W. Bruce Banerdt

July 19, 2022

Erik M. Conway,
Interviewer

Q: So it's Tuesday morning. I'm talking to—you go by "Bruce," right?

Banerdt: Right.

Q: Bruce Banerdt. This is Erik Conway, and we're going to talk today mostly about the InSight mission. Bruce, you did an oral history with Susan Niebur about your previous project effort, the NetLanders, back in 2009. So I'd actually like to pick the story up there, and the first question is, you were already working on getting a seismometer to Mars back then. So, why? What's interesting about that?

Banerdt: Well, when I first was in graduate school doing a degree in geophysics in graduate school back in the mid-seventies, the Viking mission was going to Mars. In fact, the first summer I worked at JPL was 1976. I was actually doing a summer internship at JPL when Viking landed, and for a geophysicist, the exciting thing about Viking was not all that life stuff, but the seismometer, because seismology is how we know about the interior of the Earth, all the details about what's going on inside the Earth. Seismology was peeling back the layers of the Moon and giving us an understanding of the interior of the Moon. The thing I was enthusiastic about was interiors of planets, deep interiors of planets. I love geology, I love the sky, but what's going on under my feet down to the core of the planet, that's really what floats my boat.

So the idea of going to Mars with a seismometer and starting to look inside the planet, to use a seismometer, to turn Mars basically into a looking-glass where you can see down to the depths of the planet was really exciting to me. When that didn't work out, for a bunch of different reasons, it was a big disappointment for me and for hundreds of other geophysicists around the world. So I always thought, "Well, it would be really great if someone would send another seismometer and fix the issues with the Viking seismometer, and I'll be here when that happens. I can't wait for somebody to do that," which is what we usually do, right? I mean, something like that is a huge undertaking, and especially as a student. I, at least, didn't have enough imagination to imagine that I would be at all useful in that particular endeavor. I was still learning how to use the data, much less generate it.

So I was just going along that way and going through my research and coming up against dead ends, you know, just about everywhere I turned with my analyses to figuring out what's going on in the planet, in that I didn't have the data. I used gravity data, I worked on MGS [Mars Global Surveyor] to get topography data, which I could use with the gravity data, looking at the magnetic data, looking at all kinds of things to try to figure out what's going on inside, but basically always kind of butting my head against a brick wall and waiting for somebody else to fix that by getting the data.

Then at some point, somebody at JPL was trying to develop a space seismometer, and they needed sort of a science figurehead, and I thought, you know, I can be a figurehead as good as anybody, and that's how you get sucked into things like this, you know. [laughs] And once I was the guy that was working with seismometers, then, you know, I would get called by the Program Office if they had a study, and I started thinking, "Oh, wow! This could actually happen and I could be on the boat when it sails."

And before I knew it, I'd spent a couple of decades getting deeper and deeper and deeper into designing and figuring out how to do a seismic mission and figuring out how to sell a seismic mission and trying to get the community onboard. My own scientific output started suffering, and I essentially just let that do what it did, because it always seemed like a seismic mission was right around the corner. It's like a Lucy-and-the-football situation. There's like one mission after another after another. Seems like every two years there was like a really good chance to fly one of these missions, and I always wanted to be in on the ground floor.

So that's how I got to 2009 and having basically lost NetLander. 2009, I think I had already put in a couple of more sort of Discovery-level proposals, all of which got some good marks and some bad marks, and none of which got selected, to 2010,which was the Discovery round where we put in the InSight proposal. So 2009 is kind of a good jumping-off place if we're going to talk about InSight the mission in particular, but if you want to talk about what InSight is doing at Mars and why it's doing it, you have to go all the way back to the seventies.

Q: So just to recap, the problem with the Viking seismometers is they weren't coupled well to the surface. Is that basically right?

Banerdt: That was the main problem. First of all, one of them failed because mechanically it never uncaged. It had a mechanism that kept it safe during launch, and then once it got to Mars, it decided to stay safe for the rest of the mission as well.

Then the other big problem was that it was sitting on the deck of a spacecraft that had both springs and dampers between it and the ground, so that you didn't get very good coupling with ground, but you got excellent coupling with the atmosphere, and it wasn't that sensitive a seismometer either. They knew that they had issues with coupling, so they didn't need a super-sensitive seismometer, and what we found with InSight is that the tremors on Mars are not very large, and you really need a sensitive instrument to pick them up, at least in the location that we're at right now.

