NASA Discovery and New Frontiers Oral History Project
Edited Oral History Transcript

Jessica M. Sunshine
Interviewed by Sandra Johnson
Potomac, Maryland – February 25, 2025

JOHNSON: Today is February 25th, 2025. This interview with Dr. Jessica Sunshine is being conducted for the Discovery and New Frontiers Programs Oral History Project. The interviewer is Sandra Johnson and Dr. Sunshine is in Potomac, Maryland, and talking to me today over Microsoft Teams. I appreciate you agreeing to talk to me and I'd like to start by asking you to briefly describe your education and how you first became interested in geology and what led you to planetary exploration science.

SUNSHINE: I think it's a good story. I was going to college and I knew I liked math and science, and the university I went to, Brown [University, Providence, Rhode Island], had this new program where they wanted you to have a faculty adviser that you actually took a course from when you were a freshman. I was going through the catalog, back when the catalog was an actual book, with my brothers. I eliminated math, even though I liked math, because I didn't want a math adviser. Long story short, my brother said, "Why don't you take a geology course? Because they have field trips and you'll meet people." I thought well, that's as good a reason as any. I remember liking Earth science or whatever it was called in eighth grade, and so I did, and then I really liked it. I really really liked it.

In the second semester there was a course called chemical geology, I think. Someone was on sabbatical, so the person who was partially teaching the course was a woman named Carlé [M.]

Pieters, who was a planetary scientist, and a guy named Mac [Malcolm] Rutherford who among other things did a lot of planetary work. I got the bug, because I happened to have them as teachers, and Carlé said, "If you ever want to do any research let me know." I came back then the next semester to work with her and Carlé eventually became my PhD adviser. It was really an incredibly lucky thing. I was an applied math major actually as an undergraduate and I got my master's and PhD in geological sciences.

JOHNSON: Did you know what you wanted to do at that point? Or that you had visions of working with NASA on projects?

SUNSHINE: When I took the course, no, not really. It was something I thought was fascinating, and I just kept finding interesting things about it. I worked for—actually I don't know if you know [NASA] Goddard Institute for Space Studies. It's in New York. During summers I was doing some work with Venus atmospheric stuff. I went to my adviser there, Larry [D.] Travis, and said, "I don't know. Should I go to grad school? I'm not sure what I want to do." He said, "There are people in the world who know what they want to do when they're born and they do it. Then there are people like you that just keep walking and you trip somewhere and you see something interesting and you follow the path. Don't stress about it. Just keep going." It's true. I've had a career of stopping in the middle of the path I was on and changing directions.

JOHNSON: Talk about that a little bit because you were in school in the '80s, right?

SUNSHINE: Yes.

JOHNSON: You were getting your education late ’80s. Things were changing some then. I know when I talk to women engineers it’s always interesting because they were few and far between still. There were starting to be more women in the field. Did you see that in this field also?

SUNSHINE: Geology was a little bit I think farther advanced than some places. I was again just lucky that I literally ran into Carlé, so I had that role model right there. It was never a huge issue for me in an immediate sense because I was in an environment where it was not really a problem. It changed in a broader scale. It became more obvious. But from my cocooned world I was actually fine. Had a really nice start that way.

JOHNSON: That’s good. Talk about after you got your PhD. What were you doing after that?

SUNSHINE: I decided I didn’t really push to stay in academia. I wanted to go off in some other directions. In particular I was interested in Earth remote sensing. I was interested in the business world. I actually did that for about 12 years. All the while I was actually able to continue to do some planetary work at 20, 25 percent of my time.

The transitions were easier than it might have been otherwise. I had my own grants at some point even though I wasn’t still in the university. But something important happened to me personally. When I was in grad school there weren’t many missions. Galileo was it. I remember when the Shuttle [*Challenger*, STS-51L] accident happened. Everything stopped and Galileo stopped. But I want to say 1990, and ’9[2] Galileo did two Earth flybys. These gravity assists. Carlé was asked to join the Galileo imaging team because she was a lunar scientist and knew what

to do with the multispectral cameras. When she was on the team, I was also able to join that team. I worked both of those. I really learned so much from those. I got to work with some really incredible people who are now major giants in the field who were young at the time. The team was entirely giants. It was run by Mike [Michael J.S.] Belton. As it turns out.

To be perfectly honest, Mike saw me and he saw my work. I don't think my PhD was dry yet. Literally like a month after my PhD he called me up and asked if I wanted to be part of his Discovery proposal. That was because Mike, before anybody had buzzwords about DEI [diversity, equity, and inclusion] or whatever you want to call it, he felt that there needed to be young people and young women involved in science, which was very much unlike the Galileo team. For example. He looked at me and another scientist Karen [J.] Meech as people he'd worked with before, and he wanted us to be part of the future he was trying to build with Discovery.

If you're a historian of the Discovery Program you know Mike was the person who ran the Decadal Survey that codified the Discovery Program and the New Frontiers Program.[1] Not only do I owe a lot, most of my career, to Mike, but we all do. Because he really felt after having been appointed Galileo imaging head and appointed with a team having what he called tenure because it went on forever that we needed to have this competitive process, smaller missions, more of them, that allowed younger people to get involved, to have multiple opportunities. He very deeply believed that. I owe a lot to Mike. He was a wonderful person. He had faith in me when I was a very very green scientist.

[1] Michael J.S. Belton chaired the first National Research Council Decadal Survey of Solar System Exploration in 2003, which established the foundation for the subsequent decade of planetary science and exploration.

JOHNSON: It's interesting to me that he had that vision, not only to bring in younger people, and people that didn't look like him, into these missions, but also to understand this PI [principal investigator]-led model that was new to NASA at the time when they were talking about Discovery. It was kind of crazy compared to these huge giant NASA missions.

SUNSHINE: I talked to him a lot about it. I think it was partially because he had experienced the opposite. Even though he had benefited personally greatly. He was the head of the imaging team for Galileo. He had all that stuff to work with all the years. But he didn't see that it was growing the community in the way that he thought it needed to be grown. I think on a personal level he really believed that competition was the best way to get the best done. Also the PI part of it, which we sometimes forget the control that the PI was responsible for, but also could make the decisions, and that you had a scientist who was making those decisions, not a project manager or [NASA's] JPL [Jet Propulsion Laboratory, Pasadena, California] or somebody in Headquarters. In partnership with all those people, but ultimately there was a scientist deciding what was important—because there are always trades that have to be made. I was privileged to work with Mike from 1990 through just when he died. Not only did he—as I said, he propelled me to what I ended up doing. But he was always there to make sure everything was going okay. He was a wonderful mentor and the epitome of a gentleman scientist.

JOHNSON: It's great to have a mentor like that that sees the value and knows how to help push people along that path.

SUNSHINE: Yes. He did the same I think for many other people, not just me. Certainly Karen as well. But others. I think he propelled the field. He really got this thing going. It took somebody of his stature and his conviction to really break the mold and not just talk about it but actually make it happen. Having the National Academy, the Decadal Survey, codify that, that's what NASA needed to make it happen.

JOHNSON: That relationship between of course Discovery and New Frontiers and the Decadal Survey. He was pushing for planetary science. That's the portion that he started?

SUNSHINE: Yes. I remember him saying, "We have to get our act together. Astronomy has got its act together. If we don't have our act together with a Decadal Survey as a community, we're never going to get change. We're never going to get anywhere." Yes. He both believed in the Decadal Survey as an important community thing to get the National Academy behind giving NASA science direction. But of course the biggest part of that first decadal was the Discovery and New Frontiers Program. It remains that way. The Decadal has a lot to do with what we do with the Discovery and New Frontiers Programs.

