NASA Johnson Space Center Oral History Project
Edited Oral History 2 Transcript

Dr. Gary Lofgren
Interviewed by Jennifer Ross-Nazzal
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Ross-Nazzal: Today is June 10, 2009. This interview with Dr. Gary Lofgren is being conducted for the Johnson Space Center Oral History Project in Houston. Jennifer Ross-Nazzal is the interviewer, and she is assisted by Sandra Johnson. Thanks for joining us again. Sorry about the little technical snafu there. That happens sometimes.

Lofgren: You're welcome.

Ross-Nazzal: We did talk about crew training last time, but I thought I'd ask you about the flight controllers who went with you on those field trips, if you could add some of that detail on tape.

Lofgren: Yes. The flight controllers were on the trip to the Coso Hills, which is a volcanic terrain in the east of the Sierra Nevada Mountains, just north of the Mojave Desert. So it's in very eastern California along the Nevada border, sort of about two-thirds of the way down the state from the north end. I can't describe it better than that. It was a neat area, and it was on a Navy base or Air Force base. I'm not sure which. China Lake is the name of the base [officially called the Naval Air Warfare Center Weapons Division in China Lake and Point Mugu]. Coso Hills are the local name for the area where we had the exercise, but it's the China Lake Base.

This was getting late in the training. This was April of '71; the launch was in July. This was the last big exercise that we were going to have, short of a full-op simulation which was run out of Flagstaff [Arizona], where the Houston Mission Control was involved in talking directly with the crew from Mission Control in Houston—which was a far bigger operation than we ever ran for the routine training. But they did this out in Flagstaff kind of as the last exercise before the launch. So the crew was very well trained by this time, and the idea was to invite some of the people who were going to be in the back room to watch what the guys were going to actually be doing. Rocco Petrone was there from [NASA] Headquarters [Washington, DC]. He was kind of in charge of the day-to-day operations of Apollo from Headquarters [as Apollo Program Director].

There was two or three other people who kind of came along with those guys, so I can't remember exactly. Those were the three names that I remember, and there were two or three other people there. I might be able to get those names out of a history that was written of the crew training by Bill [William C.] Phinney. He's got a document that I could maybe look that up in. I forgot about that. I could have looked that up, but I still can. They walked with the crew in the field or sat in the back room. We had a couple of days of exercises, so they could trade off. One EVA [Extravehicular Activity] they could go in the field with the crew and see how they performed, what they did. The other day they could sit in the back room where Joe [Joseph P.] Allen, the CapCom [Capsule Communicator], would sit, and talk to the crew with a couple of geologists simulating a back room operation. They could sit and listen to it or watch it from that side. So they had a chance to really see what the crew was doing, how they collect samples, how they interact with the CapCom, and just watching the guys perform, how well they do. They were very proficient at collecting samples by that time, and they were very good at describing what they were seeing by that time.

This is probably the only time that these flight controllers would ever actually see how these guys were going to be functioning, because this kind of work did not happen at JSC or at the Cape [Canaveral, Florida]. This was only done on our training exercises. I mean, they had exercises where they deployed instruments that were going to be deployed on a surface, but they never did traverse-type operations at JSC. So this was their really only chance to kind of see what the crew would actually be doing, what kinds of operations they would be doing. They really enjoyed it. Gerry Griffin gets very enthusiastic about his opportunity to go out and see that.

We had a meeting here early in April of last year where we brought in some of the old Apollo guys who did training, and sat around a table and talked about what we did. Gerry was there, Alan Bean was there, John [W.] Young was there, Dave [David R. Scott] was supposed to come but he didn't, and there were several of the crew trainer guys there, a lot of the retired fellows were there. Gordy [Gordon A.] Swann, Bill [William R.] Muehlberger, George [E.] Ulrich were at this meeting. We talked about that. Gerry was talking really enthusiastically about how he got the chance to do that, and how much it meant to him and how much it was important, and certainly advocated that that happen in the future for all flight controllers that would be involved in any kind of new operations on the lunar surface. It was just a valuable experience for him. That pretty well covers the topic. Do you have any other questions about it?

ROSS-NAZZAL: Why did Gerry Griffin say that it was invaluable? What was beneficial about that?

LOFGREN: He just really had no concept before he went there. He didn't have a clue what the crew was actually going to be doing when they were out doing geology. He didn't even tell me what his idea was, but he said it was so wrong that he couldn't believe it. I mean, actually watching what they were doing and seeing, he said, "What I imagined they would be doing was just so far wrong I don't even know how to describe how far wrong it was," he said. He really then understood what they were going to be doing.

They saw them on TV clearly during the mission, but to actually see the operation and to watch them do this and watch them get it—we had a kind of rover there for them to work with, and it wasn't a high-fidelity simulated rover, but it worked. I forget which one. We might have actually had that USGS [United States Geological Survey] one at that one, too. This rover, they called the Grover. We had it at Taos [New Mexico], and I think we did have it at Coso Hills as well. It wasn't bad. It was a reasonably good simulation of the actual rover, obviously made to operate on Earth rather than on the Moon, but as far as traveling somewhere, getting off, collecting samples, describing stuff, getting back on, and driving to the next place, it worked very well, which is really all you needed to simulate the rover part.

He said he just didn't understand what collecting samples was like. He had a much better concept of what they were going to be talking about all the time. He got to hear them talking. They actually would only hear them if they were in the back room, unless you were awfully close to them in the field, and it's hard to get that close because outside, the voice doesn't carry all that well, so unless you're right on their tail, you don't hear what they're saying. So that was the advantage of sitting in the back room, because you really heard the conversation back and forth better than you would walking with them. But when you're walking with them, you actually see physically what they do. Both aspects of that were important to do.

ROSS-NAZZAL: As we were closing out last time, in our last session, you mentioned that you were the Apollo 12 test director.

LOFGREN: Yes. We had several different operations in the LRL [Lunar Receiving Lab], and one of the operations was what we referred to as the SNAP line. I think it was Sterile Nitrogen [Atmospheric] Processing, but it was a room in which most of the science was done, the first look at the samples. This was a room with one long nitrogen cabinet in the center of the room, and it had somewhere between six and eight gloved working stations, where you had the gloves and you would have a microscope where you could look at the sample really closely then do descriptions. That's where the scientists would sit down—they didn't sit down, they actually stood—but they would do their systematic description of a sample.

Different guys did it differently. They would dictate into a tape, a little tape recorder they had, or they would hand-write out notes or whatever. But ultimately, they would go to a data terminal and type it in. Now, a data terminal in those days isn't anything like a data terminal today. This was a teletype machine that typed at ten characters per second. I don't know if you've ever seen one of those little teletype machines. When it starts typing out a message, the machine actually kind of bounces on the floor. It's pretty incredible, but those were computers in those days. But it worked. They would type in their descriptions, and we made catalogues with that.

The point of this operation was the rocks would come in there, scientists would be assigned a rock to describe, and he would probably spend as much as an hour or maybe even two hours studying the sample, writing his description. He had a list of specific characteristics he was supposed to comment on so that there was some similarity between the descriptions. Then after they did that formal part, then they could say anything they want, but we did ask them to do some specific kinds of observations. How tough was the rock? Did it have any fractures in it? What was the shape? Some standard descriptive things that we wanted to have to compare one rock to another, but after that they could say pretty much what they wanted. They could speculate on what they thought it was or how it was formed or whatever. They were free to say anything basically that came in their mind, and they did. Some of the descriptions are kind of interesting, especially when you go back and find out what the rocks really are later, but they did a good job.

This was an important part of the description because this formed a catalogue that was then published to the science community, and that's how the scientists knew what samples to ask for. This was the description that they would go into this catalogue, and if they wanted to study a basaltic rock or they wanted to study a breccia from the Moon, they could go to this catalogue and find out which sample was the one or two or three samples they would like to request for study. This is how they did that, so it was important to get this catalogue out. We tried to do that within about two months of when the samples came back.

This was a very intense period where people were working incredibly hard. It wasn't uncommon to work twelve, fourteen-hour days when we were doing this. On Apollo 12, we were behind the quarantine barrier—quarantine was still in effect—so you had to shower in, shower out. When you went into the lab, you were in scrubs just like a doctor's type scrubs.

You'd work there for probably five or six hours, go out and have a meal break, and then come back in for another five or six hours. Something like that was a typical day.