Q: The other small follow-up question is, you said somebody was interested in building a seismometer for planetary stuff at JPL, and then you became kind of the figurehead. Who was that person?

Banerdt: That was Bill Kaiser. He was one of the founding members of the Microdevices Lab at JPL, the MDL. He was an engineer, sort of a research engineer that JPL got, I think from GM or Ford Motor Company or something like that, and he came to JPL and he was really enthusiastic. He had a displacement transducer, something that actually measured distances at the atomic level. It's called an atomic force microscope. He looked at that and said, "Well, a seismometer is just something that measures the displacement of the ground. I have the perfect device for that. Let's turn it into a seismometer." So he needed a scientist to get requirements for him and tell him what it was exactly we needed to measure, so it was sort of a hammer-in-search-of-a-nail kind of a situation, as far as he was concerned, and I guess I was there to hold the nail. [laughs] I don't know. So he was very enthusiastic about that.

A few years after we started working on that, he actually left JPL and went on to, I guess, greener pastures for him, and his project was taken over by several people, the foremost among was Tom Pike, who is now at Imperial College, London, and who sort of went with that technology and developed it into the SP, short-period seismometers that we have on InSight today.

Q: So I think that was also in your—there's a bit of that because Tom Pike was in the 2009 interview, so that was how some of your international partnerships came about.

Banerdt: Yeah.

Q: You mentioned you had already tried Discovery proposals a couple of times before?

Banerdt: Yeah, the first one we put in was actually a Scout proposal, which was the Discovery equivalent in the Mars Program, and that was in the early 2000s. Then we tried to put together New Frontiers proposal, which was called Cerberus, and it was four different landers, basically four InSight landers, and that one never got out of JPL. They decided they had to decrease their portfolio and concentrate their resources on a fewer number of proposals, and we were deemed to be the weakest of the lot, an evaluation that I disagreed with. [laughter] I maybe wasn't the most dispassionate observer of that.

So I guess InSight was probably—I guess it was our first actual Discovery proposal, and, in fact, I think it's the first time that a Mars mission was allowed in the Discovery competition since the very first Discovery selection, which was the Mars Pathfinder mission, which wasn't exactly a competition, but that's a whole 'nother history.

Q: I know. That was assigned just as NEAR was assigned to APL to get Discovery program going, yeah.

Banerdt: Right.

Q: So you tried a couple of times and gotten, I guess, mixed reviews, it sounds like. So then tell me about putting together the proposal that becomes InSight.

Banerdt: Well, we had a lot going for us on the InSight proposal in that we had put in a number of proposals, and in addition to the NetLander, the GEMS, which was the Scout proposal, Cerberus, we'd also put in a number of proposals to put seismometers on other people's missions to Mars, and so by the time we got to InSight, I sort of feel like we knew what most of the problems are going to be. I tell people, as a joke, that if you put in enough losing proposals, you run out of ways to lose, and what you're left with is a winning proposal. And there's some truth to that. We knew what people had complained about on previous proposals. We had studied this a number of times and we had our own set of issues that we had come up against and resolved or at least identified and knew needed to be addressed somehow, and so when we put together the InSight proposal, we had the GEMS [Geophysical and Environmental Monitoring Station] proposal as kind of a framework, and we had the very detailed critique of that proposal from the Scout Review Panel.

And, of course, the Discovery or the Scout review process is a very exhaustive and detailed process. There's probably fifty different people that are looking at your proposal and trying to figure out what the strengths and weaknesses of that proposal are. So that's actually a really powerful tool if you have the staying power to go back and resubmit these proposals over and over again, to have that many experts pointing out the flaws or pointing out the things that are really your strong points.

So we worked very hard to take those things and not just try to turnkey fix this thing, fix that thing, but to incorporate them—I don't know how you say—organically into the process and understand what's behind it both technically and what's behind it psychologically. I mean, a lot of selling proposal is not just the technical aspects. I think that we were up against—I can't remember—twenty-five-ish proposals for missions in that Discovery round, and I don't think there was a single one that was a crappy mission. They're all great missions. There's probably a handful, half a dozen or maybe a few more, that were pretty clearly technically not ready for primetime, you know, first-time-in sort of thing, but that still leaves a dozen or more proposals which are ready to go as much as we were.

