ORAL HISTORY TRANSCRIPT

MICHAEL B. DUKE
INTERVIEWED BY CAROL BUTLER
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BUTLER: Today is October 13, 1999. This oral history with Dr. Michael Duke is being conducted at the Lunar and Planetary Institute in Houston, Texas, for the Johnson Space Center Oral History Project. Carol Butler is the interviewer, assisted by Kevin Rusnak and Sandra Johnson.

Thank you very much for joining us today.

DUKE: Well, thank you for coming.

BUTLER: It's our pleasure. To begin with, if you might tell us how you became interested in getting involved with geology and then planetary geology.

DUKE: Well, sort of from the beginning of my life, so to speak. I think a formative experience that I had when I was growing up was to be fortunate to be able to live in the Owens Valley in California, which is on the east side of the Sierra Nevadas. We lived at a place where you could just walk up into the mountains. So at a reasonably early age I saw a lot of rocks.

When I got to college at Cal Tech [California Institute of Technology, Pasadena, California] after finishing high school, I thought I was going to be a chemist, but in my freshman year I met a fellow who I have known ever since, Harrison [H. “Jack”] Schmitt, who was an astronaut eventually on the Apollo 17 mission. But we were both freshmen at Cal Tech together. One day he came and said, "Come over to the Geology Club and see what's going on there." So I went with him to a Geology Club meeting, and I rediscovered that I was really interested in rocks and not in test tubes. So that was a start. Jack and I have been friends ever since, and laugh about that a bit from time to time. But that's where it all got started.

I got my degree in geology at Cal Tech and went off for a year to Penn [Pennsylvania] State University [Philadelphia, Pennsylvania], then came back to Cal Tech to finish my graduate studies, and received a master's degree in 1961 and a Ph.D. in 1963.

During the year that I was at Penn State, Sputnik was launched, and then the first U.S. space missions. That was all very exciting. I remember driving across country in the winter of 1957, I guess it was, when we were about to launch our first rocket, and just listening for hours on the radio to the descriptions of what was going on.

Space, in general, had fascinated me, but when I got into graduate school, I met Professor Leon [T.] Silver, who became my graduate advisor. One day he said, "Well, you're not doing anything right now. Why don't you help me look at this meteorite that we recovered from Mexico a few years ago." And this is a fascinating meteorite because Dr. Patterson, Clare Patterson, who is also on the staff at Cal Tech, had just the year before, or two years before, done some analysis which demonstrated that this meteorite had formed 4.7 billion years ago, right at the start of the solar system, and that was, at the time, the best age determination for the age of the solar system. He said that he did this analysis, "But we don't know what kind of rock this is, and why it should be this way, so why don't you spend some time and study it." So I got drawn into the study of meteorites and eventually did my Ph.D. thesis on the study of meteorites.

When I was in my last year of graduate school at Cal Tech, [Dr.] Eugene Shoemaker came to the institute and served as a visiting faculty member, and we got to know one another. He was in the process of establishing what he called the branch of astrogeology at the USGS [United States Geological Survey]. He had established an organization in Menlo Park [California] with the U.S. Geological Survey, and he and some of his colleagues were doing the first really good work on mapping the moon, using telescopic photographs and then ultimately putting information from the Ranger and Surveyor landers into the data set. So he had created the branch of astrogeology, and when I graduated, he offered me a job.

So I went to work for the USGS and the Geological Survey. I actually went to work in Washington, D.C., where there was a branch office, and there began to work with Dr. Edward Chao, who was, of course, also a colleague of Shoemaker's. He and Shoemaker had found the first evidence of very high-pressure minerals formed by the impact at Meteor Crater, Arizona. They had discovered that some of the rocks in the target, what they call the target material, the stuff that had been hit by the meteorite, thrown out of the crater, contained materials that had been formed at very high pressures and high temperatures, and were totally unknown previously on the earth.

So I went back and did mineralogic studies with Chao in Washington, D.C., and along the way became interested in the problem of whether you could collect cosmic dust in the atmosphere. We had some early opportunities to use sounding rockets to fly up into the atmosphere and try to collect dust that might be filtering down from space. So I was busy learning how to analyze little dust particles, and we built a clean laboratory capable of doing research and study very tiny particles, particles that are in the micrometer range, very tiny microscopic particles. We established the laboratory and learned how to analyze these things.

So in 1967 or so, the Apollo Program was getting heated up; that is, the preparation for the Apollo Program was getting heated up. The scientific community was called together to advise on the organization of the science activities. There were two major workshops. Actually, the first one was in 1965 and was held at Santa Cruz—excuse me—Woods Hole [Oceanographic Institute, Massachusetts]. Then the second one was in 1967, it was at Santa Cruz. These were both published as NASA special reports, special papers, whatever they call it. They turned out to be very influential conferences and reports in defining the procedures to be used when we actually recovered samples from the moon and talked about how we should go about collecting them and so forth.

I was invited to participate in those meetings, but I was not yet a real member of the club. But around that time I proposed to the initial Apollo Lunar Sample Program, which was established to bring people from the scientific community into the program to do the analysis of the first rocks and dust brought back from the moon, so I proposed, based on the capability that I had been developing in the laboratory in Washington. I and my team were accepted into the program, so in 1968, 1969, we spent a lot of time preparing for the study of the first lunar samples. We received tiny little bits of lunar dust and studied them in great detail, grain by grain, to try to understand what the origin and the nature of this fine-grained stuff on the surface of the moon was.

So that brought us to 1970, after the first Apollo mission, Apollo 11. We got our samples, we studied them, and then we had a big conference in Houston, in the winter in Houston. It was a very uncomfortable conference, as I recollect. It was down in the old Conference Center in downtown Houston. But there were about 1,000 people there from all over the world, and everybody excited about the new moon rocks and dust that they had collected.

So I gave my paper, and after the first day of the conference, I was approached by a couple of folks, Paul Gast, who was at that time the chief of the Solar System Exploration Division at Johnson Space Center, and Gerry [Gerald] Wasserberg, who had been a faculty member at Cal Tech when I was in graduate school and I knew pretty well. Both Paul Gast and Gerry Wasserberg were very deeply involved in the JSC's [Johnson Space Center, known at the time as the Manned Spacecraft Center] getting ready for the first lunar sample return. They were interested. They were scientists. Their prime interest was in the analysis of the samples, to learn what they could from them, but they were also concerned that NASA was not going to do the right sort of thing to take care of the samples, to preserve them, to make sure that they were available for the use of future scientists and so forth.

So they approached me at the time and said, "NASA is looking for a curator of the lunar sample collection." The first curator, Elbert [A.] King [Jr.], had resigned from NASA and gone to University of Houston just before Apollo 11, and so they quickly got together a pickup team from the people who were working at JSC in the science area, and they needed to replace that person. So eventually I said, "Yes, I'd like to do that," and that really solidified my relationship to NASA and to the lunar sample program.

So that's sort of how I got into geology and into rocks and science and the moon.

BUTLER: That's a great introduction.

DUKE: It was obviously a very exciting time, and I really had to think about a second and a half before I made the decision to come to Houston.

BUTLER: Quite an opportunity.

DUKE: Yes.

BUTLER: You mentioned Sputnik and following that closely. When did you first hear about the commitment to a manned lunar program? At the time, did you think about the geological applications of that? Or did it all just kind of—

DUKE: Well, it all just sort of flowed, but I guess it probably was not until President [John F.] Kennedy's speech that I really thought seriously about people going to the moon. Before that, though, we had been conducting unmanned probes of the moon—the Ranger Program and the Surveyor Program. At that time I was in graduate school at Cal Tech, and JPL [Jet Propulsion Laboratory, Pasadena, California] was running the unmanned lunar program, so I was connected with that and I was even at that time trying to figure out what the relationship of meteorites and the moon were.

