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ORAL HISTORY TRANSCRIPT

LEON T. SILVER
INTERVIEWED BY CAROL BUTLER
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BUTLER: Today is May 5, 2002. This oral history with Dr. Leon T. Silver is being conducted for the Johnson Space Center Oral History Project. Carol Butler is the interviewer and is assisted by Rebecca Wright and Sandra Johnson.

Thank you very much for joining us here today. To begin with, if you could tell us about how you became involved in planetary geology and lunar geology and looking at samples other than from Earth.

SILVER: Well, actually, it starts with looking samples from Earth, and I was a lucky duck, who as a very young geologist got to work with people who subsequently made contributions of the very first order. One of the guys I worked with was a colleague of mine by the name of Clair [C.] Patterson. Clair Patterson is probably unknown to you, and although within the technical circles, his contributions are very well recognized, he's not generally known. He's the man who first determined accurately the age of the Earth, and he put together the age of the Earth from a combined study of the evolution of lead isotopes and meteorites and terrestrial samples, and his determination of the age of the Earth, which was done at the time when the decay constants—those are critical elements in the calculations of radiometric ages—were not known precisely and have subsequently been modified, but his ages have stood up exceedingly well. He reported an age of 4.55 billion years for the Earth meteorite system, and he did this on several meteorites in several ways and several papers. I'm not going to cite all of them.

But, in fact, the stuff he worked on had to be selected for him. He was a very fine chemist, but he was not a meteoriticist or geologist. I had been hired as a young geologist at Caltech [California Institute of Technology, Pasadena, California] to work with a brand-new geochemistry group of which he was a member, and I helped him prepare and I helped him select a number of the key samples that were involved. He came to Caltech in 1952. I started work on a brand-new geochemical project at the beginning of 1952.

We were both working for a man by the name of Harrison Brown, who came from [University of] Chicago [Illinois], and he brought three key people with him as postdoctoral fellows, and they revolutionized many aspects of geochemistry, all of which were applied to meteorite samples, lunar samples, and all of whom produced legions of grad students and postdocs who populated the world. They're still doing the work in lunar and meteorite samples right now. So I was lucky enough to be involved at that early stage. That was actually in 1953 that the first important paper came out.

So the first thing I want to mention is, I can't just start with how I got involved in the lunar program. I got involved in the meteorite program fairly early, and then subsequently I did a number of things on meteorites, and I produced one very important student who later became the curator of lunar samples. His name was Michael [B.] Duke. I don't know whether he's appeared in there. I didn't produce him; he was himself, but he worked with me in that.

So I've been involved in [extraterrestial] things for a long, long time. That had been a major subject of interest to Harrison Brown. My wife, who happens to the Deputy Director of the Caltech Alumni Association, may have heard of Harrison Brown, but she didn't know anything about him. Is that right?

That's unfortunate, because, in fact, the essence of my story today is, I may have gotten a little better notoriety or stuff, but we [were] an incredibly complex organism of many talents, many people, and that's one thing that I didn't say very well when Andy [Andrew] Chaikin interviewed me [for the book *A Man on the Moon: The Voyages of the Apollo Astronauts*], and that's one of the things I want to get on the record. If I can use an old saw, "standing on the shoulders of giants," it's so true, it's incredible. So the focus on me is very unfortunate. I'll make that point again and again as we go in there.

However, I had gotten involved in the mad rush to prepare to analyze, investigate samples returned from the Moon, and I was building a new clean lab to do this. My background is that of a geologist and a geochemist, and my geochemistry involves isotopes, as well as broad chemistry, and the particular isotopic systems in which I worked were the same ones with which Clair Patterson worked in establishing the age of the Earth.

In fact, originally, building the lab, it was to be a joint lab for work by Patterson and myself. Lo and behold, four months before I got the first lunar samples back, Patterson says to me, "Lee, I don't want to work on lunar samples. I've got some other things that I think are more important." He was right. He's the man who's responsible for the removal of lead tetraethyl from gasoline. His interest was in the evidence that he had been finding of the impact of the industrial revolution and man's activities on lead in the environment. He undertook a long program of establishing the fact that before man got interested first in silver, then in lead, because silver and lead are produced from the same ores, you understand, that lead was being introduced to the environment.

The Greeks and Romans were smelting silver ores, which were mostly lead, and starting a redistribution of natural leads into man's environment. Just to make it cute, Patterson showed that the Romans, in particular, were using lead vessels to take the sour out of sour wine and making them sweeter. The sour in sour wine is acetic acid, and if you have acetic acid in a lead vessel, it becomes lead acetate, which is quite sweet. So the Romans for years and years were putting the wines of the times into their lead vessels. Now, if you want to make the story cuter, Patterson, along with others, but speculated that as the Caesars grew from one, two, three, four, five, they were drinking more and more wine from lead vessels, and a few of them were a little bit dotty, if not mad, and it may well have been drinking sweetened wine. That's completely speculative. But it's a nifty story, isn't it?

BUTLER: Yes.

SILVER: But I am trying to tell you that one of the great men with whom I was associated—and we were particularly good buddies—was this guy Clair Patterson. He taught me how to do isotope chemistry, and that's how I got my lab started.

So I was preparing to investigate lunar samples when they came back, and I was going to do the work that Pat and I said we would do. Patterson went off and did his "something more important than lunar samples," and I'll let you decide. I completed building the new labs and I started the work on the lunar samples.

And then comes another important part of the story. Have any of you ever heard of Eugene [M.] Shoemaker? Okay. Gene and I had known each other since 1948. .We were both brand-new, young geologists working for the U.S. Geological Survey [USGS]. Gene subsequently became very interested in features on the Colorado Plateau, which had been speculatively either volcanic features or impact features.

A very, very topnotch geologist at the turn of the century had speculated that there was a crater, which was then called Coon's Butte [Meteor Crater in northern Arizona], that might be the result of impact, and this guy—do you know what his name was?—G. K. Gilbert, one of the great figures in American science—might be the result of [a meteorite] impact. But he couldn't make an unequivocal case, and he left it hanging [between meteorite and volcanic cratering] in a famous paper he gave as president of the American Philosophical Society.

Shoemaker picked that up. Shoemaker was supported, as I was, for that summer of 1948, in the search for uranium resources, because some of these volcanic features had interesting, usually small, uranium deposits associated with them. The country was involved in the first major developments in nuclear weaponry, and they wanted lots and lots of uranium, and so he and I were supported that summer for that.

Now, why is he important? Because about two years before Apollo, Shoemaker, who had been seconded from the U.S. Geological Survey to work with NASA and to start both a NASA science program for lunar samples and to familiarize astronauts with what we knew about the Moon and what their probable overall mission targets were going to be. [He had worked] specifically what was going to be done by the Moon manned landings on behalf of answering fundamental questions about the Moon and the solar system … for at least seven years. But as he saw the program developing, he had reservations about NASA's commitment to science, as opposed to NASA's commitment to getting a man to the Moon and bringing him back, which was the original federal statement. He had no objection to that. He fully appreciated it. If he hadn't come down with a very severe kidney disease, he probably would have been our first scientist-astronaut, because he had learned so much, he was the natural. But he was ailing physically, and, to put it bluntly, fed up.

So he made a public [critical] statement which infuriated high levels in Washington, and a lot of people who knew him well enough to respect him. I'll say right now parenthetically, he turned out to be the most important planetary scientist of the twenty-first century in the sum of all of his work. One of the things he challenged was whether or not the [role] of man was being put to its best use. He was a strong advocate of the use of man, and he laid out many things that men could do that he didn't think automated remote sensors could do, and he still wasn't pleased. I'm going to use some names. He really alienated some guys who admired him. I have to think for a minute. Rocco [A.] Petrone, who ultimately became a good friend of mine. But he was a very good friend of [Gene's] and he was wounded by Gene's blunt remarks. Remember that he headed up the Apollo office in Washington, and his concern was to keep the national focus on the promising success of Apollo so that the national support for Apollo would [continue]. The kinds of things Gene was saying were not, to his mind and to a lot of other people's minds, supportive of the main goal.

So Gene quit, and when he quit, it turns out he was an alumnus at Caltech. We knew his work. We admired it. So we hired him as a professor, and then shortly thereafter we asked him to be chairman of our division. Gene, incidentally, had been helping in the selection of potential astronauts, and when the decision was made [to] acquire a class of scientist-astronauts, Gene's knowledge and his style and everything else was important in that. In fact, three people he had hired to work for the U.S. Geological Survey's Astrogeology Branch applied for appointments, and those three people had also been my students in one way or another. One of them was Harrison [H. "Jack"] Schmitt, who actually became an astronaut. Another was Mike Duke. Mike Duke didn't [possess] the ability to take high G forces, didn't pass that test, but he was absolutely suited in other ways. And the other one was a guy by the name of Tom [Thomas R.] McGetchin.

Mike had been my student. Jack Schmitt had been my student in undergraduate classes, but he got his Ph.D. at Harvard [University, Cambridge, Massachusetts]. But I'd known him since he was eleven, in New Mexico. And Tom McGetchin had been a joint student of Gene Shoemaker's and mine. I supervised one aspect, and Gene [guided] him [in another aspect]. He was an extremely creative and provocative man in other ways. So between him, we produced Tom McGetchin. Tom McGetchin—eventually all three became involved in the lunar program. Tom McGetchin went to MIT [Massachusetts Institute of Technology, Cambridge, Massachusetts]. He eventually developed cancer of the stomach, which, although it went into remission for a while, killed him. He had been, among other things, Director of the Lunar Science Institute down here. So these were good guys.

Harrison Schmitt, "Jack," as the success of Apollos 8, 9, and 10, all those successes, which were fantastic achievements, came along [to Gene Shoemaker] and said, "We've got to start to getting ready for the science we've got to do." In talking it over, they decided that they had to go into another phase of astronaut training. They wanted to prepare the astronauts for their missions, and they felt that someone was needed who would work with the astronauts in that training, and between them, they speculated—because of my interest, I was preparing to do lunar samples anyway—whether I might be willing to do that.

So Jack made the first approach, but the guy who helped me make my decision was my boss, the chairman of the division, Gene Shoemaker. So that's how I got into the game. Now, Shoemaker was an old hand in this whole game since he'd been the original science—what will I say—attaché, and Jack was deep into the business of meeting the requirements to become a successful astronaut, and you have to recognize it as that.

