NASA JOHNSON SPACE CENTER ORAL HISTORY PROJECT
ORAL HISTORY TRANSCRIPT

WILMOT N. HESS
INTERVIEWED BY CAROL L. BUTLER
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BUTLER: Today is April 22, 2002. This oral history with Dr. Wilmot Hess is being conducted for the Johnson Space Center Oral History Project at his home in Berkeley, California. Carol Butler is the interviewer and is assisted by Rebecca Wright.

Thank you very much for talking with us today and inviting us to your home. We appreciate it.

HESS: Happy to do it.

BUTLER: Thank you. To begin with, if you could tell us a little bit about your background, and how you became interested in science in general, and physics in particular.

HESS: My father was a biologist in a college in upstate New York, and so I grew up with science around me. It was maybe more biology than physics, but I sort of became aware of the fact that biology was a very imperfect science, and I didn't like it because it wasn't structured enough, I guess.

So I knew the physics professor at Hamilton College [Clinton, New York] where I grew up, and he showed me some of the things they used in their laboratories and demonstrations, and I thought they were pretty fun. So I leaned toward that. Then when I got in college, well, I started doing physics right away. That seemed to be the thing which was most fun for me. And then after a period in the navy, in V-12 program, I got back into physics. I had studied engineering under V-12. Then in graduate school, here in Cal [California], I did physics, because that was, at that point, what I was focusing in on.

BUTLER: Some of your early experiences, —one of your first jobs was at the Lawrence Radiation Laboratory here in California. What type of research were you involved with while you were working there? What sort of projects did you work with?

HESS: First, I started as an assistant working with other students, and you'd have two or three people, a senior guy who was doing his thesis project, or maybe had finished his degree and was working on staff as the head of this team of people, and you would work together doing an experiment. I was the last generation of people who were able to work on sort of individual experiments. That means that the research team would think up the experiment and decide it was meritorious, worth doing, then develop the instruments to do it, carry out the experimental runs to collect the data from the machines here in Berkeley, analyze the data, publish the results, all of that with a group of two or three people.

Now in the big labs, and Berkeley is no longer a big lab because it doesn't have a big machine, but at the big labs, there are teams of fifty people or so that work on an individual experiment, so you've lost, in some sense, the sense of self. You're doing the whole thing, instead of just a little piece of a big project. I liked it that you could do the whole thing.

The kinds of projects I worked on when I first came here, I saw the 184-inch Cyclotron, which was, at that point, the largest high-energy physics machine in the world, and pretty impressive for that period of time. Now, of course, machines are a lot bigger. But I was very impressed by that, I remember. When I first walked into the building where the machine was, I literally sort of trembled and felt awestruck, I guess, I don't know.

So then I did experiments on the 184-inch, where you would build your counters, Geiger counters, scintillation counters, what have you, to make some measurement on particles. One of the things I did was to, with a couple of other guys, hit a proton beam on a nucleus, a target of carbon or aluminum or various other things, and then look for the particles coming out from the explosion, and looking to see what their characteristics were.

Now, if you go to a billiard table and hit a billiard ball, you shoot one ball, hit another ball, the two balls bounce off and the angle between the two balls bouncing off is 90 degrees, always 90 degrees. It couldn't be one ball straight ahead and one ball sidewise or two balls at 45 degrees, but it's always 90 degrees.

Now, if you hit a moving ball, that is to say the ball that you strike was moving, they would go off at different angles. That's what happens inside a nucleus, because you hit a particle in the nucleus, which is moving. So proton comes in, hits a proton, two protons go out, but instead of being at 90 degrees, maybe they're at 50 degrees. You measure that and you can measure the characteristics of the particle which was struck. So by doing this experiment, you can measure the momentum, the velocity of the particles moving inside the nucleus. That was the first time people had done that, and that was sort of fun.

BUTLER: That certainly sounds like it would be. Very interesting. Were there other projects that you were involved as well at that time?

HESS: Oh, yes. When I was working the staff at the Radiation Lab, I worked on ten or fifteen different experiments like that. If you want some more examples, I can give you. Maybe that's enough.

BUTLER: Okay. Sure. That's fine. We can always come back if we need to. At any point during this time, did you begin working on researching the upper atmosphere or space? Was that during this time, while you were—

HESS: No. That was much later, after I left Manned Spacecraft Center, Houston.

BUTLER: Okay. Well, you were here. For a while you also were working in the Plowshare Division.

HESS: That's after I went to Livermore [Livermore Radiation Laboratory, Livermore, California].

BUTLER: Okay. At Livermore. What sorts of projects were you involved with there? How did the projects you were working on at Livermore vary from what you had been working on when you were here?

HESS: They were a lot different. At Livermore, I was working on weapons.

WESTER HESS: It's not clear that there's two different times you were working at Livermore and two different times here in Berkeley.

BUTLER: Okay.

HESS: But when I got my Ph.D., which was 1957—

WESTER HESS: '54.

HESS: Oh, '54, that's right. Then I got a job, aside from being a graduate student, went to Livermore. Had more money than I knew what to do with, and started working in weapons. Now, my particular part of the game was to run a high-explosive test bunker in the desert near where the bombs were set off, at the atomic test site north of Las Vegas [Nevada]. The thing that we did in our explosive test bunker, we would—you're inside the bunker. The bunker's armored. Outside the bunker, you set off explosions. Now, the explosions were taking a part of a design of a weapon and exploding it, and watching the parts move. And the way we watched the parts move, was to have a very intense X-ray beam which came out from the bunker, and the explosion was carried out outside the thing. On the far side of the explosion, you put some detectors, and you looked at the detectors as the explosion was going on, and you would see that the X-ray beam got weaker, in some sequence of different detectors, and how that implied—when it got weaker, that meant that some of the metal inside the exploding gadget was getting in the beam, getting in the way, so you could measure when the exploding parts moved past you and what the density was. That told you something about what the nature of the explosion was and how the metal inside the gadget was working, and that was useful to the designers.

I did that for a couple of years. I guess it was a couple of years, yes, traveling every week from here in Berkeley to Las Vegas. —At that time in Las Vegas, you could still get a hotel room for five dollars a night, and five o'clock in the afternoon you go to Happy Hour, and there was all the food you could eat for the cost of one drink. So that was fun.

But I did that for two years, and then at the end of that stint, I came back and worked part-time here at Berkeley and then got this job at Livermore, a new job of being head of the Plowshare group. And the plowshare, from the Bible, "Beat your swords into plowshares," and the idea was to try to find peaceful uses, industrial uses, for nuclear weapons.

There were three or four kinds of things. One was trying to do excavation, and so you would dig a canal or build a harbor with some underground explosions, and the underground explosions moved the dirt, and you're left with the hole, which is what you want. We got set up to build a test harbor on the coast of Alaska, and this was to be three medium-sized bombs under ground, several hundred feet under ground, to connect to the ocean on one hand, and then to connect to the basin on the other, and the basin was to be made with one or maybe two bigger, deeper buried bombs, to dig a bigger hole. So you had sort of a canal leading into a basin. We did all the design work on that, and test work, and went up and studied the coast of Alaska.

Then enough people got annoyed at us enough so that we canceled the project, and it was very interesting to see who got annoyed. There were a bunch of environmentalists who thought it was a bad thing to spread radiation around northern Alaska, and were worried about the caribou eating the lichen, which would be radioactive and causing trouble with the caribou. We said, "Well, we'll build a fence around the whole thing." And all that wasn't good enough.

The other thing that was interesting was that polar bear hunters didn't like us. A polar bear hunter, a guy who had a light airplane, would find a rich Texan who wanted a polar bear skin rug, and get a few thousand dollars out of him, and then take him out on the ice floes in the ocean near where our harbor was going to be, and find a polar bear, and then run the polar bear until he was so tired he could hardly walk, then land the plane on the ice, and the Texan would get out and go "bang!" and then you've got your polar bear rug. Well, that was not especially a sport, but the end product was what the guy wanted.

The local people were making money off this, and they didn't want anybody coming up and messing up their scheme of things and causing undue newspaper notoriety and all that. So they were against us, and after a while, the noise became enough that we decided, "Well, we'd better not do this thing." So we quit that. But there were a variety of other projects also in Plowshare.

WESTER HESS: I remember about the Russian testing and the amount of radiation that came into Alaska.

HESS: True. After we had abandoned our test, the Russians shot a very big—fifty-megaton—explosion on their north coast, and the fallout from that test was more [radiation] over all of the Brooks Range than it would have been right alongside our test harbor, and nobody said a word. And that annoyed me, that there was this kind of a double standard. But that's the way life goes.

So you want to hear about more Plowshares things? Probably not.

BUTLER: Sure. Well, one or two other examples might be interesting. Then we can move on.

HESS: Well, one of the things that we thought about doing, and eventually abandoned it also, was to see if you could have an underground explosion which would make a great big hot zone in the earth, and then mine heat. But we thought of doing this in a salt dome where you'd melt the salt and have a great big puddle of molten salt, and then stick pipes down into it and circulate water and bring out hot water and use that hot water to drive turbines or whatever.

Well, we carried that out, down to the place where we were sort of ready to do an experiment, but we decided, "Well, let's go and see one," what happens naturally when something like this occurs. So we went out to Hawaii and studied the lava lake that had been made in Kilauea Iki, [when] the crater Kilauea erupted. This goes back into the fifties, whatever. It erupted into a little subsidiary basin away from the main basin, and then that froze over, and here you had underground this molten lava. We thought, "Well, okay, we'll go and see if we can put a pipe down into this, circulate water into it and get hot water out and how all that goes."

So we set up, had a great big husky Hawaiian carry down the equipment to do this, which was fairly primitive equipment. It was a drill about two inches in diameter, I guess, did the drill with a little electric motor, drilled down twenty-three feet, and the drill started sinking all by itself. "Ooo, oops, it's hot down there." [Laughs] Well, all right, we find the heat. A little bit bothersome to be standing on the top of this. You're out in the middle of a lake, and there's only twenty feet of ground underneath you.

So then we tried to circulate water into this thing, and the water circulated, but we found out that the lava near the pipe froze, and that was good insulator so you couldn't transfer much heat to the pipe and couldn't get very much hot water out. Ooo, not good. So that, among other things, made that experiment not work.

Then we thought about digging a Panama Canal with a bunch of underground explosions. You say, why do you want a Panama Canal? The present Panama Canal can only accept ships of a certain size, because it has locks. You go from ocean, through locks, up to Gatun Lake, locks down the other side to ocean. The locks limit what you can do very severely, and they worried about in wartime having to go through the locks, and big ships couldn't make it because they were wider than when the canal was built.

So, let's do a sea-level canal. So we went down and made a survey of the area, and it turned out that where the people made the present canal was the best place by far for a route for a canal, because it had the lowest middle of the isthmus. It was only about 300 feet, if I remember right, and most of the rest of the isthmus was the better part of a thousand feet. And that was very difficult, and so after a while, we decided we weren't going to do that either.

So what did Plowshare accomplish? Very little. Did a bunch of plans, but never did anything.

BUTLER: Well, learned some things about different properties, and things that wouldn't work, and things that might work if you were able to do them. Sometimes it's not always coming out with a physical product, it's some of the knowledge you get just by doing it.

Well, at what point did you learn about the opportunity at Goddard Space Flight Center [Greenbelt, Maryland]?

