ORAL HISTORY TRANSCRIPT

JOHN O. ANNEXSTAD
INTERVIEWED BY CAROL BUTLER
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BUTLER: Today is March 15, 2001. This oral history with John Annexstad is being conducted for the Johnson Space Center Oral History Project at the offices of the Signal Corporation in Houston, Texas. Carol Butler is the interviewer [and is assisted by Kevin Rusnak.]

Thank you very much for joining us today.

ANNEXSTAD: Well, thank you for asking me. I'm terribly flattered.

BUTLER: Well, we're glad to have you. You've got some interesting insights, I'm sure, to share with us about the Apollo Program in particular, but some of NASA's other programs as well.

To begin with, if maybe you could tell us about how you got interested in science, in the geosciences, in particular geology.

ANNEXSTAD: Well, it's a rather interesting point in some ways because I took science all the way through school. I went to school quite a few years ago, and at that particular time I think the educational system was quite a bit different than you'll find it today. So consequently I had some excellent teachers in high school, and I took every bit of science I possibly could, and all the mathematics and everything else. Then I went on to my undergraduate work, and

I stayed only one semester. This was the year that the Korean War started, and so, of course, I left school, joined the service, was sent to Korea for a while, and finally came back and returned to school, at which time I realized that I was not going to, say, major in biology.

What I ended doing was awakening one day while I was in my undergraduate school and realized I had so many credits in physics and mathematics, I stayed on and I actually took a degree in classical physics. Then to fill it out, I took a little bit of education work, but that's a different story.

From that particular standpoint, I went on and went to work for the United States Government. But my first job was with an oil company in Texas when I left school. I was on the six-year plan because, you know, two years in the Marine Corps and four years in college and that sort of thing. I took a job in west Texas.

Well, in the intervening period of time, I had applied for a job with what is known as the U.S. Coast and Geodetic Survey. This is not the USGS [U.S. Geological Survey]. This is the Coast and Geodetic Survey, which is the oldest scientific organization in the entire United States, and they did everything, including geomagnetism, seismology, they measured the Earth, all of the geophysical types of things. It was just a rather interesting organization, and on a flyer I had applied for a job there when I was in college.

Well, the oil company came through first. Then while I was working in west Texas, the Coast and Geodetic Service came back and said, "We think you're a prime candidate for our first expeditions to the Antarctica in 1956." And I said, "My God, I've got a chance to go to the South Pole. I'm going."

So I ended up quitting my job and went to work for the Coast and Geodetic Survey in February 1957. In October that year, when Sputnik went up, I was in New Zealand on my

way to the South Pole, actually to Antarctica, and I spent thirteen months down there. Well, by this time I was working for IGY [International Geophysical Year]. I was in science. I was totally and completely enmeshed in it and that sort of thing, and that was how the career had actually started.

Of course, you can imagine, Sputnik is in the air. I knew about the NASA system, and we'll probably get into that a little bit later, because I had done my training at this time out in the East Coast. A very close friend of mine was one of the first people that had been hired by the Goddard Space Flight Center [Greenbelt, Maryland], and he was involved with the early satellite work that was going to go on. I had heard about this. We were doing Earth magnetic field studies. So, of course, when [Dr. James A.] Van Allen got going and discovered the radiation belts, we were right in this whole mess at that time. That's kind of a longer part of how I got into it. But I guess I came about my science direction naturally.

BUTLER: Well, it certainly sounds like it got an interesting start, and then it went to quite more interesting things as time went on. You mentioned going down to the South Pole, getting that chance as one of your early starts. What was that like, and what were some of the things that you were involved with on that particular venture?

ANNEXSTAD: The program was put together during the International Geophysical Year. I don't know if you're aware of this or not. A lot of people have only read about it. But it was a program that was designed to just basically study the Earth in a comprehensive sense, literally pole to pole and the equator and all over, with a strong focus in polar regions. So there was a lot of work both in the Arctic and then specifically the Antarctic. At the

Antarctic we put in a number of stations, and we actually did a lot of geophysical work at that particular time.

Going down there, I was among the first group of people to ever live a full year in the interior of the Antarctic. I lived at 80° south, 120° west, a place called Byrd Station, just 700 miles from the Pole, and I was there for thirteen months. There were nineteen other people at the station, and we were the earliest groups, you might say, that started to live in totally isolated environments for long periods of time. So it was a little like being an astronaut on the way to Mars. We had to live with just a few other people with only what we had, and we couldn't get out. You probably remember the woman that was a doctor at the South Pole that had breast cancer. They were able to get in and get out. They couldn't get us out. We were in total and complete isolation for ten months right in the middle of the Antarctic.

So what started me on this whole career of exploration, I started out in oil exploration and then I went into Antarctica exploration. So this was all a natural consequence of where I was going and that sort of thing. I was out of the country about a year and a half. I came back home. I was unmarried at the time. After I got back, shortly after that, I met my first wife and we married, I think that following year and stayed together until she died thirty-four years later.

But at that time I had decided to go to graduate school when I came on back. That didn't work out. Then in the meanwhile, the Coast and Geodetic Survey called again and said, "Would you go south again?" and I said, "Well, what do I need to do?" They were going to send me on the first over-snow traverse from McMurdo [Station] to the Pole, and I was to be the navigator on that. I don't know how much of this you want to hear, but before I went down there, President [John F.] Kennedy had made an agreement with the Chilean

Government that he would send someone from the United States to help them build a base in the Antarctic, and I ended up getting chosen by my organization. So I was the United States' representative to the Chilean expedition in 1960 and '61, which was another phase in all this. After I came back from there, in the subsequent period of time, we'd had a daughter, and my wife was sitting back here, and we were transferred as soon as I came back from South America to Fairbanks, Alaska. All of this is leading up to my eventually coming to the Johnson Space Center [Houston, Texas]. But we moved to Fairbanks, Alaska, in the spring of 1961 and lived there until 1968.

BUTLER: It's certainly an environment that you would be familiar with now, having been down in the Pole.

ANNEXSTAD: You're absolutely right.

BUTLER: What did your family think of all of this, your traveling down to the Antarctic?

ANNEXSTAD: Well, my mother and father, my mother especially, was probably my guiding light, more than anything else. My mother brought me up on things such as the stories of the Arctic and all of that sort of stuff. She was quite a woman. She taught school in North Dakota for one year when she was eighteen years old and had to function in a small, little one-room schoolhouse, where she had to carry the wood to light the fires and carry the water for the children through a North Dakota winter. She was quite an adventuresome type. In the 1920s, my mother and three other women got in an old Ford car and drove to the West

Coast through all the national parks and, you can imagine, there were no paved roads or anything like that at that time. She started in this direction. And my father, of course, was very supportive.

Then after I got married, my wife was also very supportive, but she had wanted to also move to Alaska, so she said that's one of the reasons she married me. She said, "Well, if you'll take me to Alaska," and we got transferred there. So that's what was happening.

BUTLER: Well, that's fortunate then. That worked out.

ANNEXSTAD: It was a lot of fun.

BUTLER: Good.

ANNEXSTAD: When I was in Alaska, and that's probably part of it, I was still working for the Coast and Geodetic Survey. We were doing a lot of wrap-up work in relationship to IGY. We were studying the Earth's magnetic field. We were doing seismology at that particular time, and I was actually basically a seismologist.

In 1964, I was a seismologist at the observatory in Fairbanks when the '64 earthquake destroyed Valdez and Anchorage, which was a rather interesting experience, because I had sent out the first indication of the earthquake and all I could send out was the initial p-wave, the first thing, because our instruments all fell off the piers, which was quite a thing.

Well, also along about that time the Coast Survey had asked me would I consider going to the University of Alaska and doing at least a master's and beyond. So I had started

work there. While I was down there, I studied under one of the men that had put together the IGY program, a fellow by the name of Dr. Sydney Chapman. Dr. Chapman—and there are a lot of these little stories that go on here. I hope you don't mind these.

BUTLER: No, not at all.

ANNEXSTAD: Dr. Chapman and his cohort, [Vincenzo C.A.] Ferraro, were the people that predicted the outer boundary of the Earth's magnetosphere being compressed on the sunward side by solar radiation, and they predicted also that there would be radiation belts that had been found in there. When the Van Allen belts were discovered by Van Allen from Iowa City, which was our first satellite [Explorer I] that had been put up during IGY, he had predicted this back in the 1930s. And I was his student when I was at Alaska. So this again kept bringing me back into this whole space-ambient system.

Another person who was there at that time, because we were also studying the aurora and various things like that, was a man by the name of Syn Akasofo, and Dr. Akasofo, who eventually became the director of the center up there, was also very, very prominent in a lot of the work with Van Allen and that sort of thing. So, you know, this was a marvelous situation because our first satellite that went up was a predictive thing. They told the world we were going to put up this satellite, and before we realized what was happening in October, Sputnik went up.

Then shortly thereafter—and this, of course, set everyone agog. This was a very, very different type of situation, and the entire United States was panicked at this time. Well, less than two months later they put up a second satellite, and that second satellite that went

up was so large that we recognized that Soviet Union had heavy lift capability, which we did not have, and that completely changed the equation as far as education and everything was concerned.

Of course, here I was in my late twenties and all, and into exploration, and I was very aware of all of these things as they were going on and had the opportunity to study under some of the same people. That was part of it.

BUTLER: Certainly some very interesting opportunities that you had there, being in the right place at the right time, per se, to get in on the very, very beginnings of a lot of that.

ANNEXSTAD: Right.

BUTLER: Working in Alaska, you mentioned the '64 quake. That must have been, from your standpoint as a seismologist, a premier opportunity for some study.

ANNEXSTAD: I had a lot of fun with that, and I continued on and finished my master's. But in my master's work I started looking at involvement with what we call the Sun-Earth relationship. So I was working in plasmas and magnetic field work and that sort of thing. So I had the opportunity to be working both as a seismologist and as an upper atmospheric geophysicist. So I finished my M.Sc. then in 1966 and continued on with some of the research that I had been doing and was setting up for doing my Ph.D. while I was up there.

Well, during this time we had had two other children, and so now we had three children. We'd been traveling back and forth, and the career was going very, very well. Our

son who was the middle child needed special education, and there really wasn't anything available up there. So we'd had him tested one of the years we came down, had him tested at the University of Minnesota, and they recognized that he needed a broader educational base. So because of that, we decided to move. We then started to trying to find positions elsewhere.

Well, back in 1967 and '68, there were a lot of things available. I'd had an offer from Boeing. They have a scientific research lab in Seattle [Washington], and I was very interested in going there. I knew the director of the lab, and so I was looking at that. I then was talking to the people in Washington, D.C. There was another group in Boulder, Colorado, and they had talked. These jobs were, in some cases, I had a tentative offer and then I didn't. We were not really sure how this was going to work out.

Then I heard through a friend of mine that Bill [Wilmot N.] Hess was putting up a special scientific organization at the Johnson Space Center [Houston, TX]. I knew about the existence of the Johnson Space Center. My total association with NASA prior to this time had been through Goddard, and it had been because I had friends that were working at Goddard. One of my old professors had taken a position at Goddard, and we all kind of knew that the Goddard system was where the scientists were. Well, Bill Hess had been hired. Now, I don't know you've heard his name before or not. But he's an upper atmosphere geophysicist, and I didn't know him personally but I knew him by reputation. Having come out of Alaska and with the work with aurora and various things like that, Bill Hess was somebody that stood rather tall, both physically but also academically. This friend of mine, who was actually on the faculty at the university, said he knew him, and he said, "You may want to just check this out," and I did.

I checked it out, and I filled out a request to go back with the government. See, I'd been away from the government now for just a couple of years, but I was an ex-government employee. So I sent things down, and I heard back from them. But the people who wrote back to me were P.R. Bell, who was the newly appointed Director of the Lunar Receiving Laboratory, and he literally hired me sight unseen. He had looked and realized I had a background in upper atmospheres but also in seismology, and that was kind of a key. So he hired me and I decided to take the job.

I asked my wife. I asked, "What do you want to do?"

She said, "Well, I've never lived in Texas. Why don't we move there." And I had. I'd been living in west Texas, but I'd never been in the Houston area, so we didn't know anything about this. So in 1968, in June, we packed up and headed from Fairbanks, Alaska, to Houston, Texas.

BUTLER: That's quite a change.

ANNEXSTAD: What a change. You would not believe what a change this was. My children would not go outside until winter. Sixty-five degrees was warm to them, and they moved down here, and their faces were just as red as beets. They wouldn't go outside.

BUTLER: Well, they had had most of their lives in Alaska, hadn't they by then?

ANNEXSTAD: Well, two of them were born there. One of them was only nine months when we took her on the Alcan Highway. This was quite a—

BUTLER: This was quite a shock.

ANNEXSTAD: It was quite a shock to them, yes, absolutely, without question.

So we came to JSC then in the first period of July. Now, it was a rather interesting situation, and in a way it kind of told me a little bit about how—you're going to hear this again—about how JSC actually takes care of their people. I arrived and I had not signed on yet. We came down just to look for a house. But I went over and I talked to the personnel people, and they said, "Do you realize that you will have been out of government for three years on such and such a date?" and I said, "Yes." They said, "Do you realize also that if we can put you on board before that date, you will retain all of the sick leave that you had earned from the previous seven and a half years with the government?" which was substantial, and I said, "Can you do this?" They said, "Absolutely. We're going to put you on without pay." I mean, they found this out and they put me on and I retained all this sick leave and these other benefits that I had earned up to that particular point. Didn't lose anything. The end of three years, you lose it. So I came back at the right time, which was very, very fortunate for me, because I walked in here with something like 700 hours of sick leave. Wasn't that a wonderful way to start? Plus, I was also in the next area as far as annual leave, and that sort of thing was concerned.

I was immediately assigned to the LRL, Lunar Receiving Laboratory, under P.R. Bell, which was in Building 37, but I then actually had my office in Building 31 at that time.

BUTLER: What was your impression of your job and your tasks before you came on board, and how did that meet with your expectations once you got going? What things were you involved with right away, your projects?

ANNEXSTAD: Before I got here, I thought I was being hired as an upper atmospheric geophysicist, somebody that had looked into or continue my work in magnetospheric physics, plasmas and things like that. When I got here, I found that was not the case. In fact, it was a rather interesting situation that P.R. Bell had hired me. This is an interesting story, too, because at the time that I was here, he asked me first and foremost if I would help them set up a program in seismology because they wanted to do seismicity on the Moon.

Well, unknown to me, they already had contractors doing all of this work, and these people had already been defined. But apparently they were trying to set up a situation where people here at the Johnson Space Center would be doing similar kinds of experiments to the point that they would come up to speed, so to speak. Their principal investigator was a fellow at Lamont[-Doherty Earth Observatory, Columbia University, Palisades, New York] out in the East Coast, and, of course, he's just a topflight, world-renown seismologist and that sort of thing. But we were supposed to kind of follow along and become part of this. The orders were not very defined. You didn't really know what you were supposed to do, but P.R. Bell said to me, "Well, we have a pier here that we have seismic experiments on. We're running those. We'd like to have you take those over. We'd would like to have you give us a seismic program of some sort."