JOHNSON: I know it leads the science for the missions that get proposed and then eventually selected. Sometimes it takes a long time to get that selection. You said you started working with him in 1990. Was that on the Deep Impact at that time?

SUNSHINE: No, 1990. But the two Galileo Earth/Moon flybys, they were a year apart, or two years apart. I honestly can't remember. But after that there was a break, and in 1994 was the first

Discovery. That's when he called. For that opportunity, he called me up. It was a mission that didn't make it. I have to go back and look what the title was. But something like small SMACS [Small Missions to Asteroids and Comets] I want to say, small main belt asteroid and comet flyby or comet survey. It was this multiple target concept. In fact I didn't really know what I was doing, I was young, I was supposed to be the asteroid person. In those days working on a proposal, you listened to a telephone, and people talked about ideas. The whole process was so much simpler as well. The Step 1 proposals were I would argue much better in size and scope. We've gone way out of control. But that's another story.

I worked that first one with him. That was '94, sorry. Because I had just gotten my PhD. That was '94 when it was due. The next one was '96. That was the first Deep Impact proposal, which also was not successful. I was more involved because I understood more by that time.

Then in 1998, the next opportunity, Mike Belton stepped down as PI to deal with family issues. He felt he couldn't continue and do what he needed to do with his personal situation. Mike [Michael F.] A'Hearn became the PI. I honestly had no idea who Mike A'Hearn was except he was this voice on the telephone. When that happened, I called Mike A'Hearn and I think in one breath nervously said, "Hey, you don't need to keep me on the team. I was here because of Belton, you don't know who I am. We're now doing comets and I'm really not a comet person, and you're a spectroscopist, you don't need another spectroscopist." I went through this whole speech—he laughed in the way only Mike A'Hearn could. Mike A'Hearn had the best laugh in the universe. You could feel it. He said, "Look, I checked you out. You're good. A. B, I need help because you're going to look at surfaces and I'm going to look at the coma. Don't worry about it, you'll figure out comets, and I promise you you're going to end up loving them." All of which was true.

For Mike A'Hearn and I, that was the start of our relationship that went till 2017 when he passed away. Just phenomenal. I'm a very lucky person. We wrote the '98. We won. We won in 2000. Then I worked on Deep Impact. I was working with Mike on a numbers of things. I was at Maryland a lot physically. I did not literally become an employee of the University of Maryland till 2006. After Deep Impact happened.

JOHNSON: You were at SAIC then. Is that correct?

SUNSHINE: That is correct. What happened, for the record I guess, there was a number of proposals that needed to be written. I'm not sure the years anymore. But it must have been—it was 2006 exactly. Mike was writing the extended mission proposal for Deep Impact, which was called DIXI, Deep Impact Extended Investigation. I was the deputy PI. I was writing a proposal for which I was the PI and Mike was the deputy PI, something called Deep-Rosetta, which was an idea—I can come back to that one.

Then I was also involved pretty heavily in the M3, Moon Mineralogy Mapper proposal that round. Headquarters decided that I could no longer be at SAIC if I wanted to be part of Discovery because there was a group at SAIC that was—it's 50,000 people, they're way away from us, who are part of the review process, and somebody decided it was a conflict of interest. Even though it had been that way for a long time. In fact they were the review panel for Deep Impact and it was just written as a known conflict of interest. A stated overt statement. But something had changed. Basically if I wanted to turn any of those three proposals in, I had to leave. I resigned the day before they were due. Took a plane back. I was at JPL and came back. Became a Maryland employee.

It was very harsh. It was very difficult.

JOHNSON: Across the country too.

SUNSHINE: Yes. DeepR was—we called it DeepR. Deep-Rosetta. It was an interesting idea that we thought was worth trying. But it was something that I don't think people were quite ready for. It was really a built-to-print rebuild of Deep Impact that was going to go to the Rosetta comet after their mission was over but when they were still there. To do the impact experiment when you had all those huge numbers of ESA [European Space Agency] instruments that were just going to watch the whole thing. It was interesting but people don't like to do something twice. I learned that lesson a few times. People like to do new things. But it was still quite an experience to write it.

The reason I was the PI is because Mike, Mike, and Joe [Joseph] Veverka, who was another player in all this stuff, came up to me one day at a Deep Impact meeting in Germany and said, "Hey, we have an idea for you." I realized it wasn't really an idea. They were telling me I was going to do this. I thought well, okay, if these three guys think I can do it, then who am I to say no, I can at least try.

JOHNSON: That's pretty funny.

SUNSHINE: That's how I started that one. Everything is a good experience.

JOHNSON: Let's go back to Deep Impact since that was the first one. You were working on those other proposals but that was the first one that really got chosen. Talk about that process and what you were doing. I know you were talking about people on the phone and you hear voices and everybody's trying to decide. But as it got closer to being chosen and for the downselect and then for the select, just walk through if you can that process of getting ready for that.

SUNSHINE: I was a co-I back then. As Deep Impact became real, I sort of de facto became the deputy PI because Mike Belton was doing other things and I slowly—once it started becoming real. But that proposal stage, I was still in the back of the group trying to figure out what was going on. When questions were asked, I would go off and figure something out and write something. But a lot of those proposals particularly back then the PI and whoever, the couple of people they're working with, did the bulk of the work. You were part of it but it wasn't so personal. I wasn't nearly as involved as I was in subsequent proposals at that time.

I don't remember having lots and lots of involvement. I remember being involved in the spectrometer specifications for example. Trying to figure out what we needed to do. But the Step 1 proposal Mike and Alan Delamere, who was at Ball Aerospace, they rewrote the science case entirely about a week before it was due. They went to Kinko's and they had it printed because you had to print copies back then. They just didn't like it and they rewrote the whole thing. It was very much a tour de force if you will.

JOHNSON: When I was looking through the transcript of the [Discovery and New Frontiers] symposium, I was reading and you said that we were trying to do the same thing we do in the lab. But to a planet. I thought that was interesting. Maybe explain that a little more.

SUNSHINE: That's what I always loved about Deep Impact, so first of all I had an education in impacts because I went to Brown and was a planetary scientist, and one of my important contributions actually was bringing Pete [Peter H.] Schultz onto the team who's an experimentalist at Brown. I was taught by Pete. Pete was there. It's very different from when you're dealing with theorists. Mike A'Hearn being a telescope person also had that kind of experimental bent which is we don't know what's going to happen. We really don't.

This was the first planetary-scale experiment that somebody let us do. It's not just that we flew by something and we measured it. We were actually interacting with this comet. We had predictions about what might happen, but our range of predictions was quite large. There's a diagram in the proposal that shows the impactor going right through the comet. With a statement that says, "We don't think that's likely to happen but wouldn't that be exciting?" Something to that effect, which you couldn't do today. But back then you could do that.

It is that same excitement of you're not sure until you do it. You learn so much from any experiment because it never works the way you think it's going to. This one didn't. I just think it was stupendous that we were trying to do an experiment like that. Impacts are the most common process in the solar system, and yet we've only ever done one either on computers or in little, small laboratory experiments, and we don't know how they scale. We don't know how they scale in size. We certainly weren't sure what was going to happen on this body that was very low density, different materials, icy. There were lots of things that could have happened.

There was no way we couldn't learn a bunch. It's very rewarding because I think I'm the only one who's officially on both teams. We've done DART [Double Asteroid Redirection Test] now. Which is the other side. We knew when we did Deep Impact that it has implications for

planetary defense. But we weren't allowed really to talk about that. Just like DART was about planetary defense. But we learned a lot of science. But we don't talk about that. There's two sides.

But the fact, and LADEE [Lunar Atmosphere and Dust Environment Explorer], that we've had now three impact experiments is great. I hope that it makes people think about other interactive concepts, it's not just about calmly witnessing or grabbing samples. What else can we do while we're approaching these bodies to learn about them? That's the experimental side for me.