He was a member of a class of astronauts which included very good people, many of whom actually flew in space. Nobody else went to the Moon. He had to learn all the technicalities. He was a Caltech undergraduate. That had meant he'd had the technical training, or better, of most engineers. And he was a geologist who'd gotten a Ph.D. at Harvard, and had worked internationally. So he was very well prepared. My wife happens to know Jack very well. In fact, we talked about Jack with George [W. S.] Abbey last night.

I thought to myself, well, since my forte was geology and some chemistry, that I was in a position to talk to him about more than just teaching them geology. It was a case of trying to point out to them the potential significance of their experiments, and this was my own approach to the problem. But we'll get back to that when we talk about astronaut training a little later.

But to bring about astronaut training, Schmitt laid the critical [ground] work. In July 1969, we'd had an incredible Apollo 11 experiment, that proved we could get men to the Moon and bring them back. Then in the fall we had the Apollo 12 experiment that proved that we could not only get to the Moon, but we could pinpoint our location. We had as a target a small crater in which sat one of the unmanned [surveyor] landers, and the astronauts were to land on the rim of that crater to pick up parts of that thing and bring it back. It was a [remarkable] demonstration of capability. This is so typical of so many of the missions, that they had multiple purposes in which PR [public relations] was very important. That's in order to keep the sense of success and the capability and value alive all the way through things. That was a very successful mission.

However, although Gene Shoemaker had organized a significant number of field trips for classes one, two and three and four, before I got involved. He'd used the resources of the U.S. Geological Survey, who had many outstanding geologists working with [him], the astronauts saw it as academic preparation. But here I am coming on board, and we're starting to see the line-up of missions.

For a while there, we thought we had [flights through] Apollo 21 going, and then we thought we had 20, and then we thought we had 19 and 18, and they got cut off at the end, as you know. But there were a lot of crews involved, and NASA's approach was to take experienced, successful astronauts, those who flew well in Gemini particularly, and make them commanders and command module pilots and lunar landing module pilots and all that other stuff. But my job was to prepare them for the missions. So I had a much greater potential impact on their missions, and the first thing I had to do was to convince them that I could, in fact, help make their missions more successful, and you had to know these men.

By that time, I'd been a professor at one of the best schools in the country, probably the best school in the country in geology, and I'd had a lot of good students and I'd seen a lot of very good students. But in terms of energy, intellect, and determination, you can't beat the astronaut crews on the average. The selection process really was superb.

So my job was to convince them that I could help them, and unless I convinced particularly the team leader, the commander, it wouldn't work. Well, here's where Schmitt did one of his many important jobs. First, he persuaded a guy by the name of Fred [W.] Haise, landing module pilot on Apollo 13, that geology was interesting and fun, and there was going to be an opportunity for them to do something very, very important. Fred Haise is a wonderful guy, and his enthusiasm for the science part of his missions was very high.

The commander, however, was a key man, and that was Captain Jim [James A.] Lovell, and Captain Lovell was a hard-nosed Navy pilot, and all these guys were hard-nosed pilots. … Most of them, had been military pilots, except for one key one, and many of them had seen service in combat. Vietnam was back there. One of them went back. Maybe some of them had flown in Korea, but I don't know.

But [one] thing that was important to [each of] them was to make their mission outstanding. In the whole sequence of missions, each mission had to be conspicuously better performed than the previous ones, and that was a challenge. The reason it was a challenge was that the first mission, Apollo 11, you understand, or even Apollo 10 or Apollo 9, if you look at the guys who were on them, were commanded by guys who were just incredibly able and smart and [achievers]. You look at their subsequent careers, and you can see it.

But Apollo 11 had the one guy who [then was] not a military pilot, and that was Neil [A. Armstrong], and I have to tell you that what Neil did in the shortest period of time that anybody [had] was so brilliant from this point of view of providing the materials to the scientists, that nobody can claim to have exceeded it in production per minute. He was really outstanding. And I had nothing to do with it; it doesn't reflect on my work at all. And we didn't even begin to realize what he had done. He broke rules.

He had a very strict protocol, which said, "You will never leave the field of [view of] the camera." Neil Armstrong recognized that just beyond the field of the camera was a rim of craters covered with rocks and dust, which had been excavated from a little deeper than everywhere else, and he had a very special box for bringing back good samples with a special seal on it, and for about seven or eight minutes, you couldn't see Neil. The focus was on the second man, [Buzz Aldrin]. What was Neil doing? He stuffed that box so fast and so full [of lunar samples], and that has yielded so much material of value to science that I don't think anybody's, at the rate that Neil did, achieved efficiency. And that [was in] eight or ten minutes, you know.

If you [folks] have followed the careers of our astronauts, you know that Neil became a distinguished professor at Case Western Reserve [University, Cleveland, Ohio], and then he left the Case Western Reserve. I don't think any other astronaut in the first two or three crews could have done that. Then he went into private industry and he has done some great things since. But he's a very special guy.

I'm using all this to make the point that the people who went to the Moon were extraordinary, extraordinary talents. And there I was, asked to teach them. There were no degrees to be offered. I couldn't claim that he was going to be my Ph.D. or anything like that. It was just doing the job that had to be done to get the most [out] of the mission. On behalf of national interests, we wanted those things to be successful.

The first measure of success is getting them back alive and healthy, and everything was secondary to that. The second measures of the missions were the things we learned about our own planet, about the solar system, things like that, and those came on [like] gangbusters with a lot of dispute, typical scientific controversy, and all the other stuff, but it was healthy. It was good.

What I found was that I could work with each commander, because I recognized that he wanted his mission to be better than the previous missions, although he would never say that. Don't you believe it. These were extremely competitive guys, and the best, and that's what I have to say.

So at Jack's persuasion and because I'm going to work on lunar samples in-house, when I came down to Houston for other reasons, Jack was going to introduce me to Jim Lovell and Fred Haise, and they were going to look at me…. But a talk for thirty minutes or so doesn't prove very much. But I persuaded them to make a deal, and that deal was that they would give me a week. They'd come out into the desert with me in southeastern California, and we would see whether or not I could make a case that their working with me would be worthwhile to their mission….

This is not a deal with NASA; this was with Jim Lovell and Fred Haise. It wasn't even a deal with the full team, because Mike Collins, who was the command module pilot, didn't come out. We went to a place about sixty miles southeast of Palm Springs [California], forty miles southeast of Indio, [in] remote desert. It was hot. I had a very good postdoc working with me, who's now just retired as the Chairman of the Department of Geology at University of Pittsburgh [Pittsburgh, Pennsylvania], but he was my postdoc at the time. He and I drove two vehicles. One of them was my own personal carry-all, and the other was a [Caltech] division carry-all. This is [chairman] Gene Shoemaker still supporting the program, even though he'd been highly critical, you understand.

We put in what amounted to six field days, a week. They had one day off to go into Palm Springs and clean up; we had no showers. All the water we had was in G.I. cans and things like that. I did two important things: I created some exercises for them, and I cooked. They did several important things: they worked their butts off, and they washed the dishes. It was a successful exercise, and I made some good friends.

Fairly important in that was the fact that Jack was with us for the first three days, to make sure they got to know me. And another [very] important thing was that there was a backup crew.

The Apollo 13 backup crew was led by Captain John [W.] Young and by Charlie [Charles M.] Duke, and their interest was just as high. And so I had a set of exercises which in part was show-and-tell, but more importantly than show-and-tell was literally exercises; they had to do things. And I just had to innovate. I mean, my classes aren't taught that way, you see, so I had to innovate. And I had some moments of inspiration, and I had the feeling that the whole project, the whole Apollo project, was blessed.

For example, on that first week I needed to make them get a sense of what they could do practically. Now, this chunk of desert is very barren. Trees are sparse. Only along the rims of bigger dry washes can trees get their roots down deep enough to get some water. There's a particular kind of tree called an ironwood tree, which has thorns like everything else on the desert, very, very dense, very tough wood. And we camped in one wash called Canyon Springs Wash, because it had some water in it, not potable, but water in it. And I gave them exercises to do in that wash. It was a place I had used for training students at Caltech in the past, so I knew it quite well.

But there was a little canyon tributary which rose about seventy-five feet above the bed of the wash. In that little canyon was a stunted ironwood tree, and I said to them, "Help me and use your imagination. This stunted ironwood tree is a landing module. Now, you're in the landing module, and the TV, which you're going to have," because remember this was the way it was on Apollos 11 and 12, was not particularly flexible. It turned out that wasn't true for what would have been on 13; they never landed, of course. But it wasn't true subsequently. "But I want you to tell me what you see, because you've got 360-degree vision, and now we want to practice articulating the hard stuff you see. Now you're going to do documentation. You've got first-rate cameras. You've got all kinds of other things. But I'm in [the] science back room, and

I'm trying to see how well your landing site suits the needs that had been projected as the targets of the mission." And we started off with that.

One by one, all four of those guys went through that exercise, and in the end it was interesting. They were different in style and different in capability. But having been a military pilot, or having been a test pilot, the business of calling what they see was as natural as it could be to them, you understand, and so they took to it. So I think that little tree was one of my blessings. There was no obvious source of water for it. That tree's still there. It ought to have a monument on it, as far as I'm concerned. That's another example of how the breaks fall for you.

I'll give you another. About the third day—on the fourth day we took this break and they went to Palm Springs, and I don't what the heck I did—but on the third day, I had them walking down the edge of the main wash, and I would take turns with each of the crew members, and I'd walk behind them, and they were to [observe and] articulate. They're not talking to me, but they're articulating and I'm listening. Okay. Now we're coming down the wash, and the wash was about three times as wide as this room, and there's banks going up five, six, seven, eight feet. The wash itself is filled with dry sand, but the banks are exposures of rocks, and that's what I wanted them focusing on: what are you seeing? And some of it was very straightforward: color, shape, texture, all the other things that have to go. They came down.

Maybe I was as far away as the camera was when I saw a place where there was a ledge; it was straight, just standing up, almost on the edge. And there was something interesting on top of it. I looked at it, and I recognized it for what it was. It was an ancient clay pot, and this turned out to be a Paiute pot. But the business of seeing everything and articulating it and with a little nudging here and there about describing things and all, Jim Lovell came on that pot. He said, "I've got this." You know, he's the man that said, "Houston, we have a problem." He came on this thing, and he said, "There's something here that doesn't look natural."