I said, "Well, I really don't consider myself a seismologist," because although I had worked in the field for a number of years, I said, "I really consider myself more of an atmospheric physicist and that sort of thing."

And he said, "Of all the people on the staff," and this was his comment, "you're the only one that can spell 'seismology' and do it correctly."

I said, "Well, okay." So we were going to start in that direction, and that was my first assignment, which was really quite dismaying because I didn't really know what we were supposed to do and what we wanted to do.

So I made some trips. On one of the trips I went out to Lamont. At that time we had enormous amounts of support. Since the whole organization was new, they were just trying to set things up. We knew we were going to go to the Moon. There was no question about that. And we knew we were probably going to be going within a year or so. So everybody was desperately trying to put everything together, and we wanted to make sure that we had people, for example, here at JSC that knew what the contractors and the science people that were running the experiments on the Moon were doing and that sort of thing. So, in other words, we didn't have until that time a cadre of people that could really interface with these other folks that had been brought in to do the job. So it was a very kind of confusing type of situation. So I said, "Well, okay, I'll get started on this."

They took me around and they showed me the equipment. They had a place in Building 31 where they had cut a hole in the floor and they had sunk a concrete pier down there and they had instruments sitting on that. I can't even remember the fellow that took me around, but P.R. Bell was right. The guy didn't know anything about seismology. With great fanfare he showed me this pier, and I said, "That's not going to work."

He said, "Well, these are what our records look like."

I said, "They're useless. You're on the Gulf Coast. This whole area is filled with what we would call seiches," which are small, little oscillations produced by the tides coming in and out. I said, "You're also on an alluvial system that's just going to sit there and shake all the time. You have no way of really measuring earthquakes unless one occurs very close by."

He said, "Well, we thought this was going to work."

I said, "No. Any seismologist is going to tell you this is all useless."

So on my recommendation, we took the instruments off and they just eventually took the pier out and went with everything else.

So P.R. Bell said, "What are you going to do as far as the program is concerned?"

I said, "Well, the thing for us to do is to get away from here, if you want us to test equipment," which that was their idea, "where we're going to be able to provide backup testing of the kinds of equipment that were going to go to the Moon." Even though the principal investigator had done all this, they still wanted this additional stuff. So what I did was say, "Let's see if we can get some additional equipment." I called some friends of mine. I'd known people, you know, in various areas. And we found an entire seismic setup, a portable situation, with a trailer and everything else, that was not being used. It was sitting out in a field in Colorado. I got the organization in Washington, D.C., to transfer it to NASA.

We went over and we got a truck and I got a driver and we went out and we found this place that had been sitting out in Colorado. It hadn't been used for two years. It had a

complete portable system, seismometers. There was probably a couple of thousand dollars' worth of gear, and we got it for the price of tires on the trailers. That's all it cost us.

BUTLER: That's pretty good.

ANNEXSTAD: Yes. And we took it and we then instrumented a place in Oregon called Blue Mountain. We put the instruments up and we started taking data. Well, in the meantime, I had been trying to find out what was going on, and I think I kind of ran afoul of the principal investigator, who was sitting there saying, "Why is this person from the Johnson Space Center coming around here trying to find out what I'm doing?" you know, this sort of thing. It was a very, very strange period of time. In the process, we just said, "Well, we'll continue to do our own thing, which is measure earthquakes."

Well, in setting up this station, which we did, we not only were able to test equipment if we wanted to and bring up our own capability, which was we had things that we could work with and we could do it remotely if we wanted to, but we became part of the worldwide seismograph net, and they wanted us to provide them with information because we had instrumented a mountain in Oregon called Blue Mountain that nobody else had done. Eventually we, through NASA, turned all the equipment over to the University of Oregon [Eugene, Oregon], and they were very happy about that, too.

But in the process we were still trying to find our way, and nobody really knew what was going on. I think part of this was because Bill Hess, as good a scientist as he is, and he was an excellent scientist, there's no question about it, his love was unmanned systems and magnetospheric physics and upper atmospheric stuff and all that. It was at this particular

period of time that people were beginning to recognize a very important aspect of this whole situation was we were going to be bringing back samples from the Moon, and this was the geologists' corner, not the geophysicists'. So it was an interesting situation, the way it all developed. It all shook out throughout the years.

Eventually Bill, I think, left. We were putting together things over in the Lunar Receiving Laboratory, and I was not really involved with everything that was going on with Apollo 11, as far as the astronauts going to the Moon and that sort of stuff, but during that particular period of time we were trying to get ready for the first mission in July of 1969. The lab was put onto a kind of an alert basis, where we were playing this game of special canisters coming in, "We think the samples are coming in," and all that sort of stuff. Now, I was only peripherally involved at this time over there, because I was in Building 31.

Our very first run-through on this whole thing was an absolute unmitigated disaster. Nothing worked. It was wrong. It was a trial run in relationship to what was going to happen if we were to bring back samples from the Moon. Well, Bob [Robert R.] Gilruth, in the process, removed P.R. Bell from the position and appointed a fellow by the name of Dick [Richard S.] Johnston. I don't know if you've ever talked to Dick or not.

BUTLER: Yes, we have.

ANNEXSTAD: Brilliant fellow, but Dick, interestingly enough, had a much different mandate than P.R. Bell had. P.R. Bell was a radiation physicist, and so he came in and he was concerned about being able to look at radiation coming from the samples. He didn't worry about the rest of the lab. He was down in the counting laboratory, almost like King Midas

down there counting his gold and working with the people down there, and the rest of the lab was not being put together properly. Well, I remember when this occurred, and at that particular time I had kind of at least worked with and around the geologists quite a bit, so I knew a lot of what was going on, and we were aware of what was happening. P.R. Bell then received what is laughingly called "a lateral arabesque." It means he was just moved over to the corner without any operational responsibilities or anything, which was, in a way, too bad. But Dick Johnston came in, and Dick had a mandate from Bob Gilruth: "Get that lab in order." Now, Dick Johnston was what I would call the quintessential manager. He knew what needed to be done. He knew how the people needed to function and operate, and he knew how to make an organization work to get the job done. He may not have been the most brilliant person in certain aspects of science, but he had a wonderful background in chemistry and Dick had been in the upper levels of management for a long time. So he had a mandate from management, which, of course, helped, but the best part about it is that he literally whipped that lab into shape in like two or three weeks. It was unbelievable. And it really came down to a case that he just called in everybody and just plain said, "This is what you're going to do. I want paper. I want paper. I want exact operational scenarios. I want everything on my desk, and this is the way I want this thing to work, and this is the way you're going to do it."

We had a lot of young people that didn't know that this was the way NASA worked and how it functioned. He made it work. I had an enormous amount of respect for him because of the way he brought it about. Because of him, we were ready for Apollo 11. He did it. There was no question about it. Of course, I've always said that the very interesting thing about it was he did have a mandate from Bob Gilruth, and maybe P.R. Bell didn't have

that mandate, but I think P.R. Bell didn't have the mandate because he didn't realize that that's what he needed to do. Dr. Bell was a very, very fine person. I liked him very much, and we got along extremely well, but I don't think he could see how NASA was trying put this together at that time.

NASA's gone through some very interesting years and that was probably one of the most interesting to me because I was right next door to what was happening at the Lunar Receiving Laboratory.

BUTLER: And you could look at it sort of not quite from an outside perspective, but you weren't right in the middle of all of it either.

ANNEXSTAD: Well, one of the things that happened during that time, which is kind of interesting, was that somewhere along in there, P.R. Bell asked me to become the Acting Chief of Geophysics. Now, that in some ways was probably a mistake. I was not that experienced. I didn't come from a management organization or anything. I had done quite a bit of scientific work, but I did not have my doctorate at that time. I had a M.Sc. and I came out of the University of Alaska, which is another story, because to some of the people that came into the science system, I was literally a nobody. And that was fine. You can live with that.

In the process and in being elevated, he asked me, "Can you handle this?" and I said, "Well, we'll sure give it a try." So I did. And I still had my own work, and the branch I was in, which was the geophysics branch, was for all practical purposes kind of a nonexistent, somewhat sidelight of what was going on. The geologists down the hall were training

astronauts and doing all kinds of wonderful things and we were supposed to be involved. And we had four or five in our branch that some of them were very good and some of them were losers. That's all there was to it. We all kind of worked independently.

Well, P.R. Bell said, "Is there any way that you can kind of bring them together?"

As it turned out, in my naivete I said, "Yes, I suppose we can." Well, that never really happened.

A couple of members of the branch took issue with the fact that I had been appointed the temporary branch chief. It was not a permanent appointment. They became very upset with this whole thing, and they started to complain, and they began to play this one group against another. One of them, who didn't stay very long with NASA but at that particular time had the ear of Tony [Anthony J.] Calio, who was Bill Hess' replacement, and for some reason Tony Calio and I did not get along well for a number of years. Of course, it was always a little strange to me why somebody as high as he was, the Director of Science and Applications for the center, and I way down here in the branch level, why he was even concerned about me. He could just as well have forgotten about me.

In the process, my position was eventually taken away and they brought in another person, who was eminently qualified to run the branch. I'd never say anything against that individual. And I was reassigned to the man that eventually took over for Dick Johnston at the Lunar Receiving Laboratory, a fellow by the name of [R.] Bryan Erb, and you may have talked to Bryan Erb. That's another story within the system. But what had happened was apparently I had run afoul somehow of the higher-ups within the system, in this case, I think probably Tony Calio. I kept hearing that, but, you know, Mr. Calio and I never had a

conversation to the point that he was saying, "I don't like you," or "I didn't like what you did."

But Bryan Erb asked for me to come over to the LRL when he had taken over. See, Dick Johnston came in only for a short period of time, just to get us through Apollo 11, and then Erb was appointed as the Director of the Lunar Receiving Laboratory. Bryan was eminently qualified, eminently qualified to be the director of the lab. He came from the engineering side of the house. He was, without a question, a very experienced manager and that sort of thing.

So when he just said, "Come on over," and I thought, "Well, what is he going to have me do?" Well, he was very candid with me. He said, "I'm going to hide you for a while." That's what he said. And so I got a little office over in the corner, and he had me do some special things for him. All I was doing was special little scientific assignments, which went on for, oh, probably several months. It went on through and prior to the Apollo 11 system. By this time I was in Building 31, and, like I said, I was kind of hiding over there, but I didn't really have any well-defined duties, but when some strange little thing would come up. Like, I remember one time Bryan said, "So-and-so wants to do some seismic work on the Moon, and he wants to shoot a grenade off and do an explosion up there. Would you take a look at that and tell me what you think about it?" It was the kind of the thing that I did, and I did special, little things for him. In the meantime, I was just basically hiding, so to speak.

There's a lot between that and what I have told you before, but the interesting part about it, I hope, as far as the history is concerned, is this really set the tone for my entire career at NASA. So this is the first, you might say, year that I was there. I was kind of foundering, looking for things to do, and even during the first mission to the Moon, we had

had another idea that was fun. We set up a mock mission control that we were involved with, and while the astronauts were up there in the Apollo 11 system and even that, I think, probably it was a little political and some of it worked out and some did not. If you want to return to that later on, I can tell you about that.

But what was so interesting to me is when I was reviewing in your questions and everything is just kind of like my career actually with NASA started out in rather strange way, without a well-defined sense of duties, while a number of things were occurring around us, to the setup of the science system, and I was privy to seeing careers made and broken literally overnight, people being moved in and out, and this tremendous political climate that was going on during that particular period of time.

And it was interesting that there were one or two people that were very kind to me. I was not in a position to, say, leave NASA. I had three children and they were in school, and what was I going to do? Although it was so dismaying during that time, I applied for many, many jobs, and, of course, there wasn't much of anything that was going on.

So I finally decided, "Well, we're going to stay here, and we're just going to see how this is going to work out." So that was the start, you might say, of the rest of my, what, seventeen, eighteen years here at JSC.

BUTLER: Do you think a lot of that had to do with the fact that, as you said, the science for the program was kind of being done at a last-minute type of thing, where so much of the focus had been on the engineering aspect? From getting the program going and actually getting to the Moon and then trying to bring all this science together and trying to integrate it into a program that was already up and running more fully and more completely in other

aspects, do you thing that was what some of that difficulty that evolved for you and for some of the others?

ANNEXSTAD: Yes, I really do think that's the case. I don't know how much I'll really touch on, say, the science and engineer conflict, although there was some there. What was actually going on was something that was even a little broader. The Apollo to the Moon Program was probably the biggest thing that the United States had ever done scientifically. I was very fortunate to have been a member of IGY. Now all of a sudden I found myself a member of the Apollo to the Moon Program. This was the epitome of anything. It had the attention of the entire world, without a doubt. Everybody knew the astronauts. Everybody knew that we were going to go to the Moon. This is probably one of the most exciting things you've ever seen.

But underlying this is everyone that was not associated with NASA knew the big players. They knew who the head people were. Everyone knew this. They knew who the astronauts. But then there was this great, enormous group of people that were working down here in support of all this. I think we had something like 500,000 people throughout the United States and the world working in the NASA program or working for us, and maybe only about 22,000 to 25,000 actual NASA employees. There was an incredible pecking order throughout all of this, and people were literally climbing all over each other to get to the upper levels. It was almost a situation of, oh, if you get to this particular point, then you can get a corner office with windows and wooden furniture and things like this. These were all perks that are part of it. Or you can have a carpet on the floor and all of this sort of things.

It was so interesting, because having come from Alaska and being involved in things on a very straightforward basis, I was not ready for all of this. It was very surprising to me, and I found that even secretaries within the system would take on the mantle of their bosses' position. There were just all kinds of things. There were all kinds of people that were going to tell you what to do and you had no power in the system unless you were up at this particular level or something like that. It was far more insidious in many ways, especially with us over in the science group, which was the newest bunch that had come about. We were, in some ways, looked on with great disdain by the so-called "engineers."

But part of that comes from this. When NASA was put together, they brought the aerospace people, and these people, regardless of their education or anything else, were very close friends of Gilruth and Chris [Christopher C.] Kraft [Jr.] and all of these folks, and they were put in the positions of command. This is what happened. If you're going to establish an organization, it's obvious what you're going to do is put in the people that you know and trust to be able to run it and do this. They may not have been the best managers in the world as far as personnel was concerned, but they knew their job.

Now, that sort of thing filtered down to us because who do you suppose was brought into the Science Group but what I called at that time the young Turks? And the young Turks were graduates of MIT [Massachusetts Institute of Technology, Cambridge, Massachusetts] and Caltech [California Institute of Technology, Pasadena, California] and Harvard [University, Cambridge] and Stanford [University, Palo Alto, California] and the University of Texas [Austin, Texas] and whatever. And so here I am from a place called the University of Alaska, which is nowhere, and I came into this system, and that's exactly where I was. I was nowhere, in comparison. So I think, in a way, I was very aware of these kinds of

situations. I was aware that I could contribute, and there were a lot of things I could do and I had a lot of ideas, but I didn't have power of either position or the right school behind me, even though some of the people that were in here already had been with me in Alaska. But one of them had finished a Ph.D. at Harvard and another one in Idaho and places like that, and some of those folks are still here.

