# NASA DISCOVERY 30TH ANNIVERSARY ORAL HISTORY PROJECT
# EDITED ORAL HISTORY TRANSCRIPT

ROGER C. WIENS
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is May 15th, 2023. This interview with Roger Wiens is being conducted for the Discovery Program 30th Anniversary Oral History Project. The interviewer is Sandra Johnson, and Dr. Wiens is joining me again today from Purdue University and we're talking over Microsoft Teams. I want to thank you again for agreeing to talk to me today.

WIENS: Yes. My pleasure.

JOHNSON: Last time when we talked, you had mentioned that when you moved to Los Alamos, Los Alamos [National Laboratory] was in charge of the development of three different instruments, and that you were going to be working with those. I don't think I asked you what those three instruments were specifically. Maybe just a quick overview on what those instruments were and what they were going to be doing for the mission.

WIENS: Yes, Sandra. The three instruments are a solar wind ion monitor and solar wind electron monitor, and a solar wind concentrator. The first two are electrostatic instruments that have flown on other types of missions for space physics experiments, and they provide information about the characteristics of the solar wind, especially the hydrogen and the helium in the solar wind. Those are the main constituents.

We can figure out from that the type of solar wind that is flowing past the spacecraft at any given time. That's important because we wanted to collect different samples of the solar wind, and so Genesis eventually collected a bulk sample, for which it basically had collectors deployed for the entire two years and several months. Then several types of material collectors that were exposed only for different types of solar wind.

The easiest to distinguish were the slow solar wind and the fast solar wind. All we had to do for that was to determine the speed of the ions that were coming in—for the hydrogen and the helium. We could do that with the solar wind ion monitor.

Then we also had this electron monitor, and that was specifically for a third type of solar wind, which were coronal mass ejections. When the coronal mass ejection comes through it also carries with it electric field lines that are looping back towards the Sun. Because of that, electrons will be streaming in two different directions, because both ends of that magnetic loop are connected to the Sun.

By seeing the two different directions of the electrons streaming, we could figure out that we had a coronal mass ejection. The coronal mass ejection is a bit of a garbage collector in that it mixes material from both the corona and the sun itself. We thought we were not as interested in that. But the fast solar wind is the most solar-like, and we were particularly interested in that.

We collected samples of these three different types of solar wind. That was facilitated by when our ion monitors would tell us the speed and other things, and there was an algorithm on board that would make decisions on which collector to expose. This was actually the first time that any robotic spacecraft had made science decisions on board a spacecraft and then changed its configuration as a result, and so this was a bit novel. We made sure it was very fireproof and fail-safe, and then was set to run on the spacecraft so it could deploy those different panels of collectors.

I should describe the third instrument. It was a solar wind concentrator. The oxygen isotopes that we hoped to distinguish from the Sun required a higher fluence of oxygen than just the bulk sample of the solar wind. To collect that it was envisioned all the way back to 1991 or 1992 that we would use an ion telescope to concentrate the ions, especially of oxygen but also of similar mass elements. We also got nitrogen that way and carbon and so on. That was done with a reflecting ion telescope. It was 40 centimeters, so about 26 inches, in diameter in the active area. It had a large parabolic mirror at the bottom. That mirror was connected to a high-voltage source, and that mirror went up to 10,000 volts, 10 kilovolts. But its voltage at any given time would depend on the solar wind speed. Connecting it to the algorithm that gave us the solar wind speed was important, based on the ion monitor. The whole payload was interconnected in terms of how it worked, which was really quite novel. We were asked a lot of questions about what if something didn't work. But it all worked.

This telescope not only had the big parabolic mirror but we had to contain the electric field lines and we had to do a few other things to the ions as they came in. We wanted to reject the hydrogen because it was going to be actually too concentrated on this target at the center. First, we had a small grid that was at ground potential, then we had a positive grid to reject the hydrogen, and then we had an acceleration grid that would give the remaining ions 10 kilovolts, and then another grid that paralleled the surface of the parabolic mirror. All of that was devised in the space of about 26 months. We had some very rough prototypes before the selection.

Those were tested in Bern, Switzerland, in 1995 actually by myself and some colleagues there. I spent the summer in Switzerland. This was a new development because no one had ever done a large-aperture telescope like that out in space for ions. I mentioned these grids—they're like window screens, only much more fragile. We were flying a different version that hadn't been used before, and all of these things had to get tested and tried. We took one of these big sets of grids out to a firing range near Los Alamos Laboratory, and we actually shot bullets through it to simulate micrometeorites to see if the thing would unravel or if we'd just get tiny holes wherever the micrometeorites came through. The latter was the case, and so it was safe to fly this type of grid material.

But yes, these were the three instruments, and they were all developed at Los Alamos.

JOHNSON: You mentioned that it was the first time they flew, that the instrument was telling itself what to do, that the science was being determined by the type of wind or that sort of thing. It's interesting because now it seems like we're very almost used to that on the rovers on Mars and the ones that are going to fly to Mars. We're used to the instruments figuring out what to do for themselves because of what they're doing. It was interesting that that was the first time.

WIENS: Yes. Thinking about this, it was launched in August of 2001. It was actually set to fly six months earlier but there was a launch delay just to make sure that everything was ready to go. This was set to be one of the very first NASA payloads in the 21st century. This is the century of the smart everything, and this was a primitive version of a smart payload.

