W. Bruce Banerdt

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Interviewer

Q: This is Erik Conway, and I'm speaking again with Bruce Banerdt, the InSight mission PI. Today is the 18th of January 2023.

So, Bruce, in our previous interview, you had said the InSight lander development itself benefited from Lockheed's having built it before.

Banerdt: Right.

Q: It was basically the third iteration, as I understand it, after the Polar Lander and then Mars Phoenix. So the question is, were there any real issues with the lander development? I know with Phoenix, for example, they had a lot of troubles with the landing radar. So did you have any real problems within InSight's lander?

Banerdt: Not a lot. We had some issues with the landing system. Mostly there was a big scare when the parachute development, which was going on, I guess, for Mars 2020—I think it was 2020. Anyway, they were doing some high-altitude, high-speed parachute testing, and they shredded a parachute or two, and suddenly that called into question whether our parachutes were made out of the same material or the packings were different and lots of stuff like that, because we were depending on—testing had gone on for Viking and Phoenix and things like that, so there was a lot of consternation about the parachutes for a while, and I think in the end, we decided to go with our parachute and everyone convinced themselves it was okay.

There was also a little bit of a problem with the heat shield. When they cast the heat shield, they did some testing and they found it had some voids in it, and they weren't sure whether that was going to be good enough. But there's always testing going on for new projects, and then, you know, you find out, well, maybe that impacts the equipment that you have, and it happens for electronics parts too. Some project has a failure and then you find out that you have the same components buried in some of your electronics, and then you have to decide what the risk is and whether it's worth taking them out and replacing them with something else.

So that's sort of the way life is when you have what they call heritage hardware, right? If you have heritage hardware, that means it's all been tested and used before, but some of it is still in use and then you have to decide, when something goes wrong with somebody else's spacecraft, whether you need to fix something on yours which you didn't think was broken. Of course, there's a lot of hesitancy to do that because that can be very expensive if you costed out the whole mission based on using stuff that doesn't have to be redone. So there were some problems like that which were kind of run-of-the-mill problems, from what I understand. For people who are experienced in this kind of stuff, that happens with all missions.

Mostly it was pretty much just business as usual. There weren't any real big development issues. Some of our contractors charged four times as much the second time around as they did the first time around for Phoenix, and we had to deal with that. I won't name any names, but there's a bunch of stuff there.

We made a couple of changes to the spacecraft. We made the solar panels a little bit bigger. We did some additional life testing on it so we could be sure it lasted two years instead of the three months or so that Phoenix had to last. But it was pretty amazingly smooth, I think, for a spacecraft development.

Q: And, of course, anytime you have to go in and change something, you also break the testing you've already done.

Banerdt: Right. Exactly. And, of course, the later you get into your schedule, the more you just kind of hold your breath and hope that some of these things don't happen somewhere else that then ripple out to you and make you doubt something that's already, like I said, been tested and buried deep into your system somewhere.

Q: So sounds like that was relatively unproblematic as things go. The next pre-landing question I have is, what landing-site restrictions were imposed by the lander as opposed to what your science goals were?

Banerdt: All the landing restrictions were imposed by the lander. Our science goals required that we be on the surface of the planet. Matt Golombek would argue that there was one more requirement that we have, at least five to ten meters of unconsolidated material to bury the Mole. But that was a secondary science goal, and if push came to shove, I was prepared to green-light a landing site that might not have that much unconsolidated soil on the top. So that was one science requirement which I considered to be kind of a soft requirement, but all of our requirements—in fact, that was one of the selling points that I made for the mission when we proposed it, is that we will land in the safest spot on Mars based on our landing system, and I wasn't going to throw any roadblocks in front of the engineers in picking a spot based on what they needed, and that was going to make things a lot easier.

Turned out that there weren't very many places on Mars that we could actually land and operate for a year. There were two sets of requirements. One was just the landing requirements, and those were mostly elevation, because you can't land too high on Mars because you're depending on the atmosphere to slow you down from the parachute and the aeroshell, so there was that.

The other requirement that had a big effect—oh, and, of course, rockiness. You couldn't have an area that was too rocky. They have lots of different parameters in terms of the number of rocks, the size of the rocks, the distribution of sizes, things like that. It couldn't be too dusty in the sense that you didn't want to land in something that had tens of centimeters of dust. You want it to be relatively dust-free. It seems like there was one other—oh, and you didn't want it to be too windy. There are places on Mars that are sort of chronically windy, and that's not good when you're coming down and the wind's blowing you sideways, because you don't want to land as you're moving sideways. You want to land straight down as much as possible.

So those were the sort of landing constraints. So, elevation-wise, Mars is kind of a half-and-half planet. The southern half is high and the northern half is low, so there's kind of a line around the planet that you couldn't land south of. Then there are ways from orbit of characterizing which places are rocky and which places are smooth, and then we have global circulation models which tells us which places are windy and which places are not so windy. So that's sort of the landing-system restraints.