Paiute pottery is not very beautiful, but it was nearly a whole pot with one thing wrong with it; the bottom was out. That bottom was knocked out. That was part of the thing. This was a pot in which they had placed a potful of seeds and beans and things like that, beans being the beans of things like mesquites and things like that. One of their chiefs had died, and this pot had been placed where he had been known to frequent, and it was for him.

But Jim picked it up and handled it. I said, "Jim, that's yours." He liked that, and in later years I was at the study, and that pot's still there. But the feeling of satisfaction with the experience was enhanced by that little incident. Not me.

So as in all complex enterprises, you need the breaks to keep the human interest up. I'm sure that that contributed. I don't think Jim ever mentioned that. Jim wrote a book in collaboration. I have a copy of that book. I don't remember his ever saying anything about that. But those were the breaks.

By the time we were through with that, the crews remembered. John Young and Charlie Duke were ultimately the 16 crew, and they thought this was good stuff, and so we carried it on.

From then on, I was working not by myself; I was working with the U.S. Geological Survey, which had the NASA contract for leading the surface investigations. These were all the guys who have been trained by Gene Shoemaker. Without any question, he was the leader. But there were some very important people there, many of whom I had known before and with whom I could work very easily and readily.

So from then on, it got to be a bigger operation, and the bigness of the operation was that the people who were going into the field with us, of course, to several different spots were people who would be working in the science back room. They got to know the voice. They got to know the style. They had heard the preparations and all the other things. [Next] trips out, we used radios, and after that we got into a quite a formal communication system. There was a very important person in that. There was crew member who would be the capsule communicator, a CapCom [capsule communicator], and that was Tony England, Anthony [W.] England, who was a scientist-astronaut. Interestingly, he was a geophysicist. He did a good job.

So we did probably four or five exercises with 13, and it had gone well enough that the crew wanted me down there for the launch. I was down there and saw the Apollo 13 launch, and I left the launch site and headed for the airport and got ready to fly to Houston, and then the word came that "Houston, we have a problem." I went to Houston, but it was clear I couldn't do any good, so I came back to Pasadena.

One thing that maintained the thread of continuity for me in my role was that the crews were enthusiastic. They really regretted that they didn't get to land on the surface. And so there already had been a distinguished professor, who had been one of my professors, assigned to Apollo 14. I did a little work with Apollo 14. But Apollo 15 was beginning to start its training program, and that was Dave [David R.] Scott and Jim [James B.] Irwin. And Fred and Jim Lovell urged them to try me out. We had to go through this one week in the desert again. And they wanted to continue.

Now, this is of great interest to me. The reason it was of great interest, instead of having three or four exercises after that first meeting, we now had time for more than a dozen, actually, probably, for 15 and 16. And we did them in a variety of settings.

But you also have to remember the first missions were called "H" missions. The second missions were called "J" missions. The J missions were much better equipped. They were prepared for extended EVAs [extravehicular activities]. They had the vehicle. They had many other things, so the potential for tackling a variety of science problems was much greater. But that included then an increase in the level of complexity of the missions, and there something came along that had only been briefly touched on in the Apollo 13 experience: I started getting educated, and I can't tell you how important this is.

The number of things the astronauts are asked to do, and do always expecting to perform at better than expectation levels, was incredible. Every mission was supercharged with both requirements and expectations, and I really began to get educated in all the things the members of the mission had to do, and I couldn't expect them to work just to do my science. I had to keep in mind the original saw, "One, get them there and get them back," [okay,] first [problem], and then rank the priorities.

Now, NASA was learning, too. So they developed some new mechanisms. One of them included something called the Surface Working Panel. This was a team which included directors—flight directors, I should say—from the Flight Directorate. This included a variety of principal experimenters on the missions. It included a number of observers, and because I was involved in training, I was on the Surface Working Panel, and I was on the Surface Working Panel for the final three missions.

We had learned some things about Apollo 13 and then about Apollo 14. I only spent one day with the Apollo 14 team. Richard Henry Jahns, then a professor at Stanford [University, Stanford, California]—actually the dean of the College of, who had been one of my professors at Caltech—was responsible for them. So I didn't know them that well. They had a most complex mission: they resumed, or assumed, the mission that Apollo 13 had intended to do. They went to a place where it was expected that they would see material which [were] the deepest samples coming from the Moon, based on the limits of our understanding, because they were on the outer parts of the biggest crater seen on the Moon, and they were looking for ejecta where some giant object intersecting the Moon's orbit had dug out very deep materials. So it was very complex. We didn't know what to get, expect.

And that brings me to another point I should make here now. Every mission was different because we had retrieved samples which were being analyzed up to and through the time of the ongoing missions, and we were changing our understanding, that was usually growing—sometimes we screwed up—but it was usually growing, and we had cast a new light. And one of my jobs was, among others—I was the only guy doing it—was to, in fact, let them see where we were now and why their mission could make contributions. And that's what was relevant about the Apollo 14 mission.

There's not much more I'm going to say about Apollo 14, but one thing: the Apollo 14 mission was conducted with photographs that weren't very good, pre-flight photography taken by the previous missions, and the crews weren't properly prepared. Golf balls weren't the most important thing about Apollo 14. The first thing was to get the crews there and get them back, and the second thing was to learn as much as you could.

Now, all the missions have to make their own little boyish contribution, and I can't fault them for the golf ball, but the Apollo 14 crews did not have the right attitude, did not learn enough about their mission, had the burden of not having the best possible pre-flight photography, and they weren't ready. They got lost. They were never lost from our ability to know where they were. They were never lost in the sense that Apollo 11 was lost when it landed. You don't know that? But they couldn't tell where they were, and so they couldn't implement some aspects of their mission. That never happened again.

In the back room while Apollo 14 was going on, in the science back room, were two guys: myself and Dave Scott, the commander of the next upcoming mission. When we got through, Dave said, “Lee, that’s not going to happen on our mission.” That’s when Jim Lovell was introduced as the top man in the science back room. And you had a question: was his role important? It was important, most important symbolically because everybody in the science back room appreciated and respected Jim Lovell. But then beyond that, we [all] were doing a much better job. Now, when I say “we,” wasn’t me. I don’t know how many thousands of guys are out there doing a much better job: so that the photography was better, the orientation was better.

There were dozens of people who were briefing the crews. In fact, you have to recognize that at Apollo 15 level, these guys had so many tasks that were assigned to them, some of which were purely mechanical, some of which were pilot’s tasks, some of which were photography and documentation, which is part of the science, but mostly tasks in which they had to take advantage of the opportunities, listen to the discussions emanating via Jim Lovell, via the CapCom, and that CapCom was a guy by the name of Joe [Joseph P.] Allen. I can’t tell you how good he was. And we’d all trained together, and it did not happen again on 15, 16, or 17.

[Apollo] 16 had the advantage of two periods of training. They went to the original camp [for Apollo 13], and then we were together. But I didn’t do the whole training. At the end of Apollo 15, all the people who’d been involved in Apollo were now just starting to get tired, including me. .And I was willing to surrender what I was doing if anybody else want to come along, but several people wanted me to stay on, including Jack, who thought he might fly in the 17, and he did fly in the 17, so I stayed on, but we had to change people.

Then one of my old buddies, a fellow grad student, an old Caltech alumnus by the name of Bill [William R.] Muehlberger—you've met Bill and Sally—was appointed the PI [principal investigator]. If things had gone as they'd originally been planned, I probably wouldn't have been in any of this. Gene Shoemaker was the surface experimenter on Ranger. Ranger wasn't a very successful series. We lost the first seven I think. And then on the—I can't even remember the series name. As I told you, I'm getting old.

BUTLER: Surveyor?

SILVER: Surveyor, and there's one more, which was photographic.

BUTLER: Lunar Orbiter?

SILVER: No, it wasn't the Lunar Orbiter, but it'll come to us both after a while.

Gene would have been there, but Gene would have been tired, too. The intensity was incredible. I didn't give up teaching my classes. I didn't … give up analyzing lunar samples. Everything I did was added on. It had a profound effect on me, my personal life, and everything else. So I started to slip, not in the sense of not doing my best or not doing well enough, but I wanted other people to take over, and they did, and they were very good people, with Bill Muehlberger, a fantastically important guy by the name of Dale Jackson, and others. And talking about people who helped, I was sort of a member of the Geological Surface Investigations Team. That was my official title. I was being paid for my services by the U.S. Geological Survey, which had a contract with NASA.

There was, when Gene stepped down, a right-hand man by the name of Gordon [A.] Swann. I said that with Apollo 15 I had to start to learn about the real workings of a mission, and I had to put my efforts in that context. Well, Gordon Swann was one of my most important mentors. He is a geologist. I outrank him. I was outpaid. I was older. I'd been with the Survey long before he got into the Survey, but he had the responsibility for running the operation. He had the responsibility for making sure that the science back room would be ready.

Among the reasons that 14 had a negative impact on Dave Scott was the scientists were not well trained, and they were not particularly disciplined in recognizing the priorities in the mission, and they weren't disciplined in the sense that squabbles about what that sample meant or this or the other things were not going to help the crew, which had so little time on the lunar surface. There was just enough visible debate that that was included in what Dave Scott said, "This is not going to happen on our mission."

Among the many things that I got to learn was, the mission commander has immense clout. It isn't stipulated as immense clout. How could NASA contradict a mission commander's requests unless they were prepared to take him down, which never happened, right? So he had great clout, but at the same time he knew that in order for his crew to make the accomplishments that were required and, most importantly, to come back, he needed a successful operation.

Now, you have to know what they had to do in terms of continuous flight training, continuous work. You had to know about the discussions about contingencies, alternate options, all the other things. The key man on all of this was the mission commander. I can't say enough about the guys who flew.

Now, I'm not necessarily keeping in a continuous flow, but—

BUTLER: That's all right.

SILVER: So the mission commanders were training me. More than once—not Jim Lovell because we didn't have enough time together—more than once, David Scott barked at me. He said, "Do we really have to know this? Do we really have to do this?" He wasn't being contrary. He was continuously weighing the importance of what was being done, continuously weighing the time. Jim had a wife and two kids. Dave had a wife and some kids. Jim Irwin. I mean, you go down the list of all these guys. Just think about it, with the pressure that grows as you approach launch time and the whole thing. Incredible experiences, and I was obviously peripheral but not quite peripheral, because I could make some contribution. I learned that one of my most important jobs was to keep them happy with the goal, the science goals that we were going to do.