JOHNSON: Talk about getting ready for that. You mentioned that you had to explain what you were doing, but there were a number of tests or meetings or reviews that you had to go through to get there. The CDR [Critical Design Review], the Launch Readiness Review, those kind of things that NASA does to make sure. Everything has to be shaken and heated up and cooled down, and all the things that they have to go through. Were you out when they were testing, getting ready to fly, those final tests and maybe the launch readiness review? Were you able to be there when those tests were being run?

WIENS: I moved to Los Alamos in March 1997. The mission was selected just several months later into Phase A. Then it was selected in the fall to go for real, and so basically at the beginning of 1998 we were really on a race to get this thing going. We had a Preliminary Design Review and then followed by a Critical Design Review just a few months later, and so there was just a huge amount of work to get this all through there. Then of course the Assembly, Test, and Launch Readiness review, ATLO review.

Then we were facing a separate review because of the Mars '98 mission failures, and so that was a special what was called a red team review. That was a series of reviews actually held in Denver and at JPL [Jet Propulsion Laboratory, Pasadena, California]. I was at the Denver ones. I cannot remember for sure if I was at the JPL ones or not. Payload was reviewed as well, but especially the mission-critical aspects. Then I went to the Launch Readiness Review in Florida as well.

JOHNSON: Talk about that experience. I thought it was interesting because I did see that you wrote in your book about your experience when you were trying to get to that review. If you want to just talk about that here.

WIENS: Yes. Let's see. The red team review, I'm not sure if I described it in my book. But part of the red team review was in Denver, and there were a number of subsystems that were reviewed there. Since I was fairly close to the PI [principal investigator] of the mission I wanted to be in on it. I was still relatively new to spaceflight myself but I was soaking it all in.

The part of the red team review at Lockheed Martin that I remember was actually the EDL review, the Entry, Descent, and Landing review. This was a critical one because it would be the first entry, descent, and landing of a NASA spacecraft from deep space onto the planet Earth in many years since Apollo. This was a big deal. The review chairman was a navigation specialist, and that was important because from deep space the capsule has to enter at the proper angle in order to actually succeed in the entry. If the angle is too shallow the spacecraft will actually come back out of the atmosphere and not successfully get to the solid ground. If it enters too steeply then it has too high of a peak heating on it. The capsule can be destroyed by that. The right entry angle, something called the keyhole, was really the focus of this review, but it actually also entailed all of the hardware involved. The parachute and the helicopters and everything else.

Unfortunately I remember that this review focused very much on that keyhole and the idea that they would hit the keyhole, and the hardware aspect was minimized in that review. Much to the detriment of the whole mission as a whole, because the review did not apparently take into account whether anybody reviewed whether the accelerometers were installed the correct way and all of that. If that had been reviewed carefully, this mission would have landed much differently than it did.

JOHNSON: You did mention the launch readiness review in your book where you were trying to get there on time. You took a wrong turn I believe and ended up viewing a Shuttle ready to launch. Talk about that experience.

WIENS: Yes. It was my first time in the state of Florida actually. Obviously my first time to Kennedy Space Center. We were given passes at the gate and I was supposed to know where to go to get to the buildings where we had our review. I think I was running maybe a little bit late already for whatever reason. Probably because I was not used to Eastern time coming from out west. Somehow, I ended up taking a wrong turn or didn't take a turn where I was supposed to, and I drove a long way, because this Cape [Canaveral] is a large facility.

Then I did all of a sudden around a corner realize that I was coming up on the Space Shuttle at a launchpad. I could see that there was a guard with a vehicle and guns up ahead of me. As soon as I saw that I quickly made a U-turn in this deserted road and went back the way I came. But yes. It was a very different experience for me to be out at the Cape. Very real to see things getting ready for launch into space with these huge rockets.

JOHNSON: Yes, I imagine that would be kind of surprising. Let's talk about those launch delays and what caused those.

WIENS: Yes. I remember a few details of the preparation for the spacecraft and the launch as a whole. One was that I know that Lockheed Martin contracted for a star tracker, which is an essential navigation device for these.

But for the star tracker, instead of going with a tried-and-true company, which was actually a rival of Lockheed Martin, they went with a Canadian company that had not built star trackers before. This put us in a bind in two ways. One was it was a new company. It was inherently not well trusted. We wanted to make sure that this star tracker would work and that it would work well. Also it was a non-U.S. company, and so we could not provide certain kinds of assistance or even tell them what was wrong in order to fix it. We could tell them to do certain kinds of tests and then give us the results. If those results were not satisfactory, they could see what the requirements were and they could tell us if they were meeting those requirements or not.

This star tracker went through numerous phases where it was tested. It still didn't work right. The company worked on some more improvements. It was tested again. It still didn't work right. It was well over six months late. This is years ago so now I don't remember all of the details, but it was a major red flag for the mission.

I don't remember whether it was the star tracker or other details as a whole, but when it came close to the launch, it was supposed to be in January I believe of 2001, with about five months to go, or something like that, the mission held another review. That one just said that we would not be ready for the launch, or at least not sufficiently ready for the launch to feel like we would have a successful mission, and the launch was forced to be delayed.

That meant we had to get back in a launch queue in a relatively busy schedule with the Delta launchpads, and so then it was rescheduled for July. The launch window was in July. It actually launched in August because of some other delays including a tropical storm.

JOHNSON: That's what I'd read, that there was a storm. Of course you finally get everything ready to go and ready to be launched. Then they're predicting a tropical storm to come through. Were you out there in that August time period when it was supposed to launch?

