# 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 2nd, 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 at Purdue University and talking to me today over Microsoft Teams.

I appreciate you taking time out of your schedule to talk to me. I'd like to start by asking you about your education and your background and what drew you to working with robotic spacecraft and NASA.

WIENS: Thank you, Sandra. As many kids, I think, I was one who was absolutely fascinated by space and anything to do with rocketry. I was born at the very beginning of the space age, being born in 1960, and so we grew up watching—I don't think I got to see the Mercury Program myself—but Gemini and Apollo as young kids. Just at the advent of when families started really having television in their homes as well.

Actually I'll just mention that my story starting at that point is actually recorded in the book *Red Rover*, which I published in 2013 with Basic Books. I'll just say it's somewhat about the Curiosity rover, but the first several chapters are about the Genesis mission, the advent of the Discovery Mission Program, and it starts with my childhood fascination with the space program.

I was born into a Mennonite family in a small town in western Minnesota. We were quite isolated; this was well before the era of the Internet. The only information that we really had in our town was what we got from newspapers, magazines, school, and the library. I had a brother who was two years older than me, and we did a lot of things together as kids, and he is also a scientist. Growing up, we were really fascinated with everything scientific. We would go to the public library and we saw ads in *Boys' Life* [magazine] for model rockets, so we ordered a kit and started launching our own model rockets. I was in the third grade. Then of course at the same time we were watching the big rockets on TV at the beginning of the Apollo era. I remember that it was Roger [B.] Chaffee, who was lost in *Apollo 1* fire; he was sort of a namesake in a sense for me. Not that I was named after him, but I identified with him.

These things were really fascinating to me. We certainly watched Apollo 8, the first mission to the Moon with the astronauts rounding the Moon right on Christmas Eve, and really identified with them. They read the creation story from Genesis, which seemed most appropriate in that place and on Christmas Eve. This was a very special time for us, along with the next summer, Apollo 11 in July of '69 landing on the Moon, and of course we wouldn't miss that for anything. Everything stopped for those events.

I remember being in the sixth grade, and so a few more model rockets under my belt, and then Apollo 13 was launched and we were all ears just praying and hoping the astronauts would make it back. This was such a special time for being interested in space.

Then that changed of course, and that's a whole other story, how NASA then dropped down in their budget, as they had to after that. But then the story turned more towards robotic space exploration. I didn't think I would really have a career with this. I was interested or thought I would do something that would be useful for humans, such as going into third world relief, working on technology in a third world country, or something like that. Somehow, I kept getting moved back into science. That's how my story started.

JOHNSON: You kept getting back into science, and I saw that you wrote your thesis on the Mars atmosphere in the physics department when you were in University of Minnesota. You had an opportunity to work with those meteorites, is that correct?

WIENS: Yes, absolutely, and so I started a PhD in the University of Minnesota, not knowing really what to do. I had an opportunity to work with Professor Bob [Robert O.] Pepin, who was studying Moon rocks and meteorites in the physics department as a holdover from the Apollo era and from earlier mass spectrometer development, where it was being used to determine very precisely the atomic masses, as that helped to define nuclear physics.

Mass spectrometry was less related to physics by the time I came along. But I still had the opportunity to work in the mass spectrometry lab as a physics major, and so I took it. It was just at the time when Don [Donald] Bogard at Johnson Space Center was also involved in trying to date some of these Martian meteorites and he got a ridiculous age of something like 7 billion or 9 billion years and realized that there was some trapped argon in these meteorites. Then he started noticing that the noble gases were similar in their ratios to the Mars atmosphere, and the laboratory at Minnesota, which had the capability to measure very minute (picomole) amounts of nitrogen was asked to get involved, because the Mars atmosphere is very unique in its nitrogen isotopes.

I watched the first measurements being made and then I got in on the second set of measurements that were being made on this one particular shergottite meteorite EETA 79001. The composition of the Mars atmosphere, trapped in that meteorite, became my thesis work, and another component of that thesis work was trying to figure out how the gases got trapped in these Mars meteorites. For that I got a NASA fellowship, and I started making trips down to Johnson

Space Center [Houston, Texas] in the 1980s working with Dr. Fred [Friedrich] Horz in the light gas gun lab to do some shock work on analogue materials to see if we could capture gases if there was a small void space in the rocks. In fact I was there when the [Space Shuttle *Challenger*, STS-51L] accident happened, and Ronald Reagan came to Johnson Space Center for the memorial, in 1986, I believe it was. I was right on-site. I was just reading a letter the other day that I'd written to my parents from Johnson.

JOHNSON: It's an interesting time to have been at Johnson, when that happened.

WIENS: Yes, it was.

JOHNSON: It must have been an eye-opener about how an accident like that affects so many people and an entire Center and the ramifications of such a thing.

WIENS: Yes.