Now, did I create those science goals? Heck, no. I worked on four or five other committees besides the Surface Panel [Lunar Surface Geology Experiment Team]. I worked on a team called a Traverse Planning Panel [Lunar Surface Traverse Planning Team]. Their mission wasn't just to go up and pick up samples. They had to deploy very sophisticated instrumentation. They had, if necessary, to make corrections or repairs if they were necessary. They had to make judgments in real time whether or not they could perform the experiment as originally planned, because no one could anticipate the details of the landing site. You had a gross view of the landing site; I don't care how good the pictures were. When you got there and you found it in real time, you had to adjust.

Man, it was a complex ball game, and added to the complexity was that we were operating out of the science back room, and we had the weight of Jim Lovell's sound, stable judgments, but Jim was asking us why we wanted this, why we wanted that, and all the other things. And we were in the science back room hoping to optimize the mission, but that wasn't the number one requirement. And there was the big mission control room, and if you've been there, you know that there's a place in the back where there's a row of seats, and in the row of seats are "worthies" that include Bob [Robert R.] Gilruth, Chris [Christopher C.] Kraft, Rocco Petrone, George [M.] Low. I haven't said enough about George Low, who I grew to know very well. George Low took over after the Apollo 1 fire. It was George Low who brought the whole thing back together again.

Now, I've gone through so many names, and they come back, and I can't remember when I want them in there. Chet [Chester M.] Lee, who worked with Rocco Petrone; [Jim] McDivitt, Brigadier General [James A.] McDivitt, who ran the Johnson Space Center Apollo Office.

I remember one thing that Dave and Jim Irwin and I, but particularly Dave and I, introduced, and that's Dave Scott on the Apollo 15 mission. We were going to land at the base of what are called the Hadley-Apennines. These are the highest mountains of the Moon. That's a place now we can have called the Mountains of the Moon, but they're nothing compared to the Hadley-Apennines.

We were to land on the plain. One of the things that I had used in the training approach which the crews all seemed to appreciate was, first, stop and look. Don't jump in and start doing the things. Look around. Because Dave knew the capabilities of the landing craft, and Dave said, "Lee, what do you think if, before we get out and do the first EVA, if we open the hatch at the top of the landing module, look out, take a good long view, and take a camera and shoot the big picture before we start focusing in the details." This is a splendid, splendid suggestion, and as far as I'm concerned, indirectly, one of my achievements, because that had come out of all the things we'd been doing in there. But I'll take nothing away; it was Dave's suggestion.

However, we talked about the camera. We wanted a lens with a longer focal length, because we're now taking pictures way off in the distance. We wanted to make a picture of the higher parts of the Hadley-Apennines, which go up, just so that you can understand, 75,000 feet on the lunar surface. This is an incredible thing. So we decided we'd ask for one of those, and it went in on the routine request from the troop. But on a mission where weight was absolutely critical, the weight of another lens was considered critical, and, remember, weight could lead to the violation of the number one requirement: get them back. So he was turned down.

Well, by that part of the Apollo mission, I had a pretty good standing, so it was agreed that I'd go in and see General McDivitt. I went in to see General McDivitt, who'd had a distinguished career as an astronaut. He said to me, in sum—this is not a direct quote—"Lee, you have your mission and I have mine. Mine is to get the crews back."

And I said, "This is a marginal request. If the total planning depends on the weight of one lens, something hasn't been done right." Of course, that's already been said by the commander, who knew as much or more about the flight requirements, about the anticipated performance of the landing module, of all these other things. But the general had to do it by his likes.

I said to him, "Okay, Jim, you're running the safest and best trucking operation, lunar trucking operation, but you're not contributing to the accomplishment of it."

Well, he wasn't very happy with that, and I wasn't very happy with what I said. But in the end we got to talk to Rocco Petrone; we got the lens. And Dave got up and did a superb thing, and his pictures of the Apennines, it was the one place you could get [grand vistas]. We got something like that in Apollo 17, but it was great, and he did just exactly what he had to do. And I didn't have to coach him on that; he knew what he had to do and what he had to say, and it was just super.

I'm using this episode to sort of illustrate what I'll call the tensions that were in there and the things I had to learn, and I never would have said that to McDivitt if I'd known that he wouldn't appreciate what I was trying to do, and I kind of think he expected me to push for the best of the science, just as he was pushing for getting things back. I can't tell you how many times that sort of thing happened all through all the missions.

Even though I gave up leading all the field trips, I led some right through to the end. I was included, and I got to be on more damned committees. I was working well, working well in the sense—this happened very early on—two [exercises]. One, set up, with the aid of Gene Shoemaker because he's been very responsible for it, the Nevada test site for atomic explosions, where we were studying craters, and the other was to a place on Navajo reservation called Buell Park.

On both of those missions, and there's lots of good things about those missions, but on both those missions, several key members in the Flight Controllers Directorate, like Zeke, like Gerry [Gerald D.] Griffin, like Glynn [S.] Lunney, came along, wanted to know what I was doing with these guys and whether or not they could understand how it would relate to the mission. And that was extremely important.

They had to know what the science teams were trying to do, and they listened and they commented. They never commented to me; they commented to the crew commander, because that's where the power and the action was. But we hit it off in both those cases, and so when things came out of the science back room, he would know I wasn't top-dog [in] the science back room. They knew that I was there, and they got to meet the members of the little back rooms we were operating. I even ran a mission, an exercise in the California desert. [Addressing his wife] What's the naval station up there by Ridgecrest?

ARLANA SILVER: China Lake?

SILVER: China Lake. And for that one Rocco Petrone came from Washington [D.C.]. Chet Lee came from Washington, you understand, and they all wanted to see what the hell we were doing. And we had a full-out communication system with the CapCom working, and we worked it as if we were going to be in the science back room. And by doing it this way, by the time we got to the actual operations, we got closer and closer to reality. Now, did I design all that? No, I was part of the designing group. Gordon Swann was a critical man in that; he was a PI, you understand. But maybe the thing you have to take out of this is, we were training each other and sharpening our ability to do the job. [Tape recorder turned off.]

SILVER: —very good. He ran a research lab and that sort of stuff. But it wasn't until as old geezers we were comparing notes that I began to appreciate him. He was another very talented guy, and I met so many talented guys, and you never dismissed anything that anybody said, because you always knew that they were good and there was something there that you had to appreciate before you made your own decision. And I wasn't the decision-maker. What I said a moment ago was, as the program moved along and people got to know me and we had our series of what I call progressive accomplishments, successes, and what have you, I felt my influence was greater, and it helped me when I wanted to use it. But one of the things I learned was, you just can't throw science objectives around because there are so many objectives that had to be met in the whole thing. Maybe that's why I felt my influence was probably just saying I think they knew that I was beginning to understand all those things in there.

I want to pick up. I want to pick up talking about Apollo 15, which many people thought—that was the first of the J missions. They had the rovers and they had three EVA stays. They had far better photography, photogrametry in preparation. See, in hiring the U.S. Geological Survey, you normally hired a few people. The Survey was the finest photogrametric operation around short of the defense intelligence agencies, things like that. And I don't know what they were doing. They probably were involved, too, but we were getting first-rate assistance, things that you just can't dismiss. You needed all the tools you could get to prepare for the missions.

When I'm talking Apollo 15, I'm going to come back and talk about—I was thinking about things I haven't talked about—I've talked about science objectives, their successes and their failures, and I want to talk about human performance, and I've already started talking about the complexity of the missions, and I want to be sure that's not overlooked. I want to name some names of people who brought their special talents to bear, some of whom are now gone, long gone, thirty years, you know, passing scene and all that other stuff.

I want to talk about the romantic view of Apollo, and I'll talk about what the best view of Apollo would be. I'm allowed to do this, I think, in this thing.

BUTLER: Oh, absolutely.

SILVER: Okay.

BUTLER: Absolutely, and I've jotted those down so that we can make sure to come back to them all.

SILVER: Working with Apollo 15 was a real challenge, in retrospect, just a gem. Dave Scott. Dave Scott was already a space hero. Dave had had to do some very difficult things in the Gemini Program. Do you know about the Gemini Program?

BUTLER: Yes.

SILVER: You know that there was an experiment in which Dave Scott and Neil Armstrong, in one of their missions [Gemini VIII], one of their exercises, got into a Gemini system and, through a glitch, the control jets started firing, and they started spinning. How those two guys managed to overcome the fact that their little tiny ship began rotating faster and faster and faster and to come out of that is a miracle that I don't understand, and you'll have to get that from those guys. I hope somebody will ask them how they managed to do that.

But I take note of the fact that Neil Armstrong was the first, and then if you follow Neil Armstrong getting ready for the first—this is not Apollo 15, I know—and you saw the flying bedstead [Lunar Landing Research Vehicle (LLRV)], and you know the failure that occurred just six weeks before the mission launched, and then if you saw the actual mission, when they were starting to descend, and the landing module pilot, a famous guy, second man on the surface of the Moon, Buzz Aldrin was calling out the descent, and Neil was looking out this terribly small, I'd say a lousy port, and he could see down there were big boulders, and he knew this wasn't the [landing site]. So he took manual control and tried to fly the ship, the landing module, into a more suitable landing point. And down in the control room, they knew; they were watching the fuel. Forty seconds, thirty seconds, twenty seconds, and Aldrin was calling out, “We have dust,” and finally they’re on the ground. Whew. Very special. You can talk about all your automated remote-controlled systems. Took a man. [Silver expresses emotion] I’m an old man.

BUTLER: No, it certainly was a very unique moment.

SILVER: But there were so many of them. [Emotional]

BUTLER: Were you watching the Apollo 11 landing?

SILVER: Yes, in my bathrobe. [Laughter] Just like everybody else.

Later on, when if I come back to one of the topics I mentioned earlier, I want to talk about unmanned versus manned and about silly aspects of the controversy.

But I have to say that picking the quality of the guys was extremely important there, and then I’ll come back to Apollo 15 and say Dave Scott had already proved that he’s one of those special guys. And Dave, his [father] had been a distinguished member of the military, and he had the military career picked out for himself, only he was Air Force, and he had a buddy, Jim Irwin.