WIENS: Yes. I went out for the launch readiness review, which I think was in April/May timeframe and then of course the rocket got stacked up and then the launch was scheduled for July. Yes. I was out there with my family, all of us were out there.

First of all, I just arrived at the Cape, and was supposed to do interviews the very next day. This is in my book. But the launch was delayed because of a radiation test on an electronic part that was in the spacecraft. The radiation test was done by the European Space Agency. It was done differently from radiation tests in the U.S. There was some failure, and so there was an investigation to see whether the radiation test that failed in Europe had any strong implications for the success of this mission. There was a need to go back through some records on the testing of this part and do a mini-review of it, and so the launch, which was scheduled I think for two days after I arrived there, was pushed back by about four more days while that investigation happened. So as it turned out, I arrived the next morning at the interviews, and I was told right on the spot that I needed to tell my next interview that the launch was delayed, and just say that there was some checking on some background issues. That was my introduction to the delay, right when I got to the interviews.

That delay was not very long. It was still in the timeframe that we were there as a family. But then a tropical storm came up. The rocket was fueled. There was an attempt to launch on the day that the storm was rolling in, but we got to about the 20-minute mark of the countdown, and the high-level clouds were too thick, and the launch was scrubbed. We all went back. So we had the experience of getting out to the bleachers to watch the launch, and that was it.

JOHNSON: Did you stay for the actual launch? Or was that it?

WIENS: No. I didn't stay, I was with my family, and we all went back together, and we watched it on the video from Los Alamos a week and a half later.

JOHNSON: You mentioned that you had to go talk to the press. Is this something that you were used to doing? Or was this a new experience for you? Especially having to tell them that it was delayed and why.

WIENS: I was somewhat used to the press. We had gotten press already back in Pasadena when we were in Phase A the first time. We were one of three missions, and the missions were novel. Stardust was novel. Genesis was novel. Maybe the third mission not so novel at that point. But yes. I was not talking to the press as much as Don [Donald] Burnett, but I was. Then when I was leading the effort at Los Alamos I got used to talking to the press as well. It was a gradual thing. But yes.

JOHNSON: Let's talk about the launch then. You said you were back at Los Alamos to see it. You'd been working on this a long time, to see this happen. Just describe that experience a little bit, and what it felt like to see it finally go off.

WIENS: In a sense it was anticlimactic because we had all come home and we were of course eagerly awaiting the news. There was a TV that was set on the NASA channel, and we could watch the launch. A number of us just went into a room at Los Alamos in a trailer that was our main building at that time, and we watched the launch go off.

It went off without a hitch. That was great. Of course it didn't orbit the Earth. It went straight out towards the L1 [Lagrange] point where it was going to be loitering, and so it was nice to see that was all happening.

That was very easy. It was what came next that was more challenging.

JOHNSON: Let's talk about what came next.

WIENS: Yes. It was not right after the launch. It was in the phase when the capsule was getting opened that we started to see some problems. It was in November of 2001. The Genesis spacecraft was then getting out at the L1 point, and we were starting to deploy for turn-on of the instruments. But I think the capsule was opened sometime earlier than that.

As soon as the capsule was opened, within three days we started getting messages that we had a thermal problem. We had a very special paint that was applied to the front of our open area, when the clamshell design of the capsule opened. This was an all-metal spacecraft with very little extra material in there for thermal properties because we were very concerned about outgassing of any material that could contaminate the surfaces of the collector materials. So we had a lot of metal exposed. All of the collector arrays were types of metal that could heat up a lot. Our solar wind collector had a gold-coated mirror. All that would get very hot. We had to have some way of trying to keep the whole capsule cool.

That was a special white paint that was around the canister. This white paint was supposed to have a very high emissivity, low absorption. As soon as it was out in space, we started to see it fail. The temperature kept rising and rising. Material was polymerizing in some way that was unexpected. It was basically turning yellow, turning brown, slowly. The critical aspect of this, we could start to see, was the battery for the parachute deployment. This battery was not designed to be recharged by the spacecraft. It was a stand-alone battery. It was supposed to maintain its charge until the parachute was deployed at the end of the mission.

Batteries like to stay cool, as we know, and this battery had a maximum flight temperature of 25 degrees C which is just a little bit above room temperature. Our capsule temperature was zooming up towards that temperature. I believe the decision was made to close the capsule temporarily for a while, and then we convened a panel to figure out what to do.

Lockheed Martin was in on that panel of course. We immediately questioned why is the maximum temperature for this battery so cold, why does it not have a higher flight allowable temperature. We figured out that it was because nobody had ever tested the battery to warmer temperatures, and this was a common battery that had been in different spacecraft or different applications—militarily too, probably. There were a lot of these batteries around. It turned out that Sandia [National Laboratories] owned a whole lot of them. Lockheed Martin asked if they could purchase that lot, and they got a large number of these batteries, and they said, "We're going to start a string of tests on the batteries, and let's see what their actual flight allowable temperature should be."

I loved the way they set up the test. They just went to a place like a Walmart or K-mart and bought a whole bunch of beer coolers. These beer coolers they had outfitted with temperature controllers and heaters, and they would keep each beer cooler interior at a certain temperature. We decided that some of them would be kept at a fixed temperature, and some of them were set to lead the spacecraft temperature by 5 degrees at any one time to give it a worst-case test. Then they would put a bunch of batteries in each of these coolers, and periodically they would take a battery out and test it. So they would start to figure out what is the failure rate at different temperatures for these batteries.