In fact, in my Ph.D. thesis, I concluded that it was likely that the peculiar class of meteorites that I'd been studying had actually been blown off the moon by other meteorites. To me, this set of meteorites looked like you would expect material that came from the moon to look like. That's a really interesting story, because, of course, when I made this interpretation and speculation, I was almost immediately shown wrong, because we got samples back from the moon. Thankfully, most of the scientific community has forgotten what I said then. [Laughter]

It was thought for many years that it was virtually impossible for a rock knocked off the moon to actually survive and come to earth, but in the last five or six years, we have been discovering meteorites in Antarctica that are pieces of the moon. So we now know that it is possible to knock pieces of rock off the moon and actually from Mars as well, and collect them here on earth. So it's another interesting cycle of speculating that something happened, demonstrating that you're not right, and then finding the same thing out in a totally different way.

BUTLER: You had the right idea, had the wrong rocks. [Laughter]

DUKE: Wrong rocks. [Laughter]

BUTLER: As you were working, you mentioned, with the USGS and looking at the cosmic dust and then proposed to become involved with the Apollo 11 team, there were a lot of discussions going on about the moon and the lunar surface and what it would be like and what it would be made of, and the history of the formation. How much into that had you gotten at the time, or were you—

DUKE: I was mostly interested in the chemical composition, the rocks that were present there. I had become convinced that the rocks of the moon were volcanic rocks, basically volcanic rocks, and that's why I drew the connection with the meteorites, which had very similar kinds of characteristics.

Then Shoemaker had published a number of papers. He and the group at the USGS had published a number of papers that described the mechanisms that would work on the lunar surface to pulverize and reduce the rocks to this dust. We knew before we landed on Apollo that the lunar surface was covered by quite thick layers of ground-up material, and so in a sense it wasn't very surprising to me when we discovered that there were little chunks of volcanic rock in very fine-grained dust and glass that was made by micrometeorite impact.

So in a way, the initial samples were not real surprising, because it seemed logical that they would be the way they turned out to be. So then we went on to get more quantitative data, more accurate, precise data on just what had been happening, what the rocks were, what their ages were, how long they had been there, and so forth.

BUTLER: After having had your proposal accepted for working on the Apollo 11 samples, I'm assuming you probably followed the mission pretty closely. Do you remember where you were and what you were doing when they actually landed on the moon?

DUKE: Yes. Well, I was actually in Milwaukee, Wisconsin. My wife and I had gone for our summer vacation and were staying with my uncle and aunt there. We all gathered around television, stayed up late and watched the first moon landing. It was one of the things that I remember of that sort.

BUTLER: Even before they landed on the moon for Apollo 11, they began selecting some astronauts, scientist astronauts, including your friend Harrison Schmitt. Had you considered at all applying for that?

DUKE: Yes. In 1965—1964, actually, Gene Shoemaker spent some time lobbying with the National Academy of Sciences and with NASA to create what was called the Scientist Astronaut Program. Finally NASA had an announcement, I think in early 1965, that they would select some astronauts from the scientific community. Jack Schmitt and I were both working for Shoemaker at the time. Schmitt was working in Flagstaff [Arizona] for the Geological Survey. Of course, Shoemaker encouraged all of us to apply to the program, and I think there must have been eight or ten people from the U.S. Geological Survey that applied to the Scientist Astronaut Program. Then after doing that, he, I think, turned out to be the chairman of the evaluation panel as well. I can't verify that, but he was surely influential in the selection of candidates.

So, actually, in the last group of, I think, twelve candidates, Jack Schmitt and another guy from the Geological Survey named Dan Milton and I were the candidates. We were actually nominated by the Academy of Science to be scientist astronauts. So we all got invited to come to Brooks Air Force Base in San Antonio for some tests and evaluations. We spent a week at Brooks going through all sorts of physical and psychological testing, and at the end of that time they selected five candidates, I think, and I wasn't one of them and Jack wasn't one of them. None of them were geologists. So Shoemaker went back and raised a big stink with NASA.

Turned out that both Jack and I had been eliminated because of physical problems, and mine was eyes, and Jack's was a more serious problem, but it turned out that his problem could be corrected, so they lobbied to have NASA reconsider him on the basis that he could correct his physical disability, which he went ahead and did, and became an astronaut. So I was reasonably close to becoming a scientist astronaut myself, and I was crushed at the time, but you do what you do. Jack went on to be a very successful astronaut and has done a number of other things as well.

BUTLER: Even though it was disappointing for you, it must have been good to see that he had made it into the program.

DUKE: Yes.

BUTLER: Especially going to the moon and have geologists, it would be rather important.

DUKE: Yes. That was the whole idea. So he was only geologist that went to the moon, and the only scientist astronaut that went to the moon. The other people in that group of scientist astronauts included Joe [Joseph P.] Kerwin, Ed [Edward G.] Gibson, Owen [K.] Garriott, and some others that I can't remember right now [Duane F. Graveline and F. Curtis Michel]. But, you know, NASA probably was not real happy to have scientists as part of the astronaut corps. They agreed to do that, but they were really concerned about safety.

The NASA management always considered the moon trips very, very dangerous, and they were not anxious to have some sort of accident occur. So the scientists were not trained test pilots, which all the other astronauts were, and that was considered to give enough of an edge in dangerous situations that it was better to select a test pilot than a scientist. But it all worked out in the end, and I think it did a lot for science in the sense of getting people in the scientific community really involved in the Apollo Program and then Skylab and programs that came after that.

BUTLER: They were eventually able to come to a pretty good cooperation.

DUKE: Yes, that's another thing that sticks in my mind. Throughout the Apollo Program and in most NASA programs, the engineers are in charge. The engineers are the ones that make the systems work, and they really have to look carefully at every little angle that is important to the mission. The scientists typically recognized that, but still chafed a lot at the restrictions and maybe what looked like lack of respect sometimes to the scientists, that they couldn't get their views heard by the engineers, and when they could get listened to, there was this long, complex system of reviews and reports and discussions that had to go on before even the smallest thing was changed.

So during the Apollo Program, there got to be quite a conflict between the scientists and the engineers. At least on the surface there appeared to be. As we progressed from the Apollo 11 mission to the later Apollo missions and the engineers got a little bit more comfortable with the missions, the scientists were able to play a larger part. They were, of course, involved in the actual mission operations helping to plan what the astronauts would do and where they could go and what kinds of samples they would collect and what kind of tools they would use and that sort of thing. But later on in the Apollo Program, the engineers even offered some opportunities for doing some new experiments.

By the end of the program, there was quite a lot of respect among the scientific community for the way in which the engineers pulled this all off. I remember a real highlight of those days was a party, actually, that was held after Apollo 17, which was explicitly set up to invite a bunch of the key engineers on Apollo and a lot of the key scientists together. It was quite an emotional kind of event where the scientists essentially got up and thanked the engineers for doing what they did. I think it was a memorable event.

BUTLER: That's great. That's great to see that it all came together so well, and from the missions they came across very well as well.

DUKE: Yes.

BUTLER: When you were invited to become the curator at the Lunar Receiving Laboratory [LRL], and you said you didn't hesitate long to accept that, obviously it was a great opportunity, as you came into it, what had already been done and then what did you need to do for the other missions?

DUKE: Well, I said that Gast and Wasserberg had been concerned about the preparation that had been made by NASA for the samples. It turned out they were right, that they had started sort of late to get facilities prepared, and the ability to work with the samples was not in real good shape. I did not come until after Apollo 13, in the middle of 1970, and what had happened was that in July [1969] there had been the Apollo 11 mission, and I think it must have been October or something like that [November 1969], that Apollo 12 was flown. So there were two Apollo missions flown at about three months apart.

The amount of material that was brought back by Apollo 11 was not immense, but the system in which it had to be handled was quite complex because of the quarantine requirements. They built the whole Lunar Receiving Laboratory. Building 37 was the Lunar Receiving Laboratory, and it was a hotel for the astronauts. It was a biological study laboratory to see whether there was anything harmful in the lunar materials, and it was a geology laboratory to actually analyze the rocks.

There were quite extensive facilities set up for the Lunar Receiving Laboratory part of it, and there was almost nothing, very limited place to put all the samples after all of that had gone on. So the whole idea of the curatorial facility, it all fit into a room like this, whereas before that everything had been spread out in tens of feet of cabinets in the rest of the LRL.