I was just in charge of making sure all the rocks got described, and assigning people which rock they were going to describe, and I would do some as well myself, describe rocks. My job was just to make sure that everything got done. It wasn't a particularly heavy job, but I was just kind of like the clerk. I had to make sure every rock got described and everything got done. I did that for 12; the later missions, I was just in there as a worker, just as a scientist describing samples. I wasn't the test director again. That wasn't a huge job; it was just make sure everything gets done, like I say. But you didn't have any other responsibilities.

ROSS-NAZZAL: You mentioned that there were scientists assigned to a rock. How did you determine which rock they got?

LOFGREN: I knew who the scientists were. I knew what their expertise was. Apollo 12, however, had almost all the same, all igneous, basaltic rocks. So it didn't really make very much difference. There were guys that would do those better than other guys because the function of their background and what they studied. There were a couple of guys there whose real interest was studying impact rocks, and we only had a couple on 12. So I would make those adjustments, but some guys had to describe rocks they weren't good at. Sometimes we would get two or three descriptions from two or three different people for a rock. We had about—I'm trying to remember—somewhere in the ballpark of between fifty and sixty rocks that were systematically described on 12. We got more rocks on later missions. They had more time to collect rocks. But it was around between fifty and sixty rocks, and we had about four or five scientists in there working. So to try and get that done, they could describe a couple rocks a day. You'd work a couple hours, then you'd have to go to a teletype station and type in all your notes. You could comfortably do a couple, maybe three rocks a day, depending. We got all that done in a month, and then we started putting the catalogue out.

ROSS-NAZZAL: Were these MSC [Manned Spacecraft Center] scientists or were they from universities, LPI [Lunar and Planetary Institute]?

LOFGREN: There was a couple of MSC scientists. I was one. There was a couple of others. I would say three or four came from outside. There was one from the USGS, or maybe a couple, and there was a couple from universities. Some pretty well-known scientists came down to do this, and I was a young, fresh PhD, and so I got to rub elbows with some fairly famous people, which was kind of fun. Sit around talking about rocks with them. It was fun.

ROSS-NAZZAL: Who were some of the people that came down? Do you recall?

LOFGREN: Well, a fellow named Cliff [Clifford] Frondel from Harvard [University, Cambridge, Massachusetts], a very well-known mineralogist. [Stuart] Ross Taylor from Australia National University in [Canberra] Australia. He did the chemical analyses that were done in real time on an emission spectrograph, he did those. There were a couple guys from the USGS, a guy named Robert [B.] Smith who was a very well-known volcanologist, had done a lot of classic work. A guy named Ed [Edward Ching-Te] Chao who, from the USGS, studied impact rocks. I'm trying to remember. At different times, there were a couple other guys that were there, but I'm not sure if they were there on 12 or were later there on some of the later missions.

A guy named Dale Jackson, and Howard [G.] Wilshire are both well-known scientists with USGS, U.S. Geological Survey. I don't remember them being there on 12, but I do remember interacting with them on 15 and 16 when I was doing descriptions as well. I could look at the catalogue, and I could figure exactly who was there. Every rock description has the names of the people who described it on them, so you can go to the catalogue and find out. If I reviewed that, I could tell you exactly who was there.

ROSS-NAZZAL: Sure. We just like to be thorough. Was the teletype machine in the LRL or was that outside?

LOFGREN: It was outside the quarantine barrier. There was a couple of them, three of them. They were just terminals. Obviously, everything was based on a big computer with a terminal, but they were outside the barrier, as I remember. Well, actually I'd have to check on that. There was a room that was kind of a conference room behind the barrier where you could meet outside out of the room that had the glove boxes, where you could sit more comfortably and talk around a table. There may have been a teletype or two in there, where guys could go down the hall and type in their descriptions. I'm sure there were both inside and outside the barrier, now that I think about it, there were teletype machines.

ROSS-NAZZAL: This catalogue that you put together, was this something that was then distributed amongst geologists nationwide, or was this something that had to be requested?

LOFGREN: Well, actually worldwide. There was a group of people who had applied before Apollo 11, even, and were approved investigators, and certainly the catalogues were sent to everybody who was approved, but anybody who wanted one could request one because somebody who wasn't already an investigator might want to become an investigator. So he would get a catalogue, look at it, see, "Does this really interest me? Do I want to apply to be an investigator?" There was a certain number that were investigators from the beginning, and many others became investigators later on as the interest grew.

We actually got samples back, you know? Before it had happened, a lot of guys said, "Oh, it'll never happen." There was that attitude among a lot of people. Some people were convinced it was going to happen, people very close to the program, but a lot of people weren't that close to the program, so they were a little skeptical. "It'll never happen" type of attitude. But when it did, and then when we started getting later missions like 15 through 17, where we got 100 kilograms of rocks for each mission, then that really generated a lot more interest. A lot more people came in the program at that time.

ROSS-NAZZAL: In order to become a PI [Principal Investigator], did you have to suggest an experiment?

LOFGREN: No. Well, yes, you had to explain what it is you would do. Then there was a committee that would decide whether that was worth doing, and whether you were the person that should do it. Were you good enough? The people who were the chemical analysts were actually given some unknown samples to analyze, and if their analyses didn't come up to snuff, they didn't get samples. So they were tested. There were people who did experiments on samples rather than do analyses, and basically they got samples by reputation, by their proven publication record. Fundamentally, you would submit a proposal, you would propose what kinds of analyses or what kind of studies you wanted to do, and then an august committee would evaluate these proposals and decide which ones would be approved.

ROSS-NAZZAL: Were you ever on that committee?

LOFGREN: No. I was too much of a novice at that time. Years later, yes, but not in that first decade.

ROSS-NAZZAL: Of those fifty or sixty rocks that were collected for Apollo 12, how many of them were selected to be examined by PIs? Were all of them sent out, or were there a certain [number]?

LOFGREN: All of them were studied by some people. Everybody didn't study every sample. One investigator might get five or six pieces, five or six samples, or eight or ten, but nobody would get fifty or sixty samples from that many different rocks. Some of these analyses take a lot of time and effort, so they can only do a half a dozen samples a year. Some of the kinds of studies were very tedious, long, drawn-out procedures, and when you're doing complicated isotopic age dating, sometimes it can take a couple of months to do one sample.

ROSS-NAZZAL: Is there anything in particular you learned from the Apollo 12 mission that you didn't already know from the Apollo 11 landing?

LOFGREN: We learned new things on every mission, certainly. [Apollo] 11 and 12 were a long ways from each other. If you look at the Moon, we see one side of the Moon, and Apollo 11 was very much near the eastern limb or terminator, and 12 was as far west as we went. They were probably the two most farthest apart missions, and they collected very different kinds of rocks. We were looking at two areas of the Moon that were father apart than any of the other sites. There was basaltic rocks, igneous basaltic rocks, but very different ones. They weren't similar at all. So yes, we did, certainly.

ROSS-NAZZAL: I was looking at your biosheet the other day, and it said you were part of the Lunar Sample Preliminary Examination Team. Is that part of that group that you were telling me about?

LOFGREN: Yes, yes. The test director was part of that group; I was part of that group. I was not on 11, because I told you I was in Alaska when that happened. But from 12 through 17, I was on that group. That was the Preliminary Examination Team—no, you said LAP?

ROSS-NAZZAL: The Lunar Sample Preliminary Examination Team.

LOFGREN: Yes. I was on that. There's another group actually, LSAPT, Lunar Sample Analysis Planning Team, the people who actually decided which scientists would get which samples. I was on that committee a couple of decades later, but not at that time. That was a heavy-duty responsibility, and they worked a lot of long hours too. There was a lot of people clamoring for their samples, and this committee was the one that made all the allocations, so their work was cut out for them as well. But they had to have the results of the PET examination before they could do their work, so it was important to get that catalogue out because that catalogue went to this committee, too. Then they would look at the requests from the scientists who had been pre-approved before the mission, and then they would decide who's going to get what samples, what samples are more appropriate for this person's kind of study or that person's kind of study. They made those decisions.

ROSS-NAZZAL: Wow, it sounds like a lot of work.

LOFGREN: It was a lot of work. I mean those guys dedicated a lot of hours and a lot of time to that.

ROSS-NAZZAL: Yes. How many geologists were working onsite at that point?