Then the second constraint that got laid on top of that was solar power. Since we wanted to be able to survive through the winter, we needed to land someplace that was relatively close to the equator so that we would have enough solar energy by the time we got to the end of the mission, along with the degradation of the solar panels that we anticipated from dust and whatever, and when you layered that all in, we found that we couldn't land any farther north than, I think, 5 degrees or any further south than 10 or 12 degrees from the equator, so it was a pretty narrow band around the planet that we could land in and still have sufficient solar power over the entire seasonal cycle to keep the lander alive.

When you start taking a map of Mars and you mask out all the areas that are too high and make those black, and you mask out all the areas that are too rough and make those black, then you draw kind of a band around the planet near the equator, it turns out there's only like three places on the entire planet that you could even consider landing at. One is the place that we eventually landed in, which is in western Elysium Planitia. The second one was a little area of Isidis Planitia, which is a basin a little bit to the west of us, and the third one was kind of in the floor of Valles Marineris near the outflow portion of Valles Marineris. The last one was not a great site because there's chronic winds there. Isidis wasn't a great site because there was a lot of rocks there.

So from the get-go, basically the first pass that Matt Golombek made, more or less what we could do, there's really only one place on the planet that actually satisfied everything, and that's where we ended up going. So we were able to sort of narrow down our evaluation pretty quickly to that one area, which was good because it kept us from having to do a lot of extra work, but it also was a little bit anxiety-provoking because if we couldn't find a place in this area that we ended up landing in, we were kind of up the creek. We were pretty confident, because it's a relatively smooth, featureless plain and had all the things, at least on the macro scale, that we thought we needed, but until we actually got the high-resolution HiRISE imaging and they did radar sounding of it with the orbiting radars, and Arecibo, and they did thermal inertia measurements looking at how dusty it was, and until they did all that, we were a little bit on edge.

Q: So the landing requirements really did tightly constrain where you could go.

Banerdt: Yeah.

Q: That's interesting. I've known some of that from the engineers, but it sounded like some of the other missions were not as constrained as you were.

Q: Yeah, that's true. I mean, the big constraint was really having it last through the Martian year. There were very few spacecraft, unless they're nuclear-powered, that tried to last through the winter on solar power, so that was probably the thing that limited us most aerially on Mars, the 15-degree strip around the equator. That was kind of the biggest restriction, and most missions haven't had to deal with that.

Q: Right, right. I also remember, I think from your AGU talk, that the Mole, the thermal instrument, was only one of your Level 1 science requirements. Talk a little bit about that, because I guess I'm maybe a fanboy of the heat-flow measurement, but I would have thought it would have played a larger role. So talk about how that came about.

Banerdt: Well, the marquee instrument for this mission's always been the seismometer. The seismometer is the instrument that was going to get the interior structure, the physical structure of the planet, as well as a measure of its sort of geological vitality, right? So the seismometer does so many things and gives you such a rich dataset, that getting a seismometer on Mars, as we talked about before, was really a key goal for geophysicists and myself in particular for decades.

But meanwhile, the heat flow, the thermal budget, the energy budget of the planet, is a really key—what's the word? It really is a complement, really complementary measurement, okay? The seismometer mostly gives you the physical structure. The heat flow and the energy budget gives you the dynamical driving forces involved. So they're kind of two different sides of the whole question of planetary evolution. It's really great to get both of those measurements.

The problem with the heat-flow measurement is if you could actually determine the true heat flow of the planet, that would probably be up there pretty close to the seismic measurements, but to do that, you really need to make dozens of measurements all over the planet in different geological areas, sampling lots of different stuff, to actually have confidence that the measurement you're making has some global value. So making this heat-flow measurement, even if we had accomplished it and done it very precisely, would have given us a lot of food for thought, a lot of stuff to work with, but it wouldn't have answered the question of how much heat's coming out of the planet. You would still have a lot of modeling, a lot of assumptions involved in extrapolating one-point measurement to the entire planet.

One great thing about seismology is it tends to sample lots of different parts of the planet from one location. It's kind of a remote sensing type of measurement, so you do get sampling bias, but it does give you more of an idea of what the whole planet is doing, rather than just what's happening right under you. So heat-flow measurements are a really important measurement. One measurement is great, but it's not quite as compelling as the seismic measurements.

For that reason partially, and partially just for the reason that we knew this was a challenging measurement to make, and we didn't feel like we could hang the success of the mission on something that was that risky and still get selected, we made the case that the seismometer, along with the dynamical measurements, the radio science measurements, were sufficiently compelling to choose this mission. In addition, we have this other measurement, which is a great measurement, but even if we can't make that measurement—and, you know, as a reviewer, you can decide whether that's likely to be possible or not—even without that measurement, this is still a mission worth flying, and that's why you have these sort of above-the-line requirements which are the "threshold" requirements, and then you have the additional ones that are sort of below the line, what you call "baseline" requirements. So in terms of accounting, if you will, you only have to actually get the threshold requirements to be considered a successful mission, because when they make the selection decision, they're basically saying, "Okay, let's assume that all those requirements below that line are not met. Do I still want to choose this mission to fly over Mission B, C, D?" So they're kind of icing-on-the-cake things. So that's why I would categorize that as a secondary measurement.