So the issue was, after doing all this initial analysis in the Lunar Receiving Laboratory, the samples had to be taken out into this little cubbyhole and that was where the samples had to be prepared for the scientific analysis. So in this little cubbyhole they were taking scoops of soil and putting them into containers and weighing them and sealing them up in plastic bags, Teflon bags, and so forth.

There were two of three people working. There were 100 scientific teams waiting to study the samples, and it just got sort of gridlocked. That happened for Apollo 11, but then following right on it, the same people had to go over and handle the samples from Apollo 12. So by the time I got here, things were pretty tight. There was not a very big crew of people. They were in cramped space and they were not serving the scientific community the way the scientific community felt they should be, and they were presenting significant hazard to the samples. As it turned out, a lot of—maybe not terribly crucial, but a lot of the information was being lost. So there was a pretty bad situation as far as the organization of what we called the curatorial facility.

Well, fortunately, from the point of view of the samples, it was another year before the Apollo 14 mission was flown, so we had a chance to at least partly recover from that and expand the space that was available in Building 37. But it wasn't until—let's see what we did. The samples underwent this quarantine control up to and including the Apollo 14 mission, and after Apollo 14, I'm not sure I remember exactly when, we significantly expanded the curatorial facilities by renovating an area on the second floor of Building 31, which was right across the parking lot from Building 37. So after Apollo 14, we moved all of the samples from the previous missions over to Building 31. We still used the Lunar Receiving Laboratory for the initial receipt and analysis of the samples, but then at the end of the mission we'd take them out of the Lunar Receiving Laboratory and transfer them over to Building 31.

So there were several things that had to be done in 1970 when I first came. There were a series of development of laboratories and facilities for handling the samples. There was also a need for procedures. As we were able to add facilities, we were also able to add some people. Mostly these were technician people who we expected to work on the samples, and most of them had no geological training, they were just competent mechanical technicians. So we felt that we had to put together a series of very specific protocols describing how to handle the samples and what the procedures should be, so we developed those and got them going so we could document everything that happened to the samples in a systematic way.

We developed the data handling capabilities and that sort of thing. We also developed some new hardware, some new tools. We discovered—it had been known from the very beginning—we discovered that you couldn't weigh a sample. All of the samples were maintained in these nitrogen-filled cabinets with gloves that you reached in and handled the samples with. What we discovered was that all of the sample weights that were taken with balances that were inside these cabinets were either wrong or not very precise. So you couldn't account for all of the samples in the detail that the sample security procedures that we wanted to impose required.

We were essentially under very strict security procedures that said you can't let any of this material out into private hands. There is always a great fear that some of this would somehow end up on a black market for lunar samples, having escaped from the Lunar Receiving Laboratory. But we were doing lots of things to the samples. We were sawing rocks. We had saws that could actually cut through solid rock, and when you do that, you create a lot of sawdust. What you would like to be able to do is weigh the sample before you started and then weigh the pieces after you were done, and weigh the sawdust that had been made.

If you've done everything right and were able to get that back together, you could reconstruct it, and you would know within some limits that somebody hadn't taken away a piece while you were doing that. So you could keep track of it. But we found that the balances weren't precise enough to do that. We had to find a balance that would work in that environment.

We also developed procedures for sawing rocks. We had to develop new rocks. Rock saws are not difficult technology. People do that all the time. But they don't do that in conditions where you can't use water or some cooling agent on the rocks. Whenever you see a rock being sawed in the laboratory or in a quarry, they're always dumping huge amounts of water on it to cool the saw blade as it cuts through the rock. But we couldn't do that because the lunar samples would react with the water, so you didn't want to expose them. So we developed some saws. We developed a band saw that was actually a converted meat cutter saw, and used a diamond blade to cut the rocks. That worked well.

We also developed techniques for opening up and looking at the core samples that were obtained ever since the first mission, but on Apollo 15, 16, and 17 we had this three-meter drill core, which was made of—it's about this round and fairly thick walls and was made of titanium, very hard metal. It turned out that nobody had thought real seriously about how we were going to get the samples out of the core when the cores were designed. It was thought maybe I suppose you could push out the cores or something.

It turned out that when you drilled these cores into the surface of the moon, the grains got very tightly compacted and there was no way you could do anything with respect to pushing it out. So finally we developed a process where we actually used a milling machine to actually cut a very thin groove on each side of the core, laid horizontally, used the milling machine to cut the groove, and then we could sort of peel off the top of the half of the tube from the bottom, leaving this rounded compressed soil there. Then we'd go through and dissect, millimeter by millimeter, the material along the length of the tube.

So those were techniques and procedures that had to be developed. All of that happened between 1970 and 1971, or maybe beginning of 1972. So there was quite a lot going on.

It turned out that the initial failures and the problem of cramped space came back to bite us later on in probably 1972. It was after the Apollo 15 mission. The NASA management got really intent on demonstrating that we were not losing any moon rock and it wasn't being stolen and we could account for everything we had.

So we had a big investigation, and we found that we just could not trace the records from the Apollo 11 and pretty much the Apollo 12 mission. It caused us a lot of grief and sleepless nights and counting and recounting and weighing and reweighing, and looking for records in all sorts of cubbyholes where they had eventually gotten to.

We reconciled a lot of it, but never could really account for everything that we thought had been there in the beginning. Of course, it might not have been weighed precisely either. So I think that, in retrospect, the preparation before Apollo 11 was not adequate.

We are now talking just now about bringing samples back from Mars for the first time and putting them into a facility which will have the same sort of function as the Lunar Receiving Laboratory did. It'll handle the quarantine of the rocks, so to speak, the biological testing and the geological analysis, and hopefully some of those lessons that we learned thirty years ago will be applied to the Mars samples.

BUTLER: With the Mars samples, there's even more question about the water and possible life and so forth.

DUKE: Right. And even just from the point of view of accountability and can you demonstrate that you have retained all of the samples, in the lunar program we required that each of the samples that went to a principal investigator for scientific study be totally accounted for. Everything that wasn't consumed in some irretrievable manner was returnable to the archives, and the investigator was required to account for it and could be sanctioned if he lost it or lost track of it. So we were requiring all of the investigators to do that, but we couldn't do that ourselves. So that's something that was just not right. It could have been fixed if a little bit more thought that been put into it before the mission.

BUTLER: As you were developing these procedures and the tools and trying to make all this work, did you go to the scientific community a lot for ideas or did you build off of a team that you had there?

DUKE: We couldn't avoid interacting with scientific community. As I said, back in the late sixties the scientific community had been very important in helping NASA develop the overall processes that would be used in communicating between NASA and the scientific community. For the Apollo Program, there were two principal scientific groups that worked with NASA. One was called the preliminary examination team, which was commissioned by NASA and consisted of people from both inside NASA and outside of NASA, who came together to do the initial analysis of the samples while they were still in the Lunar Receiving Laboratory, while they were still in quarantine. There were some analysis and observation that could be done. So an initial catalog of the samples from each mission was put together by the preliminary examination team.

Possibly the more important group was a group called the Lunar Sample Analysis Planning Team, LSAPT, who, again, was a group of ten or twelve scientists representing a broad range of disciplines that was specifically called together to decide how the lunar samples should be distributed for study to the scientific community. That is, here you had a collection with 100 different kind of samples and you had 100 different scientific teams who wanted some of that sample to analyze. How do you bring those two things together?

So this group would understand what each of the scientific teams wanted to do, and would try to understand what was in the sample collection, and would designate what parts of the sample collection were to be provided to the scientists for analysis. That was their principal duty, but they also took it upon themselves to advise on other things—cleanliness, processes, equipment, procedures, and all of that. So everything we did was discussed very thoroughly with the scientific community.

In thinking about it, most of the actual new ideas for how to do things came from internally, but they were done many times because the scientific community had recognized a problem or, in other cases, we would just take a new idea and test it out on the scientific community and the LSAPT group, and get their advice.

Then we would be monitored by them as well. They would come and see how it was working by going into the laboratory. They would do inspections and such, give us advice and action items as things that we should fix and so forth. There was very close working relationship.

BUTLER: Sounds like you had a good pool to build from, then, from both inside and outside.

DUKE: Yes. There was a lot of tension. There were some really trying times. But it actually worked pretty well.