HESS: I was in Livermore, and I'd been trying to get the Livermore bosses to push for our involvement in space experiments, military space experiments. There was a thing called Project Vela, which was putting up detectors in space, looking for Soviet explosions. Doing the work on those things, building the detectors, putting them on satellites, getting the data back, that was fun, but also with the same detectors you could do some interesting astrophysics.

Well, so I tried to sell our lab on taking responsibility for that project, and we had a fairly good inroad onto it. I had done experiments in space by that time, and one of the other guys out there, Steve White, had also done some, so we had more knowledge of how to do space experiments than our competitor, namely, Los Alamos [National Laboratory, New Mexico]. But our management didn't want to get involved in this kind of thing, so they said no, and Los Alamos got the experiment. So that sort of annoyed me.

I had, by that time, been involved in a couple of experiments in space. We would mount some neutron detectors onto a rocket fired from Cape Canaveral [Missile Test Annex, Florida]. —This was a test rocket, which was just being fired to see if the rocket would work right. We would hang a little gadget on the outside of it, a pod, and this pod would have some detectors in it. The thing we were trying to measure was neutrons in the atmosphere of the Earth, how high up did they go, how many leaked out the upper end of the atmosphere, and was this a useful, important source for the Van Allen Radiation Belt, which you may or may not know anything about, the particles in space, which are [a] naturally occurring band of particles around the Earth.

So the idea was, can these neutrons coming out of the atmosphere of the Earth play a role in the radiation belt? Now, a neutron by itself is radioactive. It decays with a half-life of a thousand seconds. It will turn into a proton and an electron. If it's inside a nucleus, it's stable. It doesn't decay. So if you have these neutrons coming out of the atmosphere of the Earth in space decaying, then you have particles coming into the area where the Van Allen radiation belt is. Is that an important source of those Van Allen particles or not?

So we made measurements on how many neutrons are coming out, and we did calculations on—I did the calculations on what their energies should be, the distribution of energies. And this was really the first quantitative idea on how you make the radiation belt. Well, we'd done enough of that, both the experimental work on the neutrons and the theoretical study of how the neutrons are formed, what their energies are.

So that after Goddard was formed, that must have been 1957 or '58 or something like that.

BUTLER: I think about '58.

HESS: '58? The first guy who went to Goddard to be the head of the Theoretical Division there, a physicist named Bob [Robert] Jastrow, who is an old friend of mine, and Jastrow put together the Theoretical Division, a bunch of guys, theorists from various kinds of backgrounds, to study the processes in space.

Well, then after he'd been doing that two or three or four years, he decided he wanted to move to New York City, and he set up a new laboratory attached to Columbia University, which was to do many of the same things, and he could attract senior professors from several of the eastern universities to work at Columbia easier than he had found it to be to attract these guys to work at Goddard.

So he went to New York City, set up this new institute, and his job was available. And he recommended [me] to the bosses of Goddard and talked to me about did I want to go and do this, take this job over. And because I'd been doing things in space already, and because the laboratory had said, no, they didn't want to get more involved in space things—that's the

Lawrence Livermore Lab—I said, "Well, maybe I'll do it. I'll go to Goddard and see how life is on that side of the street."

So I took the job as head of the Theoretical Division at Goddard, and stayed there three years, however long it was. I don't remember.

WESTER HESS: Six.

HESS: Six? All right. But that was fun. I really enjoyed that, because we had a bunch of interesting people. I brought in several more people from the areas that I knew about to work there, including two or three people from the lab here. And we started doing studies mainly of the radiation belt. I ended up writing a book on the radiation belt out of this, and trying to understand quantitatively how the radiation belts are formed, where the particles come from, how they become as energetic as they are, and things like that. And that was a lot of fun. I thoroughly enjoyed it.

We set up a series of lectures every Friday afternoon, and we'd have invited speakers come in from other labs, or universities, to tell us about their research, new things going on. Almost every week we would hear somebody talking about something going on in space research which was new, very new. —The rate of discovery of new things at that period of time was very large. It was much larger than it had been in the nuclear physics that I did here, because it was a completely new subject, a new area, and there were all kinds of different things that people were finding out about particles in space and other things in space that just hadn't been known. And that was exciting. I thoroughly enjoyed it.

I was the head of the Theoretical Division for, Wester says six years, and I was a little bit mislabeled. I'm not really a theorist. I'm a guy who does experiments, experimental physicist as opposed to a theoretical physicist, and in most cases, those two things are fairly separate. A guy does either experiments or theory. There are few people, I fear me [not] being one of them, who combine both very well. I didn't. I was an experimentalist, and after a few years of being head of the Theoretical Division, that's sort of a fraudulent title. I decided, well, I probably ought to go do something else.

BUTLER: Well, before we move on to anything else, I wanted to ask you about a couple of the projects you were involved with while you were in the Theoretical Division, at least I think it was during the same timeframe. One of the projects was the Explorer 15 satellite that was to study an artificial radiation created by nuclear explosion in space. Can you tell us a little bit about that project?

HESS: Well, this explosion in space, —the U.S. did this and it was at least partly to find out if you could make a radiation belt in space, which could be a device for shooting down incoming rockets, the radiation from the collision of these particles with the incoming rocket enough to knock it down. So let's do an experiment to find out if you can make an artificial radiation belt, and how strong it would be. So they shot —a bomb—I can't remember the numbers anymore.

BUTLER: That's okay.

HESS: I don't remember how high it was or what its energy was, but anyway, high enough up, and they did it in the Pacific, so that it should make a radiation belt. I worked on the theoretical analysis of this before the bomb was shot.

WESTER HESS: This is Starfish.

HESS: Starfish. Project Starfish, the name of the explosion. We tried to make an estimate of how intense the radiation would be afterwards, how much of a radiation belt would be made by the bomb, and we missed it a good deal. There was a process that occurred that we hadn't thought of, that created the effect of the end product differently than the planning group had imagined.

WESTER HESS: But weren't you closer than anyone else?

HESS: Well, yes, but we were really the only ones doing the study. You don't want me to get into the details of this. Had to do with instabilities in the magnetosphere, which is a subject beyond the conversation here.

So anyway, they shot the bomb, made the artificial radiation belt, and it turned out to be more intense than they had expected it would be. But a lot of it died fairly rapidly, as the particles are low enough so that when they interact with the atmosphere, the atmosphere absorbs them essentially.

Where was I going? So, anyway, the radiation belt that was left was strong enough, people wanted to find out what its characteristics were because it was unusually intense. Okay.

Now, there were some things that would tell us right away what was going on, but after we had used that information as much as we could, we decided we wanted to put up something ourselves to find out more about what the artificial radiation belt was like. Therefore, Explorer 15.

But before I come to that, there were two or three satellites already up: Injun, which had been Van Allen's baby; and Telstar, which was a satellite put up by Bell Labs, as a communication thing, not specifically to look at particles in space. But Telstar had on it—one, it had thick cover plates, thicker than normal, over the solar cells, and that turned out to be very useful because these cover plates were thick enough so that the electrons that were in space from the artificial radiation belt wouldn't damage the solar cells rapidly.

In the Injun satellite, it knocked the satellite out. The satellite couldn't take the radiation from the electrons striking the solar cells and, in a period of weeks or months, the satellite died.

So the Telstar detectors, they had particle detectors on it, as well as having thick solar cells, that turned out to be very useful because it was the right area and the detectors were the right kind, and we got a reasonable look at what was going on with especially the electrons in the radiation belt from that. But not enough so that we still wanted to get a lot more information, so we, Goddard, took the responsibility of making Explorer 15, which was a collection of instruments made by different groups in the country, people from Applied Physics Lab at Johns Hopkins [University, Baltimore, Maryland], from Iowa, Van Allen's University, from, I guess, San Diego.

There were three or four different groups that put instruments that were fairly easily obtainable together and we made this satellite in a record time. We put the thing up in just a few months. I've forgotten how long it was, but a few months, and started getting a good set of measurements about the radiation belt, what its characteristics were, how fast it was decaying, how much of a problem it would be for other satellites and this sort of thing.

I was the project scientist for this Explorer 15, and that meant I had to go around and get the groups to agree to put the instruments together and get them on the satellite, and, of course, everybody wanted to so it wasn't any problem getting them, it was just getting the right instruments, and we got them pretty well, and seeing that they got put together correctly, and got the Cape together okay and launched okay. And so I saw the whole process of making a satellite, and getting it into orbit through, the whole system, and that was fun. I enjoyed that.

That's enough, I guess. Well, you ask me. Do you want more?

BUTLER: How long did that radiation belt last?

HESS: Well, the lower edges of it were washed away by the atmosphere fairly rapidly, but if you go up a couple of Earth radii in altitude near the Equator, you could see the residual radiation belt for several years.

BUTLER: It certainly sounds like it was an interesting project to work on.

HESS: Yes, yes, it was.

BUTLER: How much were you aware, as you were doing these projects, particularly at Goddard, although you had also mentioned, while you were still in California, how much were you aware of the Soviet programs, what sorts of things they were doing?

HESS: Well, I got to know some of the Soviet experimenters. We would have international meetings where there would be papers given and research done by different countries, and a couple three times a year we'd get together with Soviet counterparts to hear what their work was. So I knew some of the people in the business, and I got to know some of the experiments they were doing, and they were, by our standards, reasonably crude.

It's interesting that the Soviets might have discovered the radiation belt before Van Allen did if they had put what detectors they had on satellites where they would allow people from other countries to receive the data from the satellite and understand what it meant. People in Australia who could listen to the satellite, heard some of the date from it, but didn't understand it. And so it was not the Russian radiation belt; it was the Van Allen radiation belt, because they were secretive about it. But we knew some of their people, not terribly well socially—that was fairly different—but they didn't very much impress me.

I had another interesting—this is a sideline, so shut me up if you want. But back when I was working at Livermore, one of the sidelights that I got involved in, which was sort of fun, I went to Geneva, Switzerland, for three months to be an advisor to the Nuclear Test Ban Conference which was going on. It had been going on for quite a period of time, and it was getting a little stale and all that. But it was fun for me to see the kinds of things that went on. I wrote some of the speeches for our delegation to present at the meetings and stuff like that.

There were Soviet scientists at one side of the table, the U.S. and British scientists had the other side—not scientists, delegations—had the other side of the table, and the scientists were back-benchers. But we got to know Soviet scientists and politicians at this meeting rather well, because we were there a long time and we were seeing them every day.

One of the interesting things, we'd like to see them socially, outside of the formal work at the Palais Nationale, so there was some entertainment money available, and we'd go up after a meeting, three or four of our scientists would go up and approach their people and say, "We would like to invite you," indicating the group, "to come to dinner with us one day." That was the way you got to do it.

And they would look up and be happy. They liked the idea of doing it. "Very well. We will let you know."

And then it would take about two or three weeks, and they'd get an answer back from Moscow that said, "Okay. You can go to dinner with them."

Then their people would come to our side of the table and stand up there and say, "By the way, about your very nice invitation to come to dinner. We will do it next Tuesday." [Laughs] Their instructions were very specific. But it was a lot of fun. We got to know them really well that way. And they were good guys. They were friendly guys.

Enough.

BUTLER: That's certainly another interesting project there that you had an opportunity to participate in. How much did your work on the nuclear test ban affect your work at either Livermore or even at Goddard when you working on Project Starfish, in particular?

HESS: My work on the test ban?

BUTLER: When you said you were an advisor to the Nuclear Test Ban Conference that was going on.