LOFGREN: During the missions there was probably twenty or thirty, which was quite a large number. The number at MSC was about eight or nine that were permanently attached. I could give you an exact count, but it's in that ballpark. Another twenty or twenty-five came in from the outside, not all at once necessarily, but over the years, at least that many were there, either on the allocation committee or on the PET team. People on the PET team were usually people who are used to describing rocks. That's what they did as part of their science. A lot of the people who do nothing but analyze things weren't as good as describing rocks because they hadn't really done that as part of their routine kind of work. You chose people for the PET who made a profession of actually doing that.

ROSS-NAZZAL: Now I was reading in the *ASK Magazine* again, you had talked a little bit about the Lunar Database, and I was curious if you wanted to expand on that a little bit.

LOFGREN: Well, the Lunar Database is an interesting story in databases. In 1969, databases were pretty much a program that was part of a big computer and were very big and cumbersome. You would enter data into a database like that using punch cards. You've probably never seen one of those.

ROSS-NAZZAL: I think I've seen them over onsite, but they're clearing them out.

LOFGREN: They actually have a machine that looks like a big fancy typewriter, and you type on a keyboard, but this little card about, it's what, like eleven inches by five, four inches or something, and you had eighty positions on that card, and you would type a code for every position. That would then be run through a computer, and the punches on that card would input the information into the computer.

It was a very cumbersome process, and you were very restricted. You had to be very judicious in how many letters you would use for a given label, and then you would enter a number. You only had eighty characters, so you would want to have a very short abbreviation for what the number meant. You had to have a little book of what these little abbreviations meant, because you didn't always even remember them. If you use them all the time you do, of course.

So the database was done that way, but then one of the guys who was the first curator was very savvy, computer-wise, for that time, and managed to hook up with a computer downtown, even, through one of these little modems where you take your telephone and you stick it in this little box, 300 baud rate modem—which is like crawling, in computer language.

ROSS-NAZZAL: Not even dialup?

LOFGREN: Oh, if you did dialup at that—if you were to download a one megabyte file at that rate, it would take a week. It would take a long time. It would be impractical. But he did create a database, which gave some very fundamental information: a rock number, and a weight, and he could designate who had that sample. It was kind of like if you did an Excel spreadsheet and you had a list of names of rocks, and then you would have maybe a list for names of the person who got that rock, and maybe what kind of analysis was done. No data, but just what they were doing. The amount of information would probably fit on a single screen in an Excel spreadsheet, and this was the whole database at that time. That was actually pretty extravagant in those days. It worked very well. We kept track, but there was a lot of stuff that we don't have digitized on the computer that we later did. So this evolved. Into the early seventies, as computers got better, computers at JSC got better.

I remember one of the guys that worked in the curator's office that I knew very well, every day he was carrying a tray of cards about this long, over to Building 12, which is most of the way across the site from where our building is. It's almost diagonally. So this big, heavy—I mean, this thing was heavy. It probably weighed twenty-five pounds or something to carry that set of cards. Some guys used carts. Every day he would carry over a run of cards. If you made one typing error in one of those cards, the run wouldn't work. So you'd have to correct that card and then carry it over again the next day and try and get it to run. It would only go as far as that card, and then it would quit. So if you had another card down here you hadn't corrected yet, you'd correct that card, take it out, there's another card down here that might stop it. So if you were lucky, you got a run through the first time you did it. Sometimes it would take two or three times before you got the run to work.

ROSS-NAZZAL: Pretty time intensive.

LOFGREN: That was just the way you did computer things in those days. As the computers got more sophisticated, the database got better and more information was put into it. By the time we got to the early eighties, the DEC Computer Company had their VAX computers. They had a database program that came with the computer. Again, the VAX computers were smaller versions of mainframe, like the big IBMs or UNIVAX were great big things. The VAXs were smaller, but still worked on a terminal basis. You had terminals, but you could type. You didn't have to carry cards over to another building; you could have a VAX in your own building, and you could type on a terminal that looks a little more like the terminals we have today, not a teletype machine or a card punch machine. But it was still, it was a reasonable terminal, and you could enter data.

By that time, we were getting pretty sophisticated. This was a reasonably powerful database program. Our database today, which we finally actually got off the VAX just a couple years ago—our VAX was dying, and VAX had long since ceased to exist. Nobody was making new ones and there were no repairs. Everybody was scared to death. We weren't going to lose any data, but if the computer had gone down and you couldn't repair it, we couldn't do our work. The thing was backed up every day so you're not going to lose any information, but without that computer you couldn't allocate a new sample. You needed this program to go in, and say you had a twenty-gram sample, and you broke off one gram to give to some scientist, and when you broke off that one gram piece, we had to create a new number and everything. All that had to go in the database. Well, if you couldn't get access to the computer, you couldn't do that. So you couldn't allocate samples. That never happened to us, fortunately.

We are now on a new PC [Personal Computer]-based database largely based on the old construction. It's on a new modern computer program, but the way it was put together was similar. Databases still use the equivalent of an Excel spreadsheet for the basic data in your database. What gets more sophisticated is how you can retrieve data from that spreadsheet. Excel doesn't do that in a very sophisticated way. You can sort of do it in Excel, but not very well. The kinds of databases that we use allow us to retrieve and do searches and pull data out of there much more efficiently than that, and so these are much more sophisticated database programs. Database programs today are excellent.

You went from this stage where you could put in just a very minimal amount of information and actually have space to use it, to where now we do everything, obviously, on the computer and everything is there. It's like a double-entry bookkeeping system where all the columns and the vertical columns have to add up to the same number, and you don't, you've got a problem. Well, our system works the same way. If you take a sample and you take off a gram, and you then enter the new weight of the big piece and the new weight of the new sample, the small piece, that had to add up to what the numbers were before, or the computer would spit it back at you and say, "Wrong."

ROSS-NAZZAL: Error. Right.

LOFGREN: It's got to add up. There's no missing sample here. That's a sophisticated system, actually, to put that in computer language and have it all done in this complex way. Yes, the database has evolved tremendously, but because it was so simple in the first place, we don't have records prior to 1980 in our database as to who had what samples, for example. That was just beyond the scope of the early databases. We just kept fundamental information. If you took this sample and split it into two, what were the two numbers, and what were the weights of the two samples? That was sort of the extent of the database.

ROSS-NAZZAL: So there's no history of who had the rocks?

LOFGREN: It's all paper. We've got all handwritten, records, but it's not in the computer. If somebody had the time to sit down for maybe two or three years and enter all this information, do nothing but entering information into the computer, we could get it in there. But that's what it would take. You'd probably have to put a couple of clerks to work for three or four years to do that.

ROSS-NAZZAL: How long did it take you to migrate the system?

LOFGREN: It took five full-time programmers fourteen months and close to a million dollars to do that, when you consider the work that went ahead of it, too. You had to kind of write a requirements document for what you wanted it to do, then you actually had to do it. I don't know, it was about $800,000, I guess, $850,000, something like that. It was all labor. You get salaries for five people for fourteen months. That adds up.

ROSS-NAZZAL: Well, we are talking about Moon rocks, pretty expensive stuff. In comparison to—

LOFGREN: Yes, we've got the funds to do it. It had to be done, but it was surprisingly expensive. When I first started doing this job ten years ago or eleven years ago, I thought, "Well," I knew then we had to do this. That was '97. It didn't actually get done until 2007, so I was harping on this for ten years before it actually got the money and got it done. I figured we could probably put a couple guys to work on this, they'll get it done in a couple of months, and maybe it'll cost $20,000 or something like that. Wrong. We tried that approach a couple of times, and it just didn't work.

The guys didn't understand the database. It's not that they didn't understand databases, but they didn't understand what we had done, what the old thing looked like. They understood modern databases, but they couldn't fathom what it was we had created, and they weren't familiar with the old program or the old computer language.

So it took some people who were familiar with both the language of the old program, where they could translate it. We basically had to do what you always have to do when you do this kind of project. You have to write down systematically on a piece of paper what it is you want it to do. Item by item. This requirements document turned out to be about 140 pages long, and it took two programmers a year just to look at our database and to write this requirements document. That's the kind of document then you could put out for bid to a computer company and say, "Do this. How much will it cost?" That was the document they would look at, and then they would make a bid on what it would cost to them to actually do what that said. That was probably 100k just to produce that document, or 150k, something like that. Then it was about 700k for the rest, the actual project, when we did it.