JOHNSON: How did the opportunity to work with Don [Donald] Burnett come about? Was that something you were looking for, to go to someplace like Caltech [California Institute of Technology, Pasadena]? Explain how that happened.

WIENS: It was really serendipitous or a God thing you could say. I was not looking to stay in space research necessarily, I wasn't sure what to do after my PhD, still thinking maybe I would go teach in the third world or something like that. Actually I already had an opportunity to work with Don Burnett at that time. I got an NRC [National Research Council] fellowship to work at Jet Propulsion Laboratory [JPL] with another pioneer Marcia Neugebauer, one of the first women at Jet Propulsion Laboratory, and Don Burnett, who were just starting to work on what became the Genesis mission. But I turned it down because I didn't see much future in it. NASA was not doing sample return missions, and had not since Apollo, and I had an opportunity to do a postdoc at San Diego on terrestrial geochemistry and set up my own mass spectrometer there, and so I went to work with Professor Harmon Craig at Scripps Institution of Oceanography [San Diego, California].

I did that for two years, and I happened to meet—I knew Don Burnett from the fact that I had already put in the successful NRC proposal. I met him at an AGU [American Geophysical Union] conference in fall of '89, and he said, "Hey, Roger, I've been funded for this project. Would you come up to Caltech and talk with me about it in case you're interested?"

I still wasn't interested. I said, "Okay, I'll come up and visit you." We arranged a date in January for me to visit, and I was thinking well, I'll go up and get to see Caltech, which I had always wanted to visit. But that very week my adviser told me that he didn't have money to keep me on any longer as a postdoc, and that was unexpected because he kept trying to tell me that he wanted me for a long term.

I went up to Caltech with a new mission in mind, that of getting a job, and the *Red Rover* book describes how I walked into Don Burnett's office and we sat down and he said, "Well, basically you can have the job." Because he knew me already. We had already talked this over a bunch, several years earlier, and I guess he thought I was the person for the job.

I, not having a job anymore, said, "I'll take it." That was the shortest interview ever, I think. Then we talked for a couple more hours and he showed me his labs and we talked about what he had in mind for the work. I should back up a little bit and say that Professor Burnett had been working on this for a number of years in the background, but basically without funding, and so I think he had gotten together with Marcia Neugebauer back in the mid 1980s. They had put some ideas together. Of course this idea of collecting solar wind to study the Sun's composition isotopically came from the Apollo Solar Wind [Composition] foil window shade experiments where the astronauts had deployed [ultra-pure] aluminum foil from a roll during a number of the Apollo missions and brought it back, and a number of discoveries were made based on that.

By the way, Don Burnett had been the PI [principal investigator] of another experiment with the Apollo astronauts on one of the lunar missions, and ever since then he was thinking we should get a better solar wind measurement, so he started putting this together in the 1980s. At one point he had—I don't know if it was a summer student or a master's student, but it was a short-term student—do some work on feasibility. He then put together a full proposal that, I think, was submitted in something like 1986 or '87, was not selected for funding, but in 1988, '89 when he submitted again it was funded. That was when he came to me and said, "You should take this job." That was the background.

JOHNSON: Was the funding from JPL or Caltech to do the study?

WIENS: There was a relatively new program by NASA that at least in its 1990s incarnation and up through the early 2000s was called Planetary Instrument Definition and Development Program. It is now called PICASSO [Planetary Instrument Concepts for the Advancement of Solar System Observations], so it's for instrument concepts for NASA. That was known as

PIDDP. He was in on the early stages of that and so he got funded in one of the first rounds of that program.

JOHNSON: How far along was he in this idea, the sample return mission? How was that different from other things? Like you said, they had done some of this with Apollo, but this wasn't going to be humans taking a capsule out there and doing something. It was robotic. Talk about how different this was than other missions that were being proposed and talked about at that time.

WIENS: Yes, Sandra, it was really very different, and that's why I was turned off to it originally, because it was not in NASA's deck of cards at that time. NASA was doing big missions. The Voyagers[1], the Mars Observers[2], the Cassini[3], Galileo[4]. All of these missions were the type of mission that NASA did. Those were multibillion-dollar missions, never came back to Earth, and didn't have a component that would ever come back to Earth. They were not thinking along those lines and for robotic exploration, and so this didn't seem to have a future, and that's why I avoided this job originally. Our original work on it was really at the feasibility level.

We knew NASA could bring something back to Earth but the real feasibility test had to do with the fact that the solar wind is so rarefied. It's only a few atoms per cubic centimeter out in space, and it's flying at about 300 kilometers per second, so you can collect a few atoms per

[1] Voyager 1 and its twin Voyager 2 are the only spacecraft ever to operate outside the heliosphere. Voyager 1 reached the interstellar boundary in 2012, while Voyager 2 (traveling slower and in a different direction than its twin) reached it in 2018.
[2] Missions to Mars devoted to orbiting the planet, mapping, and studying the geology, geophysics, and climate.
[3] Cassini was a sophisticated robotic spacecraft sent to study Saturn and its complex system of rings and moons in unprecedented detail.
[4] Galileo was the first spacecraft to orbit an outer planet and changed the way we look at our solar system. When the spacecraft plunged into Jupiter's atmosphere on Sept. 21, 2003, it was being deliberately destroyed to protect one of its own discoveries—a possible ocean beneath the icy crust of the moon Europa.

square centimeter per second basically. It would require a long collection. It's best if it's done in the vicinity of Earth or closer to the Sun, not something that's going to the outer solar system.