So the climate for me in some ways was quite different than it was for other people, and I wasn't terribly sophisticated, so I fell into the mud hole. But thank heavens Bryan Erb came along and said, "Come on over and work with me."

I could go on and on and on, but if you have some questions you want to pick up, because I think in some ways this is quite a story before it gets done.

BUTLER: Mentioning Bryan Erb and that you were working on special projects, at that point were you able to make suggestions to him about some of your ideas and begin to work into those?

ANNEXSTAD: Well, yes, to a point. Yes. But basically Bryan was a division chief at that time, and one of Bryan's great, great strengths was his concern for his people, and this was the one thing that he had, and it's the one thing that did him in, by the way. It really did. In many ways I think he realized that I could contribute.

Well, the rocks had come back. They had gone into this situation and we now had a distribution of samples. At that particular time we were into our second curator. The first curator who had been appointed was a fellow from the University of Houston. His name escapes me right now.

BUTLER: That's Elbert [A.] King [Jr.]?

ANNEXSTAD: Elbert King. Yes, I'm sorry. Elbert was the first curator, but Elbert lost his position as a curator of lunar samples before the samples came back. He had a well-defined sense of how things should be handled, but he didn't have an organization set up. Well, he ended up, I think, going back to the University of Houston, and [Dr. Daniel H.] Dan Anderson took over as curator.

Well, Dan Anderson, who had finished his Ph.D. at the University of Minnesota, and, of course, I had been born in Minnesota, at least I knew who he was, and I was in that area down there, and Dan took over as curator.

Well, Dan Anderson's a very, very bright guy. I don't know if you've ever met him or ever heard his name mentioned. But Dan was kind of a typical yuppie. I mean, he grew up as one of these people with long hair and a degree in physics, and Ph.D., University of Minnesota [Minneapolis, Minnesota], and was against the Vietnam War, you know, but a very, very nice guy, very brilliant. The reason I say he's brilliant, he's probably the only person I've ever met that could go sit in the Piney Woods of east Texas and could tell you every single bird that was in the trees by its sound. Now, these are the kinds of people that they had brought in. I mean, our group of people was not a group of second-rate people, by any means. They were absolutely first-rate. Everybody had these little quirks. I liked Dan very much, and he was brought in as curator.

Well, at that time they were working on Apollo 11 samples, and they were trying to disaggregate the sample. They were cutting them and doing things like that. Now, I had not been involved at all.

Bryan Erb called me into his office one day. Bryan said, "John, we have to cut the lunar samples. We can't just take little pieces off them anymore. We have to cut the samples. We're going to have to take some of them out of the vacuum laboratory and try to do them elsewhere. I would like to have you watch over the cutting."

And I said, "Well, wait a minute, Bryan. Are you asking me to take a position and to kind of direct things?"

"Oh, no, no, no, no," he said, "I don't want you directing anything. Would you just kind of be a mouse in the corner and stand back and kind of watch and see what's going on and just kind of make sure things go smoothly? But I can't give you any authority or anything."

I thought, this was a kind of a strange, strange assignment, and what are you supposed to do?

So the first day that the sample was going to be cut, there must have been ten people there, and I was standing way in the background watching all of this and kind of keeping my eyes peeled and whatever. It was a Chinese fire drill. I mean, nobody knew what was going on. They were having problems, and how do you document this, and how do you do this. Yet all these procedures had been in place. But this was now something new, because nobody had ever cut samples before, although there were all kinds of people with all kinds of ideas.

Finally what I did was I went back one day to Bryan and I said, "What do you want done? Do you want that operation to be part of your shop, or do you want it to kind of just go along like that?"

And he said, "Well, to be honest with you, I'd kind of like it be part of our shop."

I said, "How much freedom do you want to give me?"

And he said, "Don't make anybody mad."

I said, "Okay, we'll see what we can do."

The offshoot of that was I eventually became Director of the Processing Laboratory, but it was me and thirteen contractors that were doing the work. Yes, this was what it was. I was actually nothing other than the NASA technical advisor to the system, but this gives you a great amount of power within the situation.

So we got into that, and I started then putting the processing laboratory, it was just a small group, on a basis of how things were going to work in a little more solid vein. Two things happened. One of the things that had gone on, the previous, we called them contract monitors, and as a contract monitor what you were supposed to do was to grade the contractor, and if the contractor got all high grades, they would get extra money and things like that. So you had a certain amount of power, but at the same time the contractor has his own administrative staff.

But you, as a NASA employee, were not supposed to come in and give orders directly to the contractor. But we had certain other things. One of the things that I had that I was supposed to do was to open up the door for the all the contractors to go into the lab and go to work in the morning. Well, one of the first things I realized is the contractors showed up at 7:30. The previous NASA people didn't show up until 8:00 or 8:15. So I thought, that's

kind of strange, and I began to look at this whole thing, and I realized what had gone on. NASA was working literally 8:-00 to 4:30, and the contractors were working from 7:30 to 3:30, 4:00. So they would sit around and drink coffee and not be ready to go to work until a NASA person showed at 8:00 or 8:15 to open the door.

So I showed up at 7:30. This was really interesting. They looked at me and said, "What are you doing here?" And I said, "It's 7:30. You guys go to work." There were only two members, and this was Brown and Root Northrup, that was the contractor at that time, and there were only two members of that entire group that were ready to go to work at 7:30, because they had to have their boots and their suits and everything on, because we were going into a clean lab. And I said, "We start work at 7:30." It took about three days, and everybody's ready to go to work at 7:30.

Then they would come out, they would try to come out thirty minutes early in the evening, because they were supposed to take their suits off and all that, and I got that changed to the point that they could come out fifteen minutes before then. Their comment all along was, "Well, why are you there?"

I said, "We're paying you for these hours, and these are hours."

They said, "Yes, but you don't start until so and so."

I said, "I'm going to be here when you're supposed to go to work," which was kind of an interesting situation, because, of course, fortunate for me, the Brown and Root people were not running back complaining except to their own bosses, and their own bosses would then go back and complain to Bryan Erb, and Bryan would say, "No, that's okay. I agree with that." You know, he would do that.

The second thing that happened was probably one of the most interesting things, and I love to tell the story because at this time, remember now, we had a sample that come back from Apollo 11, and they'd gone through it with a vacuum system and everything else. Then some of it was now being put into another laboratory where we were trying to process it and we were involved with all of this. It was really getting the material ready to go to the principal investigators. So security was a little more relaxed, because we were holding the initial examination in the area of the vacuum laboratory.

As the stuff came on out, we had doors that were secure, but you had to have a code to get in the door. We were giving out codes. Everybody had the code. Now, I was not really the formal director of the laboratory, but we were moving into this direction, and I realized that we had people that were coming in at 10:00 at night processing sample and doing it on their own and getting it all ready, working in the lab, and we'd come in 7:30 the next morning, and this stuff was done and ready to go. These were other scientists, that were even coming from Building 31, and they said, "Well, we're supposed to do this. We're supposed to tell what kind of samples there are and that sort of thing."

So I called security. In the meantime, I had been acquainted with some of the folks over at security, and I called a friend of mine over there, a fellow that I had just known about. I said, "You know, we've got a real problem over here." And he said, "Do we ever. We want to come and talk to you." I had told him who I was, and I did this without talking to my branch chief. At this time I was in a branch over there, and I hadn't told Bryan Erb anything or that sort of stuff.

The security man came over, and I told him what the problem was, and he said, "We know what the problem is. We're very concerned about this. Anybody can pick up samples

and walk off with it." And we had a number of incidences of things like this. Being an old government employee and somebody that was involved in some of the stuff before, and knowing these fellows, I had hit the right button. So he said, "What do you think we should do?"

I said, "I want you to change the locks on all the doors," and he said, "It'll be done before tomorrow morning."

So everybody goes home that night. All the locks are changed, and when everything was finished, they came to me, and he said, "Who gets the combination?"

I said, "I do." I just took it on myself to say this. I didn't ask anybody.

They said, "That's fine with us. You've got the combination. You can decide who gets it."

I said, "I'm going to see Bryan Erb. I'm going in to see the division chief and tell him what's done."

I went to Bryan, and I said, "This is what I've done. You wanted this thing under our control. We now have it. Everybody is locked out except you and me. Bryan, to whom do you want me to give the combination?"

He said, "You've got one and I've got one, and that's all that's needed right now." We locked them out.

I came to work the next day specifically thirty minutes late. People were standing outside screaming. They were angry. My branch chief was absolutely beside himself. I didn't tell him this. The section chiefs were running up and down the hall. What was going on? And I calmly went over and opened the door and let everybody in and whatever, and

they were lining up outside Bryan Erb's office, and Bryan said, "No, he did what I told him to do." And that's when security came down on top of the whole thing.

The interesting part of the story was that we were able to use security to actually put a damper on free access to the samples. So I think the secure system moved on from that particular point, which was interesting.

Then the next part of the story involved Mike [Michael B.] Duke. Now, Mike Duke had been brought as—remember now, in the meantime I was director of the laboratory, but my boss was the branch chief in Building 37. Mike Duke, by this time, had been brought in as curator. They had put together the whole Lunar Sample Analysis Planning Team and all of this stuff on who's getting samples and they'd gone through much of Apollo 11 and by this time Apollo 12 was coming in.

I think it was during the first part of Apollo 12, and Mike came over to the lab one day. I had never met him, but I'd heard about him. Graduate of Caltech, top-flight geophysics background, Ph.D., expert in contamination, very, very highly recommended by Jerry [Gerald] Wasserburg. You've heard the name Jerry Wasserburg before. Jerry Wasserburg had so much power in the system it was unbelievable. Those are other stories, too. He's the one man that succeeded almost individually in getting Bryan Erb removed and some of those people.

Well, in any event, Mike Duke came over. The process had been in the past, Mike had two fellows working for him that would write out sample orders because everything was supposed to be on a paper trail, and the sample orders would come over to my boss, who was the branch chief, and he would then give them to me, and I would fill the sample order or

have my people in the lab fill the sample order and then it would go on back and out it would go.

Mike asked me a very interesting question. I don't know if Mike even remembers this. But he came over to me one day and it was just shortly after he had taken the job over as curator, and he said, "I know you're over here in this branch, but who do you respond to?"

I said, "I respond to your people."

He said, "Well, you don't work for us."

I said, "As far as I'm concerned, I do, because you're calling the shots as to what goes with the principal investigators."

He said, "Well, what happens if you get conflicting orders?"

I said, "I ignore them. As far as I'm concerned, you folks are calling the shots on the samples, and that's what we'd do." Apparently I said the right thing, because the eventual result of that was my whole position was taken off the branch over in 37 and I was put on the staff of the curator, which is the next part of this whole scheme of where we went with everything. It's kind of funny. Rather interesting years.

BUTLER: Yes, certainly sounds like it. I'm sure there's quite a few tales that surround it. A good overview that you've been able to us there with it. That move certainly makes sense, to put your section under his. They certainly relate to each other, as you said. Your orders would naturally come from that area.

With that, during that whole stages of Apollo 11, Apollo 12, and bringing all of this in order, getting it all functioning and flowing well, how much did you follow the actual missions while they were going on? Did you have a chance to watch any of those?

ANNEXSTAD: Oh, yes, we followed them, without question, and the reason for it is that wherever they're going to land, who's going to do this, I mean, these people were very, very highly trained and were going up to actually bring back pieces of the Moon and that sort of thing. It became pretty obvious after we had gotten into this thing, how incredibly important this whole process was and what was going on, plus the fact that the eyes of the world were on us.

While I had my little office in Building 37, sitting over there, and I had a little tiny office, but I could see right out in the hall, it was not uncommon to see the Vice President of the United States or senators going by, or Walter Cronkite and his staff and a constant run-through of people from Hollywood. The big guns were just back and forth, and it was not uncommon to see them in the hall all the time. All of this, it left an impression on you, and it made us all realize just how much the world was looking at all of this.

The astronauts, of course, would be quarantined in there, and the processes were very, very well defined. But it was like living in a fishbowl. We were paid basically forty hours a week and we were all government employees, but we were all young, and this whole system was so new, and it was so exciting that I think the average person was working fifty, sixty, seventy hours a week. We'd come into the lab and we would work extra hours. We would work in the evenings. We would come in on weekends. Nobody ever worried about time or anything like that. We were all mission-oriented, very definitely.

It was a wonderful experience from that standpoint to find yourself as a part of this whole system in history, and the Johnson Space Center was a key. This was NASA. This was not just the Johnson Space Center. I mean, to us the focus of the entire of the entire

agency was right here at JSC. I think it started at the very top and it worked its way down. There were all these little things I was telling you about. I think they're side notes and that sort of thing, but the overall feeling was one of incredible pride, almost sitting there and just saying, "My goodness sakes. Look at what we're doing. Look at what is going on."

Nobody knew how to do anything. Just how do you take apart a lunar sample? I remember being asked that, and I said, "The only experience I've ever had in taking apart rocks was I used to work in a stone quarry once." They said, "Well, maybe you've got some ideas." Everything was new. Nobody could tell you, "Don't do that," or, "You can't do this," because we didn't know if it could be done or not. So it was just a wonderful, wonderful experience.

BUTLER: Certainly sounds like it. As you mentioned, you had to figure out how to do some of these things, how to process some of these, because it hadn't been done before, and there were a lot of different things to take into consideration—the contamination factors that were an issue and trying to keep the samples pristine. Did that all come from just teamwork, from talking things out, getting ideas from various people for those processes, and they then evolve into formal procedures?

ANNEXSTAD: Well, it did in a way, but one of the things that you had, which was probably so important, was a team called LSAPT, the Lunar Sampling Analysis Planning Team. And one of the things that NASA has always done is if they had large problems—and JSC was no different—they would go out and they'd find the experts and ask them to come in and say, "What do you think should be done?"

They put together a team called Lunar Sample Analysis Planning Team, LSAPT, and this was the advisory board to the curator on the dissemination and handling disaggregation and all of the aspects of the lunar samples. Now, this team at the time I first became really associated with it and kind of knew what was going on, I knew they existed but I didn't really know who these people were. It was pretty well run by Jerry Wasserburg, who is a scientist. Jerry is still active. I saw him at the meeting, and I've known Jerry for a long period of time, but he's an incredibly competent individual who was a principal investigator that NASA had been funding at the rate of about a million dollars a year for years and years and years, probably one of the most important people in the lunar game out of Caltech. Well, here you are with credentials that cannot be denied anywhere, and he was brought in as the Lunar Sample Analysis Planning Team. He was part of this whole team.