BUTLER: As you were working with getting all of this organized and getting the curatorial facilities set up, what involvement, if any, did you have with the astronauts who were preparing for the later missions? Did you have any of them coming and looking at moon samples or anything?

DUKE: From time to time the astronauts would come around and look. Apollo 14 was the only one that I fully participated in, where there was still a quarantine, and we had interaction with them in the Lunar Receiving Laboratory, used to try to add information to what they had recorded or talked about during the mission.

For Apollo 14, that wasn't terribly successful, not that they weren't interested in helping, but my impression is that the experience on the moon was so intense that in some ways it just made everything a blur, that they really were not very good at remembering details after they got back. So you couldn't ask them a question like, "Do you remember picking up that rock? Why did you do that?" It was just a blur to them by that time.

Some of the astronauts took a great interest in the samples afterwards and even wrote technical papers on their missions. Some of them didn't take all that great interest in the science afterwards.

BUTLER: I'm sure Harrison Schmitt was one of the ones that did take a good deal of interest.

DUKE: Yes. John Young, of course, has been great. He's been a devotee of the moon ever since he was there, and has interacted over the years with the scientists and science community. I have a great deal of respect for John.

BUTLER: That's great. And he's still so involved over at JSC.

DUKE: You bet.

BUTLER: It's good to see him interested in the science aspects. Did you have any contribution or any involvement at all in looking at some of the lunar landing sites, any of the selection in that?

DUKE: No, I didn't participate in that part of the process. That had been going on since the mid sixties. At that Woods Hole conference, already there was discussion of the landing sites, and the Geological Survey organization in which I worked was actually developing the maps and identifying potential landing sites and was, in fact, involved in astronaut training as well for the missions. But I didn't take part in that.

BUTLER: During the missions, while you were curator, what did you do at that time? Did you follow the missions closely, go to Mission Control at all? Or did you just wait for the samples to come back?

DUKE: We're usually in a panic mode, because there were always things that weren't quite ready for the samples, so we were always busy doing the last-minute details, waiting to receive the samples. We followed the people that were actually in the back room in the Mission Control Center, where a small group of people, they'd mostly been involved in training the crews and in doing the site science strategy planning. So they were—I don't remember the name of that team, but they were closely involved.

We, of course, watched all of the EVAs on television to the extent we could and listened to the discussions between the crew and the Mission Control Center, and just generally tried to understand what was going on and what we were going to expect to see when they got back.

BUTLER: When they did get back and you were looking at the samples and organizing them and processing them and so forth, were there any surprises?

DUKE: Well, not really. There were some samples that had been described on the lunar surface that everybody was really interested in seeing, and two that come to mind were the white rock on Apollo 15 which Dave Scott picked up and immediately named the Genesis Rock. It turned out to be a white rock, very old, so he had called that one pretty correctly.

The other one I remember was when Jack Schmitt stumbled over the orange soil and got all excited about that. We, of course, were all waiting to see whether it was really orange when we looked at it in the laboratory.

I think the things that concerned us when we opened the samples was how coherent they were, because the crews pick up a sample on the surface of the moon and put it in a bag, you'd open the bag at the other end and sometimes it was a rock, but sometimes it could be just sort of a pile of dust because the rock they had picked up was not all that coherent. Then you immediately set to work trying to piece together the pieces and see whether it really was the sample that they picked up on the surface. So just detailed handling kinds of things were of immediate interest.

The samples all came back very dusty, of course, so you couldn't do a lot when you first saw them, except to look at their shape. Later on, we developed ways of blowing off the surface dust so that you could look at them. But even so, the samples exposed to the lunar surface were somewhat altered by dust, micrometeorite impacts, things of that sort.

In addition, most geologists don't study rocks much anymore just by looking at them. You need to make a microscope slide, a thin section, get some sample and analyze it in a machine of some sort before you're really willing to say what your interpretation is of that sample. So just looking at the samples is not a major part of the analysis.

What we would do is, many of the samples came back in individual Teflon bags, sample bags, and, one by one, those would be taken out and weighed and opened up. You'd weigh the bag and then you'd open it up and you'd take out the sample and you'd weigh the sample. Then you'd weigh the dust that was in the bag. You'd again try to make a balance between what you thought you had when you started and what you ended up with. Then you would photograph each of the rocks. Some of the soil samples were then sieved so that you took out the rock pebbles that were in the soil, because they could be studied separately.

So those very mechanical kinds of things went on in the laboratory, and they went on for days and days and days. There were lots of samples, and the processing of the samples was quite slow because we were trying to take great care, not lose anything, and not destroy anything particularly for some of the samples that were not as coherent as others. Then a lot of effort was put into the processing of the core samples. That was something that when you opened up a segment of the core, which would be a segment that long or so [Duke gestures], you could literally take a month to do all of the subsampling of that sample. So these were not things that were done in a short period of time; they took days and days.

BUTLER: Were the investigators who wanted to look at the samples later, I'm sure they understood most of the process you would go through, but I'm also sure that they would send their requests in pretty quickly.

DUKE: The process was to send in the requests beforehand and then the LSAPT had the authority to suggest what samples should be given to each investigator, with a rationale for why each one would be given. Then that would take, actually, for the later missions, probably ten days of effort by the team to understand all the samples and make an allocation plan, and then the allocation plan had to be approved by NASA Headquarters. So it was sent up to NASA Headquarters. There was an approval cycle.

When everything had been approved, then the PIs would be notified as to what their sample allocation was, and I think everybody accepted the initial allocation of samples. It was clear that if a PI identified a sample that he really wanted to study, that hadn't been given to him, he had the right to request a piece of that sample. The Lunar Sample Program is still going on, with people requesting samples that they think could answer a particular problem that they're working on. They make the sample request based on their own ideas, but also on all of the other information that has been gained on the sample collection. So as long as these people were approved to be in the program, they had the authority to make requests. Then the LSAPT would review the requests and approve or recommend something different.

There were very few cases in which requests were ultimately denied, but the LSAPT and the curator could put significant barriers in the way of a PI getting a particular sample that he or she wanted to investigate. All of that to try to conserve the sample, to make sure that the same analysis wasn't duplicated by more than one person, because he wanted to preserve as much sample as possible and so forth.

BUTLER: Were there any requests or proposals for evaluation that came as a surprise, that were very forward-thinking, like someone hadn't thought of taking this approach before, or anything along those lines?

DUKE: There were a lot of unique analyses and analytical approaches that were developed by people working in the program. It was a period in which there were very rapid advances being made in mass spectrometry and in chemical analysis and in microanalysis. I don't recollect any instance in which the nature of the sample was what drove the development of the technique, but it was surely a time where there was a lot of new technique being developed by the community.

I guess the one thing that might be in that category is the way in which the cores were handled. That was something that really people hadn't worried about until after they realized what these core samples were and that they required special kinds of processing.

BUTLER: Sounds like a good time for learning a lot, then, both technique-wise and about the moon.

DUKE: Yes. One of the things we had to do with the core is figure out how you could keep some of it intact. I mean, this is really just dust. It's lying on the bottom of a tube, and it would fine as long as it were left in one place and not moved or jiggled or tilted or something, but it would be very easy to change the nature of the core by handling it.

So we invented a process whereby most of the core was just scooped out millimeter by millimeter and put into little vials, which are hundreds and hundreds of vials from each core, with material from each little interval. But then there would be some material left in the bottom of this core tube, and we developed a way of encapsulating that, of essentially pouring epoxy on it and into it so that the original structure would be preserved. So we now have a collection of—there's like thirty or forty of these encapsulated cores which are available for study. I don't know that anybody ever has studied them, but they are available for study.

BUTLER: And that's probably something they would have to come here to do?

DUKE: Well, I suppose. It turned out that people thought beforehand that there would be a lot of obvious layering in the regolith, the broken-up stuff at the lunar surface, that you would be able to see layers of debris from impact craters or from other things. So a lot of effort went into preserving what they call stratigraphy, the stratigraphic relationships. But it turned out that after intense study, it proved that there was not a lot of obvious layering in the cores, and so that did not become a major research area. So I suppose if you wanted to study that, you would just make cuts of these cores with a saw and turn them into microscope sections and study them that way, but I don't think anybody has ever done that. The reason to do it seemed to vanish just because the surface dust is not obviously layered as it was assumed to have been.