Q: Fair enough. So I'm going to skip forward a little bit, I guess, because we should talk about the launch delay. I've gotten a lot of technical details out of Ken Hurst on the seismometer's developmental issues, so we don't have to talk about that. What I want from you is the story, how do you discover the seismometer problem, how is it transmitted to NASA, what's their response, your story of all that.

Banerdt: Okay. This is an interesting story, and I'll have to try to really be brief, because I could probably talk for hours on this. [laughter]

So the seismometer had two components, had the very broadband seismometer, the VBB, which was developed by the French, and then it has the SP seismometer, short period, which was developed originally at JPL and then taken to Great Britain by Tom Pike and was being built at Oxford in the U.K. So, again, the SP was sort of icing on the cake. We really had sold this mission, based this mission on the performance of the French VBB seismometer, which they had been developing for more than 20 years. This was a descendent of the seismometer that actually had flown on the Russian Mars '96 mission. I guess it was finally Mars '96.

CNES had the longstanding support of the development of the seismometer which was being done at the university, Institut de Physique du Globe in Paris. They had an aerospace contractor that was building prototypes for them, and so they had a lot of development work, a lot of testing behind it. They had a flight model of a previous incarnation of this, which had been made bigger and better and everything since '96, but the basic technology was the same. So we had a fairly high degree of confidence that this seismometer was pretty much ready to go. We had the design, they had testing that showed that it had the performance, it had the ability to withstand launch loads, it had all this stuff. It was cool. That wasn't going to be something we had to worry about.

We went over and sort of set up things with the French after we got selected. They were very gung-ho. They were very committed to this. CNES put a lot of money into it, and this was a prestige instrument for them, and to get the opportunity to fly it was a big deal, so CNES was behind this really strongly, which made us feel really good. I have a long both personal and professional relationship with the folks at IPGP, scientists and engineers there that developed this seismometer, so this was a great situation.

The French said, "Leave us alone. We know what we're doing. You can come to our reviews, but please don't make much noise. We have work to do." [laughs] It was relatively arrogant, no more arrogant than JPL is, but, you know, relatively.

Q: Fair point. [laughs]

Banerdt: Yeah, fair point. A lot of our engineers were kind of chomping at the bit. They're not getting enough insight into what's going on, they can't do oversight, you know, working with foreign partners, plus they can't understand the guys' accents. It was all kinds of stuff, but, all right, everything's fine. Then their schedule kept on slipping and slipping and slipping, and, again, that's not unusual in these flight projects, but it was kind of annoying and concerning.

So by the time we got to where the contractor, Sodern, which is the name of the company, was ready to deliver the first potential flight models of the VBBs—they were going to deliver them to IPGP, which does all the testing, because they have the scientists and engineers who really know the instrument; they were going to do the testing and that stuff to great ready for flight—I think they were about a year behind schedule, but we still had time to do things. It was annoying. We'd lost a good chunk of our margin, but it was okay.

So they delivered, I think, four of these units, and basically out of the box, they're broken. I mean, they delivered articles that just don't, didn't, and would never work. It's like you buy a new car, you turn the key, and smoke starts pouring out from under the hood. It was that egregious. Of course, we were upset. The French were upset because turns out that CNES, as a space agency, and IPGP, as sort of the managing scientific technical authority on it, they were basically being put off by Sodern, their contractor, the same way we were being put off by them. "We know what we're doing. Leave us alone. We'll deliver something." And then they delivered crap.

Of course, there was immediate huge "We've got to get to the bottom of this," and we had excellent cooperation by the French in general. I mean, there was embarrassment there, and I think there was an understanding right away that they really didn't have much of a fig leaf to stand behind at this point. If they wanted this to be successful, everybody had to get their fingers in and cooperate and collaborate, find out what's wrong and fix it.

So almost immediately, JPL was given complete full access to everything that was going on. There were ITAR issues with us helping them, and I'm not exactly sure exactly how we managed to get through all that, but basically we were able to go in and tell them how to fix stuff, which is supposed to be against the rules. Even worse, Sodern, turns out they're an aerospace contractor and most of their business is defense oriented, so they're a defense contractor, and not only are they a defense contractor, they're a key contractor for France's nuclear deterrent force, so they build components of nuclear weapons. And in the end, we showed them how to do things better, right, which is supposed to be the whole reason behind ITAR, is not helping other people build weapons, and particularly nuclear weapons, but somehow people looked at it, and, I mean, we were completely open with the export-control people. They looked at our stuff, they looked at the situation, and they gave us the green light to work on this stuff. So we did.