It was certainly worth doing. It certainly runs much better now. We use the Web to get access to the database from our desk. Anybody can get it from their computer if they're authorized to go there, and of course different people are authorized to do different things. Like I'm only allowed to go in there and get information out. I'm not allowed to change anything. The people who actually do the work are the only ones that are actually allowed to enter new data or actually change numbers in the database. You've got to restrict it that way; you only want people who know what they're doing actually changing numbers.

ROSS-NAZZAL: Right, yes. You don't want to lose any samples. When Apollo 13 didn't land on the Moon, did that provide any sort of challenges for you and the other people working in the LRL?

LOFGREN: Not really. Obviously it was a trying time for us, but no, it really didn't. In fact, if there's a benefit from something like that, it was we had a little more time to think about what we should be doing on the lunar surface on the next mission. They were scheduled pretty close together, and when the Apollo 13 mission didn't happen, then we had a little more time to plan.

That was probably one of the few benefits that came out of that. No, it didn't really affect us directly in terms of what we did. People got ready for the mission, it didn't happen, and so they just were—Apollo 14 went to the same place Apollo 13 was going to go, so nothing really changed. It just got postponed.

ROSS-NAZZAL: You were working in the LRL again in Apollo 14?

LOFGREN: Yes. Just describing. I wasn't a test director or anything like that, I just was one of the people who spent time. I did that for the rest of the missions. We would spread the work out among a bunch of people, and then more and more samples were brought back, so there was a lot more work to do for more people. We still tried to get the catalogues out in that same two to three month time period with a lot more samples, so we needed more people to do that.

ROSS-NAZZAL: Were you ever concerned about back-contamination working in the LRL during 11, 12, and 14?

LOFGREN: I would have to say personally no, because from my own scientific experience and people I talked to, the environment on the Moon was such that nobody could really imagine that any kind of life as we know it could exist on the Moon. You hear about the ozone layer and the atmosphere, how it protects us from the cosmic rays of the sun. Well, the Moon has absolutely no atmosphere, so you're absolutely unprotected from the cosmic rays from the sun. The astronauts were only there for a few days. They got a dose, but not enough to be harmful. If there had been a serious solar flare, we're not quite sure what we would have done. It would have been a serious problem.

Solar flares can sort of be predicted. Not absolutely, but scientists can kind of anticipate when solar flares are going to occur. That's when you get a much more intensive burst of radiation going off from the Sun. There was one particularly intense solar flare between Apollo 16 and 17 that caught people's attention and said, "Boy, if that had happened while a crew was there..." We don't know how serious a dose the astronauts would have gotten, but it would have been of high concern. Radiation, it's still a tough issue. If we go back and spend months on the Moon, protecting crew from that radiation is still a serious problem.

So no, I didn't really imagine there would be anything on the Moon that was dangerous to me. There was some concern that maybe bacteria that went up with you, for example, exposed to that radiation might have suffered radiation damage that when it came back with you, could have been a problem. There was some concern about that, but not really concern that people would be harmed from it. There was this air of caution, that we're not going to take a chance, and so the quarantine was instituted. If you think about the recovery of the astronauts, when the Apollo capsule dropped into the ocean, they just opened the lid, the astronauts climbed out, got on the helicopter, flew to the aircraft carrier, got on the aircraft carrier, then went into a quarantine facility. Think about that. If there was something on the spacecraft that was harmful, what's the worst place to set it free? Probably in the ocean.

The quarantine was dealt with very rigorously in the LRL, but there were interesting side effects. One of the stories I like to tell—and I don't know if I told this last time or not. I don't think so.

ROSS-NAZZAL: I don't think we talked about working in the LRL at all.

LOFGREN: OK. The way you sterilized samples was to heat them up. Without damaging the sample as little as possible, you would heat it up to about 200 degrees Fahrenheit, and you would bath it in ultraviolet light. If you want to take a sample out from behind the barrier, that's what you would do. You would walk into this little room, and out in the middle of the room was this oven that you would put the sample in, it would heat up, and it would also bathe it with this ultraviolet light. You would walk in from one door on the quarantine side. You'd walk to the center of the room, put the sample in this oven, and then you would exit. Then a person would come from another door on the other side and get it.

Now, there was no real barrier. It was just this little table with this thing out in the middle of the room, and there was this yellow tape that went up the walls and across the floor. People joked that if there were bugs, they were not allowed to cross the yellow tape.

ROSS-NAZZAL: They knew the rules? (laughter)

LOFGREN: They had to know the rules. Those are the little things that just kind of made you, "Huh? Is this really a quarantine?" I'm not an expert in bugs, so maybe that was adequate. From the people who were doing the quarantine, they thought that was an okay system. When that's not your field of expertise, you look at that, and you kind of think, "Hmm, somehow that doesn't seem right." Then on top of that, if there was a bug on the Moon, on a monthly basis it was bathed in temperatures of 200 to 250 degrees and pure ultraviolet light. What we were using to sterilize the samples was what the samples experienced all the time. If that would have done anything, it might have revived the bugs rather than killed them. There was these inconsistencies. That's the way you took care of bugs on Earth.

There's a certain logic to that, too, because there is some thinking that for bugs to really affect us, they have to be enough similar to us that they can affect our biological systems. They can't just be totally, totally different, probably. The two systems might not interact at all. I don't know if I'm being clear, but if you can imagine, the kind of life that we have which is based on carbon, nitrogen, and oxygen, and our bodies are made using those elements and water. If you had another kind of life form that was based on different chemical elements and a different structural system—they don't have calcium and phosphorous for bones, they have silicon for their bones—that's just totally different—there's no reason that if life forms on one planet, and it assumes to be derived from certain kinds of elements that are the basis for that life, on some other planet or another solar system it might be totally different. Those two systems might have no danger to one another because they're so different that a bug on one wouldn't be able to be hosted or couldn't interact with the other. That's the point. So there is that.

You were looking for life, you were trying to kill life that was kind of like us, and so to harm us it would have to be enough like us that that would probably have killed it. If it hadn't killed it, it probably wasn't harmful to us anyway. Nobel Prize-winning biologists basically put forth that hypothesis, so it's not a crazy idea. But some guys, like in any science discipline, there are people who don't agree with that, and they didn't want to take a chance. So the quarantine was done. They did this for three missions, found absolutely nothing that could even be the least considered even [dangerous]. The level of carbon in the sample, and carbon is one of the major elements in our life system, was in the parts per million range. It was all inorganic. There was no organic molecules on the Moon. Well, if you don't have organic molecules, you can't have life, and for three missions we didn't find any organic molecules. It became pretty safe that there wasn't going to be any life, and so they stopped doing the quarantine.

ROSS-NAZZAL: Did procedures change in terms of preparing to work in the LRL as a result of the end of the quarantine?

LOFGREN: Yes. Sure. Yes, you could just walk in. There was no barrier. You didn't have to shower in, shower out. The barrier disappeared. You were just allowed to go in, in your street clothes, and work.

ROSS-NAZZAL: No bunny suits?

LOFGREN: Right. I'm trying to remember, actually. There were actually, well...

ROSS-NAZZAL: I see pictures of people in Building 31.

LOFGREN: I know. I'm trying to think. I actually don't remember. If you look at the pictures from Apollo 15, for example—

ROSS-NAZZAL: That's right, that photo we had of you.

LOFGREN: It seems to me like I was wearing a normal shirt. We had on these hats, but I think we were wearing street clothes, as I remember. I'll look at the picture again. I'm not even sure. It's funny how some things you just don't remember.

ROSS-NAZZAL: Well, it was over forty years ago.

LOFGREN: But it would be easy to look at some of the pictures and see what people were wearing, and I've got movies of people, so I'll have to go back and look at the movie again. That's one of the things I didn't pay attention to. I was watching samples and watching them work with the samples, and didn't pay attention to what they were wearing.

ROSS-NAZZAL: Did you change the way you handled the samples at all?

LOFGREN: No, no. We still worked in the cabinets. We still had gloves. The one thing that changed is when we were under quarantine, the cabinets were under negative pressure. The cabinets were at lower pressure than the room, so if there was a leak, the room leaked into the cabinet. The cabinet didn't leak out into the room. Once we went to no quarantine, that was reversed. The cabinets were positive pressure to keep contaminants from the room from leaking in. So if there was a leak, the cabinets would leak out into the room rather than stuff leaking in.

That's the way we operate the Lunar Sample Facility today. All the cabinets are under positive pressure. So the gloves stick out. When it was negative pressure, the gloves would always be in, and it was easy to just put your hands in and get in the gloves. When the gloves point out, it's far more difficult to get your hands. You've got to kind of go finger by finger.