BUTLER: I guess that in itself was a learning process that you wouldn't have known that without the cores.

DUKE: Learn stuff. Yes.

BUTLER: As you came to the end of the Apollo Program then, at what point did you then move into—you later became chief of the Solar System Exploration. Did you stay curator for a while after the program ended and then move over?

DUKE: Well, like I said, the program hasn't ended yet.

BUTLER: That's true.

DUKE: There's still a curator, and the Lunar Sample Program continues, but we've added other sample collections. So after the Apollo flight missions ended, we still had a large amount of work. We went through the analysis of the Apollo 17 mission rocks, but then it was clear that there were a lot of samples from the previous missions that hadn't been properly studied, and it was a time when a lot of people went back and looked at what they had done and said, "Here we could make progress if we study this sample." So the period of intense study continued for at least three or four years.

This sequence of things that happened was that Paul Gast was the chief of the division during the Apollo missions and he, unfortunately, passed away. It's hard to remember when. In 1973, I guess, or thereabouts. One of the people from the scientific community, Dr. Larry [A.] Haskin, was appointed to be the chief of the division, to follow Gast. Larry had the position for a couple of years, two and a half years, maybe, and decided that being a NASA bureaucrat was not cut out for him, and so he went back to the university, and he's now at Washington University in St. Louis [Missouri], and he's been a continuing contributor to lunar science throughout his career.

So there was a vacancy. I was curator. We looked around for somebody from the outside for a while and didn't find anybody that suited us to fill Haskin's position, and eventually I was chosen to do that. So it was sort of an evolution, a promotion, if you will, within the JSC organization.

BUTLER: You mentioned that he didn't like being a NASA bureaucrat. Were you able to balance your geologic and scientific interest with the management?

DUKE: Well, being the curator was almost totally a management job. I did not do very much in the way of science from 1970 to 1976, and by that time I had sort of settled into mostly doing management organizational kinds of things. I always enjoyed doing that, so it was not a big problem.

There were some real issues and challenges at that time. We entered, after Apollo, a period in which it was very difficult to keep planetary exploration going. Obviously there was no human exploration of the planets, but there was a series of unmanned missions, the most important of which were the Viking missions to Mars in 1976, the Voyager mission to the outer planets, and then Galileo, which is still around Jupiter now, and Magellan, which was the Venus radar mapping mission. Each one of these was really like pulling hen's teeth to get approved, and none of them were to the moon. There were no lunar missions. In fact, it was over twenty years between the end of Apollo and the next lunar mission.

So here we were, a science organization, sitting in the middle of the Johnson Space Center, which was totally a human exploration kind of center, and which reported to the Office of Space Flight in NASA Headquarters, now the Office of Space Science. So we had a significantly difficult time keeping momentum going in the scientific program at JSC. I distinctly remember one time when the center director came to visit us, and I'm pretty sure he had on his mind the possibility that he could eliminate the division and use the manpower for something that he was more interested in.

So we went through a significant series of difficulties, and we managed to keep the program going by being innovative. At the time that Haskin was leaving and I was taking over, we were just getting word of the discovery of meteorites in Antarctica. We jumped on that and were able to work out, in 1976 or 1977, an arrangement between the National Science Foundation and the Smithsonian Institution and JSC whereby we would become the curatorial facility for the Antarctic meteorites, and we would do for the Antarctic meteorites more or less what had been done for the lunar samples, characterizing the samples and making them available for research.

So that turned out to be an important program and brought a lot more visibility in the late 1970s and early 1980s, a lot more visibility to JSC, and helped with the relationships between JSC and NASA Headquarters, the Office of Space Science. But we were always a little bit scared that something was going to really fail and we would not be able to continue the program that we had undertaken.

That was another reason why we were strong advocates for the Mars sample return mission, which we first brought to the attention of the scientific community at the time of Viking in 1976 and 1977. We started advocating Mars sample return missions partly because we wanted to learn about Mars, but partly because that was something we knew how to do and we knew how important it was to do it. So we advocated Mars sample return missions, which that was 1976, and now it looks like we may get samples back from Mars in 2008. So that would be thirty-two years from the time we first started advocating that kind of mission.

Then we developed also the cosmic dust program. I mentioned that early in my career I'd been interested in cosmic dust, and then in the late seventies, early eighties, a fellow at the University of Washington [Seattle, Washington] named Don Brownley [phonetic] demonstrated that you could actually collect samples of cosmic dust in the stratosphere. We had tried sounding rockets in the early days, but they did not spend enough time in the stratosphere to collect samples, but Brownley discovered that you could collect samples by flying aircraft through the atmosphere for long periods of time, for hours.

Eventually we decided that JSC had some airplanes that were able to fly at high altitudes, the RV-57, and so we developed a program to build a new cosmic dust collector capability. That has been used ever since to collect dust particles. We have a cosmic dust laboratory at JSC, and they look at these collectors and pick micron-size particles off one by one and analyze them. So that was another sample program that we brought to JSC in the eighties.

Out of that has grown the recognition of JSC as being *the* place for planetary sample materials, and now that is firmly established and we, JSC, will be the place where the Mars samples are taken care of, curated. In addition, JSC is the place where the samples from the Genesis mission, which is to collect solar wind in space, bring it back for analysis, and the Stardust mission, which is direct sampling of cometary dust. Samples from those missions will be brought to JSC also.

So the work we did in the Apollo Program and Antarctic meteorites and cosmic dust and holding everything together during the 1980s has led to the current situation where JSC will be a unique facility in history. There's never going to be another one like it, with the range and type of materials that are in its collection.

BUTLER: A great facility and a good history to build on.

DUKE: Yes.

BUTLER: We're going to pause here real quick, if we can, and change the tape.

DUKE: All right. [Recorder turned off]

BUTLER: You were talking about the various programs that JSC was able to expand into and get into, to keep planetary science going.

DUKE: And to keep sample science going, basically.

BUTLER: As these ideas would come in, were these again coming from both people in your department and from the scientific community, people putting proposals for different sample collections?

DUKE: No. The basic ideas came from people at JSC. There aren't many new ideas. What happens is that people get attuned to different things and so they recognize opportunities when some new information comes about. I think that that's, for example, what happened with the Antarctic meteorite program. People at JSC had been working on lunar samples and recognized that the techniques that we had and capabilities would be applicable to this new set of samples, and that there was a valid scientific reason for at least proposing to do that. So we just proposed it. You interact with people as you develop a proposal. You don't just get an idea and write it down most the time and send it off to somebody. But the ideas for these new things basically came from people at JSC who recognized, because of their experience and heritage, that they had something that could be done and would be good to do.

BUTLER: Were there any new—you talked earlier how you had developed all the different procedures and different methods and tools. As these other—the Antarctic meteorites, and then looking at the Mars return, return from a comet, were there new considerations that had to make any drastic changes?

DUKE: Oh, yes. Well, the Antarctic meteorite program was easier than the lunar samples. That was not a difficulty. A minor difficulty was that the samples were coming from a place that's cold, and they were actually returned cold and you had to sort of evaporate the ice out of the samples when they got back from the Antarctic. But that's a minor problem.

But some of the other samples that will eventually be in the collection, and the cosmic dust samples, are quite distinctive, and special laboratory facilities were built for those. In particular, there's a dust-free laboratory for the cosmic dust collections, so you don't contaminate it with other very fine dust particles. The solar wind collection mission has very stringent contamination control requirements.

Now, this is stuff that I haven't done. This is stuff that came along after I was gone. But they have a special laboratory that is just now being built, in which the actual—they do this sample collection by making a thin metal foil of a very pure metal and cleaning it very well, putting it in a container, flying it out into space, and opening the container so the foil is exposed to sunlight, to the particles that are coming out of the sun, and it'll collect that for a couple of years, then it'll be folded back up and sealed back up and brought into the laboratory. Practically any exposure to stuff on the earth will potentially foul up their measurement. So a very special laboratory is built for that.