Well, the other people that were on the board at that particular time, they were from Harvard, University of Chicago [Illinois], Stanford, all the big names and the big people that you can imagine, but they were brought in. They were geologists, people that had been involved in meteorites, people that had been involved in various kinds of things such as geochemists and whatever, and every one of them had international reputations. There were some younger folks. One of the people that was on there was not so young at that particular time, but it was somebody who had written a number of books on the solar system; this was John [A.] Wood from Smithsonian [Harvard-Smithsonian Center for Astrophysics]. Another fellow who was probably one of the finest mineralogists in the country, a young fellow by the name of James [J.] Papike, was on there. Jim today is the Director of the Meteorite Laboratory in the University of New Mexico. People like that, and they were brought in to advise and to handle everything.

Well, this kind of brings me to the next part of the question, because at this time, even though I had been directing the processing laboratory, the processing laboratory started to move over to Building 31, not completely, but in part.

My first association, for example, with the team was when they decided that we didn't have an inventory of the lunar sample. We didn't know what we had. I was brought into the meeting one time as director of the laboratory. Mike wanted me over there. Jerry Wasserburg asked me, he said, "We need an inventory of the sample. How are you going to handle it?"

I said, "Well, what I thought we would do is we would open every single sample bag that we have. We would put the samples on a scale and weigh them, and at the same we're going to take a picture so we have a back-up of everything."

He said, "Well, how much sample do you think we have?"

And my answer to him was, "We have what we have. That's all I can tell you. Nobody's going to run any of it out of here. But we'll do the inventory, and that's what we have, and that's it."

He said, "Well, that's good enough for me." So we went ahead and we did it.

Eventually things got much more sophisticated than just that. This was a very crude attempt at trying to do things, but we'd never done it before. So how are you going to start? Once you start, then you can get better and better as you go on. Well, eventually we started to move our operations over to Building 31. About that time I was actually appointed as an curator for processing, and Mike Duke brought over. Well, I was still running the laboratory but working very, very closely with him at that particular time and becoming very involved with the larger questions.

One day Mike had said to me, "You know, we've got a problem with the planning team."

I said, "What's that?"

He said, "Well, the planning team functions as an individual unit and one of the members has been appointed as secretary. He doesn't get everything down the way it supposed to be, and we have to use his notes to then define what are known as sample allocations," the samples that are going to the people, because we had a complete operation where these things would come up and they would mail samples out and some of it would have to be hand-carried and that sort of stuff. But the actual sample allocation was a series of charts that had been made up. Everything was being done by hand.

But the words that were coming out of the lunar sample analysis meetings were nothing other than what the secretary was trying to write down, and at this time I think he was somebody from the USGS or so, and he wasn't terribly interested in it.

Mike asked me if I'd being willing to come over and just kind of sit like a mouse in the corner and make some additional notes so we'd know what was going on. I said, "Okay," so I did. They were meeting about once a month, and they would meet for two or three days at a time a month. So I would sit over there during their entire meeting.

One of the fortunate things I had was that when I was in school, I had learned how to take good notes, and I was able to take very complete notes during these meetings. I was way over in the corner. I wasn't anywhere near this whole situation. That eventually became my next assignment, and I became the Executive Director of the Lunar Sample Analysis Planning Team and held that position for almost seven years, which was an interesting point,, and it started out only because I was able to sit in a corner and take fairly complete notes.

When we worked out the charts with everybody, after the planning team had gone, I had all these things laid out, and I was privy to an awful lot of the decision things that were made. I was privy to the day that Bryan Erb got fired and that sort of thing, or was removed from his job.

I became very well acquainted with all of the people on the team. This worked so much to my advantage that today many of the people that were on LSAPT and had done those things still remember those years and all the favors that I used to do for them. Eventually I got to the point where Mike Duke had me doing all the correspondence for the team. He used to come to the meetings, and after a while he stopped even coming to the meetings. He'd come in and he'd address them and then I'd stay there. As new members would come in, I was the one person that was not going on or off the team. So here you are with this situation. You know, knowledge is position and power, and it became a whole new career for me, which was very, very heavily involved with all of the aspects of all of the missions. Now, we had changed curators at that time, and Mike had moved on up to become division chief and that sort of stuff.

BUTLER: Now, while we're talking about the sample analysis and divvying it out and sending it around to the different investigators, how did that process work, since you were here involved first by just observing and then taking notes but then being more involved? What were some of the determining factors as to how the samples would be allocated?

ANNEXSTAD: People would write letters to the curator. These letters would come in. These letters could come from anybody, but basically the letters that we were using were letters

from laboratories and people of the laboratories that had been accepted as principal investigators for lunar samples. They were actually NASA grantees. They got NASA money. They would have to write their proposals for NASA money, and then, that said, that they were capable, once they got that, of working on the lunar samples, then they had to go through the next process, which was to request a sample. They had to send in a request letter, which had to be very precise. "This is what I want. This is what I want to do. This is how I'm going to do it, and this is what I hope to find." In other words, it was a second proposal, and these were two- and three- and four-page letters.

Now, the letters would come in, and then the letters would be cataloged and everything, and we would make copies and send them out to the individual members of LSAPT. Now, LSAPT was broken up into three or four different groups. There would be somebody on geology and somebody in geochemistry and somebody on age-dating and mineralogy and petroleum, various things like that. We would divide the request letters up and then we would get these request letters to the particular people, especially the chairman of that particular subcommittee. They would then be responsible for having the letters passed out to everybody else on the committee.

When the full committee met, even though the subcommittees had been meeting up until this time, the full committee would then sit down and they would vote on and decide not only who was going to get the sample, but how much sample they were going to get. This was an interesting point, because the people in the beginning that had been requesting sample were used to asking for hundreds of grams, and now we're dealing with something that costs so much money to come from the Moon, they were going get like ten milligrams or something like that, very small amounts, and they were to get only certain kinds of samples.

In other words, the team would not only take these requests and look at them and say, "This is what they want," they would say, "We're going to tell you what you're going to get." So they were involved with even designing experiments.

That was an enormous amount of power scientifically within the system. They had a greater hold on what had gone on as far as lunar sample analysis than anybody in the entire world. So eventually, only because I showed a lot of loyalty not only to the center, but a lot of loyalty to Mike Duke and to the science and things that were going on, I was very fortunate, I kept kind of stepping up and stepping up. Eventually I was in the position where all the letters would come to me. I would send them out to the subcommittee chairmen. I would keep a record of everything. I would read every single letter and made notes on who wanted what. I would make sure that everything was set up for the LSAPT meeting. They would get there, and when the decisions were then being made, and they would vote on them individually, individual samples, I would take down all the information.

Then after the meeting was over, I would turn all of this over to the people that were writing the orders for processing, and that was going to go on. I would then sit down and write letters of acceptance or rejections to all of the PIs [principal investigators] throughout the world that had sent these things.

Mike got this to the point that—I don't know how well you are aware of this, but if you're in an engineering group and you're down, say, at the level that I was at, I mean, I was not a branch chief or anything like that, although the curator was in office, so I suppose we were up a little bit better. If you wanted to write a letter to anyone, you wrote a letter for your branch chief's signature, and your branch chief then had to clear it with the division chief. Mike Duke got it to the point that I was writing letters to people all over the world

under my signature without having to go through anybody. Absolutely unheard of, and he defended it without question. He said, "We're dealing with these people everywhere. Somebody from Germany wants this, and they come on in. They'll write to us." He followed what I did, and he said, "This is the way we would like to have it done," and all the way on up, everybody knew that we were handling official correspondence directly out of our office. Mike would look at some of the letters in the beginning. After a while, I had full rein of everything. It was a wonderful experience of just plain, you might say not only cooperation, but trust in the things that you could do, and it made for a very interesting career.

So for somebody that had thought he was going to find himself on the way out the door, all of a sudden I was sitting here signing official letters to people all over the world. The correspondence I had was, oh, heavens, forty, fifty, sixty or more letters per month in correspondence. It got to the point that people eventually would start calling me. Well, you've got a problem when you do something like that. It's very easy to sit there and take all of this responsibility on yourself and you can easily get in trouble if you don't let your bosses what was going on.

I learned very early on that Mike wanted to know what was happening, but as long as he knew what was happening, then everything was just fine. So I would constantly keep him informed, and I would never let something go on that he didn't know about. This man is so sharp and so bright, that I could tell him something in passing in the hall, and two weeks later he'd remember the exact words of the conversation, as busy as he was. I felt that with Mike Duke I had an absolutely wonderful, wonderful working relationship this way.

BUTLER: It's good to be able to have that kind of relationship, especially in a job that was intense like this and that was so important as well.

ANNEXSTAD: Yes, it was just great. It was working, and it was marvelous. We had this wonderful opportunity to set procedures on our own, and that's what we did. It worked out very, very nicely. That's really what I did more than anything else, and that's how I got involved with working with these institutions, organizations that were doing this work. To this day, I still see, there are half a dozen or more people over there that were doing lunar samples all the way back to Apollo 11, and they still remember this. The fellows that were on the LSAPT team, many of them became very close personal friends. Today, now, I can call on them and I can send students to them, which is fun.

BUTLER: That's great.

ANNEXSTAD: Yes, that's great.

BUTLER: This is a good point for us to take a break and change out our tapes. [Tape change]

ANNEXSTAD: …working so strongly with LSAPT and everything, and because I was involved with processing, they asked me to be on what they called the preliminary examination team. Now, I was not what you would consider one of the premium members. The team was run by Paul [W.] Gast, who was the division chief at that time, and Mike, of course, was on it, and some of the other people. This was about a year before, I think, Paul

died rather tragically of cancer. Paul had been a professor of geochemistry at the University of Minnesota, and when he came in a few years before, he was brought in really as the division chief, and he brought with him a number of his people that were very, very good. These were folks that were heavily involved with all of the systems, including astronaut training and absolutely everything else.

Well, this was my first real association with him, and he was an incredibly brilliant individual who, I think, had a Ph.D. from Columbia [Columbia University, Palisades, New York] or had been at Columbia, you know, one of these sorts of things, and very, very well liked. Tony Calio was a very good friend of his, as was Jerry Wasserburg and this crowd. And I found myself on the team, and to me it was almost an honor to be associated with this kind of a situation. I don't think I had a lot of input. I recall going to meetings and being asked questions on "How are you going to do this?" and examination and whatnot.

But one of the offshoots of this whole thing was really kind of a funny assignment. When Harrison [H. "Jack"] Schmitt was on the Moon, he found what was known as orange soil, and, of course, everybody got excited. He put in the bag. It was on a small, little crater. When we were ready to open samples, the decision was that the orange soil was going to be opened in a setting in the LRL with everyone around. Now, Mike told me, "I want you to be in charge of just plain organizing this system. Now, you're going to be on the floor. The technicians will be there, and Jack Schmitt will be there," and whatever.

The day that all this was going to happen—of course, I wasn't on the preliminary examination team, but they had kind of told me, "We just want you to organize this," and it was kind of like this was not your moment of glory necessarily, but an opportunity that was going to present itself. Well, I was on the floor and I looked around and there were catwalks

around in there and the center director and the LSAPT people were there and the press was there. I mean, this was an array of higher-ups and bigwigs. Everybody's sitting in there. "Jack Schmitt's going to open up the orange soil." And I had to kind of control everybody. I had to make an announcement from the floor that this was going to happen and all that sort of stuff. I felt kind of funny about all of this, but after a while you get used to going all of these sorts of things, and I did. You know, "Ladies and gentlemen. This is going to begin now," and whatever.

Anyway, they went ahead and they're opening this bag and Jack Schmitt is standing there, and everybody's just waiting with bated breath. "What's Jack Schmitt going to say?" And he said, "Didn't look like that on the Moon." [Laughter] And I thought, all of these things were going on, and everything went smoothly from that standpoint. But it was the opening of the orange soil that Jack Schmitt had found, which was really kind of interesting. Of course, I had become acquainted with him, and he was a wonderful, wonderful person.

I was fortunate, I had met every one of the astronauts, some of them more than once. I had coached Little League football around here, and one of the better football players we had in the little league was Buzz Aldrin's son, and so we had met them. I've met Buzz Aldrin probably ten times since then, and he never remembers me. That's Buzz Aldrin. I mean, what can I say? I met all the other astronauts.

I don't know if you had seen this or not, but Fox News put on an hour special here several weeks that stated we did not go to the Moon.

BUTLER: We saw that.

ANNEXSTAD: And I not only became dismayed while I was looking at it, I became very, very angry and very upset. I told the story to my class here about a week ago, two weeks ago, that Apollo 12 went to the Moon. Really their biggest thing was to recover the Surveyor camera, and the parts from Surveyor camera were in my office in a locked file cabinet for years. Because of my position within the system, we had these things there. Of course, everybody was interested in the camera and the pieces of it and were looking at the lenses and that sort of thing, because that's the only thing that we had that had been on the surface of the Moon for quite some time and we could tell about such things as decay, tracks, and that sort of thing. So it was very interesting pieces of gear. Everybody kind of forgot about it, and it's since been sitting in a file drawer in my office for a long time till finally we ended up putting it away. It was in bags or that sort of stuff, but nobody ever even thought about it, little things like that that would occur. So, anyway, that was part of what I had to do with Apollo 17 preliminary examination team.

BUTLER: It's certainly an interesting part.

ANNEXSTAD: That was fun.

BUTLER: Well, mentioning and specifically for Apollo 17 the orange soil and having built up there, up to Apollo 17, developing these procedures, building it from the start, how much was applied from one mission to then either changing procedures or different ways of doing things or anticipating certain things for the next?

ANNEXSTAD: We tried to change. The question was improvement. Could we make things better? Could we handle the samples a little bit better? Of course, we also had the additional constraint that as you would go from one mission, they were going to bring back more and more sample. This became an additional problem.

I can tell you one story about Apollo 13, which was an interesting point. The missions were very close together at that time, and we were in many cases really just barely into sample distribution, and we did not have the facilities that we eventually had over in Building 31, where we had the more expanded facilities. In that situation I remember when Apollo 13 went up, we were not ready to accept more sample. We were really scrambling what was going to go on. In some ways, when that mission aborted, it gave us time to get ready to do a proper job for 14. So we were overly committed as far as sample was concerned at that time with just 11 and 12.

That's when, I remember it was Apollo 12, was when Jerry Wasserburg had asked me, "How would you take inventory?" So, I mean, we had a lot of work to do, and we were literally taking a look at sixteen-, eighteen-hour days and seven days a week to get ready for Apollo 13. If they had gone to the Moon and brought the samples back, we would really have been scrambling. So I remember that desperately.

Well, that gave us little bit more time, and by the time Apollo 14 came down the line and then, as you know, 15 went on and brought back a lot of sample. Dave [David R.] Scott was involved, and that was a very, very significant mission. [Apollos] 15, 16, and 17 had brought enormous amounts of sample back, and we were able to handle it by that time. Our staff was better. We were much better trained. Our people knew what was going on. I think our people had settled into positions where they could do the work and do it a lot more

efficiently, because back in 11 and 12 we didn't even know what we were doing. So you can do only just so much and get so much done during the day. You can have somebody working in these glove boxes, but you can't have them sitting in a glove box for eight hours. There's no way. And every time they wanted to do the slightest thing, they had to go and get a drink of water or they had to go to the bathroom or something like that, it was a question of getting in and out of the suits and moving back and forth. Everything was very laborious. So we just got a lot better at it, and that's one thing that tended to occur.