So right away, we found out the big problem was that these seismometers, these exquisitely precise and sensitive devices, were full of dirt. They were filthy. We have all these tiny clearances of microns, where you have these pendulums that go up and down between two places, and it was all just full of crap, like had been left on a piano for a while and collecting dust. There was just so much dust. So our people went to their lab and they looked at their contamination control, and it was a joke. It was terrible. And the crazy thing is, the company actually knew how to do contamination control. If you look on the other side of the classified/unclassified divide, the people in the classified side had perfectly good, clean labs, with effective controls and stuff, the people on the other side did not, and there was no communication between the two sides of the company, which were literally in adjoining buildings on the compound.

This is all my understanding of listening to people explain things in the hallways. We never got anything that was quite that detailed officially.

So we brought our contamination people from JPL, who are world experts on that. They taught them how to clean up their labs and how to operate them in a clean fashion, and we had our people sit in those labs for the rest of the program, doing spot checking to make sure it was maintained at the proper levels and so forth. We had contamination-control people over there and quality-control people over there, three at a time, for the next two years. Well, more than two years. Anyway, a constant JPL presence.

Okay. So as soon as we got that problem fixed, started getting into the design, and we found that—I don't even know if I can go through—there was a series of about four or five different issues, and every time you'd fix this one issue, it would reveal the next one, okay? So when the contamination was done, we tried to operate it, and the springs were not working properly. The springs were this really complex series of like 24 overlapping little leaves that had to go like this [demonstrates], and if there was a tiny—well, Ken [Hurst] I'm sure gave you details about that. So the manufacturing process on that had to be altered. The testing process had to become much more rigorous, and we started throwing out more and more—they would build a dozen of these to get one or two that would actually work out of that.

So once you got the springs, then there were clearance problems. There were still issues that even if there was one bad particle left, it could get into a certain place and stop the whole thing. One piece of dust could actually freeze up the instrument. We had to redesign so that things were unlikely to get in there.

What else? What else? Then we found some cracks that we had to evaluate and change. There was one thing after another.

Finally, we had everything fixed. The VBBs were working properly, the actual sensors. And then one day—they put them in these vacuum vessels, okay. Vacuum technology does not seem to be cutting—you wouldn't think that's a cutting-edge thing, but one day one of these things, a sphere about this big [demonstrates] collapsed. It just "pow!" And it turned out that when they'd done the structural analysis with these CAD programs and Finite Elements, they'd done the structural analysis and figured out how thick the thing had to be, and they put all kinds of loads on it, and everything was fine. It was quite thin, but they hadn't, like, clicked the box where it says "Do the buckling analysis," which is you have a bridge, and a bridge is perfectly strong against this arch, but if you pull the keystone out of it, the whole thing collapses, right? Because it's very tightly balanced.

So the buckling analysis says, okay, if you just push in a little bit, does it pop back or does it collapse? It's sort of a local equilibrium, right? How wide is your local equilibrium? If you push it this far [demonstrates], does it come back, or you get over the side and it runs down the hill? Well, they had failed to do that analysis, and it was unstable to buckling. So they had to quickly redesign and rebuild these spherical shells, which were quite difficult to do.

So, finally, that was done, put it together. At this point, the rest of the spacecraft was in ATLO, was being built, is put together, they're starting the testing, and so they have dummies, seismometer dummies on the spacecraft, and were constantly reworking the test schedule so that we could do all the tests, and then at the very end, put these seismometers on and do the sort of bare minimum numbers of tests with the seismometer, because it's getting later and later and later. I think at this point, it's like January of 2015 or something like that, and we're getting up into the spring of 2015. Normally, this thing is supposed to go to the launch site in the fall, and we're looking at getting the seismometers there in the summer and then going through whole things were—we're shuffling all this different stuff.

We finally get the seismometers together, start testing them. They're great, it's wonderful, everything's working out. Get to the last test, which is the thermal vacuum test where they put it in a vacuum chamber in a freezer and get it down to Mars pressure and Mars temperature, and this is literally the last test that you do before you put it in a box and put it on a plane to go to Denver. And it started not working. The performance just started just cratering.

Took about two days to figure out that there was a leak in this vacuum chamber. And these seismometers were built to operate in vacuum for two reasons. First of all, just the air resistance for this pendulum to move by nanometers is a damper on it, and that hits the performance. The bigger problem is that vacuum's also an insulation. We had like five different layers of insulation around this, because thermally, if you're thinking we're looking at motions of 10^{-10}, 10^{-11} meters kind of on the molecular radius of scale, the thermal expansion/contraction is a *huge* noise on top of that, plus just the Brownian motion of this mass on a spring, you know.

We have about 50 grams, but when all the atoms line up in a certain direction when they're vibrating, actually one of the major noise sources is just the Brownian motion of the atoms in this mass.