HESS: Yes. We were advising—the ambassador was the head of the U.S. delegation, about the technical side of what a test ban should be like. How much did that influence my work? Not at all, I think. Just a different subject.

BUTLER: Okay. As you were working at Goddard and you were working on the various satellite programs, particularly in the Theoretical Division, did you have any work at all in the Mercury or Gemini Programs, any of the experiments that they did?

HESS: No.

BUTLER: Were there any other, particularly satellite programs?

HESS: It would have been rather different if I'd been involved in the science in those things. I'll tell you a story. I'll tell it now. At a meeting later on, this now jumps down to Houston. I'm in a meeting with the Houston management about some part of the science business, and Chris [Christopher C.] Kraft, the head of [Flight] Operations down there, asked me something about the science program that was done on Gemini, and I said it was lousy. It was really bad. He looked very upset and said, "What do you mean, it was lousy?"

I said, "It was a fiasco."

And he stomped out of the room. And he came back a minute later with a piece of paper written on it, "Fiasco: a ridiculous and utter failure." He had gone to a dictionary and looked up the definition of "fiasco." And he said, "Is that what you mean?"

And I said, "Well, maybe it wasn't quite 'utter.'" And that didn't seem to help very much. But it's true. Science that they put together to do on Gemini was worthless, not useful at all.

BUTLER: Certainly very limited.

HESS: As an example, they put a magnetometer onto the spacecraft to try to measure the Earth's magnetic field. Fair enough. That's fine. People have done that some. And it turned out they had not done a good job putting the magnetometer far away from the steel body of the spacecraft in order to cut out the local affect of the satellite, so what they were seeing was at least partly due to the satellite, and the data that came back from it, some of our people looked at it and it was very poor quality.

So I told Chris Kraft, I guess, "If you had given me the location of the satellite, I would tell you what the magnetic field was better than you measured it." And he didn't think that was a very nice statement, either. [Laughs]

BUTLER: One of the other projects that you participated in while at Goddard was a Lunar Exploration Conference. This was in July of '65. You were in charge of the Particles and Fields Working Group at that particular conference. If you can tell us a little bit about what the goals of the conference were and what the results were.

HESS: The goals were to try to develop a science program for Apollo, for the missions going to the lunar surface. And they gave us rules, I guess you'd call it rules—a limitation. We were told we could take, I think it was 250 pounds, down onto the Moon, which was in the science budget, and could bring back a hundred pounds, which was in the science budget, and we weren't to exceed those limits. Now, there were also—well, no, were there any other limits? Well, —space, but that was the important limit, was how much weight we were allowed. I don't think we argued much about that, and we said, "Okay. We'll see what we can do with that."

So a bunch of people talked about what kinds of experiments to do. Bringing the stuff back from the lunar surface, that was completely the lunar samples, rocks and dust and what have you, and so we could bring back a big aluminum box like this, which itself weighed maybe ten pounds, filled with rocks and stuff, clamp it together, put it in the upper portion of the landing vehicle and bring that home. That was easy coming back.

Coming down onto the Moon, there were more thoughtful process at that conference, and it ended up designing a set of instruments which were to go down on the Moon and be left there, and make measurements and come back home, and that was called ALSEP [Apollo Lunar Surface Experiment Package]. I think that's right.

There was a discussion about what kinds of instruments and what capabilities and all that. So there was a seismograph, there was a lunar corner reflector. Take a piece of glass that has sloping sides like this, and they come in at forty-five degrees and their flat plate up here. You shine light down onto it. It will come down, bounce off one of the pieces of glass, sides of the glass thing inside, bounce to another one and then come back, directly back at you. So you have a retro reflector; it comes back.

If you then have this on the surface of the Moon and have a laser at McDonald Observatory in [Fort Davis,] Texas, was one of the places, shine the laser at this gadget on the surface of the Moon and measure how long the time is for the pulse to go to the Moon and come back, measure that accurately, you can tell how far it is to the Moon.

Well, that by itself isn't very interesting, but the changes in that distance are fun, because it can tell you about the libration of the Moon. The Moon keeps one face pointed at you, but it isn't absolutely one face; it wobbles a little bit. Well, studying that wobble is fun for astronomers. Okay, that was one thing that could be done.

Another thing that could be done is if you put one of these laser reflectors on one side of the San Andreas Fault, and one on the other side of the San Andreas Fault, you could measure if one of these was moving with respect to the other, and you could do that to a couple of centimeters. Well, that's a pretty nice measurement for a gadget that's 250,000 miles away. But that was okay to do that.

So there were several things about motion of the Earth and of the Moon which could be measured with this corner reflector. So we adopted that as one of the instruments. Seismograph, corner reflector, magnotometer. Well, there was a question about whether that was useful or not. Some particle detectors looking for the solar winds which are low-energy particles coming from the Sun and some higher-energy particle detectors also. Have I left out something?

But anyway, we put together this package of instruments which was to go down and be left on the Moon with a power source, and the power source was a radioactive thermoelectric generator, RTG. So we keep the heat from this which you could use to generate electricity would keep this gadget running forever and ever daytime, nighttime. You couldn't use solar cells because it wouldn't work at night, lunar night.

So the conference put together that plan of the instruments and eventually the people who would do the instruments, and that was the most important single planning thing for the science project attached to Apollo, because it laid out with fairly reasonably strong restrictions what the experimenters could do. And that's a good thing to do. You don't let an experimenter have everything he asks for. You buy more than you want.

So that was a good conference, and it produced a very useful result. In fact, it wasn't necessary to do anything more on the general planning level of science for Apollo after that conference. That was a good piece of work.

BUTLER: And this was all primarily done by scientists from the community?

HESS: By scientists. It was pulling together a variety of scientists from the community who had the proper skills to try to think through what should these experiments be and how much does a lunar seismograph have to weigh. Can you do one for ten pounds that has enough sensitivity that you can measure moonquakes okay? That kind of questions. You have to get the right kind of people to do this, but we had a good set there.

BUTLER: How was it determined who would be invited to participate in this conference?

HESS: Who would be—I don't know. I wasn't. I guess I suggested a couple of people to come, but I'm sure it was the Houston management and plus maybe Washington management, but I don't know. I wasn't involved in that.

BUTLER: Okay. Before we move on to more details about Apollo, were there any other major projects that you were involved in while you were at Goddard that you'd like to comment on?

HESS: [Pauses] No.

BUTLER: Okay. Well, while you were involved in planning now for the Apollo Program, or at least participated in this conference, were you involved in any other Apollo activities while at Goddard, before you moved on to the Manned Spacecraft Center [Houston, Texas]?

HESS: No.

BUTLER: That would make sense, saying there wasn't a lot of need for more planning since the conference went so well.

How did the opportunity arise for you to accept the position as the Director of Science and Applications at the Manned Spacecraft Center?

HESS: Well, George Low, who I've known for some time, asked me if I would like to come down as Director of Science to work with him. And he, at that time, was the head of the Apollo Project. That was before the fire that killed the three astronauts, after which time, he—I'll get this backwards. Joe [Joseph F.] Shea was the guy who was in charge of Apollo, but after the fire, he stepped down, because he was broken up. He was really upset by the fire. And George [M.] Low went to become Director of Apollo. Before that time, he had been Associate Director, number-two man, at Manned Spacecraft Center. It was in that capacity that he asked me if I would come down and be Director of Science.

They really hadn't had any science program before that down there, and they needed an in-house program. It didn't have to be a strong program, because most of their work was going to be done at laboratories and universities away from Houston. The analysis of the lunar rocks, for example, would mostly not be done at Houston, some, but mostly at other places where the specialists were.

So anyway, George asked me would I come down and be Director of Science, and that was after I'd been doing the research at Goddard for six years, and I don't know, it was a new challenge and it sounded fun, different, and I wanted to see the Apollo Project do good science. It had the capability of doing interesting things, and there wasn't very much apparatus in place to help it to do science. Okay, so make a new science director at Houston, and bring in some good people there to be the nucleus for the process of getting the science done. And so he convinced me. I was, at that point, going to give up doing research. I had been doing my own research program at Goddard, and that was a lot of fun. I thoroughly enjoyed it.

But I knew that as soon as I went to Houston, I would be caught up in administrative, engineering, political, all kinds of other kind of things, to the point where I wouldn't do any research, and that was completely true.

So anyway, George asked, and I said yes.

BUTLER: You mentioned you'd been doing your own research program at Goddard. So you had left the Theoretical Division then, and moved on to do some of your own research?

HESS: I was doing the research as part of the Theoretical Division, so I was a mixture of theorist and experimental.

BUTLER: Well, with your new position at the Manned Spacecraft Center, as you mentioned, this was a new branch that was being formed. They hadn't really had that before. What was the reaction from everyone on site to this new directorate and to this new focus for Apollo for gearing up on the science side?

HESS: "Stay out of my way and don't bother me."

BUTLER: Did that persist throughout?

HESS: Yes. Science had the lowest priority of any of the mission's projects at the [Manned] Spacecraft Center. And that's not necessarily wrong. Of course, it caused me some personal annoyance, but you look at the job they had to do, [President John F.] Kennedy said it, he said, "Put a man on the Moon and bring him home safely." [Robert R.] Gilruth or Chris Kraft or one of the bosses down there, and the first thing you think about is safety. And that's okay.

That's okay. Safety should be at the top of the list. But it meant that you had to compromise or do other things differently than you would do. For example, any experiment that was going on a manned spacecraft, the LM [lunar module] going down to the surface of the Moon, for example, had to be man-rated, the seismometer. What do you mean man-rated? It had to be built out of parts that were traceable, that you could tell where they came from so that they're not going to get in trouble and burn up, or it had to go through a series of very severe tests to make sure that it was going to function okay.

This was half…for safety purposes and [half] for PR [public relations] purposes. They wanted to make sure that the experiments would work, that you wouldn't have failures. And that was the PR. But the safety did come first. I shouldn't say half and half. Three-quarters safety. But they were very PR-conscious, the management down there. They didn't want failures to come back and haunt them through the newspapers.

In the unmanned spacecraft program, failures were okay. You could have one out five experiments on a spacecraft going up fail and people, "Oh, damn it. That's annoying to have that fail." But that was all it was.

Now, in the manned spacecraft, if it failed in a way that was going to hurt the guys, that was a no-no. But if it just failed and didn't produce any useful data, that turned out to be a no-no also, but it was a PR no-no. So as a result of this man-rating of experiments, a magnetometer which flew on Gemini, cost about 10 million bucks, and a magnetometer which flew on Explorer 8 or one of the unmanned satellites, was about a half million, a factor of ten, twenty, fifty difference in cost. Well, so you just accepted that and went on. It was what happened.

Where was I going? What are we talking about?

BUTLER: Well, we were talking about —a couple of different things there, but we'll just move on to the next topic, I think. Obviously this was some of your responsibilities in your new position as working these safety and operations standpoints into the actual science, actually integrating the science into the operation.

HESS: That wasn't my responsibility; it was the responsibility of a bunch of the engineering guys who worked with us on seeing that the experiments were put together properly and doing the testing for man-rating and that sort of stuff. It was done, but it wasn't my personal responsibility.

BUTLER: Were these individuals that worked under you, or were they in the Engineering Directorate?

HESS: Yes and no.

BUTLER: Okay. Some of both.

HESS: Some of both, yes.