By the time 17 went on, we were in pretty good shape. We knew what was going on, and things were going very, very well. By that time the missions were becoming—they were never tedious—the missions were always very, very exciting, and this was a wonderful period of time. But we were becoming more and more comfortable with our entire job, and there was on-the-job training, is what it was, more than anything else.

BUTLER: Certainly sounds like it was. Since it was all new and hadn't been done before, it was a lot to build off of. Being that everything was new, and you're looking at these samples now from the Moon, and while there had been meteorites and such that they had on Earth, this was the first mass quantities, what are some of the first things that were learned from them? And then maybe if you could talk about some of the later big discoveries or changes in thought.

ANNEXSTAD: It was very interesting, because I was not a geologist, so I was in, for all practical purposes, kind of learning all of these things as we went along. We had these kids that were—or people that were so expert in mineralogy and petrology and they would just

look at the samples and they would call it this and that and they would do the descriptions and various things. So I was kind of ancillary to a lot of that part of the system. But it was very exciting and very time-consuming. So we were involved. Everybody was reading and trying to pick up and to learn, you know, where we were within that part of the system.

I guess probably what we learned more than everything else right from the very, very beginning was that the samples, especially that came back from Apollo 11, look basically uninteresting. They were simple basalt. They were something that related to much of what we have as a, say, sea floor area today, fine-grained and that sort of thing. They were very dusty, very dirty. All of the procedures that were run, we had to do such things as clean the samples.

We began at that particular time also to recognize that if we were to break the samples apart, how were we ever going to be able to identify what we had in the beginning? That's something that we started which was a little different. I guess they hadn't gone into that. We started to make models of the lunar samples. Then we developed procedures for taking them apart in such a way that we developed a method of being able to catalog each individual piece of sample in relationship back to the parents, and we developed an entire genetic history of the lunar samples, so this was something that went on.

I used to liken it to keeping a grocery store, because we had so many of these things that had been involved. You sit back and you say, "We have 100,000 items, and how do you put everything together?" which was involved. In the process I'm learning all of these about various parts of the sample.

The other thing that we picked up rather early on is that there were not as many minerals on the Moon as you'll find on the Earth, probably about 100. But they are also

forming in what we call a non-hydrating environment, so water was not a part of the formation part of it. The age-old question of was the Moon volcanism or was it impact was something that constantly came on up, but they began to realize very early on that primarily impact that had formed the outer portions of the surface but then a number of other processes were involved, including flood basalts that filled in the large marias and that sort of thing. So we were all learning more and more, although we had the good fortune of having the experts that had been speculating about the Moon for a very long time around us.

The other thing that happened was that the people that really had a handle on what was going on were those that had been working in meteorites. I didn't know anything about meteorites, not at all, but it became a very fascinating subject for us in relationship to everything, because the people that had worked in meteorites had been looking at cosmic materials for years, so they had a leg up on everybody. The words were up and down the hall. Everyone would hear these things and then working with the samples.

I have to tell you one story about the samples that was kind of an interesting situation. Because the samples would come in, we had our jobs and we were doing these things, and I was not in the position of looking at a sample and telling you that it has pyroxene or various other minerals within it or whatever. I was not involved with that because that was not something that I could do. I just didn't have that background. But by the same token, you find yourself not as enamored with the material you're working with as probably some of these people that had been working with meteorites and that sort of thing all along.

Then one day I was involved with the cutting of a large piece of Apollo 11. That was a 300-gram piece that was supposed to go to Washington University in St. Louis [Missouri]. The man who was the head of the McDonnell Center for Space Science was a fellow by the

name of Bob [Robert M.] Walker. Now, Bob Walker is a member of the National Academy of Sciences, who was an honored member of the Analysis Planning Team, was very, very well known and has good friends all over the system. He's a top-flight scientist. He came in that day to see us cutting this sample. Now, we're cutting a sample with what we called a wire saw, and one of the things that we found out about wire saws and anything else, and that's some of the things that we had developed, is that the wire saws have impregnated diamonds as the cutting tool. But what happened is the wire saw would run out of diamonds within about twenty minutes. That's something we eventually found out, and so I developed procedures for them to change the saw wire as quickly as possible. And you would say, "What takes all this time to do it?" I said, "Yes, but after about twenty minutes it's not cutting. All it's doing is just wearing away and it'll cause problems." So that's just a just side note on this.

But the day he was there, he came on in. He had stopped at my office and I met him for the very first time and I knew who he was. He was a very, very important individual. We have subsequently become very close friends. I told he and his wife this story, and they got quite a kick out of this. Bob doesn't remember this. They came on in and while we were cutting and Bob's looking at this sample. Now, we're not in a perfectly wonderfully controlled environment, but we're in an environment whereby at least he can't put his hands on the sample, but he's close to it, you know, like about this far away [Annexstad gestures], and he's watching what's going on. I looked over at him, and he's shaking, literally shaking. His face is gray, and the sweat is just pouring off his forehead. Here's a man who has devoted his life to cosmic materials, meteorites and everything else. He's looking at his sample from the Moon, that he's going to be looking at, a beautiful piece, and some work

that he's going to be doing with it, and he's almost beside himself. I've never forgotten this, because at that instant it was probably one of the clearest message that I had ever gotten that told me of the incredible importance of our work and where we were in relationship to science. It's something that you'll never forget.

I've shared the story with Gislane [phonetic], his wife, and with Bob, and Bob said, "No, I don't remember that." I said, "Bob, you stood there, and I was afraid you were going to get too close to the whole operation and start sweating all over it." It was just pouring off him. But that was really interesting. We had a lot of little things like that. As we go along, I hope you don't mind if I remember some of those. I'll throw them in.

BUTLER: No, those are great stories, because that shows some of the human element of all of this, and it is a great exploration program and it's science, but is involving people. Did you ever have a moment where you were working with these rocks and holding them and realizing, "Hey, these came from the Moon. These are something so different"? Or was that your moment?

ANNEXSTAD: I don't really recall being myself so impressed with just the rocks as much as I was with the people. I think it's how these things hit many of these other folks. See, I was not a laboratory scientist. I was a field person, and that was the entire situation. This was the first time that I had ever spent my time in laboratories. Everything else was field exploration, and that was my game, and it had been and has always been my first love. So it never quite occurred to me that this was something that I was so impressed with, although I saw certain samples and we saw some of the things that had gone on, and this was fun. I enjoyed this. I

was impressed by it, but not as I was, because I was more process-oriented. Processes, that's one of the reasons I was a geophysicist. I was involved with that.

One of the things that brings in one of the questions that you asked, is "Did you ever want to be astronaut?" and I said no. No, I was very happy with my work in exploration there, and for some reason I never really wanted to be an astronaut, although those were incredibly wonderful people. So many people want to be astronauts. They say, "I want to be an astronaut. I want to go into space." Well, I was very happy with everything else I was doing.

BUTLER: Well, you certainly had plenty of interesting things to do. Talking about the processing and, again, all the procedures building up, were you involved at all with the discussions or the handling of the suits when they would come back? You mentioned the dust on the rock, and the suits were covered as well. What was done on those?

ANNEXSTAD: Well, usually the suits went back to other people, and then what happened is we had technicians that would vacuum them, and they would take the samples and all away. Well, eventually the samples came to us, and we had to do that. But, no, I wasn't really involved with the suits in any way, not until at one time I started working with—and that will come much later in our interview, but when I started working with the Antarctic program again, I started to design clothing and worked with the suit people and tested some things for them. But that's another story.

But, no, we got the vacuuming material. We had to save everything, and we saved some of the worst stuff you can possibly imagine. People did all kinds of things with lunar

sample, and then when they were finished with it, they were supposed to send it back to us, and we'd get these jars of unexplained liquids that were within this system and we were somehow supposed to save all these things, and I think we basically did. But we got a lot of junk back, which some of it we got rid of. I think Mike Duke probably told you about some of these awful things that would come back, and what were these people doing in their laboratories with the lunar samples.

BUTLER: When they would send the samples back, would they send back any sort of report giving a rundown of what they did and the results?

ANNEXSTAD: Oh, yes. Oh, yes. We had full documentation on all of that, and they had to send the samples back. We would then put them back into storage and that sort of thing. We had to account for everything. So we did have a way that we could stand there and say that the sample had been used during the investigation process, that we were allowed that, but everything else had to be accounted for. Where was it? We'd sent so much out, and so much came back, and the real question was, did anything ever get into the hands of private people? We had hoped this would not happen, but my guess is that it may have happened on one or two cases.

But I remember when I was part of the processing laboratory in Building 37, I remember one of the down-the-line administrators calling me into his office one day and saying, "Look what we found," and he had a jar of lunar rocks that somebody had found somehow on his way out the door. That was an interesting experience, because he said to me, "You know, I got a hold of this. You could get fired over this."

I said, "Why?"

He said, "Well, it came out of your laboratory."

I said, "I have no idea what had happened."

He said, "Is there a way you can get it back into the inventory without anybody knowing about it?"

I said, "No, but give it to me." And he gave it to me. I went straight to Mike Duke, and I said, "Look. This is what happened."

Mike said, "Good. We'll handle it in this way." I mean, there were people that were working kind of against the system, but I had an association with Mike that was just such that I was very straight and forward with him, and I said, "This happened," and Mike said, "Absolutely no problem," and we never heard another word about it.

I don't know how the guy got the sample. I really don't. I'm not going to mention his name. I don't know how he got the sample, but, I mean, he had a stainless steel canister that was this tall, this big around [Annexstad gestures], and it was filled with pieces of moon rock. I don't where it came from. I don't know who had done it, or what had happened. We didn't ask any questions. We were just happy we'd got it back and it became part of the inventory. Of course, he was afraid if I hadn't been able to spirit it back in, that I would get blamed and I would get fired, and, no way. I just went to Mike Duke, and I said, "This is what happened," and Mike said, "Good. We'll do it this way."

BUTLER: Well, it's always good to be straightforward.

Over the years that the lab built up and built through all these procedures, and we've talked about how the procedures have changed and how the processes have changed, but how

did the people and the feeling of it all change as time went on, and now that it's continued and grown into other areas?

ANNEXSTAD: One of the things that I discovered rather early on is, there was a great amount of loyalty among everyone. It didn't make any difference whether you were a contractor or a NASA employee or what. There was a great amount of loyalty to the program and to what we were doing, and over the years we were able to build up a group of people that were part of the processing system, and folks that would come in and would work and would give us input on how to change procedures of various kinds of things that would go on.

This loyalty has extended over the years to the point that there are still a number of people that are working in the processing laboratory that I worked with when I was here back in 1984 and '85, or even before that. They're still here. This is a good place to work. It really is. It's a beautiful center. Maybe no one really likes to live in Houston heat or anything like that, but the work is very different. The work is very exciting, and you know that you're on the cutting edge of a number of different things. And the people that we had were just absolutely marvelous.

One of the things that we developed over a period of time, we had a number of women that came in that were hired as processors, and in many ways they turned out to be a lot better than the men because women are much more capable of handling fine work. They were very interested in doing this, and we had some marvelous detail people, and some of them are still there, as I say. We would try to listen to them. There were always little personality things going on back and forth. You always have that within the system. But we actually had developed, I think, in the processing game a rather well-functioning team.

We had our problems at times. I remember one particular period of time we had a curator that had strange kinds of ideas, and after he was gone, everything settled down again. Then every so often Brown and Root, the contractor, would bring in a new supervisor that didn't know what was going on, and everyone would live through that until they finally got the supervisor to the point that the supervisor knew that he shouldn't come in and tell them what to do because they knew their jobs. This was part of a whole situation.

It was a team effort, and I think this was the kind of thing that justified just about everything that JSC ever did. This whole center is a series of teams and people basically working together. You can kind of shunt a lot of the negatives off to the side, although they may make interesting stories at times, but it's the total team effort.

I remember one time we had a major meeting at the center. We used to have them up in the big auditorium, in Building Number 2, the place right next door. Chris Kraft came in and was addressing the entire center. We had everybody there, hundreds of people. He opened his comments with something like this—I can't remember the exact words—something like, "We have been accused of having a superior attitude here at JSC over the scientists and the other parts of NASA." I remember him saying "superior attitude," and he said, "I don't want you to lose that." And that was basically, I mean, there was a level of pride all over the center, and it extended from the administrators to the astronauts and all the way down to the people at the lowest possible level.

Even though you were here and all of us would take vacations and we were from all over and you'd go back, and people would just kind of look at you like, "Oh, boy, you work for NASA. You work at the Johnson Space Center," and that sort of thing. Like I said, and

many times it was like living in a fishbowl. People were there but there was something about being able to say—you're traveling somewhere and someone says, "Well, what do you do?"

"Well, I work at JSC."

"You do? What do you do there?"

"Well, I'm the Associate Curator of Lunar Samples."

"You are?" All of a sudden it's—and after a while you don't know what to say. All you do is you know that this is what you do, and that's where you've been.

BUTLER: Well, it certainly is a unique opportunity that, as you mentioned, the Apollo Program captured the whole world.

ANNEXSTAD: It did.

BUTLER: So having a job like that would certainly be a draw for folks. With the Apollo Program, we've talked now up through Apollo 17. As the Apollo Program was coming to a close, obviously your job was still going to continue. The processing was still going to continue. Still to today, it has. But did you have any thoughts on the end of the program?

ANNEXSTAD: We were quite disappointed. The money was really going away. The powers that were were doing everything they could to move us into the next direction. Of course, the Space Shuttle was that, and it was a very important step, but it became obvious to us and to everyone here at the center that the so-called glory days of Apollo were gone, and that people were standing there saying dumb things like "Twenty-six billion dollars for a bunch of rocks"

and things like that, yet everyone here knew the importance of the program, and we knew those things should go on. We had a number of missions on the board that were truncated at Apollo 17, which was a great disappointment.

And the money went down, and the money went down so far that even though the Space Shuttle was on the drawing boards and they were moving in that direction, they could not stand here and say, "Three or four years from now, we're going to need this much money to do it" and convinced Congress to do it. So we were in things like budget shortfalls and various things like that. And we were all looking for other points of business, although in the processing game, we were just trying to get more and more efficient. That was continuing to go on, although we didn't have as many laboratories involved. During Apollo 11 and 12, I think we had something 110 laboratories throughout the world that were getting samples. I don't know the exact number; I'm just throwing that out. And those numbers stared to falter and go back.

We did such things as the Lunar Sample Analysis Planning Team used to meet once a month. Eventually it started to meet every three months, and then eventually about once every six months. So, money for consultants was going. Money for travel was down. Even though you wanted to do more things, they started to doing such things as saying that if you wanted to do research here at the Johnson Space Center, then you had to take the same chances as the people that are on the outside and you had to apply for your money through NASA, which made it a very difficult thing. We were trying to increase our scientific base while we were still here, but money was becoming a very serious problem.