So it just was not going to meet our requirements, so we had to pull it out and put together another tiger team that says, well, what's happening? We finally figured out that there was a leak in one of the feedthroughs. The way they designed it, you need about 80 different wires going into there, which is maybe not the best way to design something in a vacuum system, and, again, another engineering failure, they had the plugs that all these wires go through were vacuum rated, they're vacuum plugs, but their thermal rating was only down to -20C or something like that, and we had to go to -80. When they went down to -80, they cracked, and when they crack, they leak.

So, again, we're saying, "Okay, can we fix this? Well maybe we can fix it." So we're reshuffling all the testing programs with the spacecraft. We have about 15 different engineers that are going back and forth to France all this time, and we have four or five of them in place at any one time. They're kind of cycling in and out. We've got a guy there, Jason Feldman, and Ken basically around the clock. They're living in France for months at a time, and they're the de facto team chiefs over there, even though the French are nominally in charge, which provoked a lot of resentment from people, understandably. JPL's culture is to be very direct and can feel very harsh to people who are used to being quite polite with each other and deferential and so forth. There was a lot of friction, a lot of friction with various different people from JPL. There were a lot of people on the French side who welcomed that and who thrived in that kind of a situation, and there were some who didn't work with it as well.

There was friction between CNES and this contractor. Originally, they had very little leverage with this contractor because the contractor gets some huge portion of their funding from their defense department and not from the space agency, so they tended to say, "Leave us alone. If you really want something, go to the defense minister." I think finally they got the defense minister to tell them to "Get in line. This is a black eye to France and an embarrassment to the country." So we finally got rid of their project manager, who had privately told people on the French side that he couldn't see how this thing could possibly work from the beginning and he's doing it because he's told to, but, you know, "It's crazy. This can't possibly work." So the project manager didn't really think the project was possible, so he wasn't really directing it that well.

Q: That's a bad sign. [laughs]

Banerdt: That is a bad sign. So he finally left the company to go to greener pastures, and we got someone in there who was a younger guy who was eager to work with the Americans and was very cooperative and very competent.

So we went through about four different iterations of trying to fix this leak. We were finally at the point where if it works at this time, we were going to ship it straight to Vandenberg, where the spacecraft was already being prepped for being put on the rocket. We had a two- or three-week testing program that would sort of test all the interfaces with the seismometer, do a nominal performance test, and then just bolt it on and blast off, instead of the six months of testing we originally planned.

But the final test of our final attempt to fix it didn't work, and so that happened I think like three days before Christmas that we got back the results of that final test. They called us up and said, "No. We've got a leak still." I had to march down the hall with my project manager and tell the head of 4X that this launch was not going to happen, we needed to call NASA, and went on from there.

Q: Was this still Jacob van Zyl? Oh, no, that wouldn't be right.

Banerdt: I think it was Jacob. I think Firouz had left at that point and Jacob was in charge by then. I can't remember now.

Q: I've got it wrong. You should have been under Fuk Li, the Mars program, or were you under Solar System?

Banerdt: We were under Solar System, so we were under Firouz for a while and then Jacob. I can't remember when the handover was.

Q: I'll find it out. So tell me about having to tell NASA and what their response was.

Banerdt: It was, obviously, uncomfortable for me. I was devastated by the whole thing. I mean, it was just a horrible outcome. I didn't feel too bad about talking to NASA, because, fortunately (in my opinion) we had kept NASA right in the loop with what was happening every single step of the way, and basically getting their buy-in, because every time there was a new problem, we would go and we would explain it to the people at Headquarters, explain what went wrong, why it went wrong, and how we want to fix it and why we think that's going to work, okay? And we would talk to the people in Planetary Science, and every once in a while we would brief John Grunsfeld, who was the AA at the time for the SMD, and these were all technical people, as it turned out, in that iteration, and they all said, "Okay, that makes sense. You're not asking for more money." At this point, we're still burning our reserves. We never asked for extra money before the launch slip. So they would give us their blessing and we'd go on.

So when we finally got to tell them that this didn't work, they were disappointed, as we were, but they did not feel like, you know, this was something that was brought to them out of the blue. So they were disappointed and sympathetic. I think they felt like they were on our team, honestly, you know, so we did have a very good relationship with Headquarters and we did try to make sure that they felt like they knew what was going on and had a stake in it besides just seeing what happened at the end.

The harder thing was the press conference that followed shortly thereafter. It was just an audio call, but that was hard. I don't remember much from that, but I dreaded that and just got through it somehow.

Q: Then I had a question about JPL's institutional support, because it sounds like you had to put a lot more people on the seismometer from JPL than you intended, you had expected, and so forth. How did JPL arrange that?