You've got to get your fingers into the things and then push your hand in and get it in. Then you've got to figure out how to do that with the other hand when you don't have a hand to help you. So it becomes quite, actually, a little bit of talent and skill to get into gloves in a reasonable period of time. The processors that do it all the time can do it in about a tenth the time it takes me to get into a pair of gloves and get into a cabinet.

ROSS-NAZZAL: Wow. I'm wondering, can you walk us through the procedure of looking at a rock in the LRL? What was that procedure?

LOFGREN: Well, the rock would be in a tray, and the technicians—and we had a lot of technical people who were the highly trained people for handling—would put the rock at a scientific work station. They would move the rock from a collection box or storage box to the station. Then the scientist would go to the station. The way it was set up is you had a microscope that was on the outside, and you had the rock sitting inside the cabinet. You had a flat surface, and you had a little lab jack inside where you could put your hand in the glove and crank the lab jack up and down to focus. You would want to get the rock into the focal range of the microscope, then you could focus the microscope on the rock, because you always wanted to look at the rock carefully with the magnification. You could see so much more when you could magnify fifty, a hundred times over normal vision.

The scientist would probably spend a good hour, probably, just staring at the rock and making notes: identifying the minerals, looking for fractures, looking for textures, other structures in the rock, all the things that we requested that they describe. Did they see any shock effects? What did the weathered surface look like versus a fresh surface? Describing the differences, and then try to identify the minerals as best you can. Later, we would have techniques for doing that absolutely, but these guys were skilled at identifying minerals and could do a good job of identifying which minerals were there in roughly what proportions, so you sort of knew what the rock was.

Initially, they thought that they might actually do a lot of—there were a lot of kinds of tests that geologists tend to do or mineralologists tend to do, where you put stuff in, determine the refractive index of minerals, where you have these specials oils that you put them in. They did a lot of practice with that kind of thing, but in the end they decided that, no, those oils can contaminate things. So we never really did much of that kind of thing. We basically would just spend an hour or two looking at the rock and trying to examine everything you could about it, identify the minerals, what were the proportions between the minerals and all the various features of the rock. You'd discuss what you think the origin was, what kind of name would you give it.

A lot of these are qualitative judgments that you make, but these are guys who do this for a living, so they're good at it. They would do quite a good job of that, as borne out by the fact that the rocks were reasonably well described. Eventually, when they started doing more detailed analyses, they would get the minerals right and the proportions basically right, but they had other techniques that determined what was the precise chemical composition of the minerals, and those kinds of details would be determined later. You couldn't determine that at this stage of the game.

So a scientist would be at this work station for a while, and he would just do these qualitative judgments of the rock and descriptions, and he might write two or three pages worth of information about it. You might think, "How does he do that?" There's a lot you can say about a rock. You might not think so, but there is when that's what you do for a living. We still have these catalogues, they're still available; in fact, they're now available on our website, electronic versions of them.

ROSS-NAZZAL: We'll have to put a link in your transcript.

LOFGREN: Yes, yes. We can have links to the curation website. The only one that's not on there yet is Apollo 12, interestingly enough. But it's in the final stages of being ready to put on. I'm so busy right now I can't order the last few photographs we need from the photo lab to finish up the catalogue. I've got to get that done. It just seems like I've got a million things to do.

ROSS-NAZZAL: Well, we're happy you came to see us this afternoon, then.

LOFGREN: This is one of the million things. (laughter)

ROSS-NAZZAL: Other than describing and working on those catalogues, what else were you doing during the Apollo missions and crew training, of course?

LOFGREN: Well, I was doing crew training at the same time all this other stuff was going on. I worked with the 15 mission, and I did a little bit with 16 and 17, but not too much. I actually was building the laboratory that I came to NASA to build for the science I was going to be doing for the next thirty years or forty years, so when I wasn't doing all those things, I was working on this laboratory, basically my science.

ROSS-NAZZAL: Why don't you tell us about that laboratory? You mentioned last time that it was an experimental petrology laboratory?

LOFGREN: Yes.

ROSS-NAZZAL: Was that something NASA wanted or you convinced them to build?

LOFGREN: Well, it's interesting. I think I told you how I came to come to NASA. Did I tell you that story?

ROSS-NAZZAL: With Jack [Harrison H.] Schmitt? Yes.

LOFGREN: I wrote the letter to the head guy here, Bill [Wilmot N.] Hess, and described what my expertise was, and came down and interviewed here. They decided that yes, to study the formation of rocks in an experimental lab where you could determine what temperatures and pressures things happened—you can quantitatively duplicate features you see in the rock in the laboratory so you can get very precise pressure, chemical, temperature histories of the rocks. This was something that they wanted to be able to do. That's the kind of thing that I would do in my lab.

So I put together a laboratory where I would crystallize synthetically prepared compositions that mimicked the lunar rocks. I didn't use actual lunar rocks to do these studies. It was easy enough to take chemicals off the shelf, put them together in the right proportions, and make the bulk composition of a Moon rock. That was perfectly adequate to do these kinds of studies. Now, you might work with a little bit of the real thing to confirm, do just a few experiments to confirm the experiments on the synthetic materials, but in general that technique works very well. If you make a bulk composition, this combination of chemical elements in a melt, that's what it is. That worked very well.

Then you would devise experimental regimens that allowed you to duplicate features that you see in the actual rocks. So you could then talk about the precise history: pressure, temperature, physical events that occurred to form that rock. I did that for many years. I was able to determine exactly what rates of cooling in terms of temperatures, how much the temperature would change over a period of time while these rocks were forming, the lavas that had come out on the surface. Even some of the impact melts that were formed in the big impacts, the kind of work I was doing, I could talk about what that process was like, how rapidly it would happen. What were complex interactions going on in this melt, combined with crystals, to form the features that we see in the rocks. It was a complex interaction of having these melts, and you're injecting crystals in them, and that affects how and when certain crystals start to grow, and what the ultimate textures or relationships between the minerals in the end product turned out to be. I would mimic that in the laboratory, so then I could put more precise histories.

This was interesting for the samples because samples from the Moon, we did not have the context. Like on Earth, you would go to a lava flow and you would collect a sample from a given position in the lava flow. You knew it was from the center, you knew it was from the edge, and you knew precisely where it was within this lava flow. Well, on the Moon, all we had were random samples just sitting on the surface, and we didn't have a clue where in a lava flow they came from. So with the work I was doing, I could reconstruct approximately where a lava was from. If a lava flow was ten feet thick, I could estimate where in that lava flow the sample came from. Based on the extreme limits, I could say, “Well, this had to be from the center of a lava flow that was maybe twenty feet thick,” or something like that, or, “It came from the edge of a thinner lava flow.” You couldn’t always be absolutely precise, but you could put limits on, or ranges, on that kind of thing. I wound up doing that for some particles of meteorites as well, eventually, after I studied the lunar samples as much as I could. I did similar kinds of things on meteorites.

ROSS-NAZZAL: When was the lab finally finished?

LOFGREN: That kind of lab is never finished, but by ‘72 or ’73. The whole time I was here, I was working on it some. When I was training astronauts, not very much. But they had given me a couple of technicians, so I could give the technicians tasks, and they would do them while I was off training astronauts, so that was a big help.

ROSS-NAZZAL: Where is the lab located? I couldn’t find any information about it when I was searching for it.

LOFGREN: Most of the time I had it, it was over in Building 261. But in ‘94, the lab was moved over to Building 31. The experimental lab, the one that I built, I’m not working in it anymore. I’ve sort of turned it over to a younger guy. It’s still there, and it’s in Building 31, in a windowless portion of the building, the high bay.

ROSS-NAZZAL: I think I know where that is.

LOFGREN: If you go to the Ares website and look at the experimental petrology lab, they describe it.

ROSS-NAZZAL: I was looking at the *Roundups* in the news [clips].

LOFGREN: The website for our directorate.

ROSS-NAZZAL: Can you give us some description of what the specifications and requirements were for the laboratory?

LOFGREN: I had several kinds of equipment. I did high pressure studies where I tried to duplicate conditions as much as 25 to 50 kilometers deep in the Earth, so I had pressure devices where I could put a sample at pressures up to about 150,000 pounds per square inch. That's a pressure that you would find tens of kilometers deep in the Earth. Anywhere down to one atmosphere. There's different kinds of conditions that you want to look at. Sometimes you want to look at the function of pressure versus temperature, so then you would have these devices where you would have contained gas pressure on your sample together with temperature. I did a lot of experiments that way, duplicating conditions in the crust of the Moon and the crust of the Earth, where minerals form, and duplicating those features.