We began to recognize that for some reason we were not the darlings of the world any longer, and that in many ways kind of hurt. I remember we went through a reduction in

force, and it was a very painful type of thing. It was painful for everybody, because all of a sudden, I remember the word came down, and the word was, "Do you realize that the Johnson Space Center has the highest individual GS grade of any installation that the United States has in the world?" We were highest average grade. It was something like 11.3 or somewhere along in there, of all of the GS grades. No one else in the world was at that level. OMB [Office of Management and Budget] came down and said, "You've got to get these numbers down." Friends of mine were moved from GS-15s to 13s, things like that. If they wanted to keep their job, they lost several grades and things like that. It was a very dismaying period of time. Morale was really quite down, and nobody asked the question, what did it take to put people on the Moon and to bring back samples and to handle programs that were so difficult and so unknown that we were literally thinking on our feet for years. And this was our reward: "Well, you're going to lose your job," or, "You're going to be cut in pay." I had a friend who lost $10,000 a year in pay. He was making, say, 45,000 at the time, but that's substantial when you get knocked down to there. Today, I suppose, the same person if he lost 10,000 dollars in pay and was making 120, then that's a difference. But at that particular time, not even the center director was making enormous amounts of money.

So it was a very difficult period of time, and I think it was one that was dismaying to most of us on a basis of just saying, "Why are we being rewarded in this way?" But a number of us stayed on. I was fortunate. I had military service time. This put me in a different category than many of the scientists. Many of the scientists that were hired were not at my age, and, of course, they didn't have military service, so they didn't have retention, and I did, and that helped me. But we lost people. They started to go different places. The

brightest and the best started to leave, and they found other jobs. The center was cutting way back, and I think there was a general feeling of low morale, a lot of that.

BUTLER: Did that build back up over time?

ANNEXSTAD: To a point, yes. I think we're now, from what I hear, we're starting to go into something not dissimilar to that again. NASA is in a very unique position. When everybody loves NASA, then we're in good shape, and then when we kind of lose our position, then people say, "Well, why are you putting all this money into here?" Yet if you stop and you talk to school children today, talk about middle-school children. Middle-school children are interested in two things: dinosaurs and space, and that's all there is to it.

At times we hear this old question of "Too much money is being spent on space. Why don't we give it to the poor and homeless?" or do this or do that. And they forget one rather important thing. It's why a government exists. And if a government does exist, it actually exists so that you can do collectively for everyone that which they cannot do individually. If you're to be doing something like that, there should be a small amount of at least effort, if not money, that should go to something that is pure exploration, generally reaching out from yourself to do something that's a little bit different. The average commercial company is not going to go into something that's unknown unless they can amortize the cost in five years or less.

The government is the only thing that can go into something like this and go into such things as solar system exploration, or the things that we're doing right now, the [International] Space Station and all those things. So I feel that in some ways, like everyone

else, that we were blindsided and almost left adrift. Because of the people, because of the type of management I think that we had, and because there was a loyalty to what we were doing, the agency has survived and, I think, will continue to do so.

BUTLER: Exploration is a key part of the human nature, so I certainly hope that will be a drive that does continue.

ANNEXSTAD: I agree.

BUTLER: And to some extent the work with extraterrestrial samples has continued, on a smaller scale, a much smaller scale, obviously, from the Apollo Program. But with the meteorite program, how did that evolve from what work you had initially been doing, and what differences were there in the way meteorite samples were handled from the lunar—

ANNEXSTAD: That's long story, and you're going to have to hear the whole story. I'd been involved in my life with what I feel are three wonderful major advances in science, to the point that I can honestly say that would I die tomorrow, I'd feel I've had an incredibly full, wonderful life in exploration.

The very first thing that I was involved with was the IGY, as I mentioned. The Apollo Program was number two. These were marvelous. And the Antarctic Meteorite Program was number three. Now, I have to tell you this story, because I knew you were going up with this. So I had made some notes and I wrote down here. This is the complete story of the Antarctic Meteorite Program.

BUTLER: Great. Okay.

ANNEXSTAD: Back in 1969, unknown to me at that time, some Japanese had found some meteorites in Antarctica. They didn't realize what they had. They brought them back, and the meteorites, there were nine specimens, and one of the chief mineralogists at the Tokyo Museum at that time, a woman by the name of Madame Shima [phonetic], looked at them and said, "There are four different types." Now, this took a while to get out, and it finally got out at a Meteorological Society meeting, and a fellow by the name of Bill [William A.] Cassidy saw this and said, "Oh, maybe there are meteorites over in the American sector."

Now, before I came to NASA, I'd been on four Antarctic expeditions, and I did a lot of work in the Arctic. So I was very heavily involved with that. So you mention the Antarctic and immediately, you know, that's old home to me. I've got a lot of ice time. I was very heavily involved.

Well, during all the years I was with Apollo and doing all that sort of stuff, I was still very interested in what had gone on in the Antarctic, because I had worked down there during the fifties and the sixties. Well, along about, I think it was 1976, Bill Cassidy went down, he found some meteorites and he came back with them. And I found out about this. I can't remember why I found out that he had gotten meteorites, but I looked at this whole thing, and he had said, I think in publications, where the meteorites were found. Immediately I recognized that he's probably only just touched the surface; there's probably a lot more down there.

At this time I was still in LSAPT. I was part of the processing laboratory, and I was still working, and I was doing some additional work down there. Mike Duke was the division chief at this time. He had moved on up to that, and we had another curator, who will go unnamed at this point. I went down to Mike one day, and we had talked kind of around this idea at time about, "Should we kind of get into the meteorite game? We've got some capabilities, and we can look at meteorites." This was something that everybody had just kind of talked about but never did anything or whatever. I went down to Mike one day, and I said, "Mike, I've got one of my crazy ideas."

He said, "What's that?"

I said, "Why don't we curate Antarctic meteorites?"

And his jaw dropped. He looked at me and he said, "That's a heck of an idea." He said, "Do you know anybody at the National Science Foundation."

I said, "Oh, yes, I know dozens of guys." I knew one guy. We'd been in graduate school together, and he was a program manager for upper atmospheric.

Mike said, "Well, see what you find out what they're going to do, because I know that they're funding this guy Cassidy." Mike said he knew Bill Cassidy, but he said, "I know that they're funding him, and he's been down there finding meteorites."

So I called NSF. I get a hold of my friend Ben [Robert] Fogel, and I said, "Ben, who's doing this?"

He said, "Oh, that's Mark Turner. I'll give you his number."

So I got on the phone and I called Mark Turner, and I said, "What's going on here?"

"Well, you know, we're funding him and he's planning this."

I said, "Do you know, we should probably talk about the possibility of becoming the curation center for the unearthed meteorites. I'll bet there are going to be a lot of them."

Mark said, "Well, that's a good idea. Let's talk about it."

So I went back to Mike and I told him what was going to happen. I said, "Mark told me this is going to go on, and Cassidy's going to go down again and whatever, and we should probably get into this."

So Mike said, "Well, let's invite Cassidy to come and talk to us and tell him what we've got." So we did. We set the guy up. He came walking in, and we had everybody there and we kind of wined and dined him and showed him our facilities and we could do this, and Bill Cassidy all along was kind of saying, "Well, you know, I supposed we were going to keep them at [University of] Pittsburgh [Pennsylvania], but, you know, we really can do this, but maybe you fellows know a little more about it."

Well, we started to work on this, and [Dr. Donald D.] Don Bogard and I got together. Now, Don Bogard did his Ph.D. at the University of Arkansas. He's a cosmochemist, and he was into meteorites. He knew meteorites. I couldn't tell you a meteorite from a Moon rock. I didn't know the difference between them or anything like that. All I knew was that I think we've got an opportunity. Mike used to say to me, "The only reason you wanted to get into this is you wanted to go back to Antarctica." I said, "Well, that's right."

So we sat down, and Bill Cassidy said, "Maybe we can work out something like this." Well, then pretty soon the NSF people started to come through, and one of them was a chief scientist. Mike at this time did his typical thing, he said, "Bogard and Annexstad, you go ahead and you are representing us. You just go ahead and negotiate with these people and see if you can set everything up." So we started working on it.

We began to realize that there were to be three players in the group: JSC, the National Science Foundation, and [the] Smithsonian [Instutition]. Now, Smithsonian was pulling this little game of saying, "If you find anything on federal land, because of what is known as the Antiquities Act, it belongs to the Smithsonian."

But we pointed out to them this stuff is in the Antarctic, which is covered by treaty, and the treaty said that not any one country owns or can keep Antarctica. So I said, "We offered to become actually the curation center for the National Science Foundation, and then Smithsonian eventually was going to become the repository for the meteorites after everything was done."

We sat down and set up a program based completely upon what we did with the lunar samples. Mike said, "Go get them. You can have this area in processing." We got some of the processors and said, "You're going to now work on meteorites." We went out here to the storage area and we found a bunch of the old cabinets that had been used during Apollo 11 that nobody had gotten rid of, and we grabbed them and cleaned them up and put them back in, and in a matter of several months we had set up a processing laboratory to handle Antarctic meteorites.

Well, in the intervening period of time we're sitting here looking at how was this going to work. So Don Bogard and I sat down and put together a three-agency agreement among NASA, Smithsonian, and the National Science Foundation, that said this is how this whole program is going to operate. We put together all of the procedures, everything that was going to go on, and how the meteors are going to come in, and how they were going to be handled, how were they going to be cataloged. We put together a Meteorite Working Group like LSAPT, and these people were going to make decisions on who's going to get the

samples. In other words, we had a complete administrative setup that was similar to the Apollo Program.

Well, as you can imagine, all kinds of people came through and they said, "You can't do this. These meteorites are contaminated. This isn't good. This is overkill," and whatever. We pointed out to them that our people were fully and completely trained in all of the procedures for lunar samples, and we said, "We can change things anytime you want, but where we are right now, if we follow everything that we have done before, it's going to work perfectly. Now, just remember, I know it's going to take longer. It's overkill. It's more involved because these aren't lunar samples. But we know how to do the job, and that means that we're not going to make any mistakes." So this was the perfect opportunity.

Well, the National Science Foundation got into it. We had people in Washington that put it together, and we got a three-agency agreement, the first time this has ever happened. They put this all together and we went into the formal mode.

In the meantime, I had asked Cassidy if I could go to the Antarctic with him, and NASA said, "If he wants you to go, go ahead." So I then did the next five expeditions as the NASA representative. I went down and set up the collection procedures. How do you collect the meteorite on the ice in the Antarctica? We showed them how to do it. I brought down cameras. I got all kinds of support from NASA. Wonderful. They sent down three Hasselblad cameras just to take pictures. We came up with very solid procedures of how you pick up meteorites and you don't touch them and how you put them in either plastic or aluminum foil or stainless steel containers. We set up this whole procedure, and I was the guy that went down to the Antarctic. I went down in '78, '79, '80, '81, '82. So I did five

expeditions with NASA, and then I worked with a couple of other countries later on. But we set up an entire program.

In association with all of this, Don Bogard was put on the Meteorite Working Group, and I was put on as the Executive Director of the Meteorite Working Group, and we ran the show right until 1986 in this whole thing, put this whole thing together. It was so much fun, and it worked out so that all kinds of different stories associated with it, but talk about a shot in the arm, because all of a sudden we're down there and we're finding things that we never had before.

Oh, we had all kinds of people that were angry with us, because they said, "All you guys at JSC get a chance to see these samples before anybody else." We were able to work through all of these things. We had a solid program, which is still going on, and due to all of this, every single meteorite comes through JSC. JSC had literally retained control over this whole system, some of it's going on now to Smithsonian, but JSC is a major player within the system. A number of people that got involved were folks that have been here. The woman that found the one that they think has life on Mars used to work for me. I got her into the program and literally trained her, Robbie [Roberta] Score. But the interesting thing was I was on the team that found the first Martian meteorite in 1979.

So I ran the field show. Actually, I was very fortunate. Bill Cassidy wanted me to run the field program because I'd had all of this other experience. In the process now I did ten scientific expeditions and then a couple of years I was senior lecturer on two cruises to the Antarctic, and I'd tell all the little gray-haired old men that want to go to the Antarctic and don't mind paying $15,000 for the trip what it's like to be down there.

But this was a dream come true and we had more fun. All of a sudden it was a shot in the arm for us. And the publicity we got! Then we found something that came from Mars. After that they found something that came from the Moon, and I would say the program is history. I received several awards from JSC for this, and it was just an absolutely wonderful thing. I tell you, scientifically to be able to work on three major directions, one would have been enough, but to work in all three like that. And it would never have happened if we hadn't had the Apollo experience and the Apollo base, because Apollo was our training ground. All of a sudden now we turn around and we have, for all practical purposes, quadrupled the world's supply of material from other areas, asteroid belt, Moon, Mars, and other places like that. This whole question of where we're going to go on Mars and all is tied up right with this particular processing system. So it all hinges back to the early years.

BUTLER: Absolutely. Even the processes that you've established for and now maintain started with Apollo. Working for the meteorite program is not only going to determine to some extent probably where we're going on Mars, but then what happens when all of that comes back.

ANNEXSTAD: I think that you're probably going to find that the constraints on the program will be even greater with the Mars program than they were with the Moon program, but it's probably not going to be quite as painful in some ways, because we know a little bit more about what's happening and we have people that are still there. Of course, this latest thing with do we have fossilized life on Mars, oh, that is so exciting. That's a story within itself, as is this question of did we have a Martian meteorite.

Doug [Douglas P.] Blanchard eventually became the Division Chief of the Solar System Exploration Division. Doug and I had always been very close personal friends. He was brought in rather early on. When they were ready to make the public announcement about [Dr. David] McKay, [Dr. Everett] Gibson, and Kathie Thomas-Keprta having found what they think was fossilized life on this Mars meteorite, Doug called me before the thing. I was living in northern Minnesota. He said, "We want you to know before you hear over the public press, because of the work that you had done here before." I was so flattered. I was just thrilled. I told my wife at that time—my first wife had died, and I had gotten married again, and I told my wife, "This is unbelievable. I can't believe it. They called me and they told me this." I was just overwhelmed. You think that in some ways that when you leave, that you're kind of forgotten, but in many ways you're not.

So I took an early retirement in '86 and went off, because I had a position. Somebody wanted me to go teaching.

I should tell you this, that during the years I was with NASA, when I came here, I didn't have my doctorate, and I had most of my work done for my doctorate in upper atmospheric geophysics. During the particular time when I was working for Bryan Erb and all, a program came up at the University of Oklahoma in Norman, about management, and Bryan suggested that I may want to take a look at that. He said, "Get involved in some of that sort of thing." I was still kind of hiding.