Everything Jim did was understated. He’d seen military service. He’d crashed. He was a human wreck. He rebuilt himself physically, requalified for flight, got his appointment. When we were doing our training, he paid intense attention. He always deferred to Dave, but that didn't mean he stayed quiet all the time. But he'd talk to Dave; he didn't talk to me. I had trouble pulling it out of him, because he felt it was the commander's role. Well, in every [situation], he was extremely good. So in having Dave and Jim, there was a hell of a team. They also had a first-rate backup team. They had as a backup Dick [Richard F.] Gordon, who was backup commander, and Dick Gordon, lo and behold, had a landing module pilot who was a scientist, Jack Schmitt, and they were a hell of a good team. Now, Dick had already flown on Apollo. He's been a command module pilot, and I think he was either on 10 or 12; 12, I believe it was. Well, you can correct that. And these two guys were as dedicated to the Apollo 15 mission as were the lead commander and their guy. I think one element of what I considered to be a great, successful mission was, in fact, that there was an unspoken competition, because on every exercise we did, I had to debrief each of the crews separately, and each crew would listen to what I said about the other crew, and that was good,

So the quality of the people was just outstanding, just outstanding. And Jack was learning all the way through on that, and I'm sure that Dave Scott's remarks on Jack's performance as the landing module pilot was an influential contribution to the decision, which was ultimately, I believe, made by George Low, to put Jack on the 17 mission, and that itself had so many repercussions. I have to think about that. So I had the very best kind of people to work with. The job always was to create useful, exciting exercises whose relevance could be [grasped], and always I depended on somebody providing additional felicitous circumstances.

Have I talked to you about Jim Lovell and the pop? That's not the only one. One I remember very well. Apollo 15's mission included landing a special kind of a lunar feature called a rille. In this case it was the Hadley Rille. Now, what do you do with a rille? Think about that. The rille is a winding depression, linear, serpentine in shape, with walls that gave you windows into the underlying rocks, which is always one of the most difficult things we had. The reason those windows were better than craters themselves, which also gave you samples of underlying rock, is because the [walls] were ordered, whereas everything on the rim of a crater was thrown up by an explosive impact, which was, if anything, the height of disorder.

Actually, that's not completely true, because Gene Shoemaker, among his many contributions, had shown that, in fact, the debris is thrown out as inverted flaps, which come out like this [Silver gestures] and then fall back down, and if you knew how to read them, which is what geology is supposed to be about, you could, in fact, get some idea [unclear]. Gene did that by his work, both at Coon Butte, which was Meteor Crater, and also from studying the vented nuclear test sites at the craters formed from the venting at nuclear test sites in Nevada.

But nevertheless, there was none of the disordering phenomenon of an explosion involved, we thought, and it's probably still true. So getting to go to the Hadley's Rille required finding an analog. Well, again, felicitous circumstances. We probably had the best analog in the world in northern New Mexico. It's in the Taos area, and it's called the gorge of the Rio Grande, and about five or six million years ago, there was a tremendous set of basalt flow outpourings into a depression, through which the river has cut a gorge, just about the same width and the same depth as Hadley Rille. And I remember suggesting that to Bill Muehlberger, who had done a lot of important work in the area, and he said, "Jeez, why didn't I think about that?" That was one for me. Well, finding that was extraordinarily important and felicitous, but that isn't what was felicitous.

We arranged to stay at a place called the Sagebrush Inn. I'd only driven by it. [Addressing his wife] I've pointed it out to you, but we've never stayed there ourselves. At the Sagebrush Inn, we got a suite of rooms, old adobe rooms. Everybody had their own rooms, adobe walls, very attractive place. And when we came back, it was great. But the greatest part of it was in the morning. The cook for us—we were there for three days—the cook there was a great northern New Mexico-ish cook. She was just thrilled to have the astronauts there. So she did her best all the way through, and in the morning she would feed us something that the crews just loved. It was—[Addressing his wife] what was the stuff that Cathy made for us at home? What would we call that dish that Cathy made for brunch the last time we had it? Your sister—

ARLANA SILVER: I know. I'm trying to think of what she—

SILVER: Chiles, eggs, tortillas—what would you call that? Casserole?

ARLANA SILVER: It was a casserole.

SILVER: It was a casserole. She'd come in with these giant baking dishes filled with fresh green chile, eggs, tortillas, and the crews just loved that stuff. So did I. Arlana is a fan, too, of northern New Mexico cooking.

Talk about felicitous stuff. Did I know that the cook was going to be that captured and make things work? No, I didn't know it. But we had picked a good place. We knew it was pretty good. So, anyway, it wasn't that hard to please them.

For the first time in an extended way, in the exercise along the rim, we're using a special vehicle, which was called the "Grover," and the Grover was the U.S. Geological Survey's preparation for the lunar rover, which had yet to be driven on the lunar surface. It was going to go up with them. The Grover worked like a charm, and it was electrical. It ran well. It didn't begin to have the capacity, because it was operating in one G instead of in one-sixth G. It didn't begin to have the capacity. But it was stupendous.

The weather there was stupendous, and so the photography that the crew took—and I had my own camera with me, and the pictures I've got—were just stupendous. We had these two crews working along this straight gorge, about 800 feet deep, about [1,200] or [1,400] feet across, and the river down below. They didn't have that river on the Moon, but we had everything else. We had the individual lava flows. We had interflow sediments. The stuff wasn't covered with the debris that three and a half billion years had produced on the rille, you understand, but, nevertheless, you could see all kinds of things, and you could develop the logic. We are seeing a stratigraphy, and we could determine what was older and what was younger.

We didn't know what we'd see when we got to the Moon, but we hoped we could do some of that. We didn't do as much of that as we'd liked, but from my point of view, the crew could see it. They knew what they might be able to see. They knew what to look for, do you understand? I didn't have to coach them on what you're going to do there at the rille, and we didn't know for sure, because we knew that the Moon was covered with debris layers which had accumulated for billions and billions of years, or as that guy used to say, "[Bullions] and [bullions] of years." You know who that guy was?

BUTLER: Carl Sagan?

SILVER: Carl Sagan. Carl Sagan wasn't a great fan of Apollo, but that's just an insertion.

So it was on missions like this that the crew would understood enough, I didn't have to point out what was relevant every time, and it was a damn good thing. One night after a hell of a good dinner, we all convened in my room for a debriefing on the day's work. The word "debrief" is so important in Apollo preparation and what have you. You have to go through what you've done, and you have to know what you did well, and you have to know what you did poorly, and you have to know what you would work on to make it better. And there was debriefing by me, from a science point of view, and there was also debriefing by the commander, or any other crew members who felt that something was falling short. And there was debriefing that the guys who operated in our little remote back rooms, the guys listening at the radios and all the other stuff, they'd comment, too.

Joe Allen was a critical man in this. I can't tell you how important the CapComs were, but they weren't CapComs. They had another title that I can't come up with right at the moment [support crew]. They weren't training officers. They weren't any of that. They were just younger astronauts who were lending their know-how to the preparation, organization, critique, and the whole thing was there, and they worked directly for the commander in there. And there was [more than] one for every mission.

So we'd had this wonderful dinner—I'm trying to get back to my story again—and we started to debrief, and we went through it and we went through it, and it had been a long day. We operated at 7,000 feet—that's the elevation of the Taos Plateau in there. These guys were in the Grover. In some cases, I had to run along behind them. I got kind of tired. And I remember that this one night—Arlana will appreciate this—I started talking, debriefing them, and then I stopped. You know how you'd talk and you were waiting to hear the next part of the sentence? They were all waiting. I'd fallen asleep. [Laughter] Oh, they forgave me. It was the kind of group we had and the kind of working relationship. It worked out.

BUTLER: It certainly did seem to. They brought back a lot of good science from—

SILVER: They brought a lot of good science. But that's because they got into the science. There's no way we could have told them what to do. There were so many things they did that were special. In a little foothill to the Hadley-Apennine, there's a crater. The people who did the preliminary geological analysis—there was always a preliminary geology analysis. I'll come back to that when I hit one of those topics I mentioned earlier. There's a crater called St. George's Crater, and we were going to go to the St. George's Crater because the rim of craters is where you get coarse debris, bigger samples that you can do certain things with that you can't do with the fine dust or what we called the regolith on the Moon.

These guys, there was an experiment for a group of people who were interested in what the solar wind and cosmic rays were doing to the surface of materials on the unshielded Moon. On Earth we're shielded by atmosphere, by magnetic fields, and other things. So what is the nature of this stuff? We described a potential experiment for them. We got two rocks that were shielded on two sides, collect, see if you can get a photo down below, see if we could get some of the walls of the rocks that are partially shielding them.

They did better than that. They found a rock about that high [Silver gestures]. They rolled it over and brought up the shielded side. They sampled the unshielded side. They sampled the shielded side. They sampled the soil underneath it. You understand, they revised the experiment and did it better.

I will say this very carefully, but one of our important scientists was a man by the name of Dale Jackson, an old hand at the U.S. Geological Survey, and Dale said, "Well, they found that baby, and they, and they, and they," in typical male fashion, do you understand? They seduced it, they interacted with it, and they did every else, by God, and they got everything they could out of it for free, and that's exactly what they did. Man, did that make me feel good. I hadn't taught them that. They were smart enough to do it. And that went through all the experiments.

This is anecdotal, but it's in the records somewhere. One of the things was to get as high up on the flank of Hadley [Delta] as they could get. Remember, this is the first mission in the lunar world with a vehicle [Lunar Rover]. Nobody knew exactly how well that vehicle would perform. And they drove along, and they found, much to their dismay—we'd asked them to pick up lots of big angular fragment samples. On the way, all of a sudden, there was an uncalled-for stop. Dave worried, said something, muttered something about "I'm fixing the seatbelt." Bald-face lie. They'd seen a big rock, thereafter known as the "seatbelt rock," which they stopped to pick up because it was interfering with the time lines in their mission. In the end, that would redound on the accomplishments they'd hope for. Well, they got the piece of rock.

But then they started up the slopes, and they wanted to get to a place where they could get debris which had been thrown up on the slope from the great impact event represented by the Imbrium Impact. Okay, kept going up, but it got steeper and steeper. Finally, off in the distance, they saw a rock, and they got up to the rock. They couldn't quite get up to the rock; it was too steep. And they got out of the car, and Dave got out first, and then Jim started to get out, and as soon as Jim started to take his weight off the vehicle, it began to slide down the hill. First-time experiences in an alien environment.

So Jim had to stand there for a while until he felt that he could get the darn thing set and not get away. They went over to the rock, and the rock, it was about that high [Silver gestures], and it was on a slope gentle, about like that. But you could stand on the lower side and look at the top. You could stand on the upper side and look at the top, and Jim looked at that thing, and he said, "Gee, that looks kind of green."