So how do you differentiate yourself from a pack of excellence? The way you do that, there's lots of different ways to do that, but it has almost as much to do with psychology as it has to do with engineering, and we went at it, and I specifically went at it in several different ways. One way I went at it was going to meetings and talking about this mission and talking about this science just about everywhere I could, and my philosophy was, is that if this is everybody's second-choice mission, you know, a geochemist is not going to want a seismology mission as their first choice, but if they're thinking, "Well, if they can't do this geochemistry mission, that seismology mission sounds like a good one," if we're everybody's second choice, then, you know, we've got it made.

So I tried to make it seem like this was an inevitable mission; it was going to happen sometime. I worked within MEPAG. I was selected to be on the Science Goals Document

Writing Committee, and I did work collaboratively with people. I didn’t ramrod things through, because, first of all, I’m not a good ramrodder. I’m not that kind of personality. I’m sometimes glad I have those kinds of personalities on my side, but I don’t do that. But I can be sort of quietly persuasive, I think, and so just being in all these groups and trying to be accommodating and acknowledging other people’s science goals as well as my own, I mean, I fought for seismology, I fought for geophysics, but I never tried to stand in the way of sample return, which was always sort of the gorilla in the room. I never tried to stand in the way of surface geology, which also results in great missions and a lot of great science, but I was always saying, “But also this geophysics, also this seismology. This is why you should care about it. This is why it melds with your science goals,” and so forth.

And I think after about fifteen or twenty years, people did sort of view this as a mission that’s going to happen sometime, it’s inevitable, and people were okay with that, and it was also a mission that no one thought was a dumb idea, in general, at least. So trying to sort of get the field ready, get the garden ready for the seeds, you know, get the soil all fixed up and ready to go so that when we throw the seeds out there, there’s a fertile place for them to germinate, that, I think, was a big part of getting this proposal or getting ready for this proposal to be selected.

The other part was actually writing the proposal itself. One thing that we did from the beginning was to try to be completely open and transparent. I think a lot of people tried to do that. I don’t think we’re unique that way. But I also think that being upfront about our issues, I never found that it was very successful to try to hide these things, because there are a lot of smart people out there that can dig up dirt. So if you can dig it up first and own it, that always makes everything else you’re saying more believable, and it gives people, I think, more respect for what you’re doing, and tends to get people on your side. On a review panel, you have to have some people on your side in order to win it, because it’s too easy to dismiss things that you don’t care about. So you need to have some people who are interested and feel passionately and other people who at least are inclined towards you, and that can get you a long way on a review panel.

Sort of the next thing that we did was kind of an accident, actually, and that was the whole idea of the science traceability matrix. This is a thing that had been used in proposals all the way along in the Discovery Program, and NASA and the review boards had put more and more emphasis on that, and by the time we got to this Discovery 2010, the science traceability matrix was started on one end with sort of the big-picture goals from the Decadal Survey and then went through more and more levels of detail until you got to the other side of your science traceability matrix, which basically was “These are the instruments with this capability that are going to get those goals.” And most people, I think, or certainly myself, in previous proposals going into that had sort of treated the science traceability matrix as just sort of an illustration, right? It’s like something that you sort of pull some pieces out of your proposal and it’s kind of a nice little picture, word picture, of what you’re doing.

But we got a lot of guidance from the JPL—what is the name of the group? My memory for words is getting poorer and poorer as I get older.

Q: Is it the Formulation Office?

Banerdt: Formulation Office. Thank you.

Q: No worries.