The Mars sample will be the same way. The biggest challenge there will probably be that many of the samples will have to be maintained and studied and analyzed at cold temperatures. The Antarctic meteorites were brought back cold, but then they were allowed to heat up. But some of the Mars materials they may want to keep at Mars temperatures, ten degrees below zero, twenty degrees below zero, throughout their handling.

So there are going to be a lot of challenges, and each one of these laboratories is different. Each one of the sample collections has to be kept entirely separate of the other sample collections. That's one of the rules. In fact, in the lunar sample collection, where a lot of material has gone out to investigators and has come back, the so-called return samples are kept separate from the original samples. So there's a completely separate laboratory for the return samples in the curatorial facility. So there are a lot of problems and a lot of techniques that the new chief curator is just now beginning a program of technology development for the Mars sample missions.

BUTLER: So a lot of interesting stuff coming up, and luckily they're able to build on the foundation that you established.

DUKE: Yes. The general philosophy remains the same. The details change from collection to collection.

BUTLER: As you were working as chief of the division, did you have more interaction or interaction to any great extent with some of the other NASA centers, besides Headquarters?

DUKE: There's always been a lot of interaction between JSC and JPL in the planetary program, but that's about the only interaction that we had scientifically. In the early eighties, some of my colleagues and I started working on the concepts for lunar bases and human exploration of Mars, and we were fortunate that at that time Aaron Cohen was first the director of engineering and then later on the [Johnson Space] Center director, and he was very supportive. But when he was director of engineering, and for a while actually his title was director of science and engineering, he started getting at least some of us in the science organization into contact with people from other organizations at JSC and with other centers. Before he became director of science and engineering, I essentially knew nobody from the engineering divisions at JSC, and the science organization had been quite isolated.

So in the early eighties, we started to have more interaction between the science division and the engineering divisions, and that, to me, was very beneficial. What it led to in the case of the moon, Mars activities was pretty close cooperation between several of us in science and several of the engineering divisions and collaborations that went on for several years and eventually led to collaborations in what was called the Space Exploration Initiative in the late eighties, when President [George] Bush made a speech and said, "We're going to send people to the moon to stay, and then on to Mars." We all were very excited about that, and JSC, both science and engineering, had been key players in all of the things that led up to that.

So during that period of time we formed quite a lot of collaborations with people from other NASA centers. Essentially all of the NASA centers were involved in the Human Exploration Program planning. But still JPL is the only other science center that typically JSC interacts with. But science is organized a little bit differently than the rest of NASA. Science depends on the scientific community that is dispersed, and NASA's science organizations are not particularly strong compared to the community.

In fact, over the years there's been a real push to—I don't want to say weaken NASA science, but to make sure that NASA science and NASA scientists were not dominating fields, because it's been perceived that the strength of science comes from the distribution of research in all of the universities and other government laboratories and even in industry. So you don't get a lot of science organizations in NASA centers that are collaborating in ways just because they're in NASA. There are lots of collaborations between individual scientists in NASA and individual scientists in universities all over the place.

BUTLER: Pretty good network.

DUKE: It is. And JSC, since 1970, has been the home of the annual Lunar and Planetary Science Conference. It's co-sponsored with the LPI [Lunar Planetary Institute, Houston, Texas]. LPI and JSC have been the leads of that since the first one in 1970. This last year there were about 1,000 attendees at this conference, which has grown to be the major planetary science conference. It's great that JSC is still interested in promoting and holding the conference. But this is a major place where scientists network. It's a full week of activity and 1,000 people, and you just listen to talks and you go talk to people in the hallways and have a few parties. That's a lot of the way science gets done.

BUTLER: It's a very productive conference, definitely. You mentioned working on the lunar base and then going on to Mars. While we haven't gotten there yet, what steps were you involved with taking at the time? How much focus did NASA put on that?

DUKE: Well, Wendell [W.] Mendell and Jeff [Jeffery] Warner and I, back in 1980, got very concerned because politically the planetary exploration program was still not doing very well, and there were some signs that the NASA management thought that we should forego planetary exploration for a while because there were other important things, like the upcoming space station. The space shuttle was just about to fly in the early 1980s.

We were concerned because of this tension between being scientists in a manned spaceflight center, with what was going on, so we got together and we decided that what we really needed to do was to create some areas in which the human exploration could be more involved with planetary science. After talking about a number of possibilities, we decided that we ought to focus on a lunar base.

So we put together a little presentation. We got a little bit of support from the Engineering Division, and we put together a little briefing. Went to see Chris [Christopher C.] Kraft [Jr.], who was the center director at the time. So we gave him our presentation about why we thought NASA ought to resume interest in exploring the moon, human exploration of the moon, and Chris Kraft sat there and he said, "Well, that's very interesting, but what does the scientific community think about that?"

So Wendell and I took a trip and we went all over the country, not to too many places, but to a few key places, and started seeking support from the scientific community, and we got sort of lukewarm support, but enough to continue. Early in 1984 or in late 1983, we had enough information put together that we thought we could start gathering some people together to discuss it from the science and technical community.

About that time, there was a group at Los Alamos National Laboratory [New Mexico] who was having the same sort of thoughts, but they turned out to be better connected politically than we. Turned out that the President's science advisor at the time—this was when Ronald Reagan was President—his science advisor was a guy who had been a division director at Los Alamos and was well known to the Los Alamos folks. So we got together and we sponsored a workshop in early 1984, and then a conference in, I think, October of 1984, which caught people's attention.

It turned out that lunar bases were something that there were a lot of people that were interested in, and they were not so far out that you couldn't think about them. The situation was that the shuttle was up and flying, and about that time the space station had been identified as the next big human spaceflight program. So we thought it was appropriate to ask really what comes after the space station, which we thought would be up and running by

1988, but, as everything else in NASA, the schedules get somewhat delayed. So we thought we were being timely. It turned out that we were before our time, so we struggled for a while.

But the conference that we held in 1984 fed the work that was then done in the next year by the National Commission on Space, which was a committee that was headed by Tom Paine, Thomas [O.] Paine, who was a former NASA administrator. Then they put together some really visionary ideas about what NASA and the country should be doing in space. Unfortunately, their report was completed about the time of the *Challenger* accident, which put everything into a tailspin, but led pretty directly to a study done in NASA which was called the Sally Ride Report. Sally Ride led a group in NASA Headquarters, working for the administrator, Dr. [James C.] Fletcher at the time, to look at the report that was done by the National Commission on Space, and see what NASA should be doing about it. That led to the creation of the Office of Exploration, which John [W.] Aaron from JSC was the first director of in NASA Headquarters.

So in the late eighties, I worked with John and people at JSC to define lunar bases, and the work that we did then was directly input to the planning for President Bush's speech in 1989. In fact, we were principal parts of the team that put together the technical details that backed up his speech. We had a plan that could do both sending people to the moon again and then human exploration of Mars.

Bush made the announcement and another commission was created. That was what was called the Synthesis Group. [General] Tom [Thomas P.] Stafford was the director of that. They spent a year and issued a report. So all of these things sort of were in a chain, and where we are now is that we still have a lot of interest at JSC in human lunar exploration, but the interest among the public has turned to Mars.

So we have a lot of people who are interested in undertaking human exploration missions to Mars, which are far more difficult than going to the moon, and probably way too expensive to afford, so to some extent those people are off doing not terribly rational things, not making very rational proposals in the context of where we are now and, I think, expecting that somehow the government will make a decision like it made for Apollo and will mount another big effort to send people to Mars like we did in 1961 to send people to the moon. I think that's pretty unlikely.

So a number of my colleagues and I are still out there advocating the next step of renewed human lunar exploration and, as typical, it looks like you decide that something is important, and twenty to thirty years later maybe it will get done. So I'm hoping that in the next decade we will get back to sending people to the moon. It's technically possible that it is easy to afford. We can do it for the same annual cost that we do the space station. So it would make a nice logical programmatic step for 2005, 2006 time frame. And we'll just see. It's something that's going to be done sometime. There's no question in my mind that we'll be doing that at some point. It's just a question of when and how and, to some extent, who. There's a lot more discussion of international collaboration than there ever was in the past. Just wait around for a little while longer and it'll all happen.