Now, prior to that time, color was never used in describing, because the Moon was in shades of gray from white to black. And Dave pushed his—he has a visor on the helmet to cut down the sun glare and all the other stuff. "Yeah, it does. We'd better get some of that." So he got it. That's the first time, and green was just the first color that subsequently became very important in the science objectives. But they responded, and they collected it, and it was beautiful. It turned out that this was made up of zillions—you know what a zillion is; ten to the x.—zillion beads of green glass. And nobody told them to look for green glass, beads, or anything like that. It'll come back. When we get to Apollo 17, I'll come back to that green glass. See, I've got a lot to tell you about in there.

They did a beautiful job of sampling, and that was the highest sample they collected on the Apennine. But down below there was this spot where they were wandering out, a little further down, and on a different EVA—no, it was the same EVA—and they were walking along, and Dave reached out with this device we had created where he could grab onto a rock and bring it up, and he brought it up. And Dave said, "Gee, look what I've got."

And Jim said, "That's it." What they had found was a rock made up mostly of the mineral called feldspar, and geologists have a special name for that, and it's called anorthosite. Now, if you look at the Moon, either in high-quality photography or when you've got a full Moon, you'll see darker parts and lighter parts. Well, they've already identified that the darker parts, which are called mare, are made up of lunar basaltic lavas. Fifteen was one of the beginnings of our beginning to understand the uplands. We were not keen to do many trips to the uplands, but in Apollo 11, people sorting through the finer fragments, fragments that were one millimeter to one centimeter in size, like that, in that area right there, they began to see little bits and pieces of what looked like rocks which were called anorthosite, and here it was a piece that was that big [Silver gestures]. It wasn't a little teeny piece; it was a big hand specimen.

And because we'd learned from 11 and had built it into the experiments—I had taken them to a place in the San Gabriel Mountains behind Pasadena where we have anorthosite, and they'd looked at it, and we had discussed the fact that the anorthosite might have been the most primitive or primordial rocks on the lunar surface. That was simplistic in the end. But when they picked this up, they said, "That's it," and they described it a little bit, and in the back room we knew that they had picked up a piece of what looked like anorthosite.

All of this is unsorted material lying around on the lunar surface one way or another, you understand. But the press heard that. In the debriefing between EVAs, they said, "What was that rock they found when one of them said, 'That's it'?"

And whoever was talking from the back room said, "It sounded like they found a piece of anorthosite."

"Anorthosite. What's that?"

"Well, it might well be very old stuff."

So the press created a name. They called it "Genesis Rock." That didn't come out of the science room at all; it was the press created it. But Genesis Rock, regardless of the press creation, turned out to be one of the most primitive samples, possibly the most primitive sample, we had found, we found on the Moon. Well, that's kind of interesting, because Apollo 16 then went on the visit a place where we would have had acres and acres of that stuff.

And here's a loose piece. Remember, we're on the rim of the biggest crater on the Moon, Imbrium Crater, which was subsequently flooded with basalts. So we have the Imbrian Mare, and here is this great mountain range thrown up during that impact. It was thrown up to heights that we can't match on Earth. And how knows where that piece came from? Maybe it didn't come from there at all. Maybe some other impact threw another piece, because the Moon's surface has been impacted again and again and again, and things have been redistributed over and over again. But it was an exciting piece, and again, they knew what an anorthosite looked like. And completely aside from its ultimate significance, it might not have given any answer that was very special to the work I was doing and other people were doing. It took, and it was operative, and that's what they were thinking about. So [we] couldn't help but feel good about it.

The Apollo 15 crew got to the edge of the rille, there, again, under the same constraints that Neil Armstrong had been under. You've got to watch yourself. Do not get on slopes which might slide or give away on you. They'd already run into that problem when they'd gone up on the flank of Hadley Delta, and their vehicle was sliding down. Suppose they'd lost their vehicle. Suppose it had slid. Well, the rille was a place that was much steeper, and at best all they could do was get to the rim and maybe just over the crown of the rim. They didn't take the vehicle over the crown. That's [parked] clearly on the crown, and there are some wonderful pictures, were some of the most dramatic pictures, pictures of the Moon there.

But they started to walk over because they could see rocks, and they weren't sure. But to the time that we were into Apollo 15 and probably throughout the entire string of Apollo missions, we had never directly sampled a rock that was still in place, what we would call bedrock. And these guys knew that one possibility was that they would find that in the rille. And they went over, and they sampled, and they took their pictures. There was a very important protocol, which every crew had to develop a discipline in, what you did to get a sample that you could use then for various kinds of science. They did it, and we weren't sure. But the pictures we took of the opposite side of the rille with that camera and lens, that Mr. McDivitt showed us that. In fact, there were a series of flows exposed there, just as they were in the canyon of the Rio Grande, the gorge on the Rio Grande. And we think there's a very good chance that—they took several samples—that some of the samples they got were probably, that's all we can say, probably akin to what we could see in the opposite side of the canyon. Oh, we would have liked to done more on that. But that was very, very important to us.

So when it was all over, there were several guys around—I won't name them; I've been asked many times to name them—who have been strong critics of the astronaut training program. Some of them were really the parents of lunar science. Well, I'll name two guys. One was a Nobel laureate by the name of Harold [C.] Urey. Harold Urey was a man who said, "Give me one sample of the Moon; I'll tell you the history of the solar system." That's a paraphrase. Geologists were rather skeptical. "We knew you'd learn important things." We still don't know the history of the solar system, with a lot of samples.

The other guy was a guy who'd been in conflict with the geologists, especially with Gene Shoemaker, right from the word "go." He wasn't a Nobel laureate, but he was a physicist of international reputation. His name was Tommy [Thomas] Gold. Tommy Gold was convinced that the Moon's surface was covered with dust and nothing else, whereas Gene had taken pictures from the Surveyor and the other thing, he could see rocks. He had some outstanding pictures of rocks. This is what Gene was saying: "It's not all dust. We're going to see rocks and dust." Now I'm going to insert something gratuitous. It was about—don't quote me exactly, January 4th or 5th, 1970. Apollo 11 had landed. Apollo 12 had landed. The Apollo 11 rocks, which had been so carefully collected by Neil, had been distributed. There are an enormous host of geologists at the Houston Conference Center, which is named after a congressman. What congressman is it named after? Do you know?

BUTLER: George R. Brown?

SILVER: No. George Brown was—

REBECCA WRIGHT: You mean Albert Thomas?

SILVER: No. One more. Maybe it's Thomas—

REBECCAWRIGHT: He was the congressman during the time of the NASA—

SILVER: Well, those were some great guys. Don't forget Congressman [Olin E.] Teague.

REBECCA WRIGHT: That's true.

SILVER: "Tiger" Teague was a very important congressman. But I can't remember for sure.

REBECCA WRIGHT: Sam [Samuel T.] Rayburn was—

SILVER: Sam Rayburn, but it wasn't named for Sam. I'd know that. I'm older than you guys. I knew more about Rayburn than you guys. But, anyway. And it was a most peculiar time. The world's press was there. The scientists are going to tell us what they've found from Apollo, from this incredible expenditure of funds, from these highly publicized things, astronauts who were pictured on *Life* magazine covers from A to Z and back again over and over. And we taught them a lot.

But standing there, and I'm talking to Harold Urey, and Tommy Gold bustles up and says, "Harold, what are we going to do? Listen to what these geologists are saying. They're talking about rocks, and there's only dust." And Harold had already heard the results from *n* samples, not one, and he still couldn't tell about the history of the solar system. He wasn't very happy, either, and they both agreed that they had to stifle these damn geologists. That's a first-person report. I was there.

That's all right. Harold Urey, his role was so important, you understand, in encouraging the science for Apollo, that I have to be forever grateful to him. But Tommy Gold, I didn't like for many other reasons, because he could not see the truth. And I don't have tell you, seeing the truth is only as good as your eyes are and as good as your brains are, and nobody has absolute control on the truth. But there's usually, amongst intelligent people with good eyesight and a good brain, a convergence. Tommy Gold had his blinders on because he'd already publicly stated his positions, and he could not converge. But after Apollo 11 he dropped out. By the time we got to Apollo 15, we were able to see rocks and bring home samples of rocks. We had done this at 12, we had done this at 14, and we would have done it with 13 if it had just made it.

So Apollo 15 sort of, if I can use the phrase, it was apotheosis of all the things we'd been planning to do, and we did so many other outstanding things in subsequent mission. But it was the coming together of developing the technical capabilities, preparing men to be explorers, as well as many, many other things, and then, I'm going to say, felicitous things. How did we know that they'd find Genesis Rock? How did they know they'd find that green stuff? How did we know that that boulder would be sitting there in a such a position that they could do all these things? Well, that's because the human intention, well educated, well prepared, can squeeze things out, you understand? They can extract them.

I'll come back to that again. I said that I would talk about unmanned, manned efforts. There is no robotic system, even now, thirty years later, that can do what these men were doing. But, of course, their presence represented a fantastic amount of money and investment. So it's the tradeoffs that you have to consider all around. So I'll put it this way, and I'll push this topic aside. We need all the unmanned exploration we can do. We need it done by the most capable and sensitive instruments going. But to get down to the last analysis, it's the man, and it's the man who knows what he's there for, who is driven to do it as best he can, who has the motivation, the talent, the intelligence, the education to do it, who'll get us the most per unit of time—I won't say per dollar. And I was really moved just a little while ago. Maybe you saw it, too.

Recently Shuttle astronauts replaced the lenses on the Hubble Telescope, and then all around the country they published a series of pictures, which are indescribable. Now, the camera had been built by man, is operated remotely out of Goddard Space [Flight] Center in the Shuttle Building in [Greenbelt,] Maryland. But the ability to come back and know what's wrong and to do it better is an incredible ability, and so I just have to say you need both, and you cannot do it better than a well-educated, well-trained man or woman could do. So, get that off my chest, get it said, get it behind me.

At the end of Apollo 15, I had an extraordinary personal experience. I don't know how it was set up. It's possible that George Abbey did this. I don't know. But I was working in the science back room. I got a call to come into the control room. The crew of Apollo 15 was calling me. I don't think any scientist got one of those calls. It was pretty special.

BUTLER: A testament to what your work with them had meant to them.

SILVER: Yes, I think so. But, whew. Special.

BUTLER: Very special and a well-deserved recognition.

