in terms of what I thought we could do, that Maria could then, in turn, interpret how she would see that in terms of the crustal structure, for example, which is what we were after. I was more interested in the deep interior in some senses. Maria was interested in the deep interior, but she really wanted to see the crustal structure, so one of the ground rules, in my mind, I set out was, "Maria, if you want to see in the crust, the crust is 40 or 50 kilometers thick, we've got to design a mission that will get you resolutions of, say, better than 25 kilometers. Otherwise, you will be looking at more than—if it's 100 kilometers, you're looking right through the crust, you've got the crust and everything, but you won't be able to see the crust itself, separate the crust out."

So if you want to set down to 25 kilometers, you really have to fly at 50 kilometers or lower. So 25 kilometers, 50 kilometers. Then at 50 kilometers, can you get a signal big enough and accurately enough to see a signal from 25 kilometers down? And that was the part that the ranging people, the Ka band were doing. So we brought those together, and it turned out the answer was yes for this 30-kilometer 1-degree-square on the surface. We thought we could do better, but we didn't think for a moment we could sell it any better than that. We had to be cautious about credibility.

Sure enough, within weeks, maybe a little longer than weeks, but we'd done the 180 field that we talked about, went into the second one in three months. We got up to degree and order 600, which is approximately four times the resolution that we were promising, and it's just amazing, the stuff started to drop out. That was all in the 50-kilometer orbit. That was over in three months, I think it was, three months and a bit.

CONWAY: I think that's right. Then you got another two months, I think, in a lower orbit, right?

SMITH: Yes, that's right. So the extended mission, what shall we do then? How should we handle this? Well, the engineers said, "Look, we've got enough fuel." And Lockheed Martin was good as well, very good. They said, "Look, if we want to do it again at a low altitude, we literally have to twist the spacecraft around, reverse the order and drop them down, and that's how we would do it."

"Okay, if you can do that." So we took about three months to bring the spacecraft down to about 23 kilometers circular, thereabouts, half altitude, and I knew we could only last for maybe another three months. We'd run out of fuel to control the spacecraft.

Actually, at the time that we were considering the extended mission, it was my job to put together that proposal, and Maria's classic statement was, "Don't call on me until you need me." She can't give you 10 percent of her time doing the kind of work she had been doing. "But I'll give you 100 percent of the time when you need me." Okay. And the argument goes 10 percent of her time, of course, is better than 100 percent of many people, but that's exactly what happened. And she turned it upside down, because I said, "I think the right thing to do, Maria, is stay at a higher altitude. We can likely get another six months at this altitude, and we'll really pin down the low degree field and tell us about the interior structure of the core, is it fluid, is it solid, etc."

And she looked at me. She said, "No, no, no. I want the crust." Wow! And I'd written a follow-on proposal with the high altitude and tides and all that stuff, I was at that time more interested in the interior structure. She says, "No, I don't want to do that." Okay, we've got to come down for the lower altitude. Okay, we can do that.

So I worked on then what we could actually get out of that, and it became evident that with these spacecraft, with the field we'd already got of 50 kilometers, we could control that spacecraft right down—there was never any fear of hitting the surface. It would eventually, but we had time to correct it and bring it back up. The Lab could do that all right here. So that's what we went, and it became evident that if we get down to 23 kilometers, my guess is we can do at least 10-kilometer thickness of the crust. Can't do that exactly in layers, but you can get that kind of resolution. We don't know what the crust will be like, actually, but we can get down to that, maybe even smaller.

So we went with it, and for another three months, from something like September to December or thereabouts, we flew at about 23-kilometers altitude, got another complete mapping of the Moon, and there was still some fuel left over, and I think we had then about a few weeks, we dropped it down even lower. I can't remember what it was exactly. I think we dropped the periapsis down or something. Anyway, we came down below that altitude of 23 kilometers for a short while before it actually crashed, and the two spacecraft were still operating and we saw them fly over the rim of the Orientale Basin at about two to three kilometers above the rims.