Banerdt: I guess I—I'm not sure exactly. I don't remember. Of course, Tom Hoffman, the project manager, was the one who had to orchestrate all that. But what I do remember is everybody that we needed, we got the best people available. There were never any major problems with JPL support. I mean, this is a high-profile project for JPL, and they treated as such. Honestly, I mean, that whole experience—I mean, I've been at JPL for—what it is—55 years now? That can't be right. Forty-five, 50? Forty-five years. [laughs] I've been at JPL for 45 years, and this experience really opened my eyes to what an amazing place JPL was, and I always thought JPL was an amazing place. But every time we had a problem with the seismometer, it was like, "Oh, wow, we've got a contamination problem," well—boom!—there's like three people who know *everything* about contamination control, not only theoretically, but hands-on, and have solved all these problems before in several different places, and they got it fixed. Okay.

So then we got "Oh, the micromechanics of these springs and displacements of nanoradians and, oh, my gosh, none of the analysis software can even look at this by three orders of magnitude, they just get lost in the rounding errors. What can we do? What can we do?" "Oh, well, there are three people at JPL who wrote textbooks on this and can look at this and say, "That reminds me of the—and you've got to worry about atomic defect migration here." Okay, they know what they're doing. They can fix this problem.

Then cryogenic vacuum technology. "Okay, we've got three experts here. You take your pick. Which two do you want?"

No matter what the issue is, there is someone at JPL who is *the* world expert in it, and areas of expertise that I didn't even know existed, we have experts in it. We got the people we wanted, and they went over, they spent months in France fixing this problem and getting it to work. It was just incredible.

Q: And so what would you say were the consequences of the launch delay? I mean, were there any for InSight besides the cost increase, obviously?

Banerdt: I think the consequences of the launch delay were an incredibly better instrument, incredibly better mission. I think that if we had fixed the seismometer and launched on time, I think we would probably have been successful. Maybe not, but probably I think we would have been. But I would say probably half of what we were able to do, or some fraction of what we were actually able to do, the launch delay allowed us to fix everything that we could find to fix on the seismometer, not just the vacuum. We went through the whole thing, fixed several other little details, were able to put together twice as many units to choose from for the flight unit, do extra testing, plus on the spacecraft itself, it allowed us to take out some of those components that we were kind of iffy on because of other failures, right? That gave us two years to go in and sort of do some clean-up on the spacecraft, do some extra testing on the spacecraft. I think that two years allowed us to put a new heat shield together. I could be wrong about that. I know it allowed us to do more testing on it and sort of resolve the heat shield issue that we were going into the '16 launch with some questions about.

So I think a lot of times the difference between absolute stunning success and struggling to meet what you need is razor thin, and I think that the delay allowed us to get way on the other side of that razor and make sure that this mission was the kind of—the hardware was the hardware that we really should be launching. Without that delay, I'm not sure that something wouldn't have stopped us at some point. Some overlooked issue, some problem could have stopped us early. We were so much more confident of our hardware, so much more confident of our processes with that extra two years, and it really showed in the execution of the mission. I mean, the mission went off almost flawlessly. I mean, for a prime mission, wow. I mean, we had one safe mode, which was just kind of a hiccup, on knowing when to reset the command lost timer that we recovered in two days, and cruise was completely—nothing untoward happened during cruise, allowed us to do stuff that Lockheed and JPL didn't want the scientists to do because there was time to do it. And the landing was flawless. The instrument, it performed at a level that was multiple times better than our requirement. Yeah, the mission itself went off almost without a hitch, until the dust started slowing us down after a couple of years.

So, to me, that's the main outcome of the slip. It's the best thing that ever happened to the project. [laughs] NASA might have a different idea since it cost them about another—I guess they budgeted like 130 million, but we gave about 20 million of that back.

Q: Hopefully Tom Hoffman will know the number.

Okay. So it's 1:00 o'clock my time. Do you have to go?

Banerdt: I don't have to go anywhere right away. If you have a few more questions, I'd be happy to talk a little bit more.

Q: Okay. I do have some. Let's see. I wanted to ask you, tell me about landing day. You've kind of already intimated that it went flawlessly, but tell me about your landing day.

Banerdt: Okay. Well, landing day, that was exciting. Well, first of all, you've got several days leading up to it, where you're starting to make the final course corrections and everything's looking good, and everybody's coming to town for the big circus, right? The media's there. I guess we landed on Monday, which meant that I was going in on the weekend to do interviews and stuff like that. It really did feel like the whole world was converging on JPL. Of course, other than talking to people, there's not much for me to do, even as the PI. I went to the meetings for the entry, descent, and landing, and gave the go-aheads on things, but things were going pretty smoothly.

Then the night before, they had sort of the final decision on the final course correction. There's always some issue, whether you want to fix these last little errors in the trajectory at the risk of making something worse, right? When you're in that close, the last course correction you make, you can't fix again, so you have to figure, well, what's the possibility of this might go wrong, and then you don't have another tool in the bag to fix that. So the night before landing, there was a meeting at JPL which, unfortunately because of other personal commitments I wasn't able to attend in person but I was on the Webex for, and Thomas Zuburchen was there, NASA brass was there. There were like 35 or 40 people in this room for the technical meeting, and so the team went through the data they had, and then there was a discussion about whether we do this or don't, what are the uncertainties, what do you think.