Then what we were really looking at was, do we have pure enough materials on Earth to get this solar wind, embed it in some material, and bring it back to Earth and still be able to do the measurement?

The initial work was really looking at materials like silicon wafers, germanium wafers, and other very pure materials produced on Earth that could potentially act as collectors for this material. And then how much signal to noise would we get in some kind of collection, and do we have the techniques to actually make the analyses once we collect it?

We were looking at SIMS, Secondary Ion Mass Spectrometry—the instruments are known as ion probes—to understand if they had the sensitivity to make the measurements of this rarefied material and how long a mission we would need.

It was really only after Dan [Daniel S.] Goldin and the sea change of NASA towards faster, better, cheaper, and smaller missions that we started really talking with people about bringing this back to Earth in a realistic way. Before that it was well, maybe there'll be a Mars mission that we could piggyback, and somehow send something back. But it just wasn't realistic.

That brings us up to 1992 and then into '93 with the advent of the Discovery mission born into the faster, better, cheaper philosophy of missions.

JOHNSON: Talk about that for a little bit, because like you said, Dan Goldin, when he became [NASA] Administrator, that was his push, to start doing smaller missions for less money and get more going, because at that point missions were like you said, really big and far apart. He wanted it to move quicker and for less money. But explain how that affected the scientific community. Was there a lot of excitement about that idea, and when they announced the Discovery Program, that they were going to start moving that way?

WIENS: I think there was a lot of churn, as there is when you have a major change with a leader of a major government organization. Dan had enough wits to be in there long-term, but he was making a fairly major change. People saw the need for it but it was also in a sense fairly difficult or ominous perhaps to people at some of the NASA Centers who had been in these really big missions up until that time, because this was like, well, where are we going to get our bread-and-butter funding? I saw working with JPL at that time some of that concern and fear.

But on the other hand it was extremely invigorating for the academic community, for scientists in general, because there had been a big sense of frustration with the vicious cycle where you'd get a mission suggested, then NASA would get the committees going that would define it, and then it got too big, and then some descoping or cancellation had to occur.

We were in the early stages of Cassini at this point. Comet Rendezvous Asteroid Flyby was also a big planned mission at that time. That one ultimately didn't make it and was subsumed with later Discovery missions aimed at comet and asteroid exploration.

Then what happened after Dan Goldin's start in office was that Mars Observer, which was a $1 billion mission at that time, got to the Cape [Canaveral, Florida] and got launched in late 1992 within his first half year of office. But within the next nine months it was lost. This really accelerated the recognition of a need for a different philosophy towards missions, because if you lose a big mission like that—and at a similar time Galileo was found to have the antenna that never deployed, and another big loss because we did not get nearly as much imaging from the Jovian system as we'd hoped—so both of these were really fodder for this whole idea that we needed to put less eggs in one basket and get more missions out there.

JOHNSON: Part of the idea was acceptable risk because of the lower cost.

WIENS: Very much so.

JOHNSON: Let's talk about that time period then after it was announced. All these other missions were going on, but did your team immediately start thinking okay, now we can move forward, this is exactly what we've been waiting for? Did you go beyond feasibility? I'm thinking of that time period like in November 1992 when they had that meeting at San Juan Capistrano [California]. Talk about that timing and that period and how you were moving forward for Genesis.

WIENS: Yes. Dan Goldin, as part of his new ideas, decided to convene a meeting out in California at San Juan Capistrano to get people who were interested in Discovery-class mission concepts to give their pitch for their concept. There would be a sort of incubator program where $100,000 would be awarded to 10 of the top contestants in this presentation in this meeting.

This was way oversubscribed. I think it showed how the interest was in the community in going to more rapid and smaller missions. Not everybody who was there understood the concept. They still wanted to come in with a lot of big instrumentation because that had been the tradition, and it's hard to give up and make missions so focused as this new idea was, so we saw some big missions or at least medium size missions that wouldn't fit in the cost cap proposed as well.

But there were I believe something like 110 proposals or ideas that were presented there. To me, I was sort of new in this business, but it struck me absolutely as a beauty contest. I had never seen live presentations of mission concepts before in this way, and this relatively small venue was absolutely just humming with people that were spilling out of every side of this building. It was a very interesting time. We gave our presentation there, Don Burnett did. We didn't have a lot of interest before that by places like JPL just because they were trying to support so many different ideas. Probably half of those 110 at that point.