The good news that we found out was that these batteries could handle much higher temperatures. It was found out almost right away, preliminarily. Then the question was how long-term can they last at those higher temperatures. The set of battery experiments went on for the whole duration of the mission to make sure. They had enough batteries that they could take one out periodically, test it, make sure it was still good. It turned out that these batteries could last for the several years of the mission up to I believe it was 81 degrees C.

I believe—and this is from memory from many years—I think we got up to about 76 degrees with our temperature, something on that order. But most of the time it was closer to about 69 degrees C. So we really pushed pretty close to that limit but we stayed below it by several degrees. It was pretty clear there was a limit where the first few batteries would start to degrade and fail. Some of them seemed to be good up to a little bit higher temperature.

JOHNSON: That's interesting again that that wasn't found in some of that testing or the reviews that were done. They had nothing to do with the deployment though or the problem that eventually happened either.

WIENS: That's right. But it was certainly suspected initially that the batteries caused the crash. In terms of people's, perhaps, feelings of guilt and different things going on right after the landing.

JOHNSON: During that three years before it actually came back what were you doing? When did you start knowing for sure that it was collecting what it was supposed to collect and it was working the way it was supposed to work?

WIENS: I started to say earlier that the deployment was in November of 2001 so about four months after the launch. We could see immediately—in fact I was out at Jet Propulsion Laboratory for the deployment—we could see that the panels were behaving just as they should. That was all good. The ion and electron monitors had started operating earlier.

Then it was time to turn on the solar wind concentrator. We stepped up the voltage very slowly. But we actually found that we had a problem on the concentrator: one of the voltages would not go up as high as it was supposed to. Actually we mothballed the concentrator operation for a few days. Actually a few weeks. It would work during normal solar wind speeds, but when the solar wind speed is high then it took a higher voltage to reflect the ions. All the voltages were running at higher potentials. That was when it broke down.

We eventually figured out just by testing that if we approached the voltage with small increments, it would actually run to a higher voltage than otherwise. This kind of behavior is apparently typical when spacecraft failures occur out there where you cannot do anything else.

Lockheed Martin worked with us to modify the software to operate this instrument, to do two things. One was to approach the main voltage slower. We took much smaller steps on this one high-voltage grid. That worked quite well, but it still would fail when it got to quite high voltages. We had to also just place a limit on the voltage where it would not go up to the voltage where it failed.

Then we also had it auto recover, so if it did fail at some point, it would actually use up to three attempts to come back up to the desired voltage. We nursed that thing along that way. The particular component was a hydrogen rejection grid which was not absolutely essential for this concentrator but it was quite important that it work at least part of the time, and so we rejected most of the hydrogen. We got a little bit higher fluence of hydrogen in the target but not that much and it all worked okay. We got our result out of it.

JOHNSON: Did your team have any kind of contingency planning in place if something happened with the parachutes or if anything had happened to the capsule when it was coming back? Were you part of any of that contingency planning?

WIENS: Oh yes. I was right alongside of Don Burnett with all of these discussions since I had been with him from the beginning on this. We did have discussions right from the beginning and in fact in the proposal phase: what could we do to mitigate a partial failure at the landing or any time through the mission?

Once the panels were deployed, even if the solar wind types were not identified right or something, we would get a sample of the solar wind no matter what. That was really fail-safe science. We knew we would get the baseline of science.

The two things that really had to happen to get any science was that the capsule had to be able to close successfully. That was very carefully watched because those mechanisms were mission critical. They had a very high margin of torque on them. They could basically crush your hand—if it got in the way—to get that capsule closed. Then the other thing is the landing had to happen successfully. Those two things really were watched carefully.

Like I said already, the torque was very high for that motor. The other thing was that if the landing were to not happen as we expected, first of all we had to get the material to Earth. That was absolute. Then secondly, we had to do no damage on Earth. We didn't want to hit anybody's house. You had to realize that no NASA capsules had come back since Apollo. Apollo had landed in the ocean. So had Gemini, so had Mercury. This was the first landing on land that NASA had ever done. What if this was 40 miles off? We had to think about that kind of contingency.

We talked about how many houses are within 40 miles. Salt Lake City is farther than 40 miles away, so there were basically almost no houses within 40 miles of the ground zero. Then you have other things that can go wrong. What about the parachute and so on? Or what if the helicopter didn't catch the spacecraft and it parachuted to the ground?

We knew that we would have broken material, and so then the question was, well, how do we distinguish broken material from other broken material? We had different collectors with different types of solar wind that we didn't want to get mixed up. Perhaps for each collector we could make some kind of a signature or something to identify it. We settled on having different thicknesses of wafers.

The manufacturers of the silicon wafers and the other kinds of wafers were willing to give us different thicknesses. You can change that thickness by 50 microns, that's five-hundredths of a millimeter. You can measure that with a caliper, with a micrometer. We could determine from any fragment, no matter how small, what solar wind array it came from by using different thicknesses. That was decided in the proposal phase. We talked with MEMC [Monsanto Electronic Materials Company], the silicon wafer provider, way back then. Then with the concentrator we hoped to get the target back. But that was all. It was outfitted with some springs in the target assembly.

JOHNSON: The fact that the thickness was different proved useful later, that they could identify the solar wind type by the thickness.