And since then—I've heard Zuburchen say this on a number of occasions—he comes back to this particular meeting over and over again, and he says that was the most interesting, exciting meeting that he's ever been in because he saw all these people raising—everybody up and down the line, anybody who had a question would raise a question, they would discuss it, everybody would be open, and in the end, they came to the decision based on everyone's input, everyone's expertise. There wasn't any hierarchy, there wasn't any top-down dictating. He was just amazed by the JPL process of everybody was listened to and everybody felt free to speak up with whatever they had on their mind, and it was taken seriously. So I supplied him with one of his life-changing experiences, which that's always fun. [laughter]

So I think in the end, we didn't do that course correction. We thought we were good enough. So we went in the next morning, it was a beautiful day, my whole family goes in with me, and I'm giving interviews in the media tent and running around, and everybody's asking me questions and stuff.

Then about an hour before landing, I go up to the MSA, the control room, and I take my seat in the back row. There are all these displays up there, and there's a lot of hustle and bustle, but as you probably know, it's really all just show. There's only about three people in that room who are actually doing anything, and all they're doing is looking at the communication coming in and making sure it's going up to the screens, right? [laughs] So everybody else is just fidgeting around and looking at numbers and worrying. I mean, there's a lot of anxiety, but, honestly, I felt pretty comfortable, pretty confident.

I remember taking a taxi from the Dulles Airport into town once, and I shared a taxi with, I think, Rob Manning and another one of the EDL engineers that I can't remember, and this was a couple months before the Phoenix landing. They worked the whole Phoenix landing system. We were talking, and Rob says, "You know—." I said, "How do you feel? You guys good?"

He says, "Well, you know, it just might work." [laughs]

I thought, "Whoa! This is the guy who knows as much about it as anybody." He says, "Yeah, it just might work." [laughs]

But it did work. Then we got all the input from that, and our EDL people were the most paranoid bunch of engineers you'd ever want to have on a project, and they just beat every single avenue to death, and by the time we got there, I thought that it was pretty unlikely that we would really have any problems. So that didn't keep me from worrying, you know. You don't land on Mars that often, and you're completely out of control. Of course, heard all the horror stories about the landing radar, you know, and the heat shields' thin spots and shredding parachutes, so, you know, there's always a possibility. But I was on pins and needles and following everything.

I don't think the displays quite kept up. There's one point they show the lander kind of going down this line towards the surface on the plot, and at one point it just stopped. I'm going, "I don't really think it does that." [laughter] And it's something about they lose communication at some point, then they get it back. So, anyway, I don't think it ever quite finished its little arc, that last little arc to the surface, but we were listening to the call-outs. Then they're calling out, "100 meters, 50 meters, 10." I'm trying to do the calculus in my head that says is it speeding up, is it slowing down, trying to—and, you know, "Five, four," and then you see on the screens it all goes to zero. But we were trained; we don't make any reaction for the cameras until the launch controller or—I don't remember what they called her, but the woman who was making these call-outs says, "Touchdown confirmed." You have to wait till "Touchdown confirmed" before you can move.

So you see it go to zero, and you're like [demonstrates] "Don't move." Then "Touchdown confirmed," and—boom!—that's it. I mean, it's on the surface and it's confirmed because we got telemetry back from it, which all happened 15 minutes earlier in the relativistic universe, but, you know, that's science.

Q: It's a while after landing before you get the seismometer off and onto the surface, but tell me about the moment when you knew you were going to have a successful seismometer.

Banerdt: Yeah, that was actually even more nail-biting than the landing for me, because we'd had so many problems with the seismometer, and every time we fixed a problem, we uncovered another problem, remember?

Q: Yep.

Banerdt: So the last problem that we fixed was this vacuum problem, okay? And then we didn't fix any of the problems after that, and that's either because there weren't any or because we hadn't gotten to it. And there were a couple of issues with—like I said, there were some ceramic parts that showed some cracking and some places where wires went through that we realized later were not properly glued down and could break under thermal cycling. So there were a few weak spots in the seismometer that we knew of, and there were all the unknown weak spots which may or may not exist.

So until the seismometer was actually on the ground and leveled up and let go, we didn't know what we had. There was a whole process of that, too, because we had to pick up the seismometer, put it on the ground. Then we had to level it with the landing legs on the seismometer itself to get it within half a degree of level. Then we had to center the seismometer itself, get it off of its stops and actually move its little balance motor back and forth until it got balanced. Once it was there in low-gain mode, make sure that everything's fine, then we switch it to high-gain mode, and this whole process takes at least a week. And you get to the end, and—bang!—it's working.