There was promised to be a rapid feedback, a rapid turnaround. The event was in November. I think the feedback came back by the end of December. Remember that this is in an era where there was no Internet, and so communications traveled by phone, by letters, and by fax. In late December or mid December, I can't remember exactly when it was, we got the results back from this San Juan Capistrano meeting, and the results that we had for our concept, which was at that point called Solar Wind Sample Return, or SWSR, was that other missions that were already in the planning phase would accomplish everything that we wanted to do.

This was a very frustrating thing, because we knew that wasn't the case, and so the judges of the beauty contest hadn't quite realized what the other missions could or could not do versus what we would plan to do. The other part of that blow was that we had heard that the PIDDP office that was funding new instrument concepts was going to base their next selection very strongly on this San Juan Capistrano result. If we were judged to be obsolete, we would get a low rating in our next PIDDP proposal, which had just been submitted. So the writing was on the wall for me. My job was going to be over. This was not exciting because it was going to be the second time in about two years.

Don Burnett did something that he almost never did. He decided to write a letter to the head of this Discovery Program and tell them that the judges had been wrong. I smile at his audacity because this is often done and not usually paid attention to. But he wrote a careful letter and just pointed out the different missions and what they could or could not do. He knew this well from talking with Marcia Neugebauer, who was in the space physics community, who knew what some of the in-situ spacecraft could do versus the sample return mission. We had been studying that topic.

In January we got a letter back from the Discovery concept office which said, "We did reconsider your mission concept, and you are right to say that these other missions would not cover this science of the isotopic and some of the elemental compositions of the Sun." Next, we received notification from the Discovery mission office that we were one of not ten mission concepts that they were going to pick, but we were one of eleven. We knew we were the eleventh one!

JOHNSON: You received the funding, the 100K, at that point?

WIENS: We received the 100K. Not only that, but we then received a notification saying we were the top-rated PIDDP proposal concept. Now we got full funding from PIDDP as well as the 100K from the Discovery mission concept.

JOHNSON: How did that change what you were doing then? You had this influx of money from both NASA and Caltech. How did that change? Did you add team members at that point? Did the work actually go into other than doing those studies early on. What changed about what you were doing?

WIENS: That put the wind in our sails for actually finding a mission that could do this because before that we were really focused only on the scientific feasibility, and now we were noticed by the NASA Centers. We were recognized. There was an important pedigree for these 11. JPL then contacted Don Burnett and said, "We have money internally to spend for what we think are going to be the top possibilities for the Discovery mission proposals." At that point in time the actual mission proposal timeline was announced and so we now had dates to work toward.

We had one year for the mission concept, and we would have to give a result of our mission concept in one year with that 100K. But that was then supplemented by probably several $100K of internal money at JPL. Then we also had a little bit beyond one year; we had the 1994 deadline for the mission proposals. So it was a big change.

Now we had a number of people coming to our building at Caltech and wanting to talk to us about "What are you going to do?" "How are you going to do it?" and "How can our department at JPL make it happen?" We started working with several people from the Mechanisms Division at JPL who had several different concepts for how to deploy materials to be exposed to the solar wind and then get them back into a capsule that would return to Earth.

Some of the ideas were a little bit crazy and some of them were really good. We locked onto one with Don [Donald R.] Sevilla that would deploy by rotating panels out of a capsule and then rotating them back to get into a capsule with a clamshell design, with a cover. We really started going with that, and it became the overall concept for the Genesis mission. A number of things were clarified, such as how we would mount the silicon wafers. We started talking about a solar wind concentrator that would focus a small amount of the solar wind into a very small target where we could really analyze oxygen isotopes, which was the highest priority of the mission but a very challenging one. I started working intensively on the solar wind concentrator concept. Then we started looking for partners who would work with us on the electrostatic instruments to monitor the solar wind and reached out to Los Alamos National Laboratory for that part. We were looking for a spacecraft provider and Don and I had a meeting in 1993, I think it was a DPS meeting, a Division of Planetary Sciences meeting, in Colorado. We then took a short side trip down to Lockheed Martin, south of Denver, and visited their astronautics department, and started meeting with Ben [Benton C.] Clark and with Nik [Nicholas R.] Smith and a few other of the people who became the backbone of the Genesis mission there. Then we started pulling it together for our first major proposal to Discovery.

JOHNSON: Let's talk about that. First of all I was going to ask when did it become Genesis, because that wasn't originally what you were calling it. When and how did that name come about?

WIENS: Yes. Originally, I think in the San Juan Capistrano it was just SWSR for Solar Wind Sample Return. That maybe was inspired by the SWF of the Solar Wind Foil from Apollo. But we needed a better name. Don Burnett wanted to name it after Nobel Prize winners in science. The people who had really brought together cosmochemistry as a discipline looking way back in the 1940s were Harold [C.] Urey and Hans [E.] Suess. Don wanted to name this mission the

Suess-Urey mission, and that was the name that we had for our first proposal. Suess-Urey was proposed in 1994 as a Discovery mission concept.