BUTLER: Hopefully it won't be too long of a little while.

DUKE: But there are enough interesting things going on in the meantime that you can keep yourself occupied, at least.

BUTLER: There certainly are. There's a lot of interesting things going on. After you were chief of the Exploration Division, you then became involved in the Exploration Program Office. Was this just a change in the division or was this another step up?

DUKE: The Exploration Program Office was the office that was created to respond to the Space Exploration Initiative. It was an office essentially—I think it reported to the [Johnson Space] Center Director. It was something like a program office and operated for two or three years doing studies for NASA Headquarters primarily, and then—oh, it's hard to remember now.

In 1992 or 1993, NASA was having another one of its go-arounds with Congress about the Space Station, and there was a guy who was on Senator [Barbara] Mikulski's staff, and he had a discussion with Aaron Cohen one day, said, "You guys aren't doing very well on the space station, and it doesn't seem reasonable to me that you would be studying exploration beyond the space station while you haven't been able to carry out your responsibilities for the Space Station Program."

At that time, Aaron Cohen probably had fifty people working on lunar base and Mars exploration activities, and just sort of overnight it got canceled. The word went out, "No more charging to exploration program activities." The Exploration Program Office was eliminated. Doug [Douglas] Cooke, who was the manager of that, was sent over to the Space Station Program. There was a group of people, young people in the Exploration Office, who were very dedicated to the exploration program, and we were able to preserve that.

Essentially when the Exploration Program Office was terminated, I was transferred over to the Space and Life Sciences Directorate again, and I advocated and got the director for Space and Life Sciences to create a mini Exploration Office in my old division, in Solar System Exploration Division. So that was agreed to, so half a dozen of those guys went into the division. Fortunately, again, they've turned out to be the core of the new Exploration Program Office, which was created again a couple of years ago. So another kind of organizational preservation contribution, I guess.

But there was a period when you could not talk about exploration, human exploration, in NASA, and it was entirely because NASA wasn't doing as well as they should have been on the Space Station Program.

BUTLER: Great. Sounds like a very unfortunate and hard time in many respects for NASA at that point.

DUKE: Well, we're still waiting for the space station to get out of the way. [Laughter]

BUTLER: You definitely—while the space station is going, it is important to always keep looking forward to the next step.

DUKE: That's what we thought, but, you see, that's the thought we had in 1984. [Laughter] And here it is 1999. So we're still waiting.

BUTLER: Hopefully not much longer.

DUKE: The space station, to some people, is an impediment. I don't view it that way. I wish it had been finished earlier for less money and less contention, but it really is a step in the process. Okay, so it's not the place that you would use to launch missions to Mars, but I really think that it will turn out to be a major step in human exploration, and one of the things that exploration people did not do very well was tie the programmatic link to the space station.

When Tom Stafford and the Synthesis Group made its report, it virtually ignored the role of the space station in human exploration. I think since people have really gotten interested in Mars exploration, that that space station looks a little bit more relevant to many people, because that's where you will learn about people in space for long periods of time. But it really did not get the attention that it should have, and I think we probably suffer a little bit from that now. I think we'll learn a lot of really important things in the Space Station Program and we'll look back another thirty years, you'll see that it really was an important thing to do.

BUTLER: In any of your work with the Exploration Program Office or any of your other work, were you involved in any of the remote sensing or earth evaluation projects or studies that were under way?

DUKE: Well, we did have, in the division, while I was division chief, we did, by again some sort of unplanned process, we did move into the division the little group that was working on astronaut handheld photography from the Space Station Program. So we had that group in the division, and we promoted that. We tried to encourage the use of handheld photography for scientific uses, and we developed a way for the scientific community to interact and try to help select targets of opportunity for handheld photography. And we made some observations about the earth through that program, but I have never personally been highly involved in earth observations kinds of programs.

BUTLER: You left NASA and came here to LPI to work with the HEDS-UP [Human Exploration and Development of Space-University Partners] Program. How did that opportunity arise for you?

DUKE: I made it. [Laughter]

BUTLER: That's a good way.

DUKE: This is just personal. The situation was that I was working full time for NASA, not—because the exploration program had gone away, I wasn't in the division anymore. I was a little bit undirected, okay, and sort of at loose ends, and I was working full time, or coming to the office full time. And financially, I could leave NASA with my retirement benefits and go to work half time, and still have the same amount of money as I was making before. So it seemed like a pretty good idea to try to organize that kind of transition.

So one of the things that the Lunar and Planetary Institute should do and was interested in doing was building the relationship between the scientific community and the human exploration community at JSC. So Dave Black here at the LPI and I had had a series of discussions about what that relationship should be. So I essentially proposed to him that I come and try to develop the relationship between the LPI and the HEDS Program.

One of the things that was a part of that relationship turned out to be the HEDS-UP Program. I developed the program based on a previous program that USRA [Universities Space Research Association] had run for NASA several years ago, in which universities were involved in advanced design studies that were coordinated by USRA. USRA is the parent of the Lunar and Planetary Institute. USRA is the parent organization. So this turned out to be something that the Human Exploration Program at NASA was interested in supporting, and so that was a program that could be brought into the LPI.

In addition to that, I've continued to run workshops that are relevant to the HEDS' exploration goals and objectives, and we've run a series of workshops on Mars field geology activities and on space resource development, and things of that sort. So we generally have tried to be useful and build the relationship between science community and human exploration. So that's worked out pretty well.

BUTLER: Hopefully that will help to keep the interest up and going.

DUKE: Oh, yes. Well, the universities are very interested. Just this week we're receiving proposals for this coming year. I'm hoping we'll have twenty universities involved and at least 1,000 students in one way or another involved in the program. They're always very enthusiastic.

BUTLER: That's great. Very good to hear. In your career with the space program, you've mentioned throughout our talk, various individuals you've worked with. Were there any that had a really large impact on—of course, Jack Schmitt getting you involved with the geology—but that had a large impact on the various stages of your career?

DUKE: Oh, yes. Well, going back, Jack Schmitt. Lee [Leon T.] Silver had a lot of impact. Gerry Wasserberg, sort of a love-hate relationship. He was always a very stressful person to work with, but he was always insightful and a very strong supporter of the program. I learned a heck of a lot from him. Aaron Cohen was obviously important personally and professionally. Paul Gast and Larry Haskin were both key people.

I've learned a lot and benefited from a lot of scientific colleagues, including Wendell Mendell and a whole bunch of people in the Solar System—they call it—I don't remember what they call it now. Science and Solar System Exploration Division at JSC. Another person who has been a friend and a colleague since 1984 is Paul [W.] Keaton, who was at the Los Alamos Laboratory. We formed a collaboration which continues to this day in various aspects of human exploration.

So there have been a lot of people that have been important in that process.

BUTLER: A good foundation of friends and colleagues, it sounds like.

DUKE: Yes. Well, there are a very large number of people that you interact with in one way or another. I've been in innumerable committees, and the faces tend to change from committee to committee, so there are literally hundreds or maybe thousands of people that you interact with in some way in doing this kind of work. So that keeps adding some interest as well.

BUTLER: A good group that continues to grow, like with the university students that you mentioned.

DUKE: Getting young people in is the really important thing, because some of us are getting a little older. That's why you're doing, I guess, this history.

BUTLER: Well, you have a lot of valuable information.

DUKE: While you still have me around. [Laughter]

BUTLER: Hopefully you'll still be around for quite a few years yet.

DUKE: I expect so.

BUTLER: Looking back over your career with working in the space field, what did you consider your biggest challenge?

DUKE: Well, I guess there have been several different kinds. The challenge during the Apollo Program was both a technical and an organizational challenge, and putting that together and holding that together was not traumatic, but stressful. So that was probably the biggest.

The period of time from 1976 to 1980, '81, was also pretty challenging, in that the environment within NASA for holding together a group of scientists at JSC was not very good, and there was a lot of stress involved with that. Then just getting people back into space beyond low earth orbit is a real challenge. I don't know how to do it, but I keep thinking about it.

BUTLER: Hopefully thinking about it enough and doing various things will help make it happen.

DUKE: Yes, keep talking about it.