Banerdt: The Formulation Office says "This is really key. People are going to look at this first. This is what people are going to make basically their first cut of what proposals they care about and what proposals they don't, and you have to not only show which instruments are going to address which goals, but you have to show quantitatively how it's going to happen." And that didn't make any sense to me at first, and then it took us a long time to actually develop this science traceability matrix, but we ended up putting precise numbers for if we want to understand differentiation of a terrestrial planet, what do we need to know? Well, we need to know the crustal thickness, we need to know the core size, we need to know the core density, and we need to know something about the structure of the mantle. Okay. So there's the first three columns of the matrix. Now, how do you get the core thickness? How do you get the crust thickness? Well, use seismology. And then that's as far as I would go before. Okay. Well, how are you going to do that? And the answer is, well, it's complicated. But that doesn't make a very good matrix entry, "It's complicated," right? Or, "Trust me," or, "Magic happens." [laughs] I mean, those are all kind of the same thing.

So I found that very frustrating that they were demanding that I put that kind of detail into a table where people write books about this kind of stuff. But eventually, you know, I gritted my teeth and worked with my team, and we actually came up with sort of strawman analysis techniques that would, in principle, get you these quantities, and by estimating things like the seismic activity of Mars and something about the properties of the interior, we could figure out, well, how many quakes would there have to be on a planet for us to be able to see this many quakes, and how many of those quakes we need to see in order to be able to get such-and-such a precision.

We put together a few of these sort of storylines, you know, scientific analysis storylines, to getting from a quake on Mars to a measurement of the thickness of the crust, and we were able to put numbers on that. Whether those numbers are believable or not, that's a philosophical question for the ages [laughter], but it turned out that we were pretty close, for whatever reason. But just having that much specificity, saying, you know, if we can get a seismometer that can measure 10-to-the-minus-9 meters per second-squared per root hertz, and if we can do it with this level of noise background from all these different things on the spacecraft, and if Mars is giving us quakes at this frequency, which we have some reasons to believe, and these are the reasons, then we will satisfy these goals. And it was a very specific and concrete story, and you can quibble with all kinds of steps along the way, but it was very concrete and it also looked like it was very technically possible to do it. So I think that really kind of put us on the map in terms of the Selection Committee.

We also had the advantage that we were using a lot of hardware that had already been built before, so there was a perception that the technical risk of the mission was low, and that's always been a double-edged sword in all these proposals, using old hardware, and I think in our case it did cut both ways. For the spacecraft and the lander, it absolutely decreased the risk. We've been remarkably free of technical difficulties with our spacecraft system.

With the instrument, it did not work so well. We had a seismometer which had, in principle, been built before, but there were enough changes, and it had never actually flown to Mars, so a lot of the difficulties with the design had not yet been revealed. So even though we thought and we advertised that we had a seismometer that was basically ready to go, it turned out that it was not ready to go, and it ended up biting us in the rear and causing us to slip a launch opportunity. But I will tell you, and I think I've told other people who believe otherwise, that we were absolutely truthful in our proposal. We thought that we had it in hand. We did not cover anything up. We truly believed that we were one step away from building a seismometer that was going to perform at the level that we advertised, and it was as big a shock to me as it was to everybody else when it didn't.

Q: So let's go back. We've talked entirely about the seismology, but there was another major experiment in the heat flow experiment, which personally I was really looking forward to. But tell me how that got into the proposal. Tell me that story.

Banerdt: Well, heat flow, scientifically it's the flip side of the seismology. Seismology gives you the physical structure of the planet. Waves bounce off and refract through boundaries between different materials. So with a good seismology experiment, you end up with the structure of the planet.

The thing that you'd really like to know is how the planet works. A planet is an engine. It's a heat engine. So all the geology that you see at the surface is driven by the heat engine of the interior, and what a planet looks like at the surface depends on the mechanical happenings that are going on inside the planet. You have uplift due to heating, you have the crust pulling apart due to motions in the mantle, that are driven by convection, which, again, is driven by heating. You have volcanism, which is driven by heating. Everything in a planet is driven by heat trying to get out of the planet and doing work as it does so.

So that dynamical piece of it you can infer from the structure sometimes or a lot of the time, but you would really like to know something about the dynamics, something about the energy, the way energy behaves inside the planet, and the basic measurement of that is the amount of heat coming out of the planet, the rate at which heat comes out of the planet. So it's a really super complementary measurement to make, so as a geophysicist, it's a no-brainer that that's what you'd want do to.