JOHNSON: For that first announcement of opportunity [AO] that's the name you went with. When did Genesis come about?

WIENS: Genesis came in the second round. When we did not make it in the first round—and there are more details there. But the only area where we scored lower than the top mission was in Outreach—so we needed a catchier name.

JOHNSON: Let's talk about that period.

WIENS: Among many other things we changed the name, yes.

JOHNSON: To help with that outreach?

WIENS: Yes.

JOHNSON: Like you said, you partnered with Lockheed Martin for the first AO, and also Los Alamos, but talk about that first presentation and how you got ready for that first presentation. Had you started to make any prototypes or was everything still on paper at that point for instruments or the capsule that you were talking about and the different ways it was going to work? Talk about that and actually communicating what you wanted to do for that presentation.

WIENS: Yes. Sandra, first to go back a little bit to the actual proposal, I just wanted to remind that this was not at all electronic. This was paper and pen and artist sketch pad because nothing was really done by computer except for the words at that point—the text. We would leave big boxes in the text where the figures would go. The figures would actually get pasted in before we did the xeroxing. Then the xeroxing all happened. We made the 20 some copies, put it in a big box, and that whole box was shipped by airfreight to Washington, DC. The final day we went until 3:00 a.m. But yes, we got that off and then got back to our lives.

Then we heard back several months later. We were just astonished that the Genesis mission was one of the three Phase A contestants. The crazy thing was that Caltech was known for its connection with JPL, and the Planetary Science section of Caltech's Geological Sciences Division was fully expecting that their missions would be in there. None of the professors in that Section actually made it, and there was some humble pie that was eaten because Don Burnett and I were in a different building at Caltech and we were the ones that were in this final round with two other contestants, those being Stardust and a Venus orbiter mission.

Then we were thrust into Phase A. By the way I think this is a really great way to do it—we saw then how good this was for selecting missions because you just need to have more maturity to the concepts to understand their feasibility and the relative scientific merits. It had been all on paper up to then.

Then in Phase A a lot more internal money at JPL and the $500,000 of Phase A were thrown into this and our lives took a big turn. We became so wrapped up in trying to win this competition. A consoling thought was that if we were one of the top three, and Discovery missions would happen every year, as they were suggested at that time, then we would probably have a good chance within the next several years. So it really changed our prospects in a huge way. We went from one of eleven to one of three now. Just continuing to narrow down.

Now a lot of hardware was being made in prototype. The designs were coming together. We added new partners. We gained Eileen [K.] Stansbery at Johnson Space Center for sample curation, and others. The scientific team started coming together to show how you could make the measurements—all of these things. We had a number of meetings in person at Lockheed Martin, at JPL, and so on.

There were two parts to this Phase A. One was a mission description document which was several hundred pages. Then a live presentation which was to the review panel. The mission document was due first. We got that out. Then we had another month or six weeks to prepare for live presentation.

This was a new idea—to get engineers and scientists together to do a live presentation. The first rehearsal was a full day or even day and a half, I'm not sure, but it was really poorly done, I'll just have to say, because we had engineers who would get up, and scientists—people who were not necessarily used to giving presentations. They would mumble through their part of the design. We could see this was not something that was going to make a big impact on a review panel.

So we got people who came in and really spruced it up more as a major news network presentation kind of thing. The beauty and pizzazz of the presentation started really taking an important role.

JOHNSON: I was reading Don Burnett's oral history that he did with Susan Niebur and in it he said Genesis was very mechanics-intensive, and mechanisms always scare review panels. I thought that was interesting because it was. You were doing science but as you explained the capsule, the top had to open and these wafers had to come out one at a time, right?

WIENS: Yes.

JOHNSON: Depending on what you were trying to capture. They had to move out and move back in. So it was pretty mechanical for that type of mission.

WIENS: Yes. More so than many other missions before with the exception of a very few things but yes.

JOHNSON: When you were working on all of that and you had a cap of $150 million, how did you figure out this would be under $150 million? How did you make sure that you wouldn't go over that amount for the proposal?

WIENS: Plus a launch vehicle. It was in a sense a rude awakening for the big mission people. It was just like we're going to have to really work to slim this down and not have a big army of people working on it. That was what people had started to see when Dan Goldin first put out these concepts and they realized the era of the huge mission was over.

There were teams of people, but we had to realize that somehow it was going to be skinnied down and we had to put together this budget for a NASA Center that would fit within it. I think some of the things that JPL started to do was maybe push a little bit more work out of house for some mechanisms and a few things like that. It really did become more of an industry and NASA Center partnership. Not only with Lockheed Martin building the spacecraft but with mechanisms and so on even in JPL. Lockheed Martin, I think, had definite concerns how they would do it as well. I won't necessarily speak too much to how that would work, but I think also some realism happened after the selection of the missions because then all of a sudden people wanted to grab all of the cost margin that we'd been required to show. We had to say, "No. We have to hold certain amounts of margin in reserve for certain parts of the mission."