WIENS: Absolutely.

JOHNSON: Let's talk about the landing itself. Unfortunately we had lost [Space Shuttle] *Columbia* [STS-107]. People were a little worried about what would happen in an accident over land. Talk about the reentry and where you were when you knew the capsule was closed and everything was getting ready to come in, that time period and where everyone was at that point and who was at [U.S. Army] Dugway [Proving Ground, Utah] waiting.

WIENS: Yes. It was 2004, so three years later after the launch. First of all the closing of the capsule had happened on April Fools' Day of that year. That all worked perfectly. This capsule was on its way back. It passed by the Earth and then took sort of a circuitous route. That was so that it could enter over the dayside of the Earth. If it had come straight back from the sunward side of the Earth it would have actually swung around and landed on Earth at night. We didn't want that, so this longer time period was worth the wait to get the capsule back in the daytime.

It was going to swing around in the early morning hours, because it always comes in at an oblique angle, and then come around the Earth towards the Dugway Proving Ground in Utah passing over the west coast on the way. This was all planned for the Dugway Proving Ground. There was a lot of publicity that NASA had put on this because it was the first of its kind. The whole helicopter snatch of the parachute was a big highlight because the pilots of the helicopters were known to be stunt pilots for making movies and other things where they would fly these helicopters. In fact their names were quite well known because of all of the great work that they did.

This was a really well publicized event by NASA. We were out in Dugway and the Salt Lake City area for several days before. There was a press briefing the night before. All was ready. We'd been out at Dugway already to check out the hangar, check out where the press would be, where the helicopters would take off. This was all prepared in great detail.

The morning of the landing we went out very early before it was light so we could get through the gates and be ready. It was just a little before midmorning when the landing was to take place.

There was a large crowd in the hangar because of all the press that had been prepped for this special event. This was still in the era of major news networks, the big three and so on. They were all there. From Los Alamos I had a reporter who had been following this news story for several years. He was a big guy. His first name was also Roger. Snodgrass. He had all kinds of cameras with him and he was going to get the inside story from us. Also the laboratory itself had a news handler there as well. We were well prepared.

I started giving interviews the day or two before and was there several hours in advance on the morning of the landing. NASA had set up a large screen at the front of the room with a large number of chairs for everyone to sit, and the cameras were at the back of the room.

JOHNSON: Talk about watching that screen, what you saw on the screen, and when you first realized that something wasn't right.

WIENS: About 30 minutes before the landing the helicopters took off, so there was a big to-do with a lot of photos of the pilots getting into the helicopters. Two helicopters were positioned to take turns at snagging the parachute during its descent. I would almost like to say jousting for the capsule but it wasn't quite that way.

Then there was another helicopter going along to film the event and check things out. They took off from just outside the hangar. Then the crowd was milling around and waiting for the half hour until the capsule would appear.

The Dugway Proving Grounds are outfitted with long-range telescopic cameras that were set to check out cruise missiles and all kinds of other things that had been tested there. They had very high-power lenses on them. They also knew exactly where to look for the capsule in terms of coordinates of where it would come in, and so five minutes before the landing they acquired the image of the capsule, and we had radio contact before that. I don't know the altitude of the capsule five minutes before landing, but it was many miles up, and still entering the atmosphere effectively. We saw it as a very small object initially and the audience cheered. Everyone sat down quickly.

I was talking to a *Los Angeles Times* reporter at the time, so I just said, "Let's go sit in the front row—I want a front-row seat for this!" We did. We were expecting at about two and a half minutes that first a drogue chute would appear and then the main parachute for the flying, basically, where it would be captured by the helicopters. We started seeing that the capsule was spinning. We could see that. Spin rate had been increased for stability in the upper atmosphere.

But as it came down, we could see something. It was wobbling a bit. We kept realizing that there was no parachute. We were looking at our watches and listening carefully. But as it got bigger and bigger on the screen, we started to realize that something was dreadfully wrong. The announcer was saying something to the effect that we are not seeing a chute, no drogue, no main parachute, and so this is an anomaly. Then finally they said, "Expecting impact."

Then at that point we saw the ground and then the screen went blank briefly. Then momentarily the picture came back and there was a capsule on the ground. A gasp went up from the crowd when this happened. We also heard the helicopter transmission. When we had the words, "We have impact" announced, one of the helicopter pilots did not understand what was going on and asked, "Can you give me an altitude on that, sir?" The answer was, "At ground level," in a very dry voice. When we started to reacquire the image of the capsule lying on the ground on its side, there was quite a bit of movement by the camerapeople and Roger Snodgrass, my reporter, led the way, because he knew where I was sitting.

He very clumsily, with about five cameras on him, came running to the front of the crowd and got right in front of me and started asking me, "Roger, what do you know about what is going on here?"

It didn't take but about 20 seconds and we had every microphone from every major news network. They just followed Roger right up to the front, and there were literally 20 microphones with cameras in front of me.

I don't remember exactly where others were. I think I was the main person for that room for the publicity. I think Don Burnett, and also Amy Jurewicz and some others, including Don [Donald] Sweetnam, were waiting elsewhere for the capsule to be delivered by the helicopters to that facility. So I was the lone victim at that point. I think my instincts from watching model rockets as a kid came in, which I mentioned in my book. We had lots of ground impacts with my model rockets when I was a kid.