JOHNSON: It was also planned so that your team wasn't the only one that was going to see it. It could be observed from the ground or from these other telescopes.

SUNSHINE: Yes. The whole point of Deep Impact, the specifics of what we did, was that we were not Galileo or Rosetta. We didn't have all the instruments we would like. We wanted to do it in a way that other telescopes, ground-based telescopes, the other missions could observe, because we couldn't possibly do it in a Discovery budget.

That actually worked really well. It took a lot of planning and thinking but it was not accidental.

JOHNSON: Sharing.

SUNSHINE: I think it's one of the reasons we won frankly. Because we were doing something that was bigger than us. We were creating science for astronomers while doing our own stuff.

JOHNSON: May sound like a simple question or it may be very complex. But the point of it, to do this impact. What did you think at that point? It was an experiment, but what did you think at that point the outcome would be? What type of knowledge would that add and help?

SUNSHINE: Among the personal things we were trying to find out was how the comet's composition varied with depth. There were many theories that comets were onion-skinned. Different layers of material. As it turned out, there was almost no variation with depth.

That was one piece. The other piece that we were very interested in was the physical structure. We had a lot of indirect evidence that comets were very weak. Because we saw them fall apart like Shoemaker-Levy 9. Other comets disintegrate. But by doing the experiment and then comparing the various specifics of how the ejecta came out and what it looked like, we were able to show that this thing was really porous. Something like 75 percent porosity, which Rosetta actually eventually was able to measure more precisely, but for a different comet and later. It was very porous.

We also found out that the material is very porous, very weak, so it had some constraints on the weakness, but also very fine grain, like micron size. The thing we used to say was we used to say it was champagne snow. But then we got in trouble from people. It's a trademark. Now I just say high-altitude snow. Very fluffy. Really fluffy high material. If you take that all together, and there was some evidence for example that we were able to show not in the regular material coming out but then in the ejecta that there was ice and it was micron size ice. It had to be pure. It wasn't contaminated, which tells us a lot about how the ice and the organic material in comets are put together. The fact that we did not see a lot of layering makes us think that the comet is really in the same state it was when it accreted. All these tiny grains sticking together. Lots of

void space. That really changed. I don't want to say it changed but it changed the detail of what we knew about comets.

JOHNSON: That's interesting.

SUNSHINE: I have to say though we only hit one comet in one place. That was the point of trying to do some other like we'd like to do elsewhere, just because of one place in that one comet. Because the other thing we discovered, which was very shocking at the time, is that the comet is geologically incredibly complex. We have these flow fields that go for kilometers. There's cliffs. There's just tremendous morphologic variation on this fluffy ball thing that basically has no gravity. To some extent we still don't understand why that is, even though we've seen other comets. The surface variability of the comet was a real surprise. People thought it was all going to be uniform. It's not. They aren't.

JOHNSON: The impactor itself, I was reading, there was one description, somebody said it was like putting a copper penny out there and having the comet run into it. Like a semitruck running into a penny or something.

SUNSHINE: Yes. It was.

JOHNSON: Interesting way of describing it.

SUNSHINE: That's actually not a bad analogy. We put ourselves in the path of the comet. We hit it on its head. Or it hit us. Which is still incredible when you think about the timing and all that that has to work. It's mostly copper. I talked about this a little bit at the symposium [Discovery@30 and New Frontiers@20: A Symposium on the History of NASA's Discovery and New Frontiers Programs, January 18-19, 2024]. The impactor, again you couldn't do this today, but back in the day the impactor even in the Step A proposal kind of looked like a beer can. Or a soda can. Whatever you want it to be. Because the engineers designed it.

Then once we won, the two impact specialists, Pete Schultz and [H.] Jay Melosh sat down together and redesigned the whole thing, and now it became this compact bullet-shape and we had these copper front plates that were hollowed out. So we tried to hollow out the copper so it would match the density closer to what we thought was going to be on the surface. That will maximize the energy you put in. Then we had the ability to take off mass if we needed to as the spacecraft inferred. That design wasn't done till after we were already selected, which as I said is a different way of doing business than we are in right now.

JOHNSON: Did it have to be approved again or was it just like no, this is going to work better?

SUNSHINE: I don't remember there being lots of approval. I'm sure it was in the loop. But it didn't feel like somebody had a veto—it was more of an informing thing. Because nothing had been built yet. Pre PDR [preliminary design review]. By the time it got to PDR of course it was done. But it's radically different. Radically different.

JOHNSON: Like you said, Mike A'Hearn came in. You didn't know him at that time and so things were changing there with the team. But also things were going on with Discovery as far as where the program management was going to be located. There's a lot of change in that. But as far as the team goes, how did that affect the team when those changes were going on? Didn't you have different project managers too?

SUNSHINE: We had nine in total, two once. I think because of Mike A'Hearn and both his personality but also his incredible depth of knowledge, he was able to essentially become the project manager. At least the interim one in between while the other ones—and be able to bring the new one up to speed. I'm not sure a lot of PIs could have done that. To be the transition team if you will. Then manage to make sure that things didn't get dropped off.

He did a lot more of that than frankly probably was in his job description for sure. I think it would have been a disaster with many other people who have been PIs. Mike was technically capable of handling all of that and tried very hard to insulate everybody else from it. But it took a big toll on him.

JOHNSON: I've read about, and other people that I've interviewed talked about how important the PI is as far as management because so many PIs don't know how to manage maybe as well as a project manager should. The fact that he was able to do both.

SUNSHINE: That's both the management side and the technical side. He was an impressive person. But it did take a lot out of him.

JOHNSON: I can imagine.

SUNSHINE: Because he had to watch just everything. You can imagine every time you have to start a new person, whether it was the new person at JPL or the new management office at [NASA's] Marshall [Space Flight Center, Huntsville, Alabama]—it went from a JPL management office, eventually it ended up in Marshall. I think there were a couple iterations in between.

JOHNSON: Headquarters or wherever. Kept moving around.

SUNSHINE: That's a lot of people to keep. A lot of time and effort. Also managing the distractions to make sure everything was kept going.

JOHNSON: Within the team, you mentioned how they redesigned that impactor so quickly. With Discovery and New Frontiers, a lot of things that NASA does, they put engineers and scientists in the room together and say, "Work it out; y'all communicate." I always like to ask people about the communication. The communication with people in different fields and how that works and your impressions of it and how well it worked maybe on this project and any of the other missions.

SUNSHINE: It can be problematic. I think different teams handle it different ways and different successes. For me honestly that's my favorite part of the job. It just happened last week. I was doing some instrumentation for the PRISM [Portable Remote Imaging Spectrometer] program. We were doing calibrations. There's always this part. It happens all the time, where the scientists and the engineers, they don't speak the same language. We say things that we think are clear to

one another and they aren't. It just takes face time to work it out. But the reason it's my favorite part is that when it happens that you click, and you get to that point where I as a scientist have finally been able to articulate why something is so important and the engineer gets it and goes, "Oh. Then we can do this." It happens all the time in this business, in the mission business. But you need to have the people I think in the room and they have to develop a relationship with each other.

When I was doing CHopper, Comet Hopper, Phase A, which was with Goddard, so we were all local, when we got to the Phase A study, basically we had a whole new team from Goddard on the engineering side. It was a completely different level of proposal. They came over to talk us. Mike and I realized that okay, the first thing we have to do is teach them cometary science. We spent an entire day teaching them about comets because they had no basis. They just didn't know what we were doing.