There were things that started looking like they would get cut even after selection to be able to fit in. Some of it was more risk mitigation. But yes, it was tough.

JOHNSON: I can imagine. What were you doing specifically? Building up to that first time, Discovery, the proposal. What were you doing specifically for the team?

WIENS: Yes. I started out working the science feasibility side with Don Burnett. Then when JPL became involved after we won the eleventh slot in the concept phase, then all these engineering designs started showing up for these collector concepts, and for the mechanisms. I realized that I had more ability to see how the mechanical designs came together than Don seemed to have. Plus of course Don was a professor and had other duties and other projects. I was full-time on this because I didn't cost very much. I was the cheapest person on the whole project for a long time. I became the interface between the engineers and Don, and also with the other scientists. Interpreting engineering drawings: What would this mean? How would it deploy? I started explaining things, and then trying to advise on what kind of mechanisms would be better, what risks would be inherent in different designs. That became my bread and butter along with the science feasibility.

I really enjoyed that and it basically became my career because now I've designed and developed several other instruments, working directly with the engineers.

JOHNSON: That was one of my questions I always like to ask people, because scientists and engineers talk a different language. That communication sometimes needs someone like you in between saying, "Okay, this is what we need," to either side. Talk about that for a minute, the importance of communication in this kind of work, especially planning for a spaceflight or this type of mission. Also the teams were scattered around. They weren't all in the same location. As you said, things were a little different in early '90s as far as communication at that time. But talk about how important that communication, being able to talk to each other, for the teams to communicate, and how you did it at that point.

WIENS: Yes. It was an era of rapid change or at least accelerating change. The early '90s it wasn't there yet. I can't remember the date of my first email—actually I printed out my first emails so that I would have a record of them, because they were not at my computer. They were at some server that was in the basement. I heard about this idea and eventually I ended up getting an email address and going down there. We could share time with the server with other people who were also sending and receiving emails.

My brother is in academia, so we started communicating that way. Then very soon started getting in touch with others. I don't know when Lockheed Martin came online with email. But at least the scientists: we could start communicating with them at educational institutions.

But it was in ’92 and ’93. I think by ’94 I had quite a few emails that I was—I put my fingers like this because that’s getting to be how thick the pages were that I was printing out. But then becoming a bit of a mouthpiece because Don Burnett didn’t have the time to run down there and do all of that. He would talk with people on the phone or maybe by fax.

It was when we got our first PC in Don’s office, and then eventually I got my own, but that’s several years later, but yes, we would sit at that PC and share time and do email then and start to get everyone’s email address. That was in the mid ’90s. Communication was difficult.

As I said, we lost the first round, and so then it was the second round that we started going for—the 1996 to 1997 era. By that time email had started to take over in many places. By the time we really had our second Phase A in late 1997, we were doing a lot by email and things were getting more sophisticated for communication.

JOHNSON: That first one when it didn’t get picked, and you mentioned that it was because of the outreach, did you know what they were talking about, other than obviously you changed the name? But when that came, talk about how that affected the team at that point. The funding and everything else that was happening at that point.

WIENS: It dropped back to threadbare again. Don and I had funding through the PIDDP still and I was still full-time. I became the one person again, the only person again, who was funded on the mission, for a little while. Because we were one of the top three, we knew that we would go into the next round and were possibly favored. Firouz [M.] Naderi had been the project manager the first time around. He moved on. But there were a few details of the first round that were interesting. It wasn’t like we just lost because of outreach. But part of the issue was we needed a bit stronger of a story for our risk, and perhaps a little bit better in terms of the organization, more maturity on some things.

We worked on all of those areas. But in our first competition, I remember this fairly well; we had one very strong personality who was there. More than one of course, but one that stood out. That was Jim [James S.] Martin. Jim was the project manager of Viking. He was the one who told President Gerald [R.] Ford that he didn't have time to talk to him on the phone. He was quite a cranky character in a sense, but also really good in many senses. JPL has certain other people now, who I know, who fill these kind of roles. They are very outspoken, they know their engineering well, and so people listen to them.

Jim Martin was on the review panel, because he wasn't affiliated, he was now retired. People listened to him. When we started talking about the reentry of the Genesis capsule, he started getting interested in the weather conditions. We had a backup plan where if the weather was really bad in Utah at the time we were supposed to enter, we could divert and come back several weeks later. But he actually blew this thing out of proportion because the weather in the Dugway Proving Ground is quite predictable that time of year; to have a morning storm—we were planning to come in in the morning—is very unusual. Afternoon storms yes.