I just started just telling the crowd of reporters straight up that we had contingencies for this very thing and that we were going to exercise those contingencies. I started explaining about the collector materials, about the types of analyses that we did, and the fact that we had designed the collector materials to be distinguished even if they were little shards, and that we would actually continue with our science on this mission now that we had our samples on the ground. We would get our results out of that.

I was a little bit new to how news reporting of disasters works: they wanted to report a catastrophic failure—having scientists in tears—and I didn't give them that. But eventually, of course, we would use the news media to our advantage, to give additional reports now that we had the capsule in our hands several days later. We did have samples and in fact some of our best samples were intact. That was the case with the solar wind concentrator.

But the news frenzy went on for a number of minutes. JPL had a person there who wisely eventually said, "Roger, I need you over here," and pulled me out of the crowd, and just said, "we're not going to talk to the media anymore until we have a news conference." It was then scheduled for the top of the hour.

Don Sweetnam came over and Chet [Chester] Sasaki who were the mission leads, Sweetnam for the flight aspect of it. They were going to work this hastily constructed press briefing. They were looking for Don Burnett but he was going to not show up at that press conference, and so they took that press conference themselves. That's the way it went.

JOHNSON: Was he too concerned with what was going on as far as getting things ready to be recovered?

WIENS: That, and he felt very personally responsible for the crash because he felt that he had almost certainly demanded that the capsule be left open in spite of the heating that the battery was experiencing. Myself, I was quite a lot more confident in the testing that had been done and was just saying, "No, we never went over the temperature at which it would have failed." I didn't think that was the problem. I just didn't know. But indeed it was not the problem.

JOHNSON: He just felt responsible because at that point nobody knew.

WIENS: Nobody knew any better. Yes.

JOHNSON: I know that there were contingency teams there. The people from JSC [Johnson Space Center, Houston, Texas] were there also. There was a clean room available. Originally built so they could move the capsule there and get it ready to send it to JSC. But of course it kind of acted for a different purpose after this. Were you part of that contingency to go out to the crash site? Or did you get to go out there and just see it for yourself? Talk about the recovery. I read that it was a little bit delayed because there was some kind of miscommunication between the team that was saying, "No, we know what to do," and then I think NASA management didn't know that they knew what to do.

WIENS: That part of the recovery was not well rehearsed. We had talked about the different possibilities in terms of science and even about risk hazards. But the actual details of who goes out to a crash were not rehearsed, and so that was an on-the-spot decision.

It was decided that Lockheed Martin personnel should be the ones to go out there as far as I know. They were the ones that did the cleanup. I think they were able to be on satellite phones if needed. There was some debris around and of course it was impossible to just pick up the capsule without things falling out. They brought tarps, they brought shovels, and so they tried to wrap it up. Of course I think they were wearing gloves.

There was also some safety issues because of the mortar that would deploy the parachutes. They had to make sure those got deactivated so that there was not some explosion when this was around people, so that was the first activity, and that was a Lockheed Martin responsibility. They had to do that first, and they had to have the right people to do that. Once that was done then I think they could then proceed with the rest of it. Then I guess also make sure there was no other things, any gases from the capsule that could have been hazardous.

They were the ones that were out at the crash scene. We could actually watch them on camera. It was fairly long. It took them almost until dark to take care of the site. We hung around for most of that time, and then went back to the barracks. I think we ended up in barracks because some of us were not planning to stay longer immediately. I just can't remember some of the details. We had to all make contingency plans. I ended up in some barracks.

It was a very sad occasion, people being together in barracks. We knew we got our results. But the portrayal by the media that this was a failure weighed pretty heavily on us.

JOHNSON: Unfortunately you had no control over how they would portray it. Like you said, they wanted to make it into maybe something that it wasn't, because it makes a better story.

WIENS: Absolutely.

JOHNSON: We've already talked about the accelerometers and how that caused the problem because of being installed incorrectly. But how quickly did that get determined that it was those accelerometers?

WIENS: It was very fast on a relative scale of these things. I don't know the exact day but it was I believe about two days after the crash that Pete [Peter] Doukas, one of the main people from the Lockheed Martin team, came to each of us and just spoke to us personally and said, "We know why the crash occurred. It was not anybody's fault but the design at Lockheed Martin, because the accelerometers were installed upside down." At that point that was one of our big questions. How did this whole parachute scheme not work? The accelerometers were redundant, but they were both installed upside down.

Then the question went to how did testing and reviews not reveal this. I don't know the exact story. But there is at least a story about a test that was done, but because it was done in a room with a ceiling, the test module that was testing, the board that was holding the accelerometers, did give a signal. But it's not clear whether the signal was when the board was accelerated or when it was decelerated at the ceiling. That I think was part of the faster, better, cheaper era. That's what we eventually relegated it to.

JOHNSON: Were you a part of the mishap investigation?

WIENS: I was not. No. I was at that point part of the payload, and the payload was not involved in the mishap. We were not part of that investigation.

JOHNSON: Did you get to go out to the clean room there in Utah and see it once they brought it into that area?

WIENS: Yes. I was a big part of that. The solar wind concentrator was our instrument. We had worked to install it. We wanted to be the ones to extricate that particular instrument. (I'd also been involved in the material selection for all of the payload earlier.) I was on the phone almost right away to our design engineers and technicians at Los Alamos. Our main technician, Rob Baldonado—his name is actually Juan Robert Baldonado—I was able to reach him right away and he was more than willing to come out and help us with that. He had over 50 spacecraft instruments to his credit at that point, so he had very careful hands. He brought out some equipment like gloves and clean room suits from Los Alamos, so we were set, him and me, to participate in that investigation.