Most of the ways to measure that are pretty easy if you have a couple of humans and a tractor, but if you wanted to do it on another planet with a handful of kilograms and a handful of watt-hours of energy, then you have to scratch your head and figure out how to do it. And we've looked at drills over the years, we've looked at trying to do it just using radiometers and looking at the heat radiating out just up from the surface of the planet, lots of different things like that, but this mole seemed like—and it still seems like—a really elegant solution to this problem, because what you want to do is you want to get down at least a few meters below the surface and measure the thermal gradient, measure how quickly temperature is rising, and if you know what the thermal gradient is and you can measure the thermal conductivity, that tells you how much heat's coming out of the planet at that point. If the planet's a perfect sphere, then you're done. If it's a little bit more complicated, then you've got a lot of Ph.D. theses to figure out what that means. But at least the measurement gives you a starting point, gives you something to start trying to tie things to.

So it looked like we had a method that could pull a string of thermometers, basically, down to 3 to 5 meters, which is actually a long way. I mean, if you're sitting in a room, you probably have 3, 3.5 meters up to your ceiling, okay? And we were going to go another couple of meters deeper than that. So that's pretty far down into the ground. The mole seems like an impossible thing to actually accomplish that. The first time anybody looks at that mole, they say there's no way that that thing can do it.

First, how do you hammer yourself down in the ground? I mean, unless you're the Road Runner or Wile E. Coyote, where you can take the sledgehammer and hammer yourself down in the ground, that doesn't really work that way, but there's a lot of really interesting physics that goes into that mole, and we know that it works because we have a 5-meter-high tube of soil in Bremen that we've hammered down to the bottom a dozen times or more. We have a 2-meter-deep cylinder of soil at JPL—well, it's not set up anymore, but it's around somewhere—that we hammered to the bottom of dozens of times, so we know that it works, but the conditions under which it works are limited to a certain set of conditions of soil, and we ended up finding some that it wouldn't work in, which was unfortunate, but we did learn quite a bit in the process. That was a huge disappointment that we did not get the mole down.

Honestly, I've watched the mole work in the real world, quote, "the real world" outside of the Laboratory in three different situations. Back in September 2002, I think, I went to a conference in Sicily sponsored by ESA, and one of the things they did at that conference was they had a bunch of instruments that were being designed and built to fly to Mars, and they had a demonstration of these instruments. They had some spectrometers and various different things, and they set them up in sort of a parking lot that had been bulldozed out of the sight of Mount Etna, so it was just this volcanic cindery area which could be similar to some place on Mars. A guy named Lutz Richter, a German engineer, had designed this mole, and he set it up and he put it on the ground and he started it going, and it didn't go anywhere. [laughs] It didn't work. He was messing with it and pushing it around, and never did get it to penetrate.

The second time, we took the engineering model of our mole out to Mojave Desert and we were setting it up not to show that it could penetrate, but to actually make seismic waves that we were going to pick up on a seismometer so that we could look at how we could use the pulses from the mole to look at the structure of the soil down to several to tens of meters. So we put the mole on the ground and started it going, and it didn't penetrate then either. And there were reasons in both cases.

The third time was on Mars. It didn't work then. So I've never actually seen the mole work outside the laboratory, and we've tried to simulate in the laboratory as best we can. We've taken dirt, we've taken rock, we've put things in there. It works. I'm honestly not sure what else we could have done, and we've had people analyzing it. We have a theory, a pretty strong theory as to why it didn't work, but it would be really great to get to another part of Mars and see whether those theories are valid. I still think it's a really great and powerful method for getting things down to depth, but I guess it just wasn't ready for where we landed on Mars.

Q: Since, as the PI, you put together what was going to be into the mission, what was the social process of getting that measurement added? I mean, how did you meet those folks? Were they offered by ESA? Just tell me that story.

Banerdt: It's really pretty much of a grassroots kind of a process. I mean, Tilman Spohn, the PI of the mole, he's a geophysicist from Germany, and I've known him for twenty-five, thirty years. He's mostly done theoretical stuff, but he left the university for DLR and actually headed up one of their planetology labs, for which this guy Richter that I was talking about actually worked for him for a while, and he was very interested in the same kinds of things to do with the inside of planets as I am. Whereas I sort of got into the planetary geophysical measurement game through seismology, he kind of got into it through this mole and heat flow. We were sort of doing this in parallel, but, still, we're talking to each other all the time, we're at meetings, we're going out to dinner, and so he was a colleague that I knew, and a friend, and he's talking about this kind of stuff.