BUTLER: Okay. Well, if you could describe to us a little bit about how your directorate was structured, what some of the branches were, and—

HESS: Structured. Well, there were two main groups which were related to the two main pieces. The two main science objects: one, the rocks; and two, the ALSEP, the instruments going to the surface. There was one group that had the responsibility for building ALSEP, building, testing, making sure it worked, fit in the space, the astronauts could handle it okay, and deploy it on the surface of the Moon okay. And that was a group of mainly engineering guys, because the instruments were being supplied by scientists from different universities, laboratories, but built to specifications that our people laid down so they met the safety concerns and all that sort of stuff.

And then our guys were responsible for seeing that these experiments were what they were advertised as being and they fit together, worked and were deployable, and all that sort of stuff.

The other group was connected with the rocks. We built a structure down at Houston called the Lunar Receiving Laboratory [LRL], which was built more or less according to a Fort Dietrich [Maryland] specifications.

The primary reason for doing all this was the National Academy of Science, a couple years earlier than this, had decided that there was a small but finite chance that you might bring back something from the Moon that was bad stuff, either toxic biologically or toxic chemically, or some bad material. And therefore, you wanted to study this for a while, make sure that it wasn't bad stuff before you let it go out into the open populace.

So it was decided arbitrarily that it would be kept in quarantine at Houston for two weeks. Why two weeks? I don't know. Why not a month? Why not six months? Because the public wouldn't stand for it. So in some reasonable period of time, you wanted to do a series of tests on this material to find out it wasn't very toxic and to get it out to the people that are going to do the work on it.

We had gotten proposals from scientists, sent in from all over, and the proposals were reviewed and those that were meritorious were accepted, and we had like a hundred people or research groups signed up to get samples of the lunar material to do different kinds of geochemistry, mineralogy, all kinds of things.

So we had the Lunar Receiving Laboratory built, working with some engineers from Fort Dietrich, and it was built as a barrier so that if you had bad stuff inside, you didn't want it to get out into the community. How did you do that? You have a double barrier.

The inner barrier is a great big vacuum chamber with arms going into it, which are like astronaut suit arms. So a guy could stand up alongside this, look through a sheet of glass, put his hands into these arms, open the box of lunar materials, take them out, weigh the rocks, do a little bit of close-up look at the rocks after a very cursory examination and see if they're not going to catch fire if you expose them to air. There was for a period of time a bunch of people that thought that lunar surface material would be pyrophoric. Pyrophoric means that it will catch fire if you put oxygen on it. So okay, let's not do that.

So there were tests to see that it was all right to use [the lunar rocks] inside this—this was a vacuum chamber where these arms were. They would work on [the lunar rocks] for a period of time…and after they had decided [they weren't] pyrophoric and was okay to move to a less confined area, they would send [them] down to a ordinary glove box. Now, this doesn't have a vacuum inside. This has dry nitrogen inside it, and it has ordinary gloves through the sides of it so mineralogists can work on the sample to look at it and study it and see what it looks like, the objective being to get these rocks so that you understood them well enough that you would say, "Send rock six to Professor so-and-so at UCLA [University of California—Los Angeles]." They weren't supposed to do the fundamental science there inside this facility, just to get the material understood, to get it so that they could distribute it sensibly.

All right, now, but the building was, as I said, a double barrier. You had, first, this vacuum chamber inside that you kept the materials inside and then passed them to dry air, dry nitrogen, and [second] you had the building wall itself. Now, the building was built tight, and the idea of a tight building to a guy who builds buildings, a carpenter, was, "What the hell are you talking about? All buildings leak. This building leaks. Your home leaks."

"No, no, no. We don't want this building to leak. Plug up all the holes." And you pump down inside of the buildings so it's about one foot of water lower pressure inside than out. What's the purpose of making it lower pressure? So if you did get some bugs out into the air, inside the lab, then they wouldn't be able to get outside the building, because all of the leaks would be going inward, and that would hold the bugs inside. So we built a building that was tight and we could pump down, and the building was pumped down, lower pressure inside.

I don't know where I was going with this conversation.

BUTLER: Well, you were talking about—

HESS: Oh, you asked what the structure of our group was, what they did. Well, there was a whole lot of people who were responsible for getting this big vacuum chamber to work, so that you could have some guy stand there with his hands in these gloves. The gloves themselves were terrible. We had an awful lot of trouble making them work. Astronauts' gloves on the suit when the guy is on the surface of the Moon, those work at 6 psi [pounds per square inch] differential pressure. That is to say, when the guys out on the Moon, there's a vacuum outside, but inside the suit, he's only 6 psi, not 15, which is what you are here. So the differential pressure across the glove things was 6.

Now, the glove has places where they have to flex. They're sort of metal and rubber joints and they have to be able to move on the skin with respect to each other, and at 15 psi differential pressure, which is from the ordinary part of the Lunar Receiving Laboratory into the vacuum, they didn't work worth a damn, and we had a lot of trouble with the astronauts' gloves.

Eventually we got them so that they would last fifty hours or something, but we had to replace gloves every few days.

So, guys building this big glove box and the ordinary viewing assembly for working with gloves inside it, guys being responsible for getting the box built, and the clamps work well, and all that kind of stuff, and make it so that the rubber O-rings would work even if it got all dusty, all that kind of stuff.

The directorate wasn't a very big directorate. My memory is it was less than 200 people, but I may be wrong. Don't quote that number, because I don't remember that very well.

There was another whole function of the Lunar Receiving Laboratory which was partly my responsibility and partly the medical group's responsibility, Chuck—

HESS SAYS HIS WIFE, WESTER SAID THIS.BUTLER: Berry.

HESS: Berry. That's it, Chuck [Charles A.] Berry. And we were responsible for the quarantine. Now, quarantine was in the Lunar Receiving Laboratory, and there were two pieces of it. There was part of it that was people quarantine, and that was the astronauts coming back from the Moon, and they had to stay there for two weeks before they could go out, because if you're obeying what the National Academy of Science tells you you've got to do, you've got to have all stuff that had contact with the Moon, the lunar material, stay in quarantine. Okay.

So the astronauts were in there drinking bourbon and playing poker and whatnot, and having a good time, and aside from that, there was a whole area which was biological materials for testing lunar samples. —We had an aquarium where we had little fish swimming around; —actually, three aquariums. We had little terrariums where there were Japanese quail running around. And what were these guys doing? We would have three terrariums. One, you'd sprinkle some lunar dust on the floor and leave it there for two weeks, see if the Japanese quail having their ordinary life and living in this lunar soil would get sick.

Then we had a second terrarium, which was of sterilized lunar soil. You take the lunar soil and expose it to X-rays and to ultraviolet and make it so that you'd think you'd kill all the bugs, and see if there was any difference between A and B.

Then terrarium three was of Texas soil, and see if A and B were different from C, Texas soil. And we did this with Japanese quail, with minnows, with pine seedlings, with—

WESTER HESS: Cockroaches.

HESS: Cockroaches, that was another one. We had a dozen of these different experimental animals or fauna that we tested this way, and we did this for two weeks to see if anything showed up that indicated suspicious behavior of the lunar material.

The only thing we found in all of those tests that was a difference between A and B and C, was that pine seedlings preferred to grow in A. They liked lunar soil better than they liked Texas soil, and Texas soil was pretty poor. So that was the only difference we ever found.

At the end of the two-week period of time, we hadn't found anything that was indicative of trouble, including we'd had some—there was a guy here at the university, A.L. Burlingame, who worked with Mel [Melvin] Calvin a Nobel Laureate, and he had provided a very nice GCMS, gas chromatograph mass spectrometer, very fancy chemical gadget for biological molecules in the lunar soil, not at the cockroach, pine seedling level, but at the molecular level.

He could never find any organic material in the lunar soil. The only thing organic he found was the pump oil from our vacuum pumps, and he was continually raising hell, "You guys are dirty as hell. All your oil leaking, and I can't see anything."— He was right. They leaked.

But the end of this period of time, we didn't find anything that was troublesome about the material, so in two weeks it was okay to let the guys out of the quarantine and let the rocks go out to the professors at universities.

I'm not sure if I answered what your first question was.

BUTLER: Oh, absolutely. You did. You did. While we're talking about the Lunar Receiving Laboratory and sending out the samples, there were two teams set up early on, before any of the Apollo missions came back, the Lunar Sample Analysis Planning Team [LSAPT] and the Lunar Sample—

HESS: LSAPT.

BUTLER: LSAPT. And the [Lunar Sample] Preliminary Examination Team [PET]. How were those teams set up?

HESS: I did it.

BUTLER: You did that. Okay. What were their individual responsibilities?

HESS: LSAPT, Lunar Sample Analysis Planning Team. Well, I can't remember which group played which particular role, but together they were supposed to develop the most sensible way of handling the material and getting the material out to the professors that wanted to do the detailed analysis. So we had guys like Cliff Frondel, who was the Chairman of the Geology Department of Harvard University [Cambridge, Massachusetts], and [M.] Gene Simmons, very well-known geophysicist from MIT [Massachusetts Institute of Technology, Cambridge, Massachusetts], and Paul [W.] Gast, geochemist from Columbia [Univ.], and a bunch of really well-known people who would come in and work with us and help get this thing set up. They, of course, were doing it for their own good, because they were all going to be experimenters eventually, but they also know that they wanted to do the best job they could have, having the material handled sensibly and right material go to the right professor so that he would have the best chance of doing something useful with it. Gerry [Gerald] Wasserburg from Caltech [California Institute of Technology, Pasadena, California], he was a pain in the butt, but fun.

WESTER HESS: He was the one who was downright ugly.

HESS: No, no, that was—

WESTER HESS: Wasn't it?

HESS: No. There was one of the guys. It wasn't Gerry Wasserburg. One of the other guys was all the time heckling my secretary, and she was from East Texas, and she had a southern drawl you wouldn't believe. One time, this guy really got on her nerves, and she turned to him and said, "You're just downright ugly." [Laughs] And he was brought up absolutely aback and started laughing, and he couldn't ever do anything that got under her skin again. He wouldn't dare. Completely demobilized.

Gerry Wasserburg. I was just thinking who the people were on LSAPT, and right now I can't remember what the separate functions were of LSAPT analysis team. Well, I guess the analysis team were the guys who worked on the glove boxes. I guess that's right. Yes, I think so.

So anyway, one of the things that we tried very much to do was to involve the scientists, the working scientists in the universities, in the process of getting the samples out of quarantine and having the first look at them and characterize them enough so that they could be sent out for detailed analysis to the labs. And that was new.

The people at Houston hadn't thought anything about doing this kind of thing. They were just going to package Rock A to Professor B, and all that kind of stuff. But that wouldn't have been very useful.

So anyway, we got it set up so that the scientists were going to be working, especially on the lunar samples, were closely involved with us and were happy with what we were doing. They though we were doing a good job. They had not been happy with the lab for a while before I got down there. So anyway, that all worked out very well. I never heard any real squawks about the way we passed out the lunar materials or who got what rock, and "He shouldn't have gotten that," and that sort of stuff.

We were worried for a while that there was going to be thievery of rocks and a black market, so we took some pains to tell the recipients of the samples, "Keep these things locked up.

Keep them so that they're not going to get stolen." I think most people behaved rather well on that. And there never was anything resembling black market. I never heard of any thievery.

I heard of one thing. There was a professor at UCLA who got one sample for his own analysis, as he was supposed to, worked on it, kept it locked up in the vault at the department over the weekends, came back one Monday morning, the rock wasn't there. Oops! What's happened? It turned out, one of the other professors had taken it out of the vault and taken it to a cocktail party over the weekend, and forgotten to bring it back. Oh, no, naughty, naughty, naughty. —Did we write him a severe letter? I forget what we did. We let him know we were displeased. So anyway, that all worked pretty well.