It didn't take very long for them to basically come to the conclusion that we didn't know anything about comets. Of course our response was, "Yes, that's why we're doing this. We can't make up requirements for you to measure X because we want to measure X but our uncertainties are huge." It made the entire team; it changed everything. Because we'd spent that initial investment, and a few times other like that where we worked, not in some weird team building thing, but in an effort to try to understand what was important and why. These are smart people. Especially that particular vehicle came from a telescope community where they have engineering requirements that are many decimals long. We were like, "I don't know, factor of five would be good." There was a real gulf that we had to intellectually get across. But once we did then things started happening.

Personally when I've given engineers the room to innovate, it's amazing what gets done. But that requires space, time, and face time to do that. It is a real issue. I've seen other teams where maybe it wasn't so easy to have those conversations and people didn't work exactly the same way because everybody was down in the requirements. Not really up being creative.

JOHNSON: With engineers, like you said, they're very meticulous as far as following what they're supposed to build is always laid out for them, and I imagine it is a little harder for them too.

SUNSHINE: It's just what they think they're supposed to do. I had another example and I talked about it at the symposium. It was when we decided we were going to take the Deep Impact spacecraft to Hartley 2 [comet]. The original trajectory was such that we would not have been able to use the spectrometer because it would have been in sunlight. It was really depressing for me. I know it was depressing for Mike.

But we were talking about it. A young mission person out at JPL was listening and I can't remember how long later she came back. She's like, "Well, if we do three more gravity assists, we can come in basically perfect." Which is what we ended up doing. It was a beautiful dataset. Absolutely perfect. But it was again understanding somebody who could solve the problem needed to understand the problem and be motivated to try to think about it.

JOHNSON: Also to be able to speak up about it.

SUNSHINE: Yes. Right. Have the freedom for all of that to happen. Those are the best parts when things like that happen. You're just like, "Well, that was a good day."

JOHNSON: That's interesting. You've talked a little bit about the extended mission. The two that were proposed. One was DIXI and the other one EPOCh [Extrasolar Planet Observation and Characterization]. Then it got melded into one.

SUNSHINE: Yes. Not quite right but there was DIXI which basically we had to do a full proposal to take the spacecraft to Hartley 2. But other people could have proposed other things to do with that spacecraft. Someone did. We knew about it, we were part of it. But Drake Deming who eventually actually came to Maryland the same time I did, but he was at Goddard, proposed EPOCh, which is an exoplanet characterization mission, basically the Deep Impact large telescope was out of focus, which had its issues. But it has an advantage if you're looking at bright exoplanets because it spreads the light out. He came up with this idea and he proposed it separately.

We were DIXI and EPOCh. Then NASA said we had to be one thing. Another proposal. It's not just put it together. We had to write another proposal that was EPOXI [Extrasolar Planet Observation and Deep Impact Extended Investigation]. Mike and Drake loved EPOXI because it was like glue that stuck us together. That's how it became EPOXI.

JOHNSON: Were these proposals—did you have to go through all the same process all over again that you had done for the original?

SUNSHINE: Yes. We did. At that time that's how they did extended missions. When they put us together, we had to do another formal proposal. It wasn't reviewed the same way. Then we did it

again because we ultimately changed targets. Our original target for DIXI was the comet Boethin, which turned out had probably broken up. We had to change targets to Hartley 2. That was another proposal. Then we did one more.

JOHNSON: Another full proposal.

SUNSHINE: Yes. Right. We wrote a lot of proposals.

JOHNSON: Was Hartley 2 already a possibility?

SUNSHINE: Yes. It was on our original list. But there were two reasons we didn't propose to go to it, although it was always scientifically more interesting. The first was that it was another three years. We didn't think NASA was going to want to pay for three years. As I said, we were concerned about the geometry. It wasn't as ideal a target as we would have liked. But by the time we got there it turned out it was an incredible encounter. Really incredible. It was worth the effort.

JOHNSON: Go ahead and talk about it and what the encounter was like.

SUNSHINE: Hartley 2 we knew was something we call a hyperactive comet, which means it produces more water vapor than the surface area can account for. Something we don't understand. It's a first principles puzzle. There are some theories. But it was also much closer to the Sun, so we had much better data, because it was brighter. The signal to noise was great. We knew how to use the instruments. We knew how to use the spacecraft so we had really optimized every

minute of that flyby. We did, it was amazing. We had observations every 15 minutes as the comet rotated. We really had an incredible dataset. There will probably never will be a dataset like that.

We solved the problem. Not only that, but we understand it so much better than that. But what happens on Hartley 2 is that there's carbon dioxide coming out of mostly only one end. More on one end than the other. That carbon dioxide comes from depth we think, and it's dragging water ice grains. You end up having water ice in the coma, which itself then can sublimate. So instead of just having ice water coming from the surface, you have all these tiny grains that have lots of surface area. In fact we just submitted a paper a couple days ago. It's something like 40 percent of the water production is coming from these grains, not from the surface.

Hartley 2 also turned out to be very interesting geologically. One of the things that we could show for the first time is that some of those grains, dust and water, actually redeposit on the surface. It's a sedimentary process happening on a comet. Material is moving from the inside out one end and coming back in the middle. That had never been seen before. That was pretty exciting. There's lots of interesting stuff. Hartley turned out to be a really great target. It was very much a full second comet mission.

JOHNSON: You talk about things being an experiment. You find out things you didn't expect to find out.

SUNSHINE: Always. Always.

JOHNSON: As far as those flybys for getting the gravity assist around the Earth. Is that the one where you found water on the Moon?

SUNSHINE: Yes.

JOHNSON: Talk about that. The water on the Moon.

SUNSHINE: Okay. I was wearing a couple hats. Deputy PI on EPOXI. M3 Moon Mineralogy Mapper is an instrument on the Indian mission Chandrayaan 1 to the Moon. Carlé and I had worked on the original proposal for that which was to go on Lunar Reconnaissance Orbiter. M3 ended up being a Discovery Mission of Opportunity at the same time I was writing all those other things.

In April the M3 team got together and there was some significant evidence that we thought we saw water in the sunlit portion of the Moon. Water or OH [Hydroxide], which we now just call hydration to make it short. But it's at the edge of that spectrometer, it happens at 3 microns, and 3 microns is where the instrument cuts off. You can only see the beginning of the band. It's also where—because the Moon is hot in sunlight, you start getting thermal emission. Half the light at 3 microns is from thermal. You have no way of correcting for it because you don't have any long wavelength data.

When we figured this out, I said to Carlé, "Well, we're actually doing these scans of the Moon. We're making a flatfield, what we call, using the Moon we're going to scan over the Moon with DI [Deep Impact]. That instrument we know it can see water. We know it can see OH. It goes to 5 microns. It'll saturate at the Moon but we'll get probably 4 microns.

Carlé and I went to talk to Jim [James L.] Green, who was the AA [associate administrator] at the time, and he said, "Well, let's see, let's not do anything about M3 until we get the data from

Deep Impact." Then I went back and explained to Mike what we wanted to do, sort of read him in, because this was all hush-hush. We didn't collect any different data than we would have except two things. We doubled the data. We ended up taking the calibration data basically twice. We filled up Deep Impact's memory. Everybody was like, "Why?" Mike knew why we were doing this but we didn't really tell anybody else. One of the JPL guys said, "What, you think you found water on the Moon or something?" I was like, "No, just wanted some more data."

We had a postdoc who was at Maryland who had gone back to France. Olivier Groussin. He got to the data because of the time zones before we did. By the time we got in the office—and again we were still on the phone back then, sending emails and talking over the phone. [He first said he was sorry that there was no obvious water ice in the raw lunar data. I reminded him that we were not looking for ice but absorbed water (thin layer of molecules). I emailed him a figure of a lab spectrum with adsorbed water. Then he said, "of that we have!!!"] We then set out. There was a gun to my head to get the science paper written for Deep Impact so they could be written together. It's an M3 discovery, no question about it, but we were able to confirm it.