Suzanne Smrekar, who's also a geophysicist at JPL, also had a strong interest in heat flow, and we talked a lot about a surface mission on Mars. She was actually the PI of DS-2, which was a small penetrator mission that was carried to Mars in '98 or '99 on the Mars Polar Lander and was lost as part of that mission. That had a heat flow measurement in it, a similar, more rudimentary kind of heat flow measurement, but again, getting some temperature sensors at depth, in this case maybe a meter, meter and a half deep. That penetrator was going to do that kind of measurement. So she was very hungry for those kind of heat flow measurements, and as we talked, we sort of agreed on kind of a division of labor in terms of trying to put things together for a mission. She would kind of canvass the world for heat flow measurements, I would work on seismology, and so whereas she was still interested in seismology, I was still interested in heat flow, we kind of specialized and separated a little bit.

So there was actually kind of a shootout where we had some various different groups that thought they could make this measurement, some people that did drills and so forth, and just basically made a selection based on what we thought would do the job versus what we thought we could actually fly, and the mole came out on top pretty much as the only real possible choice for a Discovery-class mission. It was the only thing that was less than 40 or 50 kilograms and could do the job.

So Tilman was working on this stuff in Europe at the same time I was working on seismometers at JPL, and then Philippe Lognonné, the PI of the seismometer on InSight, was working on seismometers since the early nineties in France, and, again, we started out as competitors in the early nineties, like the JPL seismometer versus the French seismometer, and I think we ended up knocking each other out of several possible flight opportunities by pointing out the problems with the other guy's sensor. [laughs]

Then somewhere kind of around the mid-nineties, we got together and said, "You know, our two seismometers, the one at JPL and one at IPGP, are really complementary. The IPGP one is more well suited to make long-period low-frequency measurements. The one at JPL is lighter and better at making high-frequency measurements. Why don't we combine the two and we'll have an irresistible instrument to propose to these missions." So, again, we got together and sort of worked together ever since then to try to do this collaboratively.

Q: So as it turns out, we wound up with both instruments coming from overseas providers, but it sounds like that wasn't a strategy to help sell it. That was just who you thought had the best instruments?

Banerdt: It is, yeah. I really believe that we cast the net all the way around the world, we looked in the United States, we looked other places. Really the French seismometer was the only one that was even close to being ready to fly to Mars. The German heat-flow mole was the only possible way of measuring heat flow that would fit on a Discovery mission. We ended up getting our wind sensor from Spain as well. We originally were going to go with a JPL sensor that was based loosely on the Viking design, but when we started looking at the details of the design, looking at the performance, looking at the cost as well, we ended up going with the Spanish sensor, which was essentially already built anyway. So we didn't start out with the strategy of saying, "Let's get a payload from overseas that we don't have to pay for and then that'll help us get into the cost cap," but once we recognized that that's where the best instruments were, we definitely used that to our advantage and used that as a strategy for getting the most out of the Discovery cap.

It turns out it’s really difficult to land on Mars within the Discovery cost cap. There’s just a certain irreducible amount of stuff that you have to do to get to the surface of Mars, and that comes pretty close to the cap all by itself. So I’m not sure what we would have done if there was a seismometer in the U.S. that cost 15 million, 20 million. We would have had to make some different tradeoffs than we did, because all these missions, designing one of these missions is all a series of tradeoffs. So that’s the deck that I was dealt and sort of the foundation from which our tradeoffs were done.