What are we talking about?

BUTLER: Well, talking still about the LRL, this was one of your duties very soon after you came over to the Manned Spacecraft Center. At what stage of development was the laboratory in when you came to work there? Was it already planned?

HESS: The Lunar Receiving Laboratory was being built, and its specifications had been decided on because the National Academy [of Sciences] put out this report on what we were supposed to do, so they had these engineers from Fort Dietrich working with them on how to build the lab, how to make it work right.

The vacuum chamber, which was a centerpiece inside the lab, was being built at Oak Ridge, and there was a very nice guy named P. R. Bell, who was responsible for that project at Oak Ridge. He lived just down the street from Wester and me when we were in Timber Cove [development in Clear Lake, Texas], and that was under way. It got moved to Houston some months after I got there, and then they set up a bunch of tests to see that it was working correctly and didn't leak and all that stuff.

But the building we were to be occupying, it was all up and intact. That was okay, but making the building into the final product, the Lunar Receiving Laboratory, was in pieces, partly done.

BUTLER: How much of the procedures for how the lab would operate had been worked out beforehand, or was that what these teams and you were involved in developing?

HESS: Well, the details were put together by the teams and our people. The general idea, having the vacuum chamber and people working arms inside the vacuum chamber, that had been decided sometime before. Having the quarantine had been decided sometime before. We set up the Japanese quail and pine seedlings and stuff.

BUTLER: How challenging was it in working in the laboratory, trying to meet NASA's standards on safety, on how the samples would be processed and then also trying to meet the scientific community's desires, needs for wanting to get the samples?

HESS: It wasn't too bad. You knew pretty well what you had to do, and as soon as you explained to scientists what general things had to be gone through before you could pass the material out to them. We had one big meeting where we got a lot of scientists together, I guess maybe all the people that sent in proposals for analysis, that probably was who we had pulled together. We pulled them all together and had a fairly detailed discussion of what the process was for working the materials and getting them to the point where they were ready to pass out and how we did that.

There were some questions about—it was at that meeting that Gerry Wasserburg came to the fore. He was representing a bunch of people who were mad about something. What were they mad about? They were made about money. Oh, hell, I've forgotten. That's too long ago.

BUTLER: That's all right.

HESS: But anyway, so we talked to them in enough detail so that they didn't get mad at us, and we had NASA engineers working with us on the project, so the safety part of it was going along as part of the ordinary business of the lab. So I guess the answer is, there weren't very many big blow-ups.

BUTLER: Well, that's good.

HESS: …The blow-ups—well, I was a troublemaker in general, because I wanted to get more science done. Now, the first thing that we had to accept, when we accepted the 250 pounds down to the lunar surface, 100 pounds back—and we weren't supposed to argue about that, that was just what we were given—that sort of designed the whole project for us, but then I wanted to get more science done than that, and one of the things that bothered me, from shortly after I got there, was to find out that in the command module, there was one whole sector of the command module which was empty.

Now, the command module's the thing that stays in orbit around the Moon, doesn't go down to the surface. Mike [Michael] Collins was command module pilot the first mission. So here's this big empty hunk of space, going round and round and round the Moon, and there's enough power and there's lots of space, and the weight was also not a constraint at that place. Well, I shouldn't say it like that. You had to be limited on how much you could do, but it wasn't a serious limit.

So I said, well, let's get a bunch of instruments to do remote sensing of the lunar surface and try to do X-ray, gamma ray, ultraviolet, spectroscopy of the surface, and see what chemistry we could do, and how much you could understand of the lunar surface by remote sensing. And I argued that at meetings for two years with George [E.] Mueller, and at the end of it, I won, but I won so late that it was very difficult to do anything useful.

The first several missions, [Apollos] 12, 13, 14, there wasn't any such stuff. After that, I guess I got instruments into that empty bay in the service module—command module—and did some lunar surface chemistry, but I'm not sure how useful it was. Part of the problem was that you have to take some time to design and build an instrument that has the capabilities you want for doing chemistry. It isn't something you just have sitting on the back shelf.

So maybe you take a year or something to build such an instrument. Well, I'm not sure that the timeframe was such that there was a year available for design and construction. Anyway, as far as I was concerned, that didn't work well, and the reason it didn't work well was that the engineers didn't—well, they weren't very interested in science. We were just some more guys that were getting in their way. "Another task to do? We don't need any more tasks. We've got lots of work to do. Go away."

BUTLER: Well, if we could take a moment here and pause, take a break, change out our tape. [Tape change.]

We've talked about some of your responsibilities as Director of Science and Applications down at the Manned Spacecraft Center. Early on, one of the projects you were involved with was another lunar conference, lunar study conference, out here at the University of California at Santa Cruz. Do you recall that conference and what—actually, I think out of that came a group, the Lunar Exploration Planning Group. So if maybe you could tell us a little bit about the conference and how—

HESS: Gulp. —I remember we had one. I don't really remember much about it.

WESTER HESS: That was where the Roy Rogers—not Roy Rogers—Buck Rogers guy flew. It sure impressed the kids.

HESS: Oh, you mean with the backpack? Yes. I must say, I'm coming up blank.

BUTLER: Okay. That's all right. You're allowed to say that. You're allowed to say that.

WESTER HESS: That was early, early on. I think we had gone down to Houston in May, and this was that summer.

BUTLER: We don't expect you to remember every little detail. It was a while back. So it's quite all right to say, "I just don't remember."

Do you recall the Lunar Exploration Planning Group that came out of that, or is that—

HESS: Nope.

BUTLER: Okay. That's fine. Looking at lunar site selection, did you as Director of Science and Applications have or participate in the site selection for where they would make the landings?

HESS: Well, I personally didn't have anything to do with it. The guy who was in charge of that, essentially, was Gene [Eugene M.] Shoemaker, and Gene had a bunch of guys, some of them were from Geological Survey and some other types. Gene Simmons, I think, worked with him, trying to select sites. Now, they were under fairly severe constraints. They had to select land[ing] places, where the LM could land most safely, and so that restricted the kinds of places where they could go and things they could do, a fair amount. But having said that, then I think they did a pretty good job of selecting them. I'm not a geologist, so I can't argue. They did get highland sites. They did have mare sites. They never had the guts to land in a rill, did they? There was talk at one point about landing in Hadley's Rill, but they didn't ever do that.

BUTLER: They landed within a few miles of it, I think, but—

HESS: Is that right? Well, but they never got down into it.

BUTLER: No.

HESS: I don't know if that would have been useful or not, but one of the questions that geologists had, I think they still do have, is what were the conditions which allowed you to make a river on the Moon, and what evidence is there from what's there now, i.e., the rill, that says what it was like. But that was too ambitious for the landings.

One of the whole problems with Apollo was that because safety was so important, you were forbidden from doing a lot of things you'd like to do. Like, wouldn't it be fun to land near the polar caps and make a survey for water, which they could have done? But that's not right in the easily available low-latitude region on the Moon, so stay away from it. Wouldn't it have been fun to land on the backside? No, no, that's forbidden.

There was a project that JPL [Jet Propulsion Laboratory, Pasadena, California] had put together at about the same time as Apollo, called Surveyor Block Three, which was to do more science than Apollo would do, for a lot less money. But it got canceled because it was in competition with Apollo.

WESTER HESS: It was unmanned, however, wasn't it?

HESS: That was unmanned, yes, very much unmanned. But it would have landed on the backside of the Moon. It would have landed—probably landed in the polar regions.

BUTLER: Was this one of the projects that included ideas of unmanned rovers?

HESS: I think it had a rover, yes. And it had sample collection and it had sample return. But that wasn't the purpose [of Apollo]. The purpose was, get a man there and get him back and then do a little bit of science on the side if you can.

BUTLER: How much did the scientific community accept that purpose for Apollo?

HESS: Well, there were some vocal people who thought that the whole thing was useless, but they didn't understand the political reason for doing it. Kennedy sent the—ground rules said, "Get them there this decade, and get them back safely." And if you buy that, then the game's over.

Now, if you say, "No, no, no. Use the same amount of money and do science," that's a very different thing. And some people, of course, would much prefer to do that.

WESTER HESS: Politically.

HESS: If Kennedy hadn't set the goal of going to the Moon with a man, they wouldn't have gotten the money, and the unmanned projects wouldn't have prospered as much. You can ask the question now, "What's the purpose of man in space now?" and the answer is "Nothing." Anything that you can do with man in space, you can do better without man in space, with remote sensing instruments, telescopes, what have you, which are controlled by man on Earth.

WESTER HESS: But they fixed the telescope, the Hubble Telescope.

HESS: Yes, that was useful. It was because some people screwed up. But it was a very useful thing to do, there's no question about it. But is there any point in sending man to Mars? No. It's ridiculous. Absolute bunk. A well-known geochemist from England named Lovelock, who did a very nice study of the Martian atmosphere to show that there can't have been any advanced forms of life on Mars ever, because the atmosphere would have to be different if there was any. And I don't think anybody has shot that idea down at all. They just avoid talking about it.

And so the idea of sending man to Mars is sort of like the statement of "Why do you climb Everest?" The answer is, "Because it's there." Well, I don't buy that. I think if you want to do something useful with Mars, you send a lot of unmanned satellites to do more science than you can with a man, because I don't think there's much point to having any further man in space just because it's there, just to send a guy to the Moon. All right, we've done that. Send a guy to Mars because it's there? No. Nonsense.

Anyway, what are we talking about?

BUTLER: Looking at some of your duties as Director of Science and Applications, were you involved in planning the specific surface activities that the crews would be involved in?

HESS: No. Well, in some general sense, telling the people to pick up interesting-looking rocks and all that. But one of the problems with surface activities, they didn't have very much time on the surface and they had a bunch of things they were supposed to do, like unfurl the flag. And the amount of time that they could spend studying rocks and doing things soberly and thoughtfully was almost… [none]. Jack [Harrison H.] Schmitt, a good field geologist, went to the surface of the Moon, and I'm convinced he did not, in any way, utilize his geologic training. —The timeline didn't allow it.

A field geologist on the Earth goes out into an area and he wants to study it and figure out how it was made, geologically speaking, and he sits and looks at things for a while, and tries to figure out how that's related to that. Then he wants to go and get a piece of rock here, and something down there, and see if he can explain how they're related. There wasn't any of that sitting and thinking on the Moon possible.

Jack Schmitt, just like all the other astronauts, I think, just picked up whatever rocks he saw. Now, it may be that the surface of the Moon, there's no purpose in having a skilled geologist go up there because all the rocks are related to all the other rocks because everything has been impacted and made into a stew. Everything was the same. Maybe, maybe not. But anyway, I don't think Jack Schmitt had any possibility to use his science, and that's too bad.

BUTLER: It's unfortunate that there were strict time limitations on what the crews were able to do on the surface.

HESS: Yes.

BUTLER: How much training would the crews participate in before the mission, to be able to get them to at least try and pick up some of the rocks that were more interesting or that might generate science, or were you involved much in that?

HESS: I wasn't involved in that. Shoemaker and his group, took the astronauts on various geological field trips, and they went up to—well, they went to Meteor Crater, Arizona. They went up into Oregon where there's a big lava flow area. I can't think what it's called anymore.