Really importantly we were able to show the extra data that we took, which was one week after the first set. The Moon had rotated. That means everything's temperature had changed. We could show that the water was changed as a function of time of day. It wasn't just that it was there but it was temporal. Varied with time of day. You have less water the hotter it gets, or less hydration the hotter it gets. Towards when it starts cooling down you get more of it. We could show this diurnal cycle in the Deep Impact data because we'd shoved this extra data in. That was DI going across the north pole. Then a year later we came around again and we took southern pole data. Now everybody knew why we were doing it. We took that as well. Very different scale

obviously. We were very far away. But in some ways that was an advantage because I was doing two things at once. It was crazy.

JOHNSON: I was going to ask you about that. What it made me think is that the M3 data, if it wasn't for the Deep Impact, the extended mission, how long would it have been before that was actually—would you have written about it anyway?

SUNSHINE: I think people would have. It would have been published. But we would still be arguing about it.

JOHNSON: That's what I was wondering, how long.

SUNSHINE: Ironically as we speak tomorrow hopefully Lunar Trailblazer is going to launch. It has an M3 derivative that goes to longer wavelengths. Kind of be the best of both worlds. It will have the ability to look at this over longer wavelengths. It has a thermal instrument itself but at high resolution that Deep Impact couldn't do.

JOHNSON: That's interesting.

SUNSHINE: Yes, it's coming full circle. Yes, I think it just would have been—people would be arguing about it. To be honest, Carlé populated—the M3 team was a lot of Carlé's students and people like-minded and we were skeptical. Internally we were really skeptical that this wasn't

some calibration problem or something we had done. Having DIXI come along a couple months later and just confirm there was no question of what we're talking about.

Then actually people I think found it in Galileo—on Cassini data. Once you know what to look for. Yes.

JOHNSON: Serendipitous anyway having those teams.

SUNSHINE: It certainly was. It was kind of like well, I have this spacecraft that's coming along. Again that would not have happened had that engineer not thought out of the box and redesigned the Hartley trajectory.

JOHNSON: Exactly. As you mentioned you were wearing multiple hats. Lots of stuff going on all at one time. Maybe just talk about that for a minute as far as in this field and working on these different missions and everything. You can't work on the same thing. A lot of these missions they launch and then it's six years later something happens. There's a lot of time like that that you have the opportunity to work on different things. But just talk about having especially one like that where a lot is going on at the same time.

SUNSHINE: I think some people choose to focus. If you're working on the Mars lander you got plenty of time, or your organization wants you to focus and pays you to do that. But particularly those of us who are in academia where we're teaching and we have students and we have projects and multiple projects, yes, I've never been in a situation where I wasn't multitasking in terms of projects. Right now I'm on two. One flight mission, Lucy, Discovery mission, and we're doing a

flyby in April of an asteroid [Donaldjohanson], it's going to be very exciting. I'm working on the Lunar-VISE [Vulkan Imaging and Spectroscopy Explorer] mission which is a set of instruments that we hope we're going to go on the Moon on a rover. Those two keep me plenty busy. But I still put a proposal in with someone this morning.

JOHNSON: Giving you a reason to get up in the morning, right?

SUNSHINE: I would say certainly the time I focused on anything the most it was not surprising it was the CHopper Phase A. Those proposals. The one I did after that which was called Chagall [Comet Hartley Analyses to Gather Ancient Links to Life]. Those take incredible concentration. Just incredible. I joke with people. The Deep Impact Phase A study is smaller by almost a factor of two than the Step 1s today.

JOHNSON: Talk about the evolution of making those proposals. It's changed. It has definitely changed. I've talked to some people in the program office that talked about those changes and how they even realize it's a lot you're asking people to do. Maybe just talk about that whole process.

SUNSHINE: I've talked to people about it as well in the program office over time. I think particularly the Step 1 proposals they used to basically be here's a really great idea and here's our demonstration of the feasibility. Which is different from saying, "I know exactly what I'm going to do and every detail of everything I'm going to do." Every year it just gets more and more requirements. I think there are 180 requirements in a proposal. I maintain that certainly many of

those, okay, you want me to say it, so I have to tell you what it is. But that doesn't mean I actually know the answer. I can't have possibly done the due diligence. Therefore we have this false sense of knowledge. When in reality we probably don't know really that much more than when Mike decided to rewrite the Step 1 proposal in a week. It's the idea and not only is it I think unfortunate, has unfortunate consequences. As I said I think we get in trouble because we think since somebody detailed something that it's actually known or well studied or verified. It's just there.

Then I think the other part that maybe doesn't get as much notice is that it's a huge barrier for people to enter the field. Mike Belton would not be happy about this because the cost, literal cost, of writing a Step 1 proposal is prohibitive for many many people. You're no longer getting that community set of great ideas. You're getting the great ideas from the people who can work with, already have contacts with the centers that are proposing. I think it's a shame. I don't know how to fix it, but I think it's unfortunate. The big thing we're supposed to be preventing is choosing missions that are poorly costed and have technical issues. We still do that a lot.

JOHNSON: Part of Discovery early on, part of the reason they came up with Discovery, maybe not reason, but one of the things was it was risk-tolerant.

SUNSHINE: Correct. It is not.

JOHNSON: Yes, I was going to say over time.

SUNSHINE: I think if you try to close your eyes and say we'd never done an impact experiment, I don't think there's any way a Deep Impact would be selected. Because we haven't done it yet. So

how do we know what's going to happen? All the unknowns. The inherent risks that we had I think are just not tolerated right now. I tried to talk about this as well in the symposium. We need to leave people the opportunity to not know the right answer but at the same time what we've not done is make sure in a lot of cases that the mission as designed actually is going to let us know if we were wrong. Have we really figured out all the boundaries? Sometimes the answer would be that we wouldn't be able to see this but we would still know that that was the outcome.

We should be going after the unknowns. Why are we pretending to write a proposal where everything is known? It's very frustrating.

JOHNSON: It doesn't really make sense. If you know it, then why do it?

SUNSHINE: To ask us to demonstrate that we know it just means that we're going to say things that—it's not that anybody's intentionally lying. I'm not trying to say that at all. But people are going to do their best to make it sound like they've understood a problem that they can't. I think that does extend throughout the program, and I think there's a big difference with the kinds of things people were able to propose in the beginning versus what we're doing now.

JOHNSON: You did discuss this at the symposium?

SUNSHINE: Yes, I did.

JOHNSON: Was there any agreement?

SUNSHINE: I think most people, certainly anybody who's written their proposal would agree with me. But I don't know how we change, how we walk some of this back. They did a study sometime, I don't know, maybe it was 10, 15 years ago, about simplifying the AO [announcement of opportunity] process. It ended up being more complicated, substantially.

JOHNSON: Which like you said will keep people that are newer to this maybe from proposing.

SUNSHINE: Yes.

JOHNSON: They're not going to have time to put it together.

SUNSHINE: People who don't have the resources. Many definitions of that. But somebody who has a great idea. It's really hard.

JOHNSON: It kind of goes against the whole purpose of the program to begin with.

SUNSHINE: Yes.

JOHNSON: That's a shame.

SUNSHINE: Like I said it's not clear that the outcome is any better.

JOHNSON: Just makes it harder.

SUNSHINE: Yes.

JOHNSON: These extended missions, they're cheaper because they're already there, they're already going. But you have to propose what you want to do with it. But are those extended missions ever part of that original proposal, for Deep Impact or Stardust or any of these?