He played a role right along with the JPL technicians who came up to do that as well, and we were all involved in pulling the canister apart. We had to use power tools to actually get it apart and to get at the different parts of it. For Baldonado, it was one of the most memorable events in his life, he told me later, to do that.

JOHNSON: Was everything that you needed available in that clean room, since it wasn't really supposed to be the place that things were taken apart? Did you have everything you needed?

WIENS: There were several trips to big-box stores to get various power tools and various equipment that we needed, and so the work would stop and some people would go out and they would come back an hour and a half later because we were out in the middle of absolutely nowhere—or two hours later. They would have the power saw that we needed or whatever it was. Yes. But we wanted to extricate the panels and see if we could actually find any of the collector materials intact, and we did find a number of them intact, and one that was unbroken. I think just one fragile collector wafer.

But really the important thing for me was the solar wind concentrator because it was really addressing the number one goal of the mission. When we peeked in, it was with a dentist's mirror on a long stick originally. Somebody was looking in through a crack with flashlights and this dentist's mirror on a stick. We could see the target of the solar wind concentrator, and it was almost completely intact. That was when we gave our next press briefing and we said, "We're going to get almost all of the results from this mission." That made another major headline.

JOHNSON: How often were you updating the press? Was that a daily thing during that time?

WIENS: It was daily, yes. I had the news card from the *New York Times* reporter, and he said, "Even months after this whenever we get a result, I want you to call me and let me know." I have more story to give about that.

JOHNSON: Would you like to tell it?

WIENS: Yes. After we got the concentrator out and the target was really beautiful, it had just one piece of mud on it right smack in the middle of one of the four quadrants, and one of the four quadrants was slightly broken, but we think we recovered all the pieces from it actually. I made a special call to my design engineer at Los Alamos and thanked him for putting such care into the way he had packaged that target with compression springs, which really helped it to survive.

We had that target and that was very precious. But that part of the science was going to take a long time to work out, and we wanted to start the science slowly because the material was in effect more precious than before because we had less of it overall.

Some of the initial analyses started, but the oxygen isotopes, which was the biggest result from the whole mission, that required a special instrument to be developed, it was called a MegaSIMS [Secondary Ion Mass Spectrometer]. Part of it was an accelerator mass spectrometer which takes up a whole room, the other part was a secondary ion mass spectrometer. It had never been paired with an accelerator, and so this was a first of its kind instrument. The size of a large room. It was being built at UCLA [University of California, Los Angeles], and it wasn't completely built until the mission would be successful, and so, after the landing, the money was released and the instrument was then constructed.

It took several years. In the meantime we did characterization of parts of the concentrator target to understand how the isotopes were mapped onto this target. I did a lot of work to simulate the ion trajectories. There was a software package that I had been using since the late 1990s to design this. Now we had more advanced versions of this package, so we flew millions of ions in the simulations on a computer to determine how the ions, or the isotopes, would be mapped onto the target. We did noble gas measurements in Switzerland to actually check—to verify those simulations. Those were successful as well.

All this time this MegaSIMS instrument was being built at UCLA and tests were being done and the background of the instrument was getting better. But finally, it was literally five years after the mission when this instrument was ready and the measurements started to be made.

We first got word that the results were successful. We were getting some results out of it. Then the discussion started happening between a small team of us to put together the scientific interpretation of the results. They were very interesting, but they were a little bit baffling in certain ways. What was it actually telling us? We were getting basically one number out of this and we just didn’t know exactly how to interpret it. It’s hard to do that verbally now. But it’s very true that the Sun’s composition of oxygen isotopes is quite different from the Earth’s. There was also two different effects that we were seeing in this. Then there was the effect of the instrument itself. We tried to back out the instrument effects, then we had these two different scientific effects that were left. We think we interpreted them correctly. We went through a bunch of arguments and discussion of this and wrote this paper and eventually submitted it to

*Science* magazine. It got reviewed. The reviews were responded to, and the final version was written, and now it was time to publish this paper.

It was published alongside of one other paper, which was on the nitrogen isotopic composition of the Sun. That was done a little bit earlier but it was not done with the MegaSIMS instrument, so that didn't take as long. It was done in France. But they held the results to publish them together. The two results of the two different elements made sense together as well, which was really exciting for us.

We got this result, and then when we could contact the press, I contacted the *New York Times* again and said, "We have your story for you. I know it's been five years later." Unfortunately the *New York Times* was not interested in the story at that point. I was absolutely shocked. At that point I vowed that I would get a book published for the public. It took a different form because of the fact that I was on the Mars rover at this point in time. But it was just three years later that we got this book published that included the first seven chapters on the Genesis mission. So we have a popular account of the Genesis mission in my book, *Red Rover*.

But to get to the results of the oxygen isotopes. There were several theories of why meteorites have different oxygen isotope compositions than Earth. One of them was that there was a change in the solar system materials that were condensing to form—not just condensing, but accreting—to form the planets, relative to the Sun's composition. What we call fractionation—a change in the material that is actually accreting—was due to the solar output of the Sun, the photons that are coming out from the Sun. It turns out that the photons coming out from the Sun, especially in the ultraviolet part of the color spectrum, were able to treat oxygen-17 and oxygen-18 in a different way so that they were reactive, to react and glom onto dust, which the oxygen-16, which is the majority isotope, did not. So we have this photochemical reaction that is very sensitive to oxygen 17 and 18, causes them to glom on to all the dust rings, that then form the planets, while the oxygen 16 eventually blew away or collected into the Sun.