So I did a master's of public administration, actually pretty much in political science at the University of Oklahoma, which NASA paid for. Well, after I got involved in the Antarctic Meteorite Program, one of the people that came over here was a cosmochemist by the name of Ludolf Schultz, and went down to the Antarctic with us and he realized I was

getting very involved with meteorites, glaciology, and the kind of work that went on with what we will call mechanisms of concentration in meteorites. He asked me if I would like to finish my doctorate in Mainz in West Germany. I said, "Well, I'll see what I can do."

So I came back home that year. I had been in the Antarctic again, and talked to Mike, and Mike said, "Well, I think that's a good idea. How can you work this out?"

I said, "Well, I'd like to apply for a year off, because I'm at the point where all I need to do is really do my research and dissertation.

It turned out that we couldn't do it. We already had two people from the division that were overseas for that year. But Mike said, "Go over as you can and see if you can find some money and whatever."

So I wrote a proposal for the National Science Foundation and got travel money, and NASA gave me a little bit of travel money. For the next three years I would go to the Antarctic and be gone from October till January. I would come back, wind up my work. Then about every month or two, I would travel to Europe, and I was doing my work over there and continuing on with all of this. The result was that in 1983 I finished my doctorate over there, which immediately changed the whole course of the next part of my life on that particular point. But NASA had a big hand in that, too. My dissertation was involved with Antarctic meteorites in the NASA program.

One of the other things I did was for my Oklahoma work, I wrote a thesis on the management of the Lunar Receiving Laboratory. So NASA's had a very, very strong hand in all aspects of my career, and it's been interesting. Good point.

BUTLER: It's certainly a very interesting career that you have had with NASA. Looking back over your career, what would you say was your greatest challenge and then also your most significant accomplishment?

ANNEXSTAD: I think I had made a note about that somewhere along in here, and I need to take a look and see what I had said. What was my greatest challenge? I think probably the greatest challenge that I ran into was somehow learning how to function within this large system that we have, called NASA.

This is a very good place to work, it's a very challenging place to work, and at the same time it was a very difficult place to work. So much is expected of you. If you come into this, regardless of what goes on, you are automatically in a class that is different than anything you've ever done before. The challenge is not only to fit in, but also to maintain in some ways your own capabilities, your own ideas, and to be able to be a strong member of the team without the team completely dictating every little aspect of your life.

I think I found this to probably be more difficult in some ways for me because I was a field man. I worked best on the basis of give me an assignment, tell me what my limits are, forget about me, and I'll go off and I'll do it. I'll come back. I don't take day-to-day direction very well. I like to be involved with the overall picture of just handling your own affairs and doing your own thing. I never wanted to be the director of anything or anything like that. That's not as important to me as just having an opportunity to function on my own. I think that was the challenge that I ran across as far as NASA was concerned, because I'd lived through those years of trying to, see, well, what is your own self-worth, are you so enmeshed in this, and how can you just find yourself within this system, and I think I was

able to do that. Some people never are, and many people just kind of fall right in line and never ask questions and that sort of thing. So that was it.

My greatest accomplishment, I think, was the Antarctic Meteorite Program, without a doubt. I look back on that, and frequently I just am in awe that that thing worked out, that the whole thing went. I feel that we did something there that was so unique and so different, yet so involved with what was our mission, that it's going to live for a long, long time. I had the wonderful opportunity of working with just some of the most magnificent people in the world: Dave McKay, Everett Gibson, Mike Duke, Don Bogard. Right down the line, you know, the people that were involved. Gary [E.] Lofgren, Bill [William C.] Phinney was here. These are old, wonderful friends and people that were involved. This particular program I was able to take a very strong hand in, and I look back on it and still at times I say, "Did this really happen?"

BUTLER: I can understand that. Looking at that program, and it has had a tremendous impact on the science community, especially the planetary community, from your perspective, how has that affected the way the whole procedure works, the study of meteorites and the results that are found? How much new science has been able to be done because they are processed and curated in such a controlled fashion versus just picking it up in the field and studying it that way?

ANNEXSTAD: One of the things we did is we took meteorites out of the hands of collectors. We also put some order into the program in such a way that those people that in the past wanted to work with meteorites and used to say, "We have so much trouble. I wanted to

work on such and such a meteorite and this is in the hands of this person or they're in the hands of this group and they really don't want to get it—." We put this whole situation in a manner that dozens of researchers throughout the world could just write us a letter, and we knew who they were, and they could write a letter and propose and say, "We would like some samples because we want to look at this and look at this."

We brought a semblance of order to a chaotic program that had never had it, and all of a sudden now we are seeing the fruits of our labor, which are meteorites that we never had, material that we never realized that we could get in these amounts, a direction that's moving very strongly towards life elsewhere. Who know where this is going to go. Just by putting together a program that has some sort of semblance of order to it, we were able to bring together all of this that in some ways that has not rivaled the Apollo Program, but it's a direct offshoot of the Apollo Program and I think something that all of us can be very, very proud of.

I'm very strongly committed to this direction of looking for life elsewhere and trying to understand what's going on within the solar system, and the Apollo Program gave us a baseline. It told us how old things were and how things could be different and it allowed us to be able to look at things in a proper manner. Now along comes the meteorites and it's just a little bit of real sugar coating on top of this wonderful cake that's been built. So it's a very exciting thing, I think.

BUTLER: Oh, it certainly is. It's certainly very exciting, and there's going to be many more years of new discoveries to come, I'm sure. You mentioned that you were on the team where they discovered the first Martian meteorite and then discovered the lunar and then the

Martian meteorites were discovered, the possibilities for life on Mars. Obviously when you're out on the field gathering these, you're not doing detailed study of the sample at that time, but can you tell differences between any of those in the field? Are you able to do any field examination?

ANNEXSTAD: Oh, yes, very definitely. A lot depends on where do you find the meteorites. Now, if you're away from a normal collection of rocks in this particular part of the Antarctic and you get just back from there a little bit, anything that is on the surface of the ice is a meteorite. You already know that. But you can tell the very basic types. We can tell what is an iron. We can tell what is a chondritic-type meteorite, what is an achondrite. You become quite adept at being able to tell meteorites with surficial types of information.

But one thing we can't do is we can't do mineralogy or tell what they're composed of or that sort of thing. We can tell the types and all. So you become very good at that sort of thing. I suppose individually I've picked up several hundreds of meteorites on my own. To most people that have been looking at meteorites as cosmochemists or whatever, most of them have never found a meteorite, not at all. They get them out of museums and whatever. So this is quite an experience.

We spent a lot of time looking at various meteorite types and beginning to tell ourselves what we should be looking for. Then they got out in the field, and you could recognize it right away. So this was fun. And we would say, "Oh, we think this is an achondrite," or, "This is a chondrite," or, "This is an iron," or, "This is a carbonaceous chondrite," and that's about as far as we would go. What we wanted to do more than anything else was to bag them up properly and get them back to the Johnson Space Center.

That's another one of those stories I forgot to tell you. I got involved with this. The meteorites would be found in the Antarctic, we would keep them cold, and then we said, "How are we going to get them back here without them thawing and we could put them in our cabinets?"

We thought about this and we said, "We need containers." Still to this day, I don't remember exactly how I did it, but I got on the phone one day and started to call around, and I got a hold of the people out at Long Island. There's a company out there, Grumman [Corporation], in Long Island [New York]. For some reason, I had talked to my neighbor, who was the local Grumman director, and I said something about, "We need some containers," and he said, "Call this guy, Ed, in Grumman, out in Long Island."

So I called him, and I said that my neighbor had asked me to call him. "Yes," he said, "I've got a bunch of containers. They're plastic, and they're kind of nice. We don't know what to do with them. Would you like to have them?"

I said, "How many of them are there?" There were like eight or ten.

I said, "Yes. Why don't you send them out to us."

"Oh, that's great. We'd like to get rid of them."

So these things are shipped out to JSC, and they were various sizes of containers. One of them contained a small jet engine. I didn't know what to do with this thing, you know. These things came in, and they came in through shipping, and shipping called me and said, "Annexstad, what have you got coming in here now?" We had these containers. I said, "You can keep the jet engine."

So we took these other containers and we cleaned them up, and they were very, very nice. We took those and sent them out to the West Coast and then we had them shipped to

the Antarctic, and in the Antarctic we had the meteorites put in these containers. We'd send a couple of them down, and they would then put into the freezer on the ship. They remained frozen from the time they were found all the time till they got to the West Coast. Then we had special people at the West Coast that would take these and unload them.

Now, here's where Robbie Score came into the game. I sent her out to the West Coast every year to make sure that the meteorites were repacked properly with dry ice and everything else and shipped to the Johnson Space Center. They would come into the old Lunar Receiving Laboratory, and we had a freezer space in the old LRL, and these samples would go directly into a freezer that we kept at forty below until we needed them, moved them on over to our dry nitrogen area, where we then would them in the dry nitrogen environment of the closed cabinetry.

But this was funny. I mean, we got these boxes, and the boxes then ended up being used when we moved twenty, fifteen percent of the lunar samples to San Antonio. That's another job that I had. You had asked me this question of what I have been doing also in the side. Mike had me put together the whole program of transporting the lunar samples to San Antonio. We used our new boxes, which we did use the one that had their jet engine in there. It was a small one.

BUTLER: Well, that must been an interesting program in itself, the transport and the security for all of that.

ANNEXSTAD: I was Mike's representative to the rest of the center. This was completely security-controlled and everything else. We ran all kinds of tests on various ways to move

the samples, and we finally decided to use a bus that had all the seats removed and everything else. I remember the first night. We'd decided to do it at night, and we were moving the samples. It was a big security thing. We moved a busload of these samples. Another fellow and I literally sat on the floor of the bus with our hands on the boxes to make sure that the bus didn't vibrate too much. We went at a speed of forty miles an hour from here to San Antonio with police escort and everything going all the way. Oh, it took us weeks to put this whole operation together, but it was another one of these great, wonderful opportunities. I would sit in the meetings with the security and representatives from the center director's office and everything. Mike had told me, "This is the way we're going to do it, and don't back off." Oh, we had more fun in that.

BUTLER: How did you select which samples would be sent to that facility?

ANNEXSTAD: The Lunar Sample Analysis Planning Team got into it and made the decision and said, "These are the samples that we want. We want a representative of every suite that we had." So the percentage covers so that if these samples are lost over here for some reason, and that probably shouldn't be, but we still have a suite of fifteen percent, but a suite of everything that we have. So that was the choice that was made.

See, during all of this, while I was doing all of these things with the Antarctic meteorites and everything else, we were building a new Lunar Receiving Laboratory and we were moving samples back and forth. We had an awful lot of things going on. It was a very exciting period of time, almost as exciting as Apollo.

BUTLER: Going back to the Antarctic Program, you mentioned the process of putting them in a box and shipping them here, keeping them frozen. What was the process from when they were first discovered on the ice? You mentioned bagging them? Would there be photography as there had been on Moon, photographing it in place before picking it up and bagging it?

ANNEXSTAD: One of the things that I did was we sat down and I suggested very strongly, and, of course, Mike and Don Bogard and I are talking these things through, but one of the things we suggested was that when we would find a sample in the Antarctic, we would want to take a photograph of that particular sample to identify it in the field. But the photograph should be what we would call "self-consistent." So at this particular time we found that we had something that we had been using years ago in the lunar sample game but we hadn't before, which was a small, little counter which would enable you to dial numbers into the counter. It had a small gray scale and then a six-centimeter measuring scale at the bottom. It was just a little rectangular counter like this. So I said, "Why don't we use these?" because we hadn't been using them in the lab anymore. So we took those into the field with us. Also then what we took were stainless steel, aluminum foil, and plastic bagging that could be used, and all of this was clean. It had been cleaned to the same standards that we were using with lunar sample and freon and everything else.

Then we would go into the field. I trained the people to do this. I put together a series of collection packs. We bought packs, and in there would be the collection materials. If you found a meteorite, the first thing you did was then you would take a look at what was the number that you would have in a small, little aluminum cleaned number. Any number.

Like, for example, the number could be 1001 or whatever it is. This was the field number. It was not the number that the meteorite eventually got. So you would take that number, and you would dial it into the little counter, put the counter behind the meteorite, take the photograph of the counter and the meteorite. Then you would take out either forceps, or I showed them how to do this with a scissors, stainless steel scissors, and you would pick the meteorite up, especially if it was small. If it was a little bit larger, you would surround it with plastic or something like this, and you would bag it, put it in, and inside the bag would go this clean little aluminum tag that had the same number as that. And you bagged it up.

You then sit down and you would make your notes and your field notes. "This number meteorite was found on this date at this time. These are the conditions and this is where we found them, on such and such an ice field," etc., etc., etc., and this sort of thing. Now, that's the way everything would stay. When we got back, we would turn our notes back in to our folks here. Then the meteorites would come on in. They would open this up, and they would say, "Okay, this is meteorite number 1003. It was found in Alan Hills, and it was the December, the year it was found was 1979." So as it came out of the bag, it would receive a number in relationship to when it was opened. It had nothing whatsoever to do with when it was found. So this could be called "79-010" or something like that, and it relates back to field numbers such and such and this particular photograph.

It then at that particular time was given all the necessary generic information for complete tracking. It went into the computer. It had its own file, including all of this additional information. It had its number. And they would take an additional photograph of it within the cabinet. As it got into the cabinet, we would then have a small, little one-centimeter cube, which we used with the lunar samples, which gives it not only a size

comparison, but it allows us to take a six-sided orientation of the sample. The orientation has nothing whatsoever to do with what it looked like on the ice or how it was sitting on the ice; it has everything to do with how it was going to be in the laboratory from that point.

We could then dissect it and build this whole thing back up from our complete dissection system. Remember I told you about the grocery store, and that's what we were doing. We had all these procedures in place that were just exactly like the kinds of things that we did with the lunar samples. And this paid off. So somebody could come along and say, "I want a piece of such and such a meteorite, from the central part of it or something like that." And we could show them where all of the surrounding bits and pieces of the meteorite were, all of the aspects so that we could completely rebuild everything that this person had, and for things like depth measurements and that sort of thing. It was very important.

BUTLER: Well, it's certainly become a factor now in the discussion about the life findings.

ANNEXSTAD: And so it was so important that we had learned all of this through the Apollo Program that when it came time to do this with going back with the collection procedures, even though Antarctica was different, we had this whole thing just settled down just like this. I took all that stuff down the first year and then I would train people to pick up meteorites and give them this kind of stuff.

BUTLER: How do people get selected to be on a team for doing that program?

ANNEXSTAD: What Bill Cassidy, and then today his cohort Ralph [P.] Harvey, does, is people just write a letter and say, "I'm interested in going." But what Bill Cassidy tried to do rather early on was to make sure that he had people that had strong backgrounds in meteorites. One of the reasons I got on very early on was that I was very experienced in Antarctica at that time. So I worked with him for a number of years, but eventually they started using a lot of people that were not quite so experienced, but they found that they could do a good job. They've taken down several hundred people during the years. But I was fortunate in being the one person that kind of put together everything in the early years, which was a lot of fun for me. Then I kind of moved on to other things.