SUNSHINE: Sometimes it was, I think in the beginning. But it no longer is. I think that's also a problem. I think NASA should recognize that if they're going to send a spacecraft to X, unless they use up all that fuel, somebody's going to have a decent idea of what to do with that thing. That should be part of the budget process. There should be a relatively simple way to gain entry to that for both the team and people who aren't on the team.

I think Deep Impact probably went through—in the extended mission—every version. First we had to write a proposal before the main mission, and we only had a six-month launch, so that was kind of fun, but it was a short proposal. But then eventually we had to do so many different versions. Became a mission of opportunity formally under the Discovery Program. Then it became an executive review. So much time in process. I think that's something that could be streamlined. At some point you have to turn it off. You have to say, "We can only do stuff that's not going to cost more than X." But it seems like there should be at least some sense that if the spacecraft is going to go beyond its original mission, we know they're going to ask for it.

JOHNSON: Of course Deep Impact ended in 2013?

SUNSHINE: Yes.

JOHNSON: Because of a Y2K problem. I read that and I thought Y2K, but it's 2013. I wasn't sure how a Y2K problem ended it.

SUNSHINE: Every time you store something in a computer you have to give it a certain number of bits. It turned out because we were a six months' mission—in reality and originally, we were supposed to be a year and a half. Nobody worried. The person who did the programming, makes sense, right? The date in seconds. When we hit eight years if that's what it was, can't remember the right number, we ran out of bits. The spacecraft didn't know the date anymore. The computer would not log on. Went to the backup computer. Same thing happened. It pinged back and forth. Meanwhile it drifted off the Sun. That was it. It really was a Year 2K-like problem. Not exactly that. We figured out on the ground what the problem was, but we just couldn't do anything about it. It's a heck of a way to lose a spacecraft. It's done us a lot of good, so it's okay.

JOHNSON: You can't gripe. You got a lot out of it. That's for sure. Some of the other ones that you worked on. I'll start at Stardust NExT [New Exploration of Tempel]. You were a coinvestigator on that, right?

SUNSHINE: Yes.

JOHNSON: Talk about that for a minute.

SUNSHINE: Stardust of course was a sample return mission and they had a camera. It was kind of a last-minute thing. They had the sample and the original team was not particularly interested in doing anything with the extended mission. They were happy to let us work on it. Don [Donald E.] Brownlee was part of our proposal. But essentially the comet image people stole the spacecraft and proposed this idea that we could take it back to Tempel 1 (Joseph Veverka was PI) which is what Deep Impact had hit and try to see what was different. Along the way we had to do a lot of calibration work because the particular instrument camera wasn't really very well calibrated and things like that.

We went back. It was, if I remember correctly, 52 images that we had of the entire Stardust NExT encounter. We hoped that we would come over the place where we would see the impact crater. We never saw it with Deep Impact because there was too much material coming out. It was too opaque. Those fine-grain ices became impossible to see through. We did actually go over the crater, but it was really below the resolution. There are definitely some indications of where we think the crater was. But it was not—we knew it wasn't going to be great.

But what we did see, which was really important at the time for sure, was that big flow I was talking about. We flew over again and we could actually see that it had retreated in the four and a half years since we had been there. We can actually therefore talk about how much erosion was happening on this comet. It was the first time stamp we had of cometary erosion. It turned out to be actually pretty important for that.

JOHNSON: That's interesting.

SUNSHINE: Yes. We also saw another side of Tempel that we hadn't seen. Which looks completely different. Still a bit of a head-scratcher how the two different—we call it the Stardust NExT and the Deep Impact sides. Obviously, the comet doesn't know that. But they look really different. I don't think we have a good answer for that yet. We don't.

JOHNSON: You mentioned the images on that. The images with Deep Impact. Maybe just talk about that. When you see those things for the first time, talk about those moments.

SUNSHINE: Oh. I counted this the other day. DJ, Donaldjohanson, will be my ninth flyby of a planetary body. Two of those were the Moon. In one case it was the far side of the Moon that we had never seen in color. It's exhilarating, particularly on the small bodies. I sit in an astronomy department and I'm a geologist, so this is the world that converges for me. These targets are a dot. They're a dot. They've been a dot forever. In the last, depending on how fast you're going, few minutes they resolve before your eyes into this geologic world. Every one is different. Every one is surprising.

We did the Lucy flyby of Dinkinesh and we had a satellite that turned out to have another satellite. It was a contact binary with a satellite. We got the first data down. We didn't even realize at first that it had a satellite but then it wasn't until the next day that we really figured out that the satellite was itself a contact binary. It was just crazy.

I'll never forget Deep Impact because the geologic complexity of Tempel was absolutely no way that anybody had any clue that was going to happen. It was jaw-dropping. Then we hit the damn thing. Even better as the resolution increases. Especially in the case of Deep Impact we had the flyby. We were watching the impactor. It's just getting closer and closer and closer and

then it's not. The same thing happened on DART with the impact into Didymos. Infinite resolution. Just keeps getting—it's amazing. That's really impressive. Certainly the highlights of my career would definitely be those transitions when you see an entirely new world.

Mike Belton used to say that if you got twice the resolution on a planet it was a different planet. It all happens all at once. It's pretty impressive.

JOHNSON: I can imagine those moments are memorable.

SUNSHINE: Yes, absolutely.

JOHNSON: All these years of trying to get somewhere and then actually seeing it. Having it be completely different.

SUNSHINE: Nothing's boring yet. I think Donaldjohanson is going to be pretty exciting too.

JOHNSON: Let's talk about Lucy. That one has a lot of things planned. Like you said Dinkinesh was the first look.

SUNSHINE: That was unplanned by the way. We did not plan to do Dinkinesh.

JOHNSON: Right. Talk about that mission and what your involvement was and some of those highlights for Lucy.

SUNSHINE: Like many missions, it wasn't the first proposal. There was a previous proposal with Cathy [Catherine B. Olkin]. This one, my role is pretty similar. I'm the spectroscopist. I do composition and to some extent the geologic interpretation.

This one was exciting because these are bodies that have been sitting in a unique environment. They've been co-orbiting with Jupiter for we think the age of the solar system. The hope is that their diversity represents the diversity of the Kuiper Belt objects because they've been trapped by planetary migration in the same way that the Kuiper Belt objects moved out. Again I don't think we know too much about what to expect. They're an interesting population how they compare to comets. Are they bigger versions of the same thing? Are they very different? Can we tell? How they compare to other outer belt asteroids as well.

I think the cool thing about Lucy has always been that we're doing enough of them that we're going to get statistical information or at least some sense of diversity. It's not just oh, well, we got one, what does that mean?

JOHNSON: Dinkinesh, like you were talking about the fact that it was an opportunity, it wasn't something that was supposed to happen. Then what you saw from it.

SUNSHINE: Yes. It's trying to understand how binaries form. The fact that we see this moon that's itself a binary. How did that material spin off? What is it telling us about what's going on? The primary itself actually has a lot of slumping on it which is consistent with material coming off of it.

But it's also the first. We see a lot of those size bodies in the near-Earth population. It's the first time we've seen one in the main belt. They tend to have binaries but we now have—for

example it's pretty similar in size and composition to the binary that the DART mission went to. But they're very different. Very different. It's really exciting actually. That was just one of many.

JOHNSON: I've talked to different people on Lucy. They all talked about how that team worked together. It's their favorite team. Of all the ones they've worked with that might be their favorite team. I think a lot of that has to do with the people that were running it of course. Hal [Harold F.] Levison and people like that that were working with it. But did you get that feeling also working with them?

SUNSHINE: There's a lot of SwRI [Southwest Research Institute] people on Lucy. I think those folks that are at SwRI have a certain cohesion that's a little harder for those of us—it's always a problem for people who are slightly different. My role is much smaller on Lucy than it's been on other things. I don't mean to slight anyone by saying Deep Impact will always be my favorite team.