Mind you, this is a crater that is quite big and is quite deep, so you go over the rim at two kilometers, then about thirty seconds later, you're now over the floor, which is about eight kilometers below you, so it's very strange. Actually, that presents a little bit of a problem. It's very high resolution out here, but you don't have that resolution down there, and that's an analysis issue that you have to try and fix, because you've got different resolution. It was terrific. Maria eventually wrote a key paper on the Orientale Basin from that, showing how you could see in the gravity field the ring systems that forms the Orientale Basin.

So coming down low was undoubtedly the right thing to do. I'm not sure we wouldn't have done very well at higher altitude, but this was dramatic stuff, and it really changed our thinking about the surface of the Moon. Maria was able to show that with the low-altitude period and the knowledge we had of the shape and, of course, the gravity field, particularly the gravity field itself, that the only way that that crust could, in her opinion, be interesting or as uniform as it was, that it appeared to be, is the crust is probably pulverized completely down to about 25 kilometers depth or something like that. It means that the bombardment over 4.5 billion years, and most of it in the first billion years, would have really mixed it all up like that, and any different crustal material here or there would have just about gone away, not entirely, but in many cases it was. So it raised a very good question about if this has happened to the Moon, why not elsewhere like Mars? Is Mars' crust pulverized like this? And we still don't yet know because we don't have a gravity mission, and Mars is more difficult because it has an atmosphere, not much, but it has an atmosphere, and you need to get close again, 25, 30 kilometers or something like that. That's a tough one. And that'll be sold sometime in the next couple of decades, I expect.

But yes, the scenario of going from the primary mission to the second part of the mission and then finally the—I don't know what we called it—the final phase of the mission, we knew it was going to crash, we were going to lose all control, and then to see it—well, we couldn't see it crash exactly, although it's on the near side, but LRO recorded the locations and could see that the craters on the surface of the Moon, when the two crashed, images of them show these small craters—well, they weren't actually. There were strikes somewhere along the surface, but you could identify them with the changes in the regolith color, the albedo. They were about two kilometers apart, I think, one after the other, and they ended up in slightly different positions, but well within—we knew the topography. We knew which mountain they were hitting. It wasn't going to hit a flat surface; it was going to hit some bump somewhere. And it did exactly as expected.

CONWAY: To go back to the beginning again, Dave Lehman credits your kind of rigidly-maintained goals and objectives as being part of delivering on cost too. Of course, you have a lot of experience with lots of missions in which that doesn't happen; there's endless requirements creep. What do you think enabled you to prevent the kind of requirement creep that happened in lots of other missions?

SMITH: Well, in this case, this was a very unique kind of mission, a mission in which there was only one measurement system onboard. The measurement system happened to be two spacecraft. We weren't trying to see how to fit a radar into a space that suddenly needs more space or more money or anything like that. We didn't have that problem, I'm glad to say. It was not easy for Lockheed Martin or the Lab to get these two spacecraft to literally talk together and lock together for over 200 or 300 kilometers, as the case may be, and stay that way, fly each other, and as they went around the surface like that, the gravity field torques the orbit just a little, that they would keep that orientation of the two antennas pointing at each other the whole time. And, of course, it had to be tracked from Earth as well, but relatively speaking, that was easy when it was on the near side.

Yeah, I think the mission in some senses was a simple concept of a set or single measurement we really wanted to take, and it didn't compete with itself, so we did everything to make that one measurement. If we'd had to compromise between altitude for an altimeter and the imaging size for a camera or something, I don't know what would have happened, and that does happen, compromises along the way. Other missions, an instrument, a good example would be the Mars mission that's on the surface with the seismometer, as you know, from France [Mars InSight]. That seismometer, nothing to do with the spacecraft, nothing to do with the other instruments. The seismometer turned out to be not ready, so they delayed *two years*. The impact of that was *enormous*. Everybody, not just that mission, but everybody, $200 million had to come from somewhere, or whatever it cost.