BUTLER: Would there be anything that you would consider your most significant accomplishment or achievement?

DUKE: I don't know. It all sort of runs together. There's nothing that is really independent of everything else. I think getting the system together and participating in the Apollo Program was obviously the highlight. Beyond that, I actually think that the conference that we organized in 1984 for lunar bases possibly will turn out to have been a very significant achievement. If you count the references to the papers published in that report, which actually doesn't have my name on it, but in terms of the number of times that reports presented there are referred to in the literature, I think the influence continues. Even fifteen years later, you see lots of people who refer back to the reports that were given at that conference.

It was something of a risk when we did it. I did not have complete agreement from everybody that it actually should be done. In fact, my good friend Professor Wasserberg thought it was not the right thing to do at the time. In fact, I don't think he actually came to the conference. But it did turn out very well and I think historians will have to look at other people or get other views of just what was going on in that period of time, but my sense is that organizing the conference had a significant part to play in the development of the strategy that came out through the rest of the 1980s, that led to the Space Exploration Initiative and has provided the basis for most of the thinking now about human exploration of space. So it wasn't all that difficult to do, but I think as a single activity, it probably was as influential as anything that I've done.

BUTLER: Yes, I'm familiar with the proceedings from that, and it is pretty widespread, as you said. That's good to see, that you were able to pull together people at that time.

DUKE: To this date I can't explain to you how that happened, actually. I don't remember enough of the details of how we spread the word and got people to participate, but in the end we had some really excellent people who were willing to spend their time.

We had at that meeting the last talk ever given by Krafft [A.] Ehricke. Krafft Ehricke was one of the giants of space exploration and had for a number of years been writing and giving talks on the subject of lunar development. When we first organized the conference, we decided that we wanted to have him participate, so I contacted him and he said, "Well, I'd really like to participate, but I have leukemia and I'm undertaking treatment for leukemia." A couple of weeks later he called me back and said, "I really want to participate. I've scheduled my chemotherapy so that I will be in good shape for that day."

BUTLER: Oh, my.

DUKE: And he died about a month after the conference. That was really special.

BUTLER: Special for him and for the conference participants.

DUKE: For me it was special.

BUTLER: That's good. It's great that you were able to give him that opportunity.

DUKE: Yes. Yes, gives me goosebumps to think about it now.

BUTLER: Me, too. [Pauses] Looking back, would you ever have imagined where your career would lead you?

DUKE: No. No. No way. I mean, depending on where it was, I couldn't have predicted at any point. I went to college to do something else different than I decided to do. Everything was changed by Sputnik and the Apollo speech that was given by Kennedy. There was no way of predicting. I think that, by and large, I've pretty much taken the path of least resistance in all of this. It was more or less obvious at every stage what was reasonable to do in the context of what I was able to do and what was interesting. So I don't claim to have any particular great insights into where things should go, but most of what I have done seems to have been reasonably useful. So I'm pleased about that.

BUTLER: You definitely have accomplished a lot in your career and have a lot to be pleased about.

DUKE: Well, thank you.

BUTLER: Before we close today, I'd like to ask Kevin and Sandra if they have any questions.

RUSNAK: I do have two. First of all, how would you characterize the relationship between yourself as lunar curator and the overall manager of the LRL?

DUKE: During the time I was curator, there were three managers of the LRL. You know, I'm not really sure any longer who the actual manager of the LRL was when I first came.

The person that I remember interacting with most in a management position was a fellow named [R.] Bryan Erb, who was at least responsible for managing the sample laboratories when I came. Bryan really got himself crosswise with the science community about some issues of how the Apollo 11 and Apollo 12 samples, and even the Apollo 14 samples, would be handled. Eventually the scientific community got him fired from that position, basically went and talked to NASA management and said, "We can't deal with this person. Find somebody else." And that happened. Bryan went on to do a number of things for NASA and eventually retired from NASA and served as the representative of Canada to Space Station Program, and he's a very good friend of mine. Just one of those things that happened.

But then essentially the science organization was responsible for managing the LRL, and both the interim and the final manager of the LRL were also very good friends. Pete [Peter J.] Armitage was the manager for a short period of time, and then Gus [William E.] McAllum was the manager. Both of them were very good and very supportive. The initial conflicts were left over from the very first manager of the LRL, who was the guy that built the LRL and just had a technical view of what was to be done with lunar samples that diverged from what the scientific community thought should be done with them.

So that was the conflict. It basically centered around the use of a huge vacuum chamber for the initial receipt of the samples from the moon, and it turned out that this vacuum chamber, which was called the F201, was a real kluge. He was meant to simulate the vacuum conditions of the lunar surface so that sample boxes that had been sealed on the lunar surface could be moved in there and opened up and samples would never be exposed to the earth's atmosphere.

But it turned out that the contamination levels of other things inside the vacuum chamber were very high. Scientific community got upset about that, and in addition, the system was not reliable and leaked. In fact, one time a glove broke, and one of the technicians almost got sucked into the vacuum chamber, and that didn't bother the science community, but their samples were getting contaminated in the process. So that was the conflict when I first came to JSC. But after Apollo 14, there were no real problems.

RUSNAK: While we're on the topic of the scientific community, they've criticized NASA for not allowing science to have a greater role in the Apollo Program. How did you, from inside NASA, feel about that, and how might there have been a greater role for science?

DUKE: Well, as I said, the scientific community and the engineers were somewhat at odds during Apollo, and, in retrospect, it is very difficult for me to see that it could have been any different. The engineers had to be responsible for the success of the mission and the safety of the astronauts, and those were obviously the principal requirements. The science was not exactly an add-on, but was not the prime objective of the missions.

So I don't think it could have been done any differently, and I think by the time the program was over, that the scientists realized what their role needed to be and were doing precisely the things that the science community should be doing, choosing where to go, designing the kinds of things that the crews would do on the surface, advising in real time on the collection of samples, and managing the analysis of the samples when they were returned. So I wouldn't, in retrospect, have done it any differently than it was done.

RUSNAK: Very interesting. That's all I had. Thank you.

BUTLER: I want to thank you for taking your time and sharing your history with us. It's been greatly informative, and hopefully we will see your work continue to have impact.

DUKE: Thank you. I'm still interested in doing things. My current focus is on space resource utilization, including using the resources of the moon. I wanted to do crazy things like learn to make solar cells out of lunar material so that we can create an indigenous power supply capability on the moon using lunar materials. I am interested in how we might find and mine and recover ice and water from the cold places at the poles of the moon. I'm interested in how you construct habitats for people and for plants and animals out of the native materials on the moon. I'm interested in how you do similar things on Mars as well.

So it turns out that there is more of an interest in doing such things now than there used to be, at least in NASA, and I'm hoping that I can make some contributions in that area. I'm getting back to doing research after thirty years of not doing research, which is another challenge.

BUTLER: Certainly quite a challenge. I find it interesting that you said that there hasn't been as much focus previously about using the *in situ* resources and that now there's a bigger focus on that. It would seem a logical approach to use those resources. They're already there. You don't have to get them.

DUKE: The problem, of course, is that you really can't advocate the use of resources before somebody else is advocating a program that would use them. So until we have something more permanent being done on the moon, there won't be a lot of support for lunar resource programs because you can imagine any number of things that you could make on the moon, but what you want to make is the one that will be useful to the mission that's going to go there.

So the space resource application that has received the most attention recently is to make propellant out of the atmosphere of Mars, because that could actually be used to help in a mission that returns samples from Mars. If you could take only the machinery to make the propellant there and didn't have to take all of the propellant, you could save quite a bit of mass, and you might be able to do a much better sample return mission in that way.

So NASA has actually been interested in that because it has an application to a mission that is being planned, but we don't have anything like that for the moon. So a lot of these things are just not exactly wishful thinking, but looking at the possibilities in the expectation that sometime there will be a lunar program that will be able to make use of the resources, or there will be some other program that can make use of them.

BUTLER: Absolutely. And if we have the studies and evaluations now, that makes it easier to build the programs.

DUKE: Yes.

BUTLER: We wish you great luck in pursuing those.

DUKE: Thank you.

BUTLER: And thank you. It's quite interesting.

[End of Interview]