JOHNSON: That's okay. You can pick your favorite. Who's going to tell you you're wrong?

SUNSHINE: Who you work with. That's good. Yes. I think the engineering side of Lucy I didn't have anything to do with. But getting that thing out the door during COVID [Coronavirus disease, 2019].

JOHNSON: I was going to ask you about that. Because that's one. Psyche was affected by COVID. Lucy. Just talk about that for a minute and anything else.

SUNSHINE: I really can't imagine. Again I'm not on Psyche. I'm not on the front lines of Lucy or on the engineering side, but I really can't imagine that. It's one thing communicating. We have much better tools now and we can do all of that. But not being able to be in your lab; having one person doing something where you would have had six people or whatever. The physical side. You literally have to build these things. You can't do that remotely. It's a testament to the people who were really on top of it. Lucy has certainly benefited from the fact that they had constant management, unlike what we had. But I honestly don't know how. People were incredibly dedicated to make that happen.

JOHNSON: It's an interesting—there was a lot of public outreach for Lucy. Some missions seem to have more than others. They have all the cartoons that they were putting out for schools and different things. You were mentioning before having students on M3. What do you think about the importance of public outreach, especially for NASA and to get people to come into the field? To reach out to the public and to schools and to get kids interested in pursuing some of these fields. How important do you think that is?

SUNSHINE: I think it's extremely important on the grad school level. In missions that I'm allowed the funding, I think it's really important to make sure that grad students—I have the opportunity to bring grad students and have them meaningfully participate. I certainly would not be here if I hadn't had that opportunity in grad school. That's not only for me, which I assume, I think it still stands today. Not only did it provide me the opportunity to meet all these people and work with this stuff and figure out what missions were like. But I literally learned how to do some of the

basic data analysis at the knees of people who knew what they were doing. I don't think there's a substitute for that. That's the best way to get people involved.

In terms of the general public, my gosh, our missions sell themselves. Everybody loves what we do, rightfully. It's great stuff. It's inspiring and we should continue to use it as inspiration. I always say that astronomy is a gateway drug to STEM [Science, Technology, Engineering, and Math].

JOHNSON: That's a good way of putting it.

SUNSHINE: People get excited about—it's not that we want every person or every child that we encounter to become an astronomer or planetary scientist. But it gets them going. Appreciating STEM in general.

JOHNSON: Right. Right. Kids either like stars or dinosaurs. Those are the things they like.

SUNSHINE: Yes. Right. Meet them where they want to be.

JOHNSON: Get them excited. That's the most important thing. We've got about 30 minutes in our time left. I was going to talk to you about the Comet Hopper or CHopper. Since you were the principal investigator for that proposal. Let's talk about that for a little bit and how that proposal went. Was that the one that was actually selected as a concept study? Talk about that and getting it to that point. Then having it selected, that downselect. Then getting the funds to continue developing at that point what you wanted to do, that whole process.

SUNSHINE: CHopper was a very intense couple of years. It took tremendous focus to work that program. We started DSMCE [Discovery and Scout Mission Capabilities Expansion]. A program where Headquarters asked us—people had ideas for how you could use a nuclear-powered mission in Discovery, basically giving us some seed money to help write a Step 1 proposal because it was a new idea. It was really exciting because actually Lockheed Martin came to us at Maryland, to Mike and I, and said, "Do you have any ideas?" It didn't take long for us to come up with some incredible ideas because not having solar panels opened up a whole bunch of stuff that we could do on a comet. In particular landing, repeatedly landing, which is why it was called Comet Hopper. Hop around a comet. Eventually we evolved into hopping as a function of heliocentric distance so as the comet was heating up and starting to sublimate, we would be there. First when it was basically inactive and as it became more and more active. It ended up with a total of 12 landings, which was pretty insane at the time.

JOHNSON: Yes. I think so.

SUNSHINE: The science case we worked hard on and we had a good team. We had a small team. In the beginning the science team was only about six people including Mike and I, and a few engineers. It worked really well. When we got to the Step 1, we expanded it to certain people and that worked as well.

One of the best things about a PI-led mission is you don't get a team assigned to you. Certainly on the science side you basically choose the people because you want to work with them and because you need them. In some cases there are people I brought in that I didn't know, but I

knew I needed their help. That was great. We had a great team. We worked really well together. The engineering side worked well. We were out of the box thinking. Whole thing. Nobody'd ever done this. What are we going to do? How are we going to do it? We had some really innovative engineering that happened. It worked well.

We made it to Step 2. The Phase A concept study we worked very very hard, like everyone. The other nuclear-powered mission was something called TiME [Titan Mare Explorer], which was a boat, a floater, to Titan. I thought well, that's the race. It happened to be a friend of mine from grad school who was the PI. She and I are like, "This is great. We're in a race."

The whole point was we were supposed to use this new Advanced Stirling Radioisotope Generator. Towards the end of the Phase A and before they announced selections it became clear that that particular technology was not ready. We both did a lot of work and our teams did a lot of work and there was no way that we could have been selected. That was hard. It still is hard to swallow. We went all the way through a difficult, as they are, orals, site visit, the whole thing. That's a lot of effort.

JOHNSON: Disappointing.

SUNSHINE: Yes. It's hard enough to lose. In my mind, to lose to Ellen [R.] Stofan, there are worse things in the world. But to lose because you didn't have a chance is difficult.

JOHNSON: Was there talk of redoing it again?

SUNSHINE: Not with nuclear power. We were never given that opportunity. There was small talk afterwards that they could have kept us going and let us try to use regular nuclear power. But it never happened. They selected a solar-powered mission, InSight [Interior Exploration using Seismic Investigations, Geodesy and Heat Transport]. The next round of Discovery, the team convinced me—I really was done—that we could actually do a lot of the science we wanted to do without landing. Different. We had a different approach. We had some other risks that were necessary.

We did that proposal and we called it CHagall for a number of reasons, but it never made it past Step 1. I think sometimes it's human nature. When you have a great idea and it's out of the box and it's totally new, people get really excited. But they don't get excited the same way the second time. There was nothing wrong with the proposal. It just was not selected. Whatever. That's fine. Yes. That was the last time I will PI.

JOHNSON: You were deputy PI for New Frontiers for CORSAIR [Comet Rendezvous, Sample Acquisition, Investigation, and Return].

SUNSHINE: Yes. That's a proposal that hasn't gone in yet because—

JOHNSON: Oh, it hasn't. Okay.

SUNSHINE: One round of CORSAIR went in. I was not the deputy PI at the time. We've reconfigured things.

JOHNSON: This is the second time?

SUNSHINE: Yes. The New Frontiers AO. We don't know when it's going to be announced. At some point you age out. Comet sample return missions are long. You're talking about 12-year missions.

JOHNSON: Sample returns are a little different. Yes.

SUNSHINE: We'll see what happens. But yes. That was the last time I signed up if you will.

JOHNSON: They convinced you to do it again.

SUNSHINE: I think if you talk to anybody who did a Phase A study and wasn't selected you will find that it was a personal loss is almost equal to someone dying. It's a huge investment of time and energy. One cannot write a Phase A study unless you believe you're going to win. Matter of fact, unless you think you've already won. You have to have that intensity of conviction that this is already happening. Not might happen. I think anybody who's been in that position, it's devastating. I don't think that's even strong enough a word.

JOHNSON: I can understand that because you would put everything into it. It's like it's your kid and somebody's told you your kid doesn't have any talent.

SUNSHINE: Yes. You put everything into it to the exclusion of everything else. You asked me before about multitasking. That's the one time that you can't do anything else. It's all-consuming. I've spoken to people from the last round of Discovery who lost. I get it.