So I think the cleanliness, in a sense, of this mission, in the instrument being two spacecraft with essentially no other instrumentation other than to help us understand the measurements in the spacecraft itself, two spacecraft, enabled that to happen. That doesn't happen very often, and it's not likely to happen very often in the future, in my opinion.

We didn't know we could control the cost that well, but it turned out we did, and I think Maria deserves a fair amount of credit for that. Every Monday morning we'd have a meeting with

Dave Lehman, and one of us would probably be out there going through the cost. When one of the teams came in or one of the components, whether it's Lockheed Martin or the lab, came in and said, "Well, we need a little bit more money to do so-and-so," Maria would question them and expect to see a written response. Dave Lehman would lay it out, what it is, where the money was coming from, what it would do to the reserves, blah, blah, blah, blah, all that stuff. And I don't remember ever any saying, "No, we can't do that." There might have. I'd be surprised if there wasn't some, but I don't remember any.

But it was good. Everybody got instilled in the idea of, you know, we have to keep control of this cost-wise and everything, and schedule-wise, and so I don't think it was ever a schedule slip, not that I'm aware of. Dave Lehman did a good job, and he and Maria worked out any differences, as it was. Maria was very tough, as I said, in my opinion, was tough. I could never have got away with that myself at Goddard, for example, but then being a PI at the institution you're working at is very different than being a PI for an outside institution. So I could have much more influence at JPL being at Goddard than I could at Goddard. I suppose it does make sense, but that's the case, you know.

CONWAY: I've heard that many times from different generations, even, of people.

SMITH: It's not going to change.

CONWAY: Organizational environment.

SMITH: And line management. The projects and line management—there's naturally tension.

CONWAY: Yes, absolutely. So I'm about out of time. I've got many other questions, but I'll end with this one, what didn't we talk about that you think was important? In other words, what didn't I ask about that I should have?

SMITH: I don't know. I would say one of the things we haven't mentioned is that I believe the success of GRAIL had quite an impact on NASA, on institutions like Goddard and JPL, that were able to bring a mission, a fairly complex mission when you think of two spacecraft, on schedule, on time, and on budget. "Oh, no, we did the same thing," you know. But usually they'd been fed some extra money somewhere along the line, but the thing had been increased. But I don't think there was any other mission quite that came in like that and then gave money back, and I think it was an eye-opener that it could be done. I think all of the GRAIL project deserve credit for keeping it that way, and I think even now GRAIL is a good example of how it is possible, although it's getting a little historical now, how you can keep a mission within cost and on budget.

All missions are different, so I'm not sure you can carry over too many lessons learned. I'm always being asked about lessons learned, by the way, and it's not as if you're building a dozen of these things. You'll do one at a time and each one is slightly different, they all have different requirements.

It was a unique mission in many ways, GRAIL, at a unique time. It had its share of luck just like all successful missions do, in being selected and everything going according to plan, but luck doesn't just happen by luck. You have to engineer it, if you know what I mean.

CONWAY: You have to be prepared for it.

SMITH: Exactly. And the Lab and Lockheed did a good job in seeing that happen, and, of course, Maria ran a fine project. So I look back on GRAIL very affectionately, just like I did the old MGS mission, which turned out to be also good from a science point of view in terms of what we were able to do with Mars Global Surveyor, but we never did manage on Mars Observer on the first try. That was a blow, but we all bounced back, and MGS turned out to be a great success. The measurements corrected on Mars Global Surveyor are still, in many cases, the unique measurements that were obtained. That's certainly true of the topography that we got on Mars. People are still using that as the baseline. It's usually the map of choice when people take a picture of Mars these days. They colored the blue ocean area, but it's not that way at all. But it has that kind of vision to it that people use. It's quite an attractive picture.

But, yes, GRAIL was good. Probably some things I should have told you about, but I've rambled on for an hour.

Okay, fine, Erik. Thank you.

CONWAY: Thank you. Take care.

[End of interview]