That was when I finally said, "Okay, now we actually have a science mission." There were 100 or 1,000 things that could go wrong, you know, starting from launch, like, okay, we didn't blow up on the launch pad, check that one off the list. [laughs] I had in my head, you know, probably about 20 specific things that could end the mission, starting with the launch, and so I was kind of checking them off one by one by one by one. When we got that "Touchdown confirmed," there were still about four of them left. The seismometer, when we finally got it centered, switched it into high-gain mode, and saw that we were getting the performance out of it that we expected, that was, like, the last checkmark, and at that point, I really knew that we had a mission, and that was in January, two months after landing, end of January.

Q: And then I think it's a long time before you actually get a seismic signal, wasn't it?

Banerdt: Yeah. That wasn't one of the checkboxes.

Q: I understand. [laughs]

Banerdt: There was a checkbox on my list that there aren't any Marsquakes on Mars. [laughter] That was definitely in the back of my mind. I'd spent 20 years convincing people that there were, and pretty much succeeded in convincing myself, for what I still consider to be good reasons, and they were, but we started listening, and you don't hear anything for a week or two, that's nothing. Three weeks, four weeks, six weeks, you know, now I'm in the mode where I'm getting up every morning, the first thing I do is I get to my computer and I look at the seismometer website to look at the analysis from the data that came down the day before. Still nothing, still nothing. Still keeping a bright, sunny, optimistic face on things. But when it got to a couple of months, I was really, really starting to get worried, talking to the seismometer PI about it, you know, making dark jokes and so forth. But, yeah, when we were getting into March, that was starting to be pretty tense, in my mind, as to—and we kept on going back through the statistics. Is this really what we'd expect? If you really look at statistics, how often do you expect to see something? Well, what about this and what about that?

Then we finally saw something, and it was pretty compelling. It wasn't a slam dunk. It still took some convincing to convince yourself that this had to be a Marsquake. We had to eliminate the possibility of something happening on the spacecraft.

There were still people who doubted that that was a Marsquake for a long time. I went to Seattle to the Seismological Society of America meeting and announced this. We'd only seen it, I think, a week or two before the meeting, so we kept the announcement until the meeting and were able to announce it, put it up on the screen. I could see people in the audience going [demonstrates] [laughter], kind of shaking their heads and frowning, and I got some questions. Not being a seismologist, I was a little bit nervous fielding the technical questions, but I guess I did okay. I had backup in the audience, and they were all happy with my answers. I pretty much answered everybody's question. It was a pretty compelling signal, but it was not a slam dunk. We got a pretty good slam dunk about six weeks later, and then we were off to the races.

Q: Is that just—it's not a written-down question. Is that just a matter of getting a kind of probability? You had such a long wait and then you got a whole bunch of things?

Banerdt: No, it turned out to be the way that Mars' weather works. So we landed in the middle of noisy season on Mars, which was not something that we knew existed.

Hang on. Let me just look at something real quick.

Okay. Yeah, during the—I think it's fall and winter on Mars, the winds just blow all the time. I mean, they blow and blow and blow. Actually, I don't want to give the wrong impression. The breezes blow. It's breezy. It's not really that windy. Then at the end of this particular season, the turbulence that occurs in the evening just completely collapses, and you have almost no air movement at all from about 5:00 p.m. till midnight, and then from midnight to 6:00 in the morning or so, there's kind of a little bit of a steady wind that gives you a little bit of noise. So during that basically one-year period, there's like a year where it's noisy and a year where it's very quiet for that six to eight hours, and I think I would have to say something like 95 percent of the quakes that we detected were in that quiet time, and we landed in the middle or near halfway through, three-quarters of the way through the noisy season. So Marsquakes were happening; we just couldn't see them because the noise level was too high, which is still below the noise level of the quietest place on Earth, by the way.

Q: So it was being masked essentially by other environmental noise

Banerdt: Yeah.

Q: I didn't catch on with that, because I haven't read enough yet. I've only got a few minutes left, so I guess we should end. I still have questions. We haven't talked about the mole, for example, but we don't have time to do it today, unfortunately. So I will end this, and I'll send you the transcript in a couple of weeks for you to edit and send back, and I'll also set up with Tom Hoffman. I talked to Philippe Lognonne at AGU, but I haven't interviewed him yet. I'm trying to set up a time with him. I haven't reached Tilman Spohn, though, so I haven't got there yet.

Banerdt: Okay.

Q: That's where I am. So, any questions for me?

Banerdt: No, but if you really want to learn about the mole, you might talk to Troy Hudson, because he was Ken Hurst's equivalent for the mole. He probably knows more about it than Tillman does.

Q: I had a talk with Troy, and he's already seen the transcript and edited it and so forth.

Banerdt: Okay, good.

Q: But I also try to get the PIs to tell me their background and so forth, why they did that, why they're interested in this stuff, just as I did with you.

Banerdt: Some of those guys have got a long history behind the mole as well.

Q: Okay. Fantastic. Thank you for your time.

Banerdt: Sure.

Q: Have a great day. Bye-bye.

Banerdt: You too. Bye-bye.

[End of interview]