JOHNSON: No kidding. You mentioned SwRI. But also APL [Johns Hopkins Applied Physics Laboratory], NASA, universities. When you build these teams, you do have to work with a lot of different people from different agencies. How well do you think that works when you're talking about different companies? Because you work with a company that builds something and then different institutes and different universities may be working along with NASA. That whole concept of everybody working together.

SUNSHINE: I think it's the pluses and minuses. You need people from different places and different organizations. I think when you're talking about the big three teams, whoever the PI institution is, the NASA institution, and your industrial partner, those cultures can be very different. Certainly plenty of missions, Deep Impact included, had problems with cultural differences and the way material is communicated, the way people deal with things up and down the chain when problems happen. You just have to work through them. But it would be worse if we didn't do that. In some ways having all those people from different institutions over time, and not all the time, but over the course of the mission I think you make it better. They're coming from different experiences and different perspectives. The cultures might be different; that doesn't mean they're wrong. They're just different. There's a well-known issue on Deep Impact where Ball and JPL had very different sense of fault tolerance and fault control. Almost one went from the top to the bottom.

The other went from the bottom to the top. It wasn't caught until pretty late. It's almost communication in a different way. It is cultural.

JOHNSON: Communication. I do ask a lot of questions about communication usually in these interviews. Because when you have so many different entities involved and like you said different cultures and also sometimes international, plus you have personalities and you have all these different things. It seems like a lot of it, especially if you're like you were talking about the PI institute and NASA and the different parts of each one of these missions. That communication is either made or broken by those main institutions that are running it. Seems like that would be the first hurdle to get through.

SUNSHINE: Yes. Where it usually breaks down is the first time you have a problem. But I think those are things you work through. You can't avoid them.

JOHNSON: Looking at Discovery and New Frontiers both as programs that NASA has, and we've touched on it some and talked about it, but what do you think is the importance of continuing these kinds of programs as far as the science community leading it, the PI leading the different missions? I know we've talked about how it's changed over the years. But going forward, how important do you think it is?

SUNSHINE: I think it's absolutely critical. It doesn't need to be the same words and the same mechanisms maybe. But the choice. If you look back just for a second. What was before this? It was massive missions to one place a decade. Maybe two. We've replaced it with this. I've had

this incredible career of multi destinations. Asteroids, comets, the Moon, Trojans. That's just me. The diversity of problems that we've been able to attack scientifically wouldn't happen without this creative energy that the program allows and the fact that we can have lots of opportunities. I think my concern for the program right now, biggest, is that we're not doing lots of opportunities anymore. If we can't afford New Frontiers, okay, then let's at least have Discovery every other year, so that there's a diversity of targets and science across the solar system is benefited from that.

In terms of the overall PI structure I think it would be a disaster if we went back to directed missions, to JPL. Not that there's bad people there or anything, but the organizations have their own reason to exist. When you put the PI in charge, you're by definition empowering the person who's the guardian of the scientific results. That focus is what has made Discovery and New Frontiers so important and so successful. I think those two things plus going back to Mike Belton the competitive nature. It may not be fair all the time and there's issues, but at least there's choice and people have an opportunity to go up and really pitch their idea. In some way or another those are judged against other people doing the same. As opposed to some process that is dictated not from the scientist but some other method. It's just not going to get the same results.

It certainly would be a shame if we didn't continue. But also if we didn't think about how to trim it in a way to really focus it to make it go back to what are the core principles and say, "Are we really doing that? Is this really the best way to do the core principles?" Not just continuing what we're doing because that's what we're doing without thinking about whether we can afford it or just continuing the process.

JOHNSON: If you had to think of the lessons learned, would that be one of the lessons learned? Things have changed to the point where you may have to reset?

SUNSHINE: Yes. I think it may need to be reset and streamlined with a focus on the guiding principles. Not where we are today. Which is hard. I don't know that that will happen, but I think we're at a point where maybe it should happen.

JOHNSON: If you had to choose something looking back over the missions and your roles in the different missions and everything that you've been involved in up to this point, what would you choose as something you're most proud of?

SUNSHINE: That's an interesting question. There's a lot. I'm still proud of CHopper even though it wasn't selected. I really am very proud of that experience and everything that went into it. From personally what I did even though nothing came out of it. There's very little about Deep Impact that I'm not proud of. From beginning to end it was everything that I was able to help Mike with and Mike helped me with. To be perfectly honest, the thing I'm most proud of is my relationship with Mike A'Hearn that we built from nothing into something very unique and special. Deep Impact was along the way for sure. Certainly part of it. But yes.

JOHNSON: There are people like him, unfortunately, several that have passed away that we couldn't talk to for this specific project. But it's always good to hear your impressions of him and Mike Belton, just because they're not here to talk about their missions. Other people have to talk for them basically and talk about them.

SUNSHINE: Yes. I loved being his number two. Let's put it that way.

JOHNSON: That's good to hear. Yes. Is there anything we haven't covered or that you thought you would like to cover today that you wanted to include? Or anything that you wanted to expand on? Or any anecdotes or stories? We like the stories. That's always fun getting those stories. The human side.

SUNSHINE: I'll tell you the story behind me doing Deep-Rosetta. We were at a Deep Impact meeting in Germany. We just figured out that there was ice on the surface of Tempel. Mike A'Hearn, who was a very big guy, came up to me. He said, "At the break Veverka and Belton want to talk to you." I thought uh-oh.

I'm from New Jersey. I just had this feeling of the mob is coming. What did I do? Then Belton came over and he said, "Let's go for a walk." That was when Joe Veverka and Mike Belton told me that I really should write this proposal. I was like, "Okay."

JOHNSON: They were ganging up.

SUNSHINE: Thinking to myself are you going to help, and I thought no, they're not going to help.

JOHNSON: They wanted you to do it. That's funny.

SUNSHINE: I was like, "What did I do wrong?"

JOHNSON: Those are good stories and good memories, I'm sure.

SUNSHINE: Lots of good memories, stories. Yes. M3, we talked about the water. But M3 has got some great stuff in it. Just beautiful data. Unfortunately the spacecraft didn't cooperate so it was cut short, but it's amazing what we've all gotten out of that mission. No. I'm trying to think back. I think you covered most everything.

JOHNSON: I know the one you're working on now that has something do with CLPS [Commercial Lunar Payload Services]. I know that's not part of Discovery and New Frontiers. There's so much talk about Artemis. I was just wondering, how would that affect this other mission?

SUNSHINE: I think it's separate from Artemis. For one thing we're not going to the south pole. Our mission is to go to—it's called the Gruithuisen Domes. These very interesting mountains that we don't really understand how they formed. We would deploy our instruments on a commercial rover for the first time, which is kind of exciting. NAS [National Academy of Sciences] has contracted the spacecraft and the rover. I'm hopeful that it'll continue. I think we're probably—unless they shut everything down—but we are not Artemis-dependent. We're not even helping Artemis in any way. It's also commercial, which I think might appeal to the current people in charge. It's Firefly [Aerospace] and Honeybee [Robotics] but still the concept, this is not NASA putting a mission together. They're basically I like to call it Uber for the Moon.

JOHNSON: The whole CLPS thing is interesting.

SUNSHINE: There's another one this week. We'll see. I certainly would love for it to happen. I've gotten to do many things but roving is not one of them, so that's pretty cool.

JOHNSON: It would be. Yes. I was going to say you've been a lot of places.

SUNSHINE: I've been a lot of places. I've done a few things. But I have not had a chance, and to do a rover would be really exciting.

JOHNSON: Yes. I appreciate you talking to me today about everything.

SUNSHINE: Sure.

[End of interview]