But the work that was with lunar samples, and actually with meteorites too, was in another kind of experimental device. Working together with another scientist who was also an experimentalist, we put together a furnace in which you could control the amount of oxygen that was available for the sample. In our atmosphere, it's like 21 percent oxygen we're breathing. Deep in the Earth, it's about eight or nine orders of magnitude, or as much as ten orders of magnitude less oxygen, free oxygen. There isn't any, or very, very little. The amount of oxygen determines the oxidation state of elements that have more than one valence state, like iron, manganese, chrome, basically the transition metal elements. In the Earth, you see these elements equilibrate with a certain level of oxygen in the liquids that they form in, and on the Moon it's even lower. There's even less oxygen on the Moon. The Moon has a vacuum, and we speak of the Moon as being very reduced because elements like iron on the Moon are basically stable as bright, shiny, metallic iron. I mean, on Earth, metallic iron would rust so fast it wouldn't be funny. Just setting it on the table here, it would rust in the matter of a week or two, a bright shiny metallic iron thing. Not stainless steel, now. We're talking about just pure metallic iron, it would rust very quickly just from the humidity in the air.

You're controlling those kinds of parameters, and then you're crystallizing rocks where you're controlling that atmospheric composition very precisely. It's different for the Earth than it is for the Moon. So we were able to duplicate features, the crystallization of lavas on the surface of the Moon. I studied lavas on the Earth as well and compare them on the Moon, but there are very different levels of available oxygen. The lavas are a different composition, and they crystallize more easily on the Moon just because of the difference in the composition, without getting too technical.

They basically functioned at one atmosphere, but inside a tube we could control the amount of oxygen in the chamber where the sample was actually at high temperature. Then we would cool it at certain cooling rates, and we could duplicate crystallizing in the different positions in the lava, and we could control the amount of oxygen that's available during that step. We could duplicate the minerals that we saw on the Moon quite well, and how they contrasted with this. Take that same kind of lava and crystallize it under Earth conditions, you get different compositions of the same minerals. Like the iron is different.

On the Moon, the iron might be all Fe^{2+} or reduced metallic iron. On the Earth, it might be mostly Fe^{3+}, and you've got a plus two valence of iron versus a plus three valence of iron. Rocks out on the surface of the Earth could be very highly oxidized, they turn red. You see lots of red rocks on the Earth or yellow rocks or tan rocks. Those are all various oxidation states or percentages of oxidized iron that are in that particular rock. Most of the color we see on the Earth is a result of the oxidation of iron in the rocks. That's why the Moon is gray, because the iron has not oxidized at all on the Moon, and that's why there's no color on the Moon. There's no reds, there's no yellows, there's no brown. It's all gray, because that's the color of reduced iron.

ROSS-NAZZAL: I did not know that.

LOFGREN: It's just an artifact of the conditions on the Moon versus the conditions on the Earth. There's no atmosphere up there. There's no oxygen in the atmosphere; there's no water. Those are the sources of oxygen on the surface of the Earth that oxidize the samples—the oxygen in water or the oxygen in the air. The Moon has a -13 torr vacuum. There's absolutely no oxygen there. There's oxygen tied in a crystal structure in a mineral, but that oxygen is not free to combine or oxidize something else, because it's tightly bound in a crystal structure.

Now, one of the things we want to do on the Moon is to take certain minerals on the Moon and extract the oxygen. If we're going to stay up there, it'd be nice to be able to get oxygen while we're on the Moon for breathing, or to use as fuel. We can do that. We've developed the processes for extracting oxygen from minerals. It takes energy to do that, but you've got the Sun there, the full blast of the Sun to use as a source of energy.

ROSS-NAZZAL: Was this facility that you built here at JSC a one-of-a-kind facility?

LOFGREN: Well, it was pretty close to that. We actually, this other fellow and I, Dick [Richard J.] Williams, actually developed—probably more Dick than I—but developed this furnace, which was then duplicated at other labs all around the world. We put together this kind of furnace, this specialty kind of furnace, and then to anybody who wanted them, we would tell them how to make them. It wasn't long before they were all over the place.

ROSS-NAZZAL: That's pretty neat.

LOFGREN: Because they're a very useful way to do experiments. What was unique about it was the way we measured the amount of oxygen in the environment, inside the sample chamber. That was the contribution that Dick Williams really made. The only way to do it before was to set up these very controlled laboratory system where you had a certain flow of oxygen, and you would mix it with a certain amount of CO or CO_2 to provide the atmosphere you want, then you worked on getting very precise flow rates to get your thing.

What Dick Williams did was to use a technique for actually measuring the amount of oxygen in the environment directly. It's a concept that mimics a pH electrode in principle, but it's done at very high temperature, 1,200 degrees Centigrade or 2,500 degrees Fahrenheit is the temperature we were doing it. We're measuring it in real time, so we know precisely what the oxygen content is in the atmosphere in the furnace. That's the feature that Dick developed and was mimicked, how do you that in this high temperature furnace. Actually, now it's become a commercial process for measuring in any kind of manufacturing process where the amount of oxygen in the process is important. They've used this same technique to measure the oxygen at various points on a process in a plant. They use that same way of measuring oxygen. Yes, we were a bit innovative there.

ROSS-NAZZAL: Were you ever a PI, then, while you were working in this laboratory and using some of these samples?

LOFGREN: Yes, I became a PI during Apollo 12. I actually got a grant from NASA and became an official PI, and I had my own lunar samples to work with.

ROSS-NAZZAL: Can you tell us a little bit about that?

LOFGREN: it was a standard process that anybody could do. If you had a good idea, just like the ones that did for Apollo 11, it would go before a committee who would evaluate the ideas, and then they would choose. I was asking for money, so it wasn't just asking for permission to get samples. In that day, you didn't really get samples unless you got money from NASA to study them. Now, people from foreign countries didn't get NASA money to study them, but their own governments would give them money. They would just submit a proposal. But you would see what their science was. If you were an investigator in the U.S. and you were requesting money, then you would propose what you would do in great detail, and then there would be committees that would evaluate your proposal and either decide you were going to get money or not. I was able to do that consecutively for thirty years or whatever, until I quit doing it just a few years ago.

ROSS-NAZZAL: Any experiments that stand out in your mind that you'd like to share with us on the record?

LOFGREN: One of the early experiments that I did—which was really kind of neat—there was a particular basaltic rock on the Moon that had large pyroxene crystals, the technical term is phenocryst. Basaltic rocks are usually fairly fine grain, but sometimes they'll have large crystals in them. This particular basalt from the Moon did have these large pyroxene crystals in a finer grain matrix of feldspar, and more of the same mineral, and maybe a little olivine. We actually duplicated that texture so well that you could look at our thin slice of the experiment and thin slice of the real thing and it was kind of hard to tell them apart. That was really neat. We were proud of that one. We were able to actually duplicate that texture very precisely and talk about the conditions under which those lavas came to the surface and how they crystallized.

ROSS-NAZZAL: Did you have any other researchers working with you in the laboratory that you created?

LOFGREN: Yes. I had, over the years, probably at least twenty or thirty post-doctoral fellows coming from universities that would come and spend a couple years here working. I had probably eight or ten graduate students. NASA had these cooperative agreements where NASA would pay for the graduate student to do their studies at a university, but they would come to NASA and work with me to do part of their thesis. So I had several students like that, too, over the years.

ROSS-NAZZAL: You had quite a bit of responsibility.

LOFGREN: It was almost like being a professor in a university without having to teach courses, because you had post-docs and you had students. I never had more than a couple at a time, which was nice. I wasn't like a professor swamped with eight or ten students or something. At one point I had maybe four, a couple of students and two or three post-docs at one time. That was a lot.

ROSS-NAZZAL: Were you on their dissertation committees or thesis committees?

LOFGREN: Yes. I would often go to the universities and be part of their dissertation committees.

ROSS-NAZZAL: Did NASA encourage this type of relationship?