BUTLER: What's the process of actually finding a meteorite on the surface? Do they run a certain search pattern, and how do they determine what areas of Antarctica that they're going to go to for that particular—

ANNEXSTAD: Well, the one thing we found is that you want to look for meteorites not in the snow areas, but in areas where ice has emerged. Now, ice is built through the successive accumulation of layers of snow, and below a certain depth, which is called the firm-to-ice transition point, depending upon temperature and a number of other things, I don't want to get into glaciology so completely, but the snow which is compacted eventually begins to turn clear. So way below that would be clear ice that has fallen as snow maybe, what, 10,000, 20,000, 30 or 40,000 years before. It would be very old ice. The ice just moves away from its accumulation area toward the edges of the Antarctic, and it's carrying with it debris that is

everything from volcanic debris to meteorites that have fallen on the surface and becomes encased within the ice.

Now, the ice, as it moves on, even though it would have snow cover on top of it, can run into any kind of a blockage mechanism, a mountain or whatever, a subglacial peak or something that doesn't stick up above, and the ice gets forced up to the surface. Well, one of the things that happens is the snow cover gets blown away and this old ice tends to appear in some of these areas and frequently with them are found meteorites, and that's what these people have found. They're meteorites in some of those particular areas.

The nice part about that is that you look for these ice areas which are called blue ice. So we know where to look, but not all blue ice features give you meteorites. Some are much better than others. So we would know where to look. Now, what we've been trying to do is to take a look at this and say, "What is the real mechanism of concentration? Just how does this work glaciologically?" and that's what I did my dissertation on, although this was a very early view of the whole thing.

In fact, we've got a poster tonight that my student is showing that talks about the concentration mechanism of meteorites in the Antarctica. It's over at the University of Houston-Clear Lake City area. But that was a very interesting problem, and it was something that captured my imagination rather early on, and so I started to go from just collecting meteorites—at first, I thought, well, this is okay, to really looking at this whole process. I couldn't stop the science. That got to be fun.

BUTLER: Well, we'll take one more quick break, if that's all right. [Tape change]

Just to go briefly until you left NASA and had the opportunity to move into teaching, and if you could tell us a little bit about the program that you're involved with at the Bemidji [State University, Bemidji, Minnesota] and with the NASA space grant.

ANNEXSTAD: That's another one of the more interesting aspects of things that you kind of fall into. One of the reasons that I decided to leave NASA was not because there were not challenges necessarily, but the most important thing that had happened as I'd finished my doctorate, and it kind of seemed like time to move on. We were going through a reduction in force, and I learned that anybody with twenty-five years could retire, and if you were fairly close to age fifty-five, the cost was not very much. Well, I was fifty-four years old at that time in January 1986, and I had been talking a bit about doing something else, going into teaching, or whatever.

I had talked to the folks up at Bemidji a year before, and we had discussed this, and they said would I be willing to come up and teach planetary science and geophysics. This is a very little tiny school. I kind of wanted to move to northern Minnesota anyway, because my wife and I had bought property on a lake up there back in 1979, and we were eventually going to retire up there. So we negotiated, and they asked me to come on up and teach geophysics.

Well, on the strength of that, I went to Doug Blanchard, and I said, "I think I'll take the retirement offer." This is great, because, of course, everybody was moving into a different direction, and they weren't going to be able to keep the position. I was going to be able to retire, and they were able to hire Marilyn [M.] Lindstrom, who eventually became meteorite curator and that sort of thing.

So on the strength of this whole thing I had a one-quarter contract with Bemidji State University, and I retired and went to Bemidji and taught. My wife remained here. I came home that summer and we spent the summer and enjoyed it, and it was a very different experience.

Well, as the years went on, after a couple of years, I had heard that NASA had put together a program called Space Grant. Chuck [Charles A.] Wood, your old professor, had called me and said, "John, Space Grant is going to go. Would you guys in Bemidji like to join us in North Dakota?"

I said, "Yes. What's the story?"

He said, "Well, Julius Dasch," whom I remember had been down here, and I knew Julius and had actually lectured in his class when he was out at Oregon.

I called Julius, and I said, "Chuck wants us to come over here."

He said, "Well, John, I think you ought to stay with Minnesota, because we're doing this on a state-by-state basis. Did you remember Paul Weiblan?"

Well, I remember when Paul was an Associate Director for Space Science in NASA Headquarters, and I said, "Of course, I know Paul."

He said, "Well, he's talking about putting together a program for the state of Minnesota."

So I called Paul, and I said, "What do you want to do?"

He said, "Oh, this is great. [Dr.] Ken Erickson down at Augsburg [College, Minneapolis, Minnesota] wants to work with us. Let's the three of us put together a program for the state of Minnesota." So we did.

I sat down and I thought about it, and I said, "What could we do with Bemidji that is different? I worked for two years and put together a space studies minor. We are the only undergraduate institution in the United States that does a minor in space studies. So we took the NASA money. We give it to the students for scholarships. We have a space studies minor and a complete program within this. The extra money that's been coming through, we've had upgrades throughout the whole system. I've just finished ten years of NASA grants, and we've got another five years of it coming through the state of Minnesota, and this year I have seventeen people here at the Johnson Space Center for the [Lunar and Planetary Science] Conference, that have come down and eleven of them are students, and I had enough money to pay their way to come down to the conference, and we're doing three papers at the conference. It's been an incredibly rewarding experience.

So my associations with NASA, NASA never let me go. [Laughter] It's been all this fun, and we've been doing very, very well. So out of all of this I have a solid NASA grant, and the university is very happy about it. I will retiring full-time a year from June, and there's a distinct possibility that one of our early students, who is now finishing his Ph.D. at Brown [University, Providence, Rhode Island], may be able to come in and take over the program.

BUTLER: That's wonderful.

ANNEXSTAD: Yes, I've got students all over the country. This is a direct fallout of all my work with LSAPT and the Meteorite Working Group, because I know these people, and if I've got a good student, I can call them and they'll say, yes, they'll take a hard look at him.

BUTLER: That's great.

ANNEXSTAD: It's an old-boy, old-girls' network right down the line.

BUTLER: It's worked out really well for you. You've certainly had a fascinating career.

ANNEXSTAD: Oh, I've enjoyed it thoroughly.

BUTLER: I'd like to give Kevin a chance now to ask you a couple of questions.

RUSNAK: I have one specific question and a couple of general questions. Earlier on in the interview you mentioned testing some protective clothing and such for NASA down at Antarctica. I was wondering if you could elaborate on that.

ANNEXSTAD: Well, it turned out that when we started looking at this whole process of going to the Antarctic again, I began to realize that there were to be certain kinds of things that we needed to be able to function in the field. So I was talking to people and they said, "Let's go on over and talk to our clothing people." I had some ideas of what the clothing should be like. I actually had an idea for a coverall. The fellows over there said, "Well, look, we are looking at such things as ripstop nylon, various kinds of filling types of things that we would need. We want to keep the astronauts warm when they're, say, flying or going in different areas like this, and some of this would be very useful for us. If you'll design what you want,

we will make it for you and some of the ancillary things that you want, and then when you take it down to the Antarctic and bring it back, we'll look at it and we'll see what goes on."

At that time we were using Kevlar and a number of other things like this, and we put Kevlar things on my knees and various areas where I would be down on the ice surfaces. They also made carryall bags for me. I asked for a bag that could be used to carry everything, including collection materials and cameras and things like that, and they made specialized bags and that sort of thing. This went on for about three or four years. It was just fine.

When I left, I had the remnants of one of the last suits that they made for me, but they're very different. They had very large zippers on them, and the suits themselves would come apart so completely that I could be wearing crampons and large boots. I didn't have to step into anything; I could just put things on and zip them up and it worked very, very well. They were interested in the fact that we tried all these different kinds of materials. The filling was not what they called kapok. It was something else that we have used, and they were very interested in the insulating qualities and how well it would work.

So that was an interesting period of time, because the guys over there, they'll make anything, and they just loved to get into this. Then when I'd come back, we'd sit down and I'd talk about it and "This worked," and "That didn't work," and they built up a database on it. So it was kind of fun. So I laughingly said, "Yes, I designed clothing for the Antarctic."

And they would send me down, and there's another thing that they would do, they would make sure that the NASA labels were on everything. So I was going to the Antarctic and I would have jackets, for example, that'd say "NASA" and my name on it and all this sort of stuff. While I was down in the Antarctic, they always talked about I was the NASA

person, and this was a big thing in the Antarctic, that I would come down and I was wearing NASA blue.

Then one year the director in Washington, D.C., had decided, he had heard about the program, and thought we should carry NASA flags with us. So we had little flags with the NASA logo and everything on it. We got down to the Antarctic, and everybody started to steal them from us. [Laughter] But we used to put them on our snowmobiles. So we had these NASA flags and all, and so I think it was good publicity, a lot of fun.

RUSNAK: Showing some agency pride.

ANNEXSTAD: Oh, yes. Without question. And it made a big, big splash in Antarctica. Oh, you can imagine, I mean, in interviews and that sort of stuff.

One year, in 1983, I took my daughter down there with me. I needed a field assistant, and she was attending Rice University [Houston, Texas]. I asked if she could get away from school long enough and go to the Antarctic without pay. She said she could. So we went down there, and she was part of the team for that thing. I mean, we were the first father-daughter team to ever work in the Antarctic. The press was unbelievable. We were interviewed by every newspaper and every major television station in the Houston area. I don't know if you remember Channel 13, Shara Fryer came down to the Johnson Space Center. She's on at ten o'clock at night with Dave Ward, still. Shara Fryer came down and interviewed us at JSC, and my daughter was going down to the Antarctic as a Rice University student. That was marvelous, very different.

BUTLER: That's neat.

RUSNAK: Well, now for a few broader questions. You explained earlier how the scientists fit in the organization that was managed essentially by aerospace people. I was wondering how the role of the scientists and science changed at JSC, and that's in general from the time when you started through the time you left, twenty years later.

ANNEXSTAD: I think that probably one of the largest changes came when they started to recognize the fact that the responsibilities for the astronauts far exceeded just being a person that was a pilot. We started in the early years where everybody was a test pilot or that sort of thing, and they became more and more involved with the science. I think that had a kind of a ripple effect because they came up with mission specialists and that sort of thing. I don't necessarily think that we were so heavily involved with this. There was quite a question as to whether Jack Schmitt was going to be on board. That was a political thing, and then we finally did get a geologist on board on Apollo 17, but I think the broader realization that science was going to be much more of a driver.

It was almost in the very early years that we were going to do this, because we were in an ideological race with the Soviet Union. This started to go into a situation where somebody said, "Well, we're going to get these guys to the Moon, but now what are they going to do there? They're not going to just stand there and put the flag and salute it and say 'Hello' and come home. This was kind of dumb." So science started in that direction, although it was still ancillary. And it became more and more important to our understanding of absolutely everything. It didn't shunt, I think, the engineers or the managers into any kind

of a secondary position, but it started to push science up to the point that it was equal within everything. I think it's a natural consequence of things.

In the very early years our most important problem was how to get there and could we survive. Then you bring things into it. I remember a conference I went to here at JSC, and one of the people that was talking about it said, "Where we are in the space program is similar to where we were back at the turn of the century in the Antarctic. All they were trying to do at that time was to show that they could live and survive down there. They didn't know about whether they were going to do useful work or not. That came a little bit later." And the space program has been the same way. It's been this metamorphosis that has gone on, and I've seen this go throughout the years. People may have wanted it to change quickly rather early on, but it couldn't do this. There's no way you can have this happen without going through these changes of what was happening. We were set back by accidents at times and various things that went on, and the program itself just had to mature. It was a maturation process.

RUSNAK: My final question, I guess, is more personal. You had mentioned earlier coaching, I think it was your son's football team. So I was wondering how much you were involved with work during all that was going on with Apollo and how much time you had for your family and how much attention you paid to the larger community and perhaps larger events going on in the world.

ANNEXSTAD: Well, I had children at home. They were in grade school. That within itself is a confining type of situation. I mean, you don't pick up and run off to Europe with the

family. You're pretty well stuck. That's part of it. And we were very, very heavily involved with a career. My wife was not working during those very early years. Eventually she decided to go ahead and go teaching.

So we were involved really with our local area, and it was fun. We had little activities and I helped coach Little League football, partly neighbors were doing this. Everybody that we lived around, in many of these cases, I lived here in Nassau Bay [Texas] for four years, we were all involved. All your neighbors were involved. We either worked for NASA or we worked with NASA or we had something to do with the Moon program and that sort of thing. So we all just kind of lived like a large community. In many cases we knew each other.

Then in 1972 we moved to Dickinson [Texas]. Well, I got a little further away from the people that were immediately around us, and at that time my wife went to work for the Dickinson School District and worked down there. But I lived next door to Max [Maxime A.] Faget, and many times Max and Nancy [Faget] would have people over, and we would be there, and I'd meet many of the other folks, but generally, of course, the people that were at Max's level. But even there we had other people in our neighborhood that were involved with NASA.

At that time my responsibilities had expanded, and so I was more involved with my career. My wife was in her teaching situation. So we stayed part of the community. We belonged to a church in Dickinson. We were heavily involved with that and all of the kinds of things that were part of the normal life situation.

During the Apollo Program, we all worked long hours. It was not uncommon. It was not uncommon to be in on weekends, work late in the evening, and that sort of stuff. It was

this whole idea of teamwork, dedication, and everything else. You get so involved in what you're doing that it just becomes your life.

RUSNAK: Through that, were you able to detect any sense of change in the general public mood toward the space program from, like, 1969 where you've got this general enthusiasm for "Let's go to the Moon" and then somewhat later after all the missions went well, "Maybe we should be spending this money elsewhere," and then the Apollo Program ends? Did you see any of that coming or get a sense that interest was waning?

ANNEXSTAD: I don't think that I really saw it coming, because I was at a level where we were just so busy and had so many things going on, although I could see it somewhat among the press. This was somewhat dismaying to me on a personal basis, because I was thinking the press was in many ways defining public opinion. That's not good. And we see this today. All you have to do is to follow the stock market today, and you can see that the press is defining that, but the average person is not necessarily that associated with it.

Yes, this was a bit difficult, and, as I said a little earlier, it was a real problem. We would sit and ask and almost, why? Why is it that one day we're kind of the darlings of the system and the next day you're the dregs? You just don't quite understand it. I know that many people were fighting to keep things going, and they did a wonderful job, but we went through some very difficult years. I hate to see it, but part of it comes from the fact that NASA has to go in for its budget every year. This is a rather stupid kind of a situation when NASA thinks in decades and not in years. So that's part of the problem.

RUSNAK: Those were all the questions I had. Thank you.

ANNEXSTAD: Good.

BUTLER: I want to thank you for spending some time with us this afternoon and sharing so many of your interesting experiences with us and so much of the history of the program.

ANNEXSTAD: Well, thank you. It's been fun.

BUTLER: It certainly has been for us as well.

ANNEXSTAD: I appreciate your asking me.

BUTLER: Oh, well, I'm glad we could do it. Thank you.

[End of Interview]