He was actually pounding his fist on the table at the end and saying, "I want to know what happens if you have a storm on the second time you're trying to come back." We didn't have an answer to that. We were not scored very low on that, but it made an impression on people, and it was just enough to put us kind of on a tie level with a mission that wasn't quite as delicate on its reentry, although in the end we know that Stardust actually parachuted to the ground and Genesis fell to the ground. But things like Jim's rant about the weather made an impression. We were just really tied with Stardust. I think also the politics of losing the CRAF mission—the Comet Rendezvous and Asteroid Flyby—they had it coming. We needed a comet mission. We had an asteroid mission with one of the first two Discovery missions that were hardwired (NEAR and Mars Pathfinder). So NASA needed this comet mission; Stardust was picked and we weren't.

JOHNSON: I was reading that part in your book where they were asking you about the weather and he was getting very excited about it. I was thinking about the helicopters and I think I read that the military had been doing it that way, that retrieval, when the capsule came in and the parachute. The military knew how to do that, but it was unusual. Did that throw people off too, just the way the capsule was coming in?

WIENS: In terms of the concept, the plans? Yes. When I first heard of it, I thought oh, this is crazy. It was the first of many supposedly crazy ideas that NASA and Jet Propulsion Laboratory and the robotic missions have pulled off. Some of the crazy ones also being the landing of the Curiosity rover with the sky crane, which has now been done twice, and the idea that the Mars Sample Return is going to throw their MAV [Mars Ascent Vehicle] up in the air before it actually ignites and goes up into space from Mars.

There are crazy ideas out there but many of them actually have a history. We were told that the parachute and helicopter snag had a lot of history, and some of that became declassified in the 1990s, and so suddenly we had a lot more information about what Lockheed Martin and some other organizations knew all along. I actually remember making my first call to some people in another Lockheed facility in the Washington area who knew much more about this back when we first started planning it. It was originally viewed as crazy but we got so much information eventually that it had been standard fare in the military, and we just didn't know it.

JOHNSON: After the first one you came in third, and you were preparing for the next one. But that's during the time period you moved to Los Alamos, isn't it? How did that come about?

WIENS: Yes. We had originally, I think, in the 1993 timeframe started talking with Los Alamos National Laboratory about providing the solar wind monitors, and then eventually also building the solar wind concentrator that Gensis used.

There were three different instruments that Los Alamos was going to be in charge of. They started to have a fairly significant role in this. Actually one person at Los Alamos knew Marcia Neugebauer fairly well, Marcia was basically the second person in charge of Genesis besides Don Burnett and myself. She originally recommended Los Alamos, so I got to know the folks at Los Alamos through proposal meetings and through the oral presentation itself. They got to know me as Don Burnett's sidekick who was helping all of the communication happen between engineers and scientists and working all the details underneath Don Burnett.

At one point if Genesis hadn't been selected on the round it was in, there was even a possibility that I would be the person to run it next time. They looked favorably on me. John [L.] Phillips had just left Los Alamos to become an astronaut, and so they had an empty desk and I applied to fill it. I talked to Don Burnett. The Caltech position was a research scientist but it was soft money, and so he encouraged me to go to Los Alamos with the idea that I could actually help run the development of the instruments that were there, and be just as involved as I had been.

I did that. I took the job right after we submitted the second-round proposal. Then it was a number of months after I started at Los Alamos before we were selected for Phase A again, and then I was once again running up to Lockheed Martin and flying to Pasadena to be in all the next proposal meetings, now as the Los Alamos representative. At the same time as Genesis was selected, there was a space physics mission that was selected that occupied Dave [David J.] McComas a lot, and I became the lead over all of the instruments that Los Alamos was providing for Genesis even though I was fairly new there.

JOHNSON: Talk about that for a minute, the difference in working at a place like Los Alamos compared to working at Caltech and being close to JPL. Just some of the differences of working for that lab.

WIENS: Yes. It went from where I was relatively inexpensive as a research scientist at Caltech to a staff position at Los Alamos, which has quite high overhead as do many national labs and centers. Then I started to realize, well, we have to be quite efficient with our time there and get the best people, but a good mix of senior people and junior so that we have a good cost mix. But yes, the people around me were very interested in the mission because it was a great opportunity. I had a lot of people interested in coming together to work on it.

We really were doing it with a faster, better, cheaper mentality. I was going to the hardware store literally to look at things that we could use at least in a sort of analogue sense to develop prototypes. We went out to the local sportsmen's club firing range to shoot bullets through the grid mesh that we used in the solar wind concentrator to see what would happen if a micrometeorite went through it. Would it unravel completely or would it just make a small hole?

We got our answers that way, and we almost had to, because the gas gun facilities at some of the major NASA Centers would take months to get in the queue and then do the training to work there, and we just didn't have that time in this faster, better, cheaper era. We made do with what we could find or work out immediately. It was a time when we tried to fit into the mold and we found ways to do it.

JOHNSON: You mentioned the Mars Polar Lander and then the Mars Climate Orbiter both affected this whole concept. But I think that's when the red teams were initiated.