We have a window into what was happening chemically in the solar system at this time. We never knew in the 1970s, when the anomaly was first discovered in meteorites, what it would tell us. But it did tell us something very interesting about the photochemical effects in the solar nebula. We got this only from the Genesis mission. We would not have this today if we did not have the Genesis mission.

That was very satisfying to us, and a vindication of the mission in a big way.

JOHNSON: A lot of years put into that to get those results back.

WIENS: Oh yes. Yes, you think about the fact that Don Burnett started this in the early 1980s and we got this and published it in 2010. It was really almost an entire career. For me, from 1990 on to 2010 to get this, so 20 years.

JOHNSON: Yes, and out of something that could have been a complete disaster, it's amazing what came from it.

WIENS: Yes. I say that God smiled at my career because if this crash had happened on Mars with the Mars missions that I've been on we would have never heard back. But it happened on the one that came back to Earth, and we actually still got our results. Very glad.

JOHNSON: We have about 15 minutes, so I thought I'd ask you just some general questions that we usually like to ask people. Looking back at Genesis and like you said this 20, 30 years that Don Burnett spent on it, but the 20 years that you were there or working on it or getting the results, what do you think are some of the lessons learned?

WIENS: Yes. Very good question. Persistence pays off in many cases. You have to have the long view in a lot of this space work. We are working on things now that will not come to fruition until 2050 in some cases. This is the year 2023. That's about the same timeframe actually. I think that NASA should actually look even longer-term than that.

In the 1990s we didn't have that long a history of missions. We had Voyager that was out since 1970, so 20 years, but now we have things that have been out there more than twice that long. We can plan for a 50-year mission or even potentially a 100-year mission at this point. That is something that NASA ought to be thinking about. That's one.

I also like the idea of the science-led, PI-led missions. We went through the era of faster, better, cheaper, and there has to be a balance. What came out of that is that we have a line of smaller missions, and we have a line of medium size missions—we have the Discovery missions and we have the New Frontiers missions—and we have the flagship missions. The Discovery missions have been incredibly useful and successful, and we need more of them. We are facing a bit of an issue right now with a bottleneck in these missions, but I hope that the frequency of them increases again once more, so that we have many of these missions going.

There are aspects of faster, better, cheaper that need to stay with us, so that is one, that we have these smaller missions. Not all of our eggs in one basket. Sometimes the concepts for the instruments can come out of a small mission concept. We now have several offices that will fund the instrument development. They need to continue. We need to keep raising people up into these fields who not only can do planetary science, but can speak the language of engineers and go between the engineers and the scientists to make it and bring it all together. That's actually one of the things I'm working on here at Purdue.

JOHNSON: You're working on that now?

WIENS: Yes. In terms of bringing instrumentation emphasis into a planetary science department. Yes.

JOHNSON: We mentioned last time that communication and how important it is to get people talking and understanding each other.

WIENS: Yes.

JOHNSON: You came to Houston a few times, right? To do research and spend some more time in Houston.

WIENS: Yes.

JOHNSON: Was that all with the Genesis results?

WIENS: I did my thesis work, some of that in Houston. Then I was involved with Eileen Stansbery and others who were involved in the curation aspects. I was happy to be involved in that. I'm still on the sample allocation committee and working with Judy [Judith H.] Allton on that. Yes, I'm still connected with Johnson Space Center in those ways.

I had funding to continue on with Genesis science work up until about two years ago and so that's been great to have. I'm somewhat involved, yes, even now.

JOHNSON: Looking back at Genesis and your involvement and continued involvement to a certain extent, what are you most proud of from that mission?

WIENS: The whole thing. It was started before I joined, but I was basically the third person on the mission and one of the main people to see it through. That's extremely gratifying for me. Like I said, it could have ended very differently, for different missions that I've been involved in: Every one has been a success in spite of the appearance of failure at times. That's very gratifying to see the successes of these missions.

I have to say I cannot take credit for them in that sense because one never knows if there is a mission-failing error that will cause one to never receive credit for anything on these missions. Many people have experienced those. I'm very grateful to not have. But I'm very proud of being able to be a part of this mission from beginning to end. It's many different aspects of one mission.

JOHNSON: I appreciate you talking to me. Is there anything that we haven't talked about or any anecdotes or any stories that you wanted to make sure we talked about?

WIENS: I think we've covered it.

JOHNSON: Okay. Sounds good. I appreciate you taking time out because I know you're busy at Purdue now. You're teaching now, right? Is that what you're doing?

WIENS: Yes. I'm still leading the SuperCam instrument on the Perseverance rover. We have a large team of almost 80 people involved in that one instrument. It's still keeping me very busy.

JOHNSON: Exciting time, isn't it?

WIENS: Yes. Heading towards Mars sample return, which I hope people at your organization will be very involved with.

JOHNSON: Yes. I hope so too. There's so much out there now to cover and so few of us. It would be great, I hope we do get to cover everything.

WIENS: Yes. Good luck and Godspeed on that one.

JOHNSON: No kidding. But I appreciate you taking your time and I will stop the recorder now.

[End of interview]