SILVER: Well, you're entitled to your opinion. But for me it was just overwhelming.

Well, we went on and we did several other missions, and I want to talk about Apollo 16. First of all, I had the utmost respect for John Young, and I still do. [Addressing his wife] I'm told we're going to see him tonight, honey, are we? Yes, John Young and Susan. John had been there as a backup commander to Apollo 13. Subsequently John proved himself over and over again, as only Jim Lovell had flown more than John had. John comes on as a "cracker," a country boy. He, I think, knows more about lunar science than any other astronaut ever did, because he loved it and he dug into it and he attends more than—no, other astronaut does—he attends every Lunar Sciences Conference. We've had them every year since the first one. And they were given a mission which was a follow-on to Apollo 15, which was natural.

The selection of various sites was always hotly debated. There were advocates for different sites, and they would debate with each other. But Apollo 16 was going to the highlands, okay, where all those light-colored, even white, rocks were. So, Genesis Rock, we were going to overwhelm it with other samples. And the site that was selected was selected from a number of sites based on the fact that the U.S. Geological Survey had done a lot of preliminary photogeology on the site, and there were a series of extant models about how various kinds of lunar features, especially based on morphology, had formed.

The idea was that we'd see something which we see a lot of on the Earth now, and these are great outpourings of either direct lava flows or of gaseous-driven welded [tuffs] or ashes or things like that, and these things were inferred to be at the site to which Apollo 16 was directed. And then we'd also see some of the underlying stuff. There were some fantastic craters there, very young. The younger a crater is, the better it is for us because you don't see the debris excavated by [the] crater next to it, the continuing mixing of debris that goes on over the whole lunar surface through the history of the solar system.

So they did everything they were supposed to do. Another test of the rover. Rougher terrain. There are some TV pictures of the rover—no, there aren't TV pictures; they were handled [here]—of the rover spinning its wheels and doing other things. But they got to where they were never lost, and their environment they had far fewer distinctive features than 14 did.

But then I have to make some excuses for 14. I'm not doing justice to it. That's another thing. [Apollo] 14, they didn't have a rover. They had the oldest man in the astronaut corps pulling a little rickshaw, which carried all the equipment and the sample bags and the other stuff and up slopes that were steeper and over distances that were longer, so it was physically challenging. So if I said they got lost, it wasn't as if they weren't working hard, but they had not acquired that little bit extra.

If that goes into the archives, you know, I would never want to say negative things about any astronaut who went to the Moon. I wouldn't want to say anything negative about Al [Alan B.] Shepard. But I have to call the quality of the performance was something less, and it wasn't happy for me to learn that Ed [Edgar D.] Mitchell was trying to conduct psychotelepathy experiments instead of working on what he was supposed to be working on. I don't know all the circumstances. I'm a prejudiced person in that.

It's a toughie, but I think it's going to be a matter of record. Some crews prepared themselves better, and some crews, I think, saw the mission as their triumph, rather than their mission as an Apollo triumph, and that was not what I was most interested in. That'll come onto us because we can't separate human attitude and approaches from this end. There may be a dozen of guys who can refute the basis for my remarks, and I'm willing to listen, because I admire anybody who's willing to try this thing. But that was my biggest disappointment in there.

So I've told you about Apollo 15. I'm talking about 16 now. The models we had used for telling these guys, we now had better photography, we had geologists who were spending a lot of time preparing for this site, are not very good. Now I'm not criticizing the astronauts; I'm criticizing the geologists that had prepared for. To some extent, that includes me. We were not really appropriately evaluating the models that we were doing, and, of course, everyone would say, "Oh, we're going to that place to test models," or they'll say, "You go to that place, you've got to be sure that you've got a variety of models and a variety of experiments so that you get—" Certainly it's fine to test our models, test your big-picture models. We could find nothing to support most of the models we had there.

The rocks that came back were extremely valuable. Their collecting was impeccable. They had a tragedy. I think I'll talk about tragedies later on, tragedies in the scientific sense. Anyway, they did everything they were supposed to do. Didn't turn out the way we had hoped it would turn out. They did not fail us. The tragedy had to deal with an entirely different kind of experiment, and it did affect that experiment.

Well, let me talk about it. The tragedy had to do with the diversity of experiments that were carrying on, and the first-order questions were asked. What was the energy budget in the Moon? Was the Moon still hot at its core? How hot was it? The Moon must have formed by a variety of processes which were energetic and which must have a residual energy consideration. One way to do this is to take the Moon's temperature, and the more places where you take the Moon's temperature, the better off we are.

To take the Moon's temperature, you drill a hole. In effect, you insert a thermometer, but they're not; they're a series of thermistors which measure temperatures at various points in the hole. This gives us a thermal gradient. Then you assign a temperature to the rocks that were in that hole or penetrated by that hole, and you do that by measuring the thermal conductivity of the number of rocks that you think that might represent that. Then you calculate the heat flow, the rate at which heat is moving up that gradient to the surface. The surface is looking out at the black space; it's a black-body radiator. It doesn't stay hot at the surface; it just goes out. And then from that we can calculate how much the temperature might be, by projecting the depth of the hole from a meter or two or three down many kilometers, a bit of an extrapolation, and the assumption in the extrapolation is that what you saw in that two- or three-meter hole is what you have going all the way to the center of the Moon.

Ain't true. We know it's not true and all that other stuff. But you can do something with it, and the more of those you get, the more you know something about the current energetics of the Moon. I might say we also go out and we measure the heat production in those rocks, uranium, thorium, potassium, radioactive decays. We can do something with them.

Well, this was going to be a super experiment, because in the past we had not been able to make very many thermal measurements. There's only so much you can do. The drilling process is a challenge, and I'll come back to drilling in Apollo 15. So just as the experiment was set up, there was a multi-stranded, I'll call it a wire system, but it was a band of wires, which went from the detectors, the thermistors, in the hole, to an automatic recording device, which also is tied to the broadcast from what we called the ALSEP [Apollo Lunar Surface Experiments Package].

We were all in the back room, and we were watching John, who had drilled the hole, did a much better job at drilling than Apollo 15 did, this I'll tell you. He's stepping back, and we're all watching, and we saw John's foot rip the multi-stranded connector off the drill, the assembly of things in the drill hole.

Oh! You could see it coming. You're standing there. You know how important the experiment is. The camera's focused on it. Everything is there, and he tore it loose inadvertently—John, the most dedicated, hard-working guy. He knew faster than anybody else what he had done wrong.

So down here at JSC [Johnson Space Center, Houston, Texas], I don't know how many guys, hundred of guys, working on how could we repair this multi-stranded connector, and they worked on it. Fred Haise, you remember, from Apollo 13, was suited up to try to see if he could do this with the gloves on. Didn't work.

So that's a man-made disaster. On the other hand, if it had been an automated robotic thing—I could talk to you about those, too, but that's not the point. Well, that's a disaster. We did not get that experiment, and we don't have enough data on heat flow in the Moon, in the crust of the Moon. It's certainly something that we could try to do again. I'd rather do it with men, though, myself.

And so that brings me to another drill hole, the Apollo 15 drill hole. Do you remember the Apollo 15 drill hole?

BUTLER: I remember they had difficulties with it.

SILVER: They had immense difficulties with it. The funny thing was, the two strongest men to go to the Moon were Dave Scott and Jim Irwin, Dave because he was the biggest of the astronauts, tallest and a tremendous athlete, and Jim Irwin because after he'd had that terrible crash where he had to rebuild his body and requalify, he really built himself up physically.

So they drilled the hole, and the idea of this hole was to recover core from deeper in the regolith than in any place that we'd sampled up to that point. They tried to pull it out, and they had an extractor in case it jammed. Remember, when we drill holes on Earth, we usually use a lubricant, water, oil, what have you. There's no such thing available, and we never would have wanted to introduce a foreign medium on the Moon.

So they struggled and they struggled and they struggled. They finally gave up on it for that EVA—I'll give you my best memory, recollection—and they went on and did their other things, and they came back to it. And they pulled and pulled, and, finally, they broke it loose, and they got it. But the intensity of the exercise of getting that thing out of the hole was incredible. Now, these guys are suited up. They've got all kinds of sensors on them.

And after that was done, at some point, the biomed team was learning that they were showing some anomalies. Both of them showed some heart irregularities, and Dave Scott's subsided, but Jim's continued, and they were worried about him until we got him home. And, by god, he'd had a very significant exertion in there.

Well, I don't know that the rest follows from what I've just said, but Jim Irwin died before any other astronaut, and he died of a heart attack. Jim's interests after he got back, he'd felt that he'd had literally a out-of-this-world experience. He was a religious man, and he started a program called High Flight, in which he did only good work. He tried to convert me, and, of course, I listened. He's a wonderful guy. Did that heart experience carry through to his early demise? I don't know. Can't say a word.

I have to tell you one more story about Jim. By the time we did Apollo 15, several of us were privileged to be able to go into the crew [quarters] during their three-week isolation, pre-flight, so they didn't come down with measles or anything like that. And finally we finished, and we were still prepping them on considerations that are important. Jim [James W.] Head, now at Brown University was involved, and Gordon Swann was involved, and when we finally said our goodbyes and wished them *bon voyage* and all that other stuff, I said to Jim [Irwin], "Jim, may I ask a favor?"

He said, "What, Lee? Sure."

"Talk to me." This is the quiet man, you understand, and he did. He made observations, and he talked far more than he usually did, and that was, again, gratifying. Jim was a great man. [Tape recorder turned off.]

SILVER: I hope I remember all of the things I wanted to mention one way or another, but we'll go on.

Apollo 16 brought back fantastic samples. The thing was, that somehow we hadn't prepared ourselves—I'm talking about geologists now—for the fact that our models were off-base. And that happens in science so much. You know, things get to be taken for granted. We have what we call standard models. We start from the standard model, but way down at the bottom of the standard model is usually a flaw or two which may put the model and everything else based on the model upside down and backwards. Well, this wasn't quite a standard model, but it was highly influential and everything.

Apollo 17 and the selection of the crew for Apollo 17. I wasn't party to this [selection], but I did know that people from here and from Washington and a few scientists, which didn't include me, were very influential. While, even before 16, we knew that missions were being lopped off at the end of the Apollo sequence, there had been a tradition that a good backup crew became the prime crew two or three missions later, that they'd gone through the evolution of training and the understanding, and that's in all aspects, not just in the science aspects.