WIENS: That's right.

JOHNSON: Talk about that. Did they review what was going on one time or was this multiple reviews that they had to do as you were working toward getting this project off the ground and launched?

WIENS: Yes. We went from the loss of Mars Observer to then the era of faster, better, cheaper. But then in '98 we lost both Mars Polar Lander and Mars Climate Orbiter, and there was a new idea that maybe we're going to lose multiple missions due to faster, better, cheaper—not just one here or there. There was this realization that faster, better, cheaper had gone too far in the other direction and there needed to be a bit of a course correction.

For the missions in work there was a need to review them better to understand if we could have missed something. We have the usual Preliminary Design Review, Critical Design Review, ATLO [Assembly, Test, and Launch Operations] Readiness Review, and then the

Launch Readiness Review, and all of those. But now we had to do another set of reviews, and now it was kind of like, "Oh my gosh, we got to spend more time in reviews!" But because of the losses we recognized the need.

We were looking at, "Well, what's the big risk? What could really go wrong? What would be the worst thing?" There was a decision to hold the reviews in several different places, I think because of the desire to get them done quite rapidly and not to interfere too much with the work in this still rapid timescale that we had.

I went. I don't remember if I went to JPL for some of the reviews. I think I did. But I definitely remember driving up to Denver and going to some of the reviews there. The reviews there had to do with the spacecraft and the navigation and the reentry. I remember being in the reentry review and there was a review chairperson for that review who was from the navigation community. He was very focused on getting this capsule into the "keyhole" where it's not entering at too steep of an angle, not entering at too shallow of an angle. Because how many capsules had entered Earth by NASA at that time? It was very few. None in the last umpteen years. He had reason to be very focused on that. But that review panel totally missed the hardware aspect of the accelerometers, and as we all know, the accelerometers were installed backwards. They were backwards in the design and backwards in implementation.

Thank God that the Stardust design, which actually went before us, was correct. But it was somehow turned upside down in the Genesis design. It was missed entirely in the testing, in the reviews, including the red team review, and so we didn't have a clue about that until the landing.

JOHNSON: It's interesting that like you said Stardust was correct and something was inverted for Genesis.

WIENS: Yes.

JOHNSON: I know that first scheduled launch date was January 2021. How was that date arrived at? Part of this faster, better, cheaper was the faster part. Talk about working toward a goal that quickly for this type of mission.

WIENS: Yes. We had 31 months to design, develop, and build stuff that had never been built. This was an absolute race from the beginning. Many times in an instrument and mission development there's some new information, and there's sometimes a question: "Are we on the right track? Do we need to go back and actually reverse course and do something over?" When we would get those kinds of questions in Genesis I'd pull at my hair and just say, "My God, we don't have time to do this kind of stuff."

Those are good, because there are people that come in and say, "Are you really doing this correctly?" We had that sometimes. For example, we were building the solar wind concentrator and somebody said, "But what if your one unit fails? Maybe you should have three of them."

I responded, "We can't fit three of them in the capsule, and they won't actually fulfill the purpose—each will be too small if we make three of them." Things like that. We had to go back and rethink these and maybe even make a design for three and show that it wouldn't work. Those were the things that we really had not enough time or patience for. It was just crazy.

The other thing was that if you have money, you can do things really rapidly. If you have time, you can do things more cheaply. But we were asked to do both—to do it on low budget and fast. Which is harder.

It was a learning experience for me because I had not been building space hardware before. I'd been looking over shoulders to do that. But we had a good manager in some senses. For example we said, "Well, we don't have time to build this thing, do these tests, and then build the flight unit."

He said, "Let's build several units at the same time and do different tests simultaneously." We did that for the less expensive aspects. Some of our hardware at Los Alamos was that way—it was not expensive—part heavy. That was the case with the solar wind concentrator, which was almost all mechanical and electrostatic. We could build the mechanics. We could build several of those, put them through vibration tests, shock tests—all of those things simultaneously—and we did that. That was one way to try to get faster, better, cheaper to work.

I was very intrigued by the way these things were done, and I learned some really good management skills very fast that way. But then there was there was another launch failure on a different type of mission. Not so big to planetary science, but it again caught people's attention within the sort of six-months-before-launch time period. NASA decided that we needed another several months for Genesis, to make sure that everything heading towards the launch was going to be safe, and so they moved our launch by another number of months. They gave us some extra funding to make sure that as a result of faster, better, cheaper we weren't compromising the launch and deployment somehow.

JOHNSON: I know you have another meeting coming up. Do you want to stop for today? If you can find some time for me in the next few weeks, we can pick up for another interview.

WIENS: Yes, I can do that. If you have more to ask.

JOHNSON: Yes, I do. I always have questions. But yes, I don't want to make you late for your next one, and if you need to do anything to prepare for that.

WIENS: Okay, and you said you have other meetings too.

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

[End of interview]