LOFGREN: Yes, yes they did. Very much. It was a NASA program that they funded, and it was for the NASA scientists to participate. NASA realized that the kind of synergy you get in a university where you have professors working with bright young students, how good that synergy is. They wanted to duplicate some of that synergy within NASA itself. So they created the mechanism by which scientists working at NASA could fund and bring in post-docs and students. So yes, it was very much encouraged, and it was a very good program. Still do it. It's still a program.

ROSS-NAZZAL: It makes sense. Were there any other federal agencies that you may have worked with in conjunction with your laboratory?

LOFGREN: Well, you work with the USGS a lot, but most of the people that I worked with were in the university community, almost exclusively. Other than the U.S. Geological Survey, which was really more during the training and working with the surface experiments, when we're actually on the Moon, I was almost exclusively with universities and university professors and students. I can't think of any. Yes, I was just basically university.

ROSS-NAZZAL: I noticed also on your biosheet that you mentioned that you were involved with the Geological Society of America's Penrose Conference in 1976? Do you want to share the details of that?

LOFGREN: The Penrose Conference, along with other societies had other similar kinds of things, but these were small topical conferences or workshops designed for somewhere between 100, usually no more than 200 people, usually more like 100-150 people, depending on the topic and the location. You would try to have this little workshop at a place where everybody got together in sort of a casual environment where you would have sessions during the daytime, but people would have relaxing time where they could interact as well. You were getting people together in a very specific topic, experts from all over the world in a topic where they could sit in formal environments and talk and interchange, but they could also sit informally to a large degree and chat over a beer, or just sitting around. They'd go out, and you actually had activities too. You might do volleyball or something like that a little bit. You were there for usually five days. One of the first organizations to do that was the Gordon Conferences that are run back east by AAAS. American Association [for the Advancement of Science].

The Gordon Conferences were probably one of the first ones to do that, but then most of the societies like the Geological Society had the Penrose Conferences. The American Geophysical Union had a similar kind of thing, called Chapman Conferences. I can't think of the name that they used. Most of the societies then wound up creating their own. This topic became such a popular way for people, like I say, on a very focused topic, get together. Those were very productive meetings. You'd get to meet a lot of people from all over the world in your field, where you might only read papers. You never talk to them in person.

Scientific interchange in person is just so much better. Papers are great. That's the way you publish and communicate, but face to face is better. You can just talk. You can bat around crazy ideas, which you don't do in formal, published papers. You can just really say anything you want. You can just talk. It's great, you can just throw out ideas and bat things around, where you wouldn't do that any other way. It's hard to even do that over a phone. It's better when you're face to face. There's no replacing that. That's why people go to science, even big science meetings, you might zero in on a couple of guys and go sit off in a corner and do that. But where you can get 100 people together who are interested in the same topic, it's even better. You're really focused for that short period of time.

But people go to big meetings, too. Geological Society has a big meeting where maybe 10,000 scientists come to it for a week, or I don't know, maybe it's 6,000 or 7,000. AGU, American Geophysical Union—they're really big, they're huge! You'll go there, and you may know 200 or 300 people out of all those that you interact with at some level. You may talk to a couple of them. You do that at those big conferences, but these small ones are very good, too, and this Penrose is one of those. That was one I organized on this topic, this duplication of textures and rocks and understanding formational histories of rocks was the topic that I made popular, so this was a conference that was fundamentally on that topic.

ROSS-NAZZAL: Yes, I think your biography talks about you being sort of the pioneer in this field.

LOFGREN: Yes. I don't like to say that about myself, but some people have said that, yes.

ROSS-NAZZAL: I think that's pretty neat.

LOFGREN: It is kind of neat. Before I did that sort of thing, nobody had ever really thought about that. When I did my PhD at Stanford [University, Stanford, California], I tried to convince a professor to let me do that, and this was a professor who did experimental work and was very world-famous for it. But he just looked at me and he said, "No, no, no, that's not going to work." Well, it did work, ultimately, but he didn't let me do it. I did something else under him, but I got a lot of experience at doing experiments, which was very helpful. He wasn't convinced it would work, but it did.

ROSS-NAZZAL: Well, I can see how it's been so beneficial for so many researchers over the years.

LOFGREN: Yes. It's kind of started a whole new area of ways to study things. Then the theoreticians get in and they can use some of this experimental data to then kind of make that step, to take a theoretical model and extend it beyond the parameters that you can do easily in a laboratory. But they can use the parameters that you get quantitatively in the laboratory, and then they can theoretically extend it further. It plays into a lot of different kinds of ways to study things.

ROSS-NAZZAL: One of the other items that I've picked up from your biography was you served on the NASA study of basaltic volcanism?

LOFGREN: Yes. The Lunar Planetary Institute was here in Houston. It was the Lunar Science Institute for many years, and they eventually expanded it to call it the Lunar Planetary Science Institute. They just decided that studying basaltic volcanism on all the planets would be an interesting and beneficial thing to do. We had basalts from the Moon, basalts from Earth, and we had basalts that were in meteorites. A lot of meteorites are this primitive material from which planets were made, but there was a class of meteorites where, on asteroids, these small sort of planetessimals, they're called, you did get some melting of the primitive material and you formed basaltic rocks on these asteroids. We had meteorites like that.

When this project was done, we didn't have meteorites from Mars yet or basaltic meteorites from Mars. Right about the time the project was over in 1979 and 1980, when we were writing up the final book, is when people first came out with the idea that in our collections we had basaltic meteorites from Mars. Of course, had we known that during this project, we would have studied them too, but that wasn't understood at that time.

We studied all aspects. There was a team. This was a group of probably 100 people involved in the whole project, and it was broken up into teams, and teams that studied various aspects of basalts. I was leading the team that studied basically describing the rocks and doing their chemistry, the basic characterization of basalts. There was a chapter or a group that did experiments. I did experiments too, so I contributed to that, but I was tapped to do the other kind of based on my LRL experience, et cetera. To study the experiments that people that studied the thermal environment, how you produce a lava at depth and get it to the surface, the geophysics, if you will, of that process.

There was, like, nine chapters in this book, and I can't even remember what they all are right now. Basically every aspect of basalts, there were studies of the surficial features of basalts, how you look at a planetary surface. We knew on Mars that there were basalts, and we did look at the surface of Mars from the photographs and talk about that there were basaltic rocks on Mars. It looked like there were basaltic rocks on Mercury, and as we get closer looks at Mercury, it still does look like that. From this mission that's there now, we'll get more chemical information from Mercury, and we'll know more about it. Clearly there are basalts there.

So every aspects of basalts, looking from planet to planet, that we could do. It produced a book about that big. We joked about building handles into the cover so you could carry the book because it was so big, and they decided, "Did you want it one volume or two?" It's become kind of a standard reference for basaltic rocks. From the Earth we physically got a representative suite of rocks from every major basaltic type on Earth, and then we produced a representative suite of basaltic rocks from the Moon, and got a uniform set of chemical data and experimental data on all these kinds of rocks to compare one to another. One of the nice things of the project was that all these different suites of rocks were all analyzed by the same people. We had high quality chemical data, and all the different kinds of data that were collected were all from a consistent set of analyses from the same laboratories and stuff. It was a very high quality set of data that they then used as sort of a basis for comparing to basalts for studies later.

ROSS-NAZZAL: You had mentioned meteorites, and I was curious, did you have any involvement with the Meteorite ALH-84001, that meteorite that scientists proclaimed that there may have been primitive life?

LOFGREN: I worked with a couple of guys—that was back when I still had my laboratory—and we did some experiments looking at the formation of the carbonate materials that were found in that meteorite. It was an igneous rock, coarse-grained igneous rock, probably crystallized at some depth in Mars, but it was riddled with carbonate material that clearly was not part of the original rock forming event. Something came along later, or at least the idea was that something came along later. The people who were proposing the bacterial origins said it was that this carbonate was of bacterial origin, and other people who said, "No, to us it looks inorganic."

There was some controversy. I was sort of in the camp of the inorganic origin for those. We were able to reproduce all the features in those carbonates inorganically in the laboratory, so you could come up with an inorganic origin for all those carbonate features.

That didn't negate their idea totally, because there were apparently some biological processes on Earth that could produce these things too. The issue has not been totally resolved even yet, although I think [with] the weight of the evidence, most people think that they are inorganic in origin. If there is life on Mars, that meteorite did not prove it. I think more people believe that than believe otherwise, although it's still open discussion. It's not unanimous by any means on what that rock means. People who believe that this rock's origin was inorganic still believe that there may well be life on Mars. They just didn't think that this particular rock proved that.