BUTLER: Craters of the Moon.

HESS: Craters of the Moon. That sounds right. And spent time with them. These were places which were selected to —look like the Moon. I'm not sure that's fair, but vaguely. And spent time talking with them, and, "Well, how do you sample this? What do you look for?" And all that. That was the kind of training they got, was from Gene's group.

BUTLER: Gene Shoemaker's group was working primarily through the USGS [United States Geological Society]. They weren't employed by NASA, is that correct?

HESS: True.

BUTLER: How was that structured between the NASA science side, the USGS side, but yet all coming together to accomplish the same mission?

HESS: Well, —Gene's group was given the responsibility for all of the planning-related landing sites and all of the training of the astronauts and taking them to interesting places looking for things that might be similar to the Moon in some sense. How was that related? It was just done. I don't think there was a contract or anything like that. I'm not aware of any. And we watched it going on. I talked to Gene a lot and worked with him some. Toured Meteor Crater, Arizona, with him one day, but we never gave him directions what to do. He was one of the team.

BUTLER: So the relationship there was pretty good then between—

HESS: Oh yes, it was very good.

BUTLER: Good.

HESS: And he did a good job.

BUTLER: We talked earlier about ALSEP and that conference that it had actually spun off of and a lot of the planning was done there, and not as much was needed to be done afterwards, other than fine-tuning, actually building the instruments. But on Apollo 11, they actually flew an abbreviated version of the ALSEP. It only had a couple of the instruments, rather than the full package. Do you recall the reasoning behind that, and how that came about?

HESS: No. I don't even know that it was true. But go ahead. I'll believe you.

BUTLER: Okay. I believe they flew just an EASEP [Early Apollo Surface Experiment Package] that had, I think, three instruments, including the seismometer and a couple of others, whereas the larger package was with all of the instruments which flew on later flights. I was just curious about your input on that, but we can move on.

HESS: Don't remember.

BUTLER: Okay. That's fine. Were you involved at all with the Apollo Program Control Board that would make decisions about what aspects of the missions, if there would be changes, especially on the science side of things?

HESS: I sat in on some of the meetings, but I wasn't a member of the board, and I only sat in when there was something involving science programs and that sort of thing.

BUTLER: Do you recall any major incidents—or not incidents, —but events around the science aspect on that?

HESS: No. My memory is that that was set up after the fire to be a control of materials and a control of what went inside the vehicle to make it harder to make changes so that they would have a more carefully controlled vehicle. And we didn't interact very much. Inside our constraints, the weight constraints, the space constraints, we were left pretty well alone, do what we wanted to do, and then we were going to try to do something to change the things that science was allowed to do. We didn't interact with them.

BUTLER: Okay. Going back to the Lunar Receiving Laboratory, you talked a little bit about the development, about the construction, that that was still in process as you came, and the development of the procedures, the ways to decide how the samples would be studied and allocated. Do you recall running actually simulation of walking samples through to test out some of those procedures?

HESS: There were some simulations before the landing, and at one point, they wanted to test—oh lordy, what was this? ——Dave Sensor, the group that had the responsibility for worrying about back-contamination with us, wanted to make a test where they would put some bug inside the vacuum chamber and we would do certain simulations, run rocks through it, and stuff like that, and see if anything leaked out. And the first question I asked, "Tell me about this bug." And I can't remember what it was. It was a fairly simple organism.

He says, "Well, it's not very often lethal."

Oooh! Oooh! That was enough. "Sorry Dave, we're not going to do that one." If the material got out into the room, something like that, most of the people exposed to it develop flu symptoms, had flu for a week or so, and got over it. But there was one in 10,000 or something that got something much worse. So anyway, this was a few months before the landing, and I said, "No thanks, Dave, we're not going to do that."

WESTER HESS: I thought that you were unable to keep the cockroach alive.

HESS: Yes. That's completely separate. We had trouble keeping cockroaches alive. I think they eventually worked it out. But the Lunar Receiving Laboratory was too clean, so our cockroach colony died once or twice, I guess.

WESTER HESS: And if you've lived in Houston, you know that there are cockroaches all over the place.

BUTLER: Yes. Well, I guess that's a good example that things were at least clean enough in the environment for the samples to come in and not get contaminated by something here.

HESS: You see, thinking one past Dave Sensor's putting bacteria in the vacuum chamber, we put in some little sensing device, microparticles that were fluorescent and we put some of them inside, and then went through simulation exercises and looked to see if any of the particles got out, and I don't think they ever found any. But that was not lethal. These were just looking for microparticles, the kind of things that you put on roadside signs that are back-reflecting.

BUTLER: Do you recall any particular challenges in finalizing everything for the LRL, either procedurally or equipment-wise, like a problem piece of equipment or a procedure that was hard to work out?

HESS: Well, I told you about the gloves not working well, and they never did work well. We had to keep replacing them. But we stayed ahead of that.

Procedures working on the samples? I think that all went fairly smoothly. We did some simulation things, and the people that had to handle the samples and work with them were fine. They didn't have troubles.

I remember one thing. When you send the particles down into the ordinary glove boxes, where there's dry nitrogen inside with ordinary rubber gloves going into it, and ordinary mortals can handle the things. Cliff Frondel, head of geology from Harvard, was handling one of these. I saw him, I guess, the first time he got his hands on a lunar sample, and he was looking at things, hand was shaking, and if he wasn't crying, it was the next best thing to it. He was emotionally involved. And he, at some later point, accidentally cut the end off one of the rubber glove fingers somehow by working on the materials inside, and that was a violation of quarantine, so he, Cliff Frondel, head of geology at Harvard, had to go into the quarantine section with the astronauts and stay till the end of the two-week period. It was fairly well along. He only had three or four days or something to stay. But so he went in there and lived with these guys, because that was what the rules said had to happen.

And when he got out and talked to us, he says, "That was fun. I want to do that again. I don't want to work on the damn samples. I want to drink bourbon and tell stories with those guys." [Laughter]

BUTLER: Well, they certainly would have an interesting prospective on the whole thing. That's interesting.

Well, a lot of the people that did work in the lab were scientists from outside; they weren't NASA employees. What were their time constraints? Like how were they able to work their schedules around all of the various meetings that would need to be participated in, or even tests and simulations, and then when the samples came back, how did they—

HESS: I don't know. They had to set up their constraints themselves. Like when the samples came back, the LSAPT team and the sample analysis guys, they were all down there for a week or so, I don't know how long, and their classes just had to wait. This was project number one for them to work on. Working with lunar samples was something pretty special. So they figured out how to do it, and I'm sure we had to accommodate some peculiarities of schedules and stuff, guys go home and stuff, but for simulations and especially when the samples were there, you were there and you did your piece of work. I don't remember that there was ever anybody on the LSAPT. I think I chaired LSAPT or at least I was with them a lot, and we never had any question about absenteeism or anything like that.

BUTLER: Okay. What were your daily responsibilities? Would you be able to describe an average day for you, or was there such a thing?

HESS: Well, assuming that this average day isn't when the samples had just come back to the labs, an ordinary day during the year, I'd go to the office and do the things that you do in an office. You take care of the problems that have come up about people and space and money. I don't know, being the director of a lab is a whole bunch of little jobs that you just have to keep on top of. People keep coming and giving you problems. They come to you with their problems; they're never solutions to problems. Not very exciting. Just the ordinary hum of business.

BUTLER: Okay. That's all right.

WESTER HESS: Could I speak?

BUTLER: Sure.

WESTER HESS: It impressed me that, once or twice, I went to the office and there was Bill in his office, and there was a line of about eight people waiting outside the door, so as part of the day, each one had a little problem, and Bill was able to switch his mind, which I can't do, to answer all of these different people with different problems, just, boom, boom, boom, boom, boom, and I was amazed.

HESS: Well, that's what an administrator has to do.

WESTER HESS: Well, yes, but I thought it was pretty impressive.

BUTLER: It's certainly an important ability to be able to have, to work in so many different areas within the science field and had to tie in the engineering as well, and the NASA needs, science community needs. Those take a unique individual to be able to do that, which is why we're here talking to you today.

Another responsibility that might have fallen under your area is working with the mission control room.

HESS: Mission control, yes. There's a whole other facet of our work that you haven't gotten into. We ran a room just off mission control. You've been in the old mission control room? It's a whole bunch of consoles that are looking up at the big screens up in the front, and different people take responsibility for different parts of the mission in this. —Not inside the mission control, but one of the peripheral rooms just outside the mission control was our responsibility, and what we had to do there was monitor the Sun, and what that meant was, watch out for solar flares.

The reason was that there had been, especially near the previous solar maximum, two or three big solar flares that produced high-energy protons. That's a special kind of flare. Not all of them do that. And if you had a flare that produced enough high-energy protons—and high energy means 100 MEV, the type of thing you get out of the Cyclotron. If the astronaut was out on the surface of the Moon and spent a day in front of one of these things, he'd probably die. There was enough chance of enough radiation from the big flares that it was important to watch these things and see that they didn't occur when we were going there.

Fortunately, none of them occurred during the missions, but we had a way of watching the Sun continuously. We would have solar telescopes, one at Kitt Peak [National Observatory, Arizona], one somewhere in the East, Blue Hill, maybe, run by MIT, Harvard, one in Australia, one somewhere in Europe. And these things, so that you have some telescope around the Earth, always watching the Sun, so that it would tell you if a solar flare has occurred, a big one. And you can watch the Sun in enough detail so that you see these things getting ready to go and you can tell, well, that area looks suspicious. And also the Sun rotates so that it comes up around like this, and you can see active regions, a sunspot group or something, coming up as the Sun rotates and these things appear on the edge of the Sun.

Now, you want to have enough warning so that you can get the guys out of trouble if a big flare occurs. And as I said, we didn't have any, so we didn't have problems with that. But we would have good watches on the Sun all the time and could watch active regions that we knew had gone around out of sight two weeks before, so they're ready to come up again over here now, and watch and see if they're ready to cause trouble.

If we'd ever had had trouble, we wanted to know about it enough ahead of time so that we get the astronauts off the surface of the Moon, back in the command module, turn the nose cone of the command module toward the Sun so that you would have a fair amount of shielding between him and the particles coming in, so that you would be in good shape, hopefully.

So we had a team of guys, including people in Boulder, Colorado, where I worked later, which were the solar observers, and we had a dozen observatories that worked with us on this all the time, and it was completely successful. No problems at all. But mission control, when we were getting ready for mission, they wanted a statement from us about what the condition of the Sun was and what was likely to happen over the next several days.

BUTLER: How much warning would you have been able to get?

HESS: Well, we could warn them that an active sunspot region was coming around the side of the Sun, getting to the area where if particles were emitted, they come toward the Earth. They don't fly directly toward the Earth. They come along making field lines projected from the Sun, but we can tell them where the field lines are connected to the Earth, what direction the particles will come from, and we could give them several days' warning of an active region that looked like it might produce a flare. And then if a big flare did occur, we could tell them right away, and after some minutes, I don't know how long, maybe an hour, we could say whether that flare had produced high-energy particles or not. That was the important thing to know. So we'd give them a couple of days' warning.

I don't know if they ever did a simulation of this or not. If they had had warning of a solar event, what would they have done? I don't know if they simulated that.

BUTLER: Certainly something that we could ask the simulations people when we talk to them.

HESS: Yes.

BUTLER: And how much time would it take for—and I'm sure we could look this up as well, if we needed to, but how much time would it take for those particles to reach the Earth?