And we tried to turn that into an advantage, you know. As we wrote our proposal, we knew that that would be perceived as a weakness by lot of people because if you’re not paying for something, your leverage is limited, okay? Whoever pays the bills gets to call the shots, right? And if you’re not paying the bills, all you can do is beg and threaten, right? [laughter] So that also meant that there’s a whole ‘nother layer of politics that went into this, in that if I was going to get a seismometer from CNES, I had to make sure that they wanted it as badly as I did, and only if they wanted it as badly as I did would I have any leverage at all. So I got to know the program managers at CNES, Richard Bonneville, Francis Rocard—oh, what’s her name? I can’t remember all their names now. [laughs] But I got to know those people. I was introduced by the PI over there. I went out to dinner with them, I went to meetings with them, I talked to them, and encouraged them to invest in the seismometer, and the reason why you do that is because once somebody’s invested in something, I don’t care how much you talk about sunk costs, nobody wants to give up on something they’ve put money into. By the time we wrote the InSight proposal, the French Space Agency had been putting millions and millions and millions of dollars in developing the seismometer for almost two decades, more than two decades. They'd actually flown it once on a Russian mission that only made it to the ionosphere before it came back, but they almost got it on an ESA mission, it almost went on Mars Express, so they were almost as hungry to get this thing flown as I was. They needed to see a payoff to all the money that they put into it.

So when I talked to people both at meetings privately and at NASA, I could convince them that this was not just an instrument that CNES was going to pull when the going got rough; this is something that they wanted just as badly as we did, and the politics of it were different than some of these other instruments. NASA has gotten burned on many, many instruments that they've agreed to fly from European partners. Either the funding dries up on that side of the Atlantic, or the agency there has different goals and they don't align with the goals of the NASA mission, and when that happens, all kinds of unpleasantness ensues. So one of the things that I had to convince people of was that this was a different situation, that the fact that we had international—in the United States we call it "international." [laughs] The fact that we had instruments from other countries and were being funded by other countries was actually a strength because these guys wanted it just as badly as we did. So we had a convergence of interest, and that convergence of interest is actually stronger than funding.

Q: That sounds like a tough story to sell, though, because, you're right, whoever pays the bills calls the shots.

Banerdt: Right. So what you have to make sure is that the person who's paying the bills wants the same thing you do, and if they do, then you're okay. If they want something else, then you could be in big trouble.

Q: Let's see. Do I have another short question? Because we're about out of time. In the proposal process, what kinds of demonstrations did you have to do, if any, to get to the winning side?

Banerdt: I don't know whether we had to do any demonstrations. I think one of the things that we had in our favor was that our landing system to land on Mars, that was probably one of the most critical things that we had to sell, that we could land on Mars and we could do it safely enough without failure, and we could do it within the cost cap. We had the advantage in that the spacecraft we were using had two prototypes that had already flown to Mars: Mars Polar Lander, which failed miserably, and then Phoenix, which was successful. So I tell people that we had an extensive test program going all the way back to Mars Polar Lander for InSight.

So we had a demonstration of landing our spacecraft on Mars, and we were very, very, I would say, ruthlessly diligent about not messing anything up in the landing system. We took the Phoenix landing system and did as absolutely little as we possibly could to change it, and we would change around all kinds of other things to stay within the mass envelope, stay within the dynamic envelope, stay within all the things that Phoenix had demonstrated it could do, trying not to extrapolate from Phoenix, just do exactly what Phoenix did to the greatest extent possible, and I think that went a long way towards selling our proposal.

We did have a mole demonstration, I think at our site visit, where we had the mole set up in a chamber up the hill with a video camera on it and monitors in the room so people could watch the mole penetrating down into the soil and pulling the cable down after it, which was a gutsy move because whenever you do a live experiment like that, there's always a chance that it's not going to go well, anywhere from wires breaking to it not performing the way that you thought it would. So we debated long and hard about that, and we finally decided to go with it.

The thing penetrated, and the review board watched it, basically watched that mole come down four feet into our laboratory soil, and I think that probably did a lot to sort of assuage the doubts about the mole, because that was something that always came up in every proposal that we put it, is doubts about the technical readiness of the mole and whether it would work. So whereas we had proposed it in the proposal as a nice-to-have, and that if it didn't work, we would still meet all our basic requirements of the mission, still, whenever you put an instrument on there, if people don't think it's going to work, it's going to detract from the mission. So we had to try to at least plant the seed in their mind that this thing could work. [laughs] So I think that did help to do that.

Q: Great. And we're out of time, so we'll have to schedule another appointment. Hopefully you'll see the transcript before that time. My transcriptionist has been pretty fast, so I'll schedule it once we've done that, but we've still got another hour of more stuff to do, easily. But it's been great. Thank you for your time.

Let me stop the recording.

[End of interview]