It's one of those controversies. There just wasn't enough evidence to be absolutely certain either way, and so it would be nice to get something else back from the kinds of rocks that we're seeing on Mars now with these two little rovers going around. These are rocks that were clearly associated with water on Mars, and if you're going to find evidence for life, these are the kinds of rocks you really want to have, but they're too fragile to ever get back to Earth in the form of a meteorite. An impact would destroy them. There is a nice hard version that would be bounced off the planet so they could go through space and finally come in through our atmosphere and land on Earth and survive. That takes a pretty tough rock to do that. The kinds of rocks that we're seeing on Mars that are likely to have life aren't that tough and would never make that trip. So if we're going to get them, we're going to have to go there and get them.

People are, of course, anxious to do that, but it's a very difficult thing to do, and very expensive. There's a joke—the Mars sample return mission always seems to be ten years away.

You get a little bit less than years, and all of a sudden something happens and it's ten years again. So they're still now talking 2020 or 2018 or even 2025 before we send a mission to Mars to try and get a Martian sample. There is the issue of the quarantine, and there the issue is probably more serious than it was with the Moon because there really is a chance, a much greater chance, certainly than there was with the Moon, of finding some evidence of either expired life or even current life. There's a camp that believes that if life was there, it's probably dead now, but maybe it was alive at one time. There's others that believe it still might be alive.

It's going to be below the surface. Life as we know it could never survive on the surface of Mars because of the radiation. It's got so little atmosphere that life based on our kinds of life would not survive that very well—although it might adapt. Life has a way of doing that. We are finding life on Earth in very extreme environments. After this idea of looking for life in extreme environments, which Mars would be, we're finding life in places that nobody ever thought there was life or never thought to look for it. So they start looking for things, and they're finding life where they never dreamed it could be or that life could actually exist in those kinds of environments. Whether that controversy was right or wrong, it has generated a lot of work here on Earth that has expanded our knowledge of the environments in which life can exist on Earth.

ROSS-NAZZAL: It's very interesting that you mention that. We went to a history conference in April, and we heard a presentation by someone in the Astrobiology Section out at Ames [Research Center, Moffett Field, California] talking about life that they had found that they never expected to find here on Earth.

LOFGREN: Yes. Ames is very big on studying extremophiles, the term for little forms of life that live in extreme environments, and they can. Very acid environments where our finger would burn up in it, but they survive in it. Things like that. Or very salty environments. Warm environments, things that seem to thrive at temperatures up to 300, 400 degrees Fahrenheit, that live in that and survive. We couldn't do that, but there are forms of life that do it. The ones that come out of the smokers in the bottom of the ocean, we found these vents where hot gases come out the bottom of the ocean. They had beautiful pictures in *National Geographic* of these things. They're way down deep, and these sulfur-rich gases and stuff come off, and the forms of life actually live off the sulfur, these little worms that live there. It's hot, but they still survive.

There's a lot. We're finding it in lots of places. We're finding life living two or three miles deep down in basaltic rocks at temperatures approaching 200 or 300 degrees. There's life there with no light, no—what's the process where you make chlorophyll?

ROSS-NAZZAL: Photosynthesis?

LOFGREN: Photosynthesis. They don't rely on the sun for energy, anything. It's heat or it's sulfur or it's other elements. That goes back to actually understanding the origin of life on Earth now. The idea is that early forms of life on this planet lived in environments in which they could not survive today. They lived in very reducing, oxygen-free environments, very warm, very acid, very iron-rich. Those forms of life could not exist in the way the planet has evolved today, but they lived that way for probably a one or two billion years in those kinds of environments.

Oxygen didn't become plentiful on Earth for a long time. It was about two or three billion years before oxygen started to become plentiful on Earth that we could even survive. It's probably in the last three or four hundred million years that oxygen's been around at levels where we could survive, and then that goes up and down too. There are animals from two hundred million years ago that were surviving on levels of oxygen half or a quarter of what we have today in our atmosphere and survived. They adapted to that.

ROSS-NAZZAL: Tell us about your work on chondrules and meteorites.

LOFGREN: Well, chondrules are a kind of—I'm trying to think of a general word to call them. The original classic concept is that they were little molten spheres that then crystallized in the solar nebula. Here, I get controversy already because people don't even believe that. The most common origin for chondrules is that they formed in a dusty solar nebula before there was planets, and through some process that I have to say even today is not well-understood, they would melt, and you would form droplets of liquid silicate melt on the order of a millimeter up to a half a centimeter. We're talking about things that are probably that big and maybe up to this big in diameter mostly. [Demonstrates] There's a class of meteorites called chondrites, and they're full of chondrules. Chondrites can be as much as 80, 90 percent chondrules, just littered with these little round spheres or broken pieces of round spheres.

So these spheres form in the nebula in many people's idea, and then they aggregate together by running into each other and sticking together, and form larger bodies. Basically, the planets are made of chondrules that accreted to form ever-larger, increasing things to where you could form planets, from starting out just little things, and eventually you get planets. Some of them became viable planets and some of them didn't. The ones that we see today are the ones that were viable and survived, and some planets didn't survive and collided, broke up, and disappeared.

The primitive meteorites, which is a primitive material in our solar system. After the Big Bang, you started condensing dust. The composition of that material is the primitive composition from which the planets are made, and that primitive material formed ever-larger pieces by the process of accretion until you formed planets. Chondrules were an early form of that in our solar system, where things were accreting and they were melted. The process is far more complex than that. I don't have time to give a lecture on chondrules, but chondrules are largely something that crystallized from melts.

So I studied melts, and I crystallized melts. I was already duplicating a lot of the textures you could see in chondrules even in the studies on lunar samples and other kinds of samples. I was able to reproduce textures that resembled chondrules as well, so they were a natural thing for me to go on and study after I had done what I could do with lunar basaltic samples. I spent twenty years studying chondrules and formation of chondrules. Basically, you do have these nice droplets with these crystallization textures that are fairly rapid and fairly unique and easily identified. But really, the majority of chondrules don't look like that when you start really looking carefully at them.

That's where the complexity comes in, and trying to understand the whole process and not just this smallish group. Maybe 15 percent of chondrules have these textures where they totally did crystallize from a molten droplet, and others apparently were some process in between where maybe this body of material partially melted and then crystallized again. A lot of the crystals that are there were there to begin with, and then they got bigger or they changed character. That's where a lot of the complexities in understanding chondrules comes from and understanding the whole process then allows you to understand all those variations.

One of the other prominent ideas is that chondrules come from impacts on a planetary body. Early in the solar system, some people believe that all these chondrules formed when you had an impact, and you melted some portion of that body during an impact. Droplets of melt would get up in the air and crystallize, and that's how chondrules form. Some may form that way. You can form chondrule-like bodies that way; we see spheres with chondrule-like textures on the Moon that clearly did form that way, but there are clearly chondrules in rocks that were never part or never on a body where that could happen. They're coated with primitive dust material from the solar nebula after they formed these droplets. They clearly were never on a planetary body where they could form by this impact process.

There's probably both kinds. My feeling is that the majority of the chondrules we see in chondrites are from the nebula and do not form on planetary surfaces, but clearly some do. So it becomes a very complex topic; in fact, people have been studying chondrules since the middle 1800s when they were first recognized in meteorites and recognized for what they were, as crystallized droplets of melt. People have been trying to understand them since then, and there are still things we don't understand. The process by which they heat up in the nebula hot enough to melt little droplets is still not clear.

There's still three or four or five different proposed mechanisms, any one of which could work to some degree. Some may work better than others. More than one of them may actually produce these things. We just don't know for sure because the nebula is not a place where you can go out and watch the process happen. The nebula's already done its thing. You've have to go to some other nebula that's in an early stage before planets form to actually see the process in action, so it's not something you can witness. The meteorites that fall to Earth are the only evidence we have of that process.

ROSS-NAZZAL: Were you working with any other Centers on this?

LOFGREN: No, not really. I was pretty much alone in the world on that process. I had some students that became converts, and other people have. No, it was kind of a small group, even internationally, that does this kind of thing.

ROSS-NAZZAL: So again, another sort of pioneering field?

LOFGREN: Yes.

ROSS-NAZZAL: Yes. (laughter) I can say it. You don't have to say it. I think this might be a good place for us to stop, and then next time we can talk about going back and looking at lunar samples, and your work as lunar curator, and things like that.

[End of interview]