HESS: From the Sun to the Earth? The high-energy protons, a couple of hours.

BUTLER: It definitely would have to be moving fast.

HESS: I think I'm right on that.

BUTLER: That's about right, I think. Well, another connection with mission control and a monitoring room was, there was a science room for the missions where a lot of the scientists would gather to watch what the crews were doing on the lunar surface. Were you involved in monitoring that and setting that up, as well?

HESS: I was in and out of it. It was just an observatory, if you will, to watch what's going on. There wasn't any feedback from that into the Mission Control Center, any action items, but the seismologists were always involved in that, wanting to know what was going on. I was in the solar monitoring area more than I was in there.

BUTLER: Certainly a critical area for you to be monitoring, watching for the flares, and it's good that it turned out there weren't any.

HESS: Yes.

BUTLER: Throughout your career at the Johnson Space Center, you worked with a variety of different people in many different areas, from engineering to management, to the scientists. Were there any of those individuals who had a significant impact on you personally or that you felt were key to the Apollo Program, to the science part in particular?

HESS: Well, George Low, the guy who got me to come down there, was a very good man, and he was the guy who, as I said, got me there. But he moved on to Washington [D.C.] later on, so I didn't see him that much. People in the Center, not so much. I was pretty much on my own. Low man on the totem pole.

BUTLER: You eventually decided to move on from your position at the Manned Spacecraft Center. What brought you to that decision?

HESS: My old boss, Jack [John W.] Townsend, —he was my boss when I was at the Goddard Space Flight Center, he had moved on to become the number-two man in NOAA, National Oceanic and Atmospheric Administration. And after Apollo 11, he called me and said, "Would you like to come work with us?"

And I hadn't been thinking about leaving at all, so I was, "Well, I don't know. What do you do and why would you want me?"

And so we talked for a while about what he did. And he said, "The head of our research labs has recently left, and the job of running the labs (of which there were eleven, spread all over the United States), is now open."

And I said, "Well, where is it located."

And he said, "Boulder, Colorado, but we'd sort of like to bring the guy in to work in Washington."

I said, "Well, if you talk to me about Boulder, I'm interested. If you talk to me about Washington, I'm not." And that message got through. And I said, "Well, what's the job and what would I do?"

"You'd be responsible for these laboratories, which do meteorology, atmospheric science, oceanography."

I said, "Well, I don't know anything about any of those."

He says, "Fine. You won't be prejudiced." So after thinking about it for a while, and I guess, talking it over with Wester—I did, didn't I?

WESTER HESS: Yes, you did.

HESS: With the kids, too, I guess.

WESTER HESS: No.

HESS: No?

WESTER HESS: Well, yes, they did remember driving through Boulder.

HESS: No, that question about whether we should go to Boulder or not.

WESTER HESS: They'd like to live in Boulder.

HESS: I'd been to Boulder and I liked it.

WESTER HESS: The kids—we had driven through, the kids and I.

HESS: And so I thought about it. And I said, "Well, Apollo 12, Apollo 13 are going to be a redo of Apollo 11, pretty much the same business. I've done that. Here's something new. Let's go do it." —I knew Jack very well. He was a very good guy, and I'd worked with him before, and we got along fine. And the head of NOAA, whose name was Bob [Robert M.] White, was also a guy I'd known. I didn't know him well, but he was a very good scientist, and one of the most senior scientists in Washington, head of a major agency. And I knew him, and knew he was very good. So I said, "Fine. Let's go to Boulder. Something new." I like new challenges, and the idea of having to learn about meteorology and oceanography was appealing to me. I liked the idea. So after [Apollo] 11, I left.

BUTLER: Jumping back for a moment, I skipped one of my questions. Were you involved in any other projects at the Manned Spacecraft Center, other than the main focus on Apollo?

HESS: No, I don't think so. No.

BUTLER: Well, you moved on to new challenges at NOAA, working in Boulder. If you could tell us some about what you were involved with there, what some of the projects were that you worked on.

HESS: Eleven laboratories which did broad spectrum of work in oceanography and atmospheric sciences. Laboratory in Norman, Oklahoma, which was National Severe Storm Laboratory, which worked on tornadoes, and that was one that I spent a reasonable amount of time on and came to enjoy a lot. We, at that point, were taking radar the next step forward, developing meteorological radar which had been used up to that time simply in monitoring rainfall, and now, using Doppler radar where, instead of just getting—you shine a—beam particles at a cloud and you get reflection back from the cloud, in ordinary radar you just get the reflection back, and depending on how strong it is, you can say it's raining, and it's raining an inch an hour or something like that. Now you shine the beam at the cloud, and it comes back and you measure very small change in the frequency of the radiation coming back at you. If the radiation strikes a particle which is moving, it'll come back at a different frequency—Doppler effect, which you know all about, of course.

BUTLER: Yes.

HESS: So, you could say, "Ah, the particles are moving." Well, you look inside the cloud now, and you get a map of the motion, and it's only the motion in the line of sight, not the other motion, and you can tell what the winds are inside the cloud. Now, if you take two radars, one looking at the cloud from here and one looking at the cloud from here, and combine the information from these two different directions, you can develop a two-dimensional picture of the winds in the cloud, and you can look for rotation.

And so the lab in Norman, Oklahoma, had two such radars and they studied clouds and looked for a rotation, and found that they could find tornadoes as they were developing. And you see not just the funnel itself, but you see the rotation in the mother cloud overhead, and you could see this getting intense half an hour before the funnel formed.

So you had lead time for the process, and therefore, you could warn people. Okay. And we did enough work on this to find that half an hour lead time and see that you could see this for almost all big tornadoes. You could see it for all big tornadoes, almost all medium-size tornadoes, and some fraction of small tornadoes. Well, that was the right kind of thing, because big tornadoes are the more damaging ones.

So we had enough information to—and we'd been working closely with the head of the Weather Service, who was another meteorologist, a good friend of mine, to develop this technique, and as soon as we got it to the place where it was this far along, you knew that it would give you useful signals about tornadoes and, "Okay. Let's go and start putting these things in place."

So the radars, which are now in place, which are called NEXRAD, next radars, are all radars which were developed after we had done the research showing that you could find such tornadoes. And, now, most of the radars, at least in the eastern half of the U.S., are Doppler radars. I don't know if it's complete across the country or not. I suspect it is, but I don't know.

And so you now have this lead time available warning people, to tell people to take cover. A half an hour warning is useful. It's enough so that people can take cover, and it's enough so that some people can get into mischief like driving their car in the wrong directions toward the funnel or away from the funnel. But the fundamental information is there to enable people to use it beneficially for warning. Now, that was maybe the most fun thing we did in our labs, but as I said, we have several labs in the country. There's an oceanographic lab in Miami, oceanographic lab in—

WESTER HESS: Seattle somewhere.

HESS: Seattle, Pacific Marine Environmental Lab, a smaller one in Hawaii, and combination of these labs have been deeply involved in watching ocean current structures all over the world, and the business about El Niño has been partly our discovery and partly discovery of oceanographers at Scripps Institute in San Diego, but mapping of ocean surface currents in the tropical Pacific and what their temperatures are in showing this puddle of very warm water coming into the Peruvian coast area and all that, and all the things that happen as a result of it.

And getting information about the relationship of this big puddle of warm water, the El Niño, and northern latitude currents, dry down here in California, lots of wet up in Alaska, wet, cold in the East U.S., the El Niño process, which is really a worldwide process, not just a local thing. Collecting the data, analyzing the data, showing the relationships was one of the more fun things that we did. There was a lot more of that, but I don't know how much you want.

BUTLER: Okay. Well, were any of these projects that you worked on, did any of them ever go back and overlap with NASA at all?

HESS: NASA was involved in the El Niño things. We put out a lot of buoys in the tropical Pacific. They flew airplanes overhead and measured ocean surface temperature distribution and satellite work, ocean surface temperature distributions.

The data was combined in various ways. Individual scientists would get together and combine it in their conferences, present the data, and make a unified picture of what's going on. So the answer was, the agencies would collaborate together, but there weren't any structural changes. There weren't any contracts or anything like that.

BUTLER: This was your opportunity to get back into some personal research, as well as some administrative—

HESS: Well, I was still an administrator. I didn't do any research personally, but I could get closer to the research and feel more what the research was and how important it was. That was fun. I enjoyed doing that.

WESTER HESS: You edited a book or two.

HESS: I did a book on weather modification. That was another big area we worked on—what can you do to modify weather and can you change the characteristics of hurricanes. The answer is, maybe yes. We still don't really know, but the first project we did, which we thought we knew how to change the highest winds to decrease them, turned out not to work. How can you increase rainfall in different areas? How much of a science is that and how much is black magic, and how much doesn't work at all? There were a lot of charlatans in that in the old days, but there's some truth to it. That was fun.

I did a book on the radiation belt, and it goes back to Goddard days, and that was fun.

WESTER HESS: A book about space science, wasn't it?

HESS: When I was in Goddard, we did a book, *Introduction to Space Science*, which is one chapter on different subjects written by different people, most of them from Goddard, which was bringing people aware of what was going on in space science in 1959 or something, and that was fun and useful. It was used as a text in a lot of places.

BUTLER: You certainly have been in quite involved, then, in some of your own writing, as well as research, as well as—

HESS: Yes. I wrote a lot of papers. I published—

WESTER HESS: Seventy-nine, wasn't it?

HESS: It's over a hundred research papers. Remember the National Academy of Engineering. Fun.

What else?

BUTLER: Well, you worked at NCAR for a while, the National Center for Atmospheric Research. Was that part of your work through NOAA?

HESS: Well, no. NCAR is run by the National Science Foundation. NOAA is part of the Department of Commerce. It's separate, but the two labs did a bunch of things together, scientifically did things together, like— My mind is turning to mashed potatoes.

BUTLER: That's okay. We've been wringing it out.

HESS: Downbursts, strong downward flow of air from a thunderstorm or incipient thunderstorm that comes down with big vertical velocities, comes down on the ground and spreads out. Now, if you have one of these things called a downburst—am I correct, Wester?

WESTER HESS: Yes.

HESS: If you have one of these things and a landing airplane goes through one of these things and gets in trouble, and we did—NCAR, when I was at NCAR, we were doing this, and also people at NOAA were working on it, too, did the fundamental measurements to show how these things occur and how you can detect them and then what you ought to do to avoid them. There have been a number of crashes of airplanes that encountered these things without knowing about them, and were trying to land. If you're trying to land, it's an interesting thing.

Here's the down-flowing air. It spreads out near the surface of the ground. Now, you're on an airplane that's trying to land on a runway, right into one of these things. When you first encounter it, there's air flowing toward you, which gives you more lift, and so you tend to throttle back to get down onto the runway. Then you go through the neutral point and you're over here, and the air is going away from you, so there is less lift and now you're in trouble because you've throttled back, your nose is somewhat down, you come into less lift, and you crash. Well, if you know about this, and know that one is occurring, you can avoid it. But if you don't know about it, you can get in trouble.

BUTLER: Certainly something very valuable, then, to study.

HESS: Yes. So when I went to NCAR, that was one of the projects that was being worked on there, and that was very useful project. —Another thing NCAR did, which is still going on and will for another decade or so, is climate models, mathematical models of climate. And these keep getting more and more complicated and better, but bigger computer machines all the time.

Climate model has to be a combination of an atmospheric model and an ocean model, because the two of them are both important in climate. In climate, on even an annual scale, but certainly a decadal scale or a century-type scale, you have to put both of them in, and worrying about what's going to happen with climate and can you predict it, and how well can you understand it, you have to have very complicated models for cloudiness, how much cloudiness, what different kinds of clouds, how much moisture is entrained in the clouds, what altitude are the clouds at, and that's the big bugaboo in climate models now, is doing clouds right.

But the whole business is very complicated, and NCAR was probably leading the world in the development of advanced climate models, and still is, and doing a very good job of it. But it's not a completely settled business. There's lots of work to do on it yet.

BUTLER: It's still something to strive for, to learn about.

HESS: Yes.

WESTER HESS: Was PROFS the Doppler business?

HESS: PROFS. Good lord, what's PROFS?

WESTER HESS: Oh, well, all right.

HESS: I don't remember.

BUTLER: What had led to you moving to NCAR from NOAA?

HESS: Bob White, who is my ex-boss at NOAA, asked me if I would—the head of NCAR had left. Who was he, Wester, the guy before me?

WESTER HESS: Walter. Walter something. He was a skier and he quit skiing, and he wasn't known for that.

HESS: You don't mean Walter Monk?

WESTER HESS: No, not Walter Monk. They had musical evenings.

HESS: Walter Orr Roberts.

WESTER HESS: Right. Correct.

HESS: Yes. That's right. He retired, and so the job was open, and Bob White suggested me for the job, and he was senior enough guy in Washington, so I could get it. And the question was, did I want it? Why did I go to NCAR? I'm not sure I know. Because Bob White asked me.

BUTLER: Okay. Well, that's a good reason. You had worked closely with him, and knew that he—.

HESS: It was a new challenge, and I'd been at NOAA for some years.

BUTLER: Well, eventually you moved on to work for the Department of Energy's Office of Energy Research. What sorts of projects were you involved in there?

HESS: I was the director of high energy [and] nuclear physics…. I had a budget of a [$] billion-plus. I was responsible for the programs at the major U.S. laboratories doing high-energy physics, and this means Fermilab [Fermi National Accelerator Laboratory] outside Chicago [Illinois], and Brookhaven National Laboratory on Long Island [New York], and Stanford [University, Palo Alto, California] down here, the two-mile linear accelerator, and Lawrence Berkeley Laboratory, except they, in recent years, haven't had a big machine here. Machines are getting so big, they can't fit anywhere near this area anymore.

So I had the job of the care and feeding of these laboratories, and developing the programs, seeing that the program did develop. I didn't do it personally, but seeing that the labs are doing good work in pushing forward. So the question about what new machines to develop, what new projects to undertake in connection with these machines. I had the responsibility for the research on the superconducting supercollider. When it became a sort of mature project, they put together another team who had the responsibility for the construction of the machine. I didn't ever have that, but I had the research side of it. And that went forward until it got to be so expensive that Congress killed it. —When it reached $8 billion, they thought that's enough. So that went away. Probably never will be another standalone U.S. major machine like that. They're just too much money. But this was getting me back to my own fundamental early research. I'm a nuclear physicist by training, and so I went back to high-energy nuclear physics for my final venture.

The reason for going back into government, when I was in NCAR, I was out of government, and when you retire from government, your salary at retirement is based on high three, the three years of highest salary in government. So I'd been out for six, eight years, I've forgotten how much, and salaries had gone up, and so I was getting ready to retire in a few years, and so I said, "Okay, I'd better get back in," and the job as the head of the high-energy work in the Department of Energy became available, so I applied for it as a natural my-own-field-type work, and getting high three for retirement.

BUTLER: Your career certainly did take an interesting path, starting in physics, and branching out into space science, and then meteorology, and then back to physics.

HESS: Yes. I wandered all over. Couldn't hold a job. [Laughter]

BUTLER: Well, but you had several very interesting experiences.

HESS: Yes, I did. I had an interesting life.

BUTLER: Did you at all follow any of the Apollo program after you had left NASA?

HESS: Not really. Tell me about them.

BUTLER: Well, there were a few, and they did bring back—they were able to get, as you said, more science on some of the later missions with carrying some in the command module bays.

HESS: Yes, yes, yes.

BUTLER: They were able to stay on the surface longer. They were able to achieve a few interesting things with it.

HESS: I, at one point, said the only thing we ought to do with man in space, after the Apollo game was over, was to go back to the Moon and set up a permanent observatory. There are a lots of good things. —It doesn't really have to be manned. Manned would probably help; I don't know. There are lots of good things you can use the Moon as a platform. No atmosphere, so you don't have all the problems we have here, and build big telescopes and big infrared systems and things like that, and open up astronomy in a way that you can't do it on the Earth. It would be nice to do it, but I don't think it will ever get done.

BUTLER: Certainly would be very interesting.

HESS: Yes.

BUTLER: Well, looking back over your career, particularly at NASA, both at Goddard and at Manned Spacecraft Center, what would you consider your biggest challenge, but also your most significant accomplishment?

HESS: Significant accomplishments. —I think two things. One, working on the research related to the radiation belt. We developed a quantitative understanding of several processes, things that go on to make the radiation belts, and this took that business into a firm, quantitative mode, so that that became a well-based science. And work that we did with Dungey from Imperial College in London, and Beard from Kansas, very good stuff. I thoroughly enjoyed it. Very good research, and I liked doing it, and it was important.

Then the other accomplishment, I guess, would be just getting the science done on Apollo. A number of people could have done that, but somebody had to step in and do it, and the Manned Spacecraft Center, by itself, had the capability of screwing it up pretty well, and we kept it so that it was a science-based program, and the scientists were involved in it and it worked well, and I think that was an accomplishment.

Challenges? The challenge of trying to get things done at the Manned Spacecraft Center, because, as I say, I was low man on the totem pole, so anything I wanted to do that was in anyway different, we had to fight the organization on it, and that was a challenge, but we got it done.

Enough.

BUTLER: Do you recall where you were, and what you were thinking when Apollo 11 landed on the Moon and you were able to accomplish some of what you had been working on?

HESS: When it landed, I was in the room off the Mission Control Center, the Sun Room. I was hearing the conversation between mission control and the astronaut. And remember, the spacecraft was silent. And the mission control guys said, "It's going sideways. It's going sideways. What's it doing that for? Where's it going? It's only got forty seconds of fuel left." And they were very nervous. They were really out of their mind, because instead of just coming down and landing like this, like the plan had it, he comes down and he starts going like this.

Then he puts it down when he's got about twenty seconds of fuel left, and everybody just, "Oh. Oh boy!"

But what he was doing was overflying a crater, but he didn't say that. He didn't talk at all during this period of time—Neil [A.] Armstrong. And so people were really, really upset.

It was fun. I was on the edge of that. I knew what was going on well enough to know that something funny was happening. I hadn't been involved in the simulations well enough before that to understand how funny it was, but it was funny.

BUTLER: Certainly quite a moment.

HESS: Yes. Right.

BUTLER: Well, I'd like to, at this point, ask Rebecca if she had any questions.

WRIGHT: I just have one, because you mentioned it a couple of times. While you were at the Manned Spacecraft Center, you had the opportunity to interact with so many scientists and investigators that wanted to be involved with the Apollo Program. Could you share with us, for a few minutes, their reactions and maybe some of their questions? Was there anybody who ever hesitated at becoming involved with the program, or were they all very enthusiastic and energetic about wanting to be part of what was going on in the country at this time?

HESS: I don't remember anybody being hesitant about getting involved. Essentially, all of the people we were working with, having access to lunar samples, something like this, was a next step in whatever their scientific program had been. A geologist or geochemist who's working on terrestrial samples, meteorite samples, meteorites from Antarctica, there were a bunch of guys who went down to collect meteorites in an interesting situation in Antarctica where there's one particular area where the meteorites will collect on the surface or near surface of the ice, and then they would gradually flow into one particular region and then sort of stop, and it's a natural place for going and collecting a bunch of them. Maybe you know more about this than I do.

BUTLER: I know a little bit about it, yes.

HESS: And there were several of those guys who worked with us, and they were all very keen on getting lunar materials. I can't think of anybody who was unhappy or hesitant or didn't like what was going on with the samples. They just wanted to get a hold of them and go to work on them.

WRIGHT: Did you have to turn some down that wanted to be involved in the program?

HESS: Well, yes. We got proposals from some people who weren't technically competent, and so we turned them down. And that's the normal way that science is done, that you have to develop the capability, the competence to do a certain class of work, and then you compete for new steps in this, new projects, new capabilities. If you weren't competent, if you haven't demonstrated ability, you're not going to get the material.

WRIGHT: Did you receive unsolicited proposals of what other scientists thought you should be doing in space for science?

HESS: Yes.

WRIGHT: Is there any of those that come to your mind that you maybe wish you could have suggested but knew—

HESS: Most of them were nuts. Things that we would have liked to have done? Mostly I think we had a crank file, stuff that came in that was junk.

I can't think of anything that people said we ought to have done that we wanted to do. Nothing comes to mind.

WRIGHT: Did you have any ideas of your own that you would have liked to have included in there?

HESS: Well, a different project now. At one point I personally worked on small accelerators to fly into space to generate artificial aurora. You take a not very big accelerator, a thing that puts out an amp of particles of 20 kilovolts. Now, you don't know these numbers very well, but that's not a terribly big deal. You shine that down—you're out in space out here—you shine it down on the top of the atmosphere, it'll make artificial aurora, and if you're underneath it, looking up, "Hey, there's an auroral spot up there. Hey, what's going on? That's Bill Hess up there, shooting his accelerator." Dah-dah-dah-dah-dah-dah.

And I did that a couple of times. We did it from a rocket fired from the East Coast. Shoot the beam down, make an artificial aurora. Fine, it works, we can see it, we can photograph it with some special low-light-level cameras and stuff like that. "Hey, that's great."

Now let's do one more complicated. We'll go out to Hawaii, fly a rocket up here, shoot the beam of particles along a magnetic field line, have them come down into the southern hemisphere, make an aurora in the southern hemisphere, photograph it from an airplane down there. So we went and put the rocket up and shone beams on particles, and photographed it from the other end. "Hey, it works. We can see them."

So we demonstrated the ability to make these artificial auroras in simple experiments like this. Now, we want to get more complicated? Fine. Let's do this on a space vehicle where you shoot the beam upward from Alaska, and the beam now, all these particles are going to go along a magnetic field line and come down somewhere in the southern hemisphere somewhere, and make an aurora.

Well, if you happen to have an optical gear at the right place looking for it, fine, you can see it. But now let's do an experiment where we do this on a polar orbiting vehicle and it's going further north all the time and you "pung, pung, pung, pung," and the southern hemisphere of these things keep showing up further south until all of a sudden they stop. Why does it stop? Because above the auroral zone, the magnetic field doesn't couple back onto itself on the Earth like an ordinary bar magnet does. It's coupled somewhere way far out into space to the Moon, or to Jupiter, or to somewhere else.

The idea was to try to understand the magnetic field of the Earth, to map it by sending particle beams along magnetic field lines, and to see all the peculiar things that will happen, especially at high latitudes. Never did it. It would have been fun, but never had the opportunity.

BUTLER: Well, I certainly want to thank you for sharing your experiences with us. You've had quite a few interesting avenues that you've had a chance to explore in your career, and it's certainly been very interesting for us.

HESS: Good. Good luck with your collection of materials.

BUTLER: Thank you.

[End of Interview]