I remember when they cut off 21 and 22. I remember when they cut off 19 and 20, right down to 18. In the normal spirit of things, 17 would have been flown by the backup crew for Apollo 14, and 18 would have been flown by the backup crew to Apollo 15. The backup crew to Apollo 14 was Gene [Eugene A.] Cernan and Joe [H.] Engle, and the backup crew to Apollo 15 would have been Dick Gordon and Jack Schmitt.

But money for Apollo was running out, so they were being cut back. And in terms of the view of some of the policy makers, Apollo was now down to doing science. That didn't have the same Washington value as doing other things there. [All] completely subjective analysis.

And so it was decided to cut out 18. Who was going to fly on 18? Joe Engle and Gene Cernan. But what about Dick Gordon and Jack Schmitt? What about having a scientist on the Moon? There was a gimmick in some people's minds that had not been played before. And how good was Jack Schmitt? He'd become a very able pilot, flying jets. He was a recognized expert on the landing module. He had done everything he was asked to do. Jack was publicly assertive about many things, and for some of the astronauts he was probably too assertive. I'll say that quietly and gently. Not for me. Jack was super!

Then the Solomonian decision was made. Gene Cernan was an outstanding pilot. Dick Gordon was an outstanding pilot. Dick Gordon had already flown on Apollo. Gene Cernan had not flown on Apollo. Joe Engle was a competent landing module pilot. So was Jack, but Jack was a scientist, and he was developing an overview of the big picture of science. So the decision was made to break up the two teams and to pair up Gene Cernan and Jack.

That was interesting for many different reasons. First of all, it was heart-breaking for Dick Gordon, with whom I had worked for a long time, and who I thought would have been excellent, just outstanding. And for Joe Engle, who'd been working faithfully away at it, it was really disruptive. He hadn't flown, and he wouldn't fly. He did ultimately get to go into space, but it was a real heart-breaker.

From my point of view, we had paired up two people, one of whom I knew extremely well, and the other one I'd only seen briefly for one day in [that] Apollo 14 prep. I knew how highly regarded Gene Cernan was as a commander and pilot, but I didn't know him beyond that, and, as I told you, I didn't think that Alan Shepard set a very good example as the prime commander for Gene, the backup one, although Gene did not walk in and out casually the way Alan Shepard had. And that wasn't from my briefings; there were other briefings as well.

So I had to run an exercise just to find out. I took them down into the desert again. Jack had already been there. But down in the desert I had some other interesting problems. [NASA] Headquarters had to decide who would be the backup for 17. After all, guys who back up 17 would not fly. So they made another Solomonian decision. We'll use a crew that had flown: Dave Scott and Jim Irwin. It was a big mistake, because they had done their thing. They ran into some other difficulties you may or may not know about, difficulties for which I think they've taken too much of a beating, and I don't want to get into it. They had no anticipation that they would be asked to perform. That was a dumb thing to do. But should anything befall the mission, the prime crew for the mission, they had to have competent people to back them up. Oh, it's a toughie, right? There was some wisdom in there. I'm not faulting it. Thank god I didn't have anything to do with that. But I'm still talking about preparing now.

Now, right behind Pasadena are the San Gabriel Mountains, and I had them out in the desert. Out in the desert, Dave and Jim's interest wasn't as great as it had been. Not surprising. So that wasn't a very good opportunity. The first time I had to work alone with Gene and Jack was in the San Gabriel Mountains behind Pasadena.

After I got through with that exercise, I had the following impression. Gene was his own man. He was going to learn from Jack Schmitt, an experienced geologist, as much as he could. But he was a man of his own observations and his own opinions, and they were very good. When I got through, I had them do some similar things that gave me a basis for comparable comparisons, and Gene, at this stage, was quite good. I'm not going to compare him to Jack. Jack knew so much more than he did. But at a comparable stage to 13 or 15 or 16, even, he was good.

And from then on, through the whole training experience—and I did not run most of the exercises that they were on, but I ran several—I found that I could get value from Gene's comments, and I wasn't expecting Jack to be the voice of the team, and Gene Cernan as commander would never allow him to be the voice of the team, and I found him to be very, very good. So Gene did a first-rate job, from my point of view as a science trainer or whatever you want to call them.

And we now had an incredibly powerful team down here at the Center. People had through mission after mission after mission. They knew what they had to do. They knew the preparations, and they did super jobs. So when the mission finally came off, Gene and Jack did an outstanding job. They went to a very interesting place. Jack has a canned talk he gives, "The Valley of Taurus-Littrow," which is a very good talk, except he makes it sound canned. I wish the hell he wouldn't. Cut that. I've heard the talk eight times. But Gene did a good job.

I ran into a very interesting difficulty. Their voices have very similar timbres, and telling them apart was, "Who said that first?" And they saw interesting things. There was a station on their second EVA called Station 4, Shorty's Crater, Shorty Crater, the crater with the dark rim around it. We thought it might be something that we had anticipated on the Moon, but we had never found: a volcanic crater, as opposed to an impact crater. But it wasn't. It was a crater which had a very distinctive deposit that had been thrown out [of] the crater. And these guys drove up to the rim of Shorty's Crater—and now I'm gonna run into trouble. Somebody said, "Look down there. It's orange." I don't know whether that was Gene or Jack. I think Jack said it, but I'm not sure.

That got us terribly excited. Now, why? Well, you have to be geological to know this, and that is that in basaltic craters on Earth where water is present, the presence of steam coming up through the black rocks contributes to the oxidation of the iron in the rocks and turns them reddish-orange. Boy, was I excited. Finally we'd gotten to a place where we were defining some evidence of oxidation, some water. By the time we started Apollo 17, we had the impression the Moon was waterless, it was oxygen-less except for the oxygen incorporated in the minerals in the rocks, and here was something new.

I got very excited. I was dead wrong. In fact, I was jumping up and down, and I was calling out to the PI, and it was then [he] had to talk to Jim Lovell. I said, "Get them to take a core. Get them to take a core. Get them to take a core." Well, they knew enough to take the core without my having to say a damn thing, and Bill Muehlberger, Bill's a big guy, Bill just pushed me down in my seat, and he did exactly the right thing, because they did take care of it. They did it all well.

It turned out to be orange glass and zillions, ten-to-the-fourth zillions, of little beads. We didn't know it at the time. Some of the beads were black. The black halo came from the black beads, and the crater had dumped into a layer.

Flying overhead was the command module pilot, and I think that was Ron [Ronald E.] Evans. Ron heard these guys excited about orange stuff, and he was flying over the rim of Mare Serenitatis, and he said, "Hey, I see orange out here, too." And he looks, sees orange. He took pictures which showed that.

Apparently there'd been a major, major event. This was a volcanic event. These were volcanic deposits of beads, of melt, coming out into the vacuum of the lunar environment forming perfect little spheres. That's one way to make ball bearings. Subsequently, we learned that the color came from the presence, instead of an oxidized state, the presence of titanium 3, a very reduced state of titanium, and it was one of the most important discoveries we had there.

As soon as I finally got to see the samples, which was during the lunar preliminary examination teams—I was on that, I was on all the preliminary examination teams from 15 on—I knew we had something special. And what they took me back to were the green stuff on top of the Apollo 15 boulder, which was green beads. Now, on the lunar maria, the basalts have variations in chemistry. One of the conspicuous variations was some of them were high-titanium basalts, and some of them are low-titanium basalts, and the Mare Imbrium basalts were low-titanium, and the Mare Serenitatis basalts were high-titanium. Well, we're putting all this after the fact, but that's what the scientist is supposed to do one way or another.

And then in the course of doing things, a number of us independently before the next science report discovered that these beads had coatings on them, and the coatings were so slim that you couldn't see them. They're finer than the coating on a camera lens. But they had chemistries that were unlike most of the things that we were dealing with, the chemistries which included what we'll call volatile metals. Lead's a volatile metal, cadmium, zinc, a bunch of other things. And they were there in unusual concentrations, and the lead was an unusual isotopic composition.

Now, it dawned on a number of us—I wasn't the first—that these beads had to have been superheated silicate melts thrown out into a vacuum where they formed this incredible sort of stuff and that they had to be from deep within the Moon. Now we get back to the question of how hot is the Moon now, and how hot it was at 3.3 and 3.7 billion years, which are the key times for these [two] kinds of glasses, and how deep did they come from, because if we know the thermal gradient and we can analyze the chemistry of the beads and then put them on an analogous chemistry or the beads themselves into an experimental environment, we can determine what it would take to make them superheated. We can determine their melting points and what exceeded the melting points.

You had to come to the conclusion that both those sets of beads came from several hundred kilometers below the lunar surface, the deepest samples we had, and that they represented volcanic fountaining, you understand, during the mare-forming process, which gave them very special implications.

And then I talked about the coatings, and I want to come back on that. Probably the best example to use on this, although there were many samples that would do it as well. No, the best one I'll use it is a sample that Arlana's seen a picture of. It's from Apollo 11, 10072, a [vesicular basalt]. All of our lunar basalts have cavities in them. Some of them we call vesicles. They look like the cavities we see in terrestrial basalts. Some of them have a different kind of cavity with crystals growing, the main crystals of the matrix just growing right into the cavities. We have other names for this. I won't get into them too much.

We've never yet figured out—well, we know that those cavities are related to the separation of the fluid phase. We have never identified the fluid phase. I don't know that anybody did some obvious experiments that I would do now, but it turns out that the Apollo 15 green beads also had a very fine thin film of enriched volatile metals, not only in volatile metals, but fluorine and chlorine and other things. Not a sign of a drop of water. The vesicles we see in the terrestrial lavas we all attribute primarily first to water and then to CO_2, which is oxidized carbon. We haven't seen either of those anywhere on the Moon.

Some other kind of gaseous or fluid phase, vapor phase, must have been responsible, and it must have transported these volatile metals, fluorine and chlorine, from—now add them all together—from the deep interior of the Moon. And that was telling us something about the Moon that we really didn't understand until after 17. Now, the presence of that material moving up from the interior of the Moon tells us things about the interior of the Moon that we have had no clue to prior to that, and it just represents one of a major class of unanswered questions about the vertical, three-dimensional chemistry of the Moon, about the time-dependent variations, and the thermal history of the Moon. And it turns out that the orange stuff is seen all over the western part of Mare Serenitatis, and there are orange beads of the same character blown all over the Moon. We find them in the regolith everywhere.

That's one thing about the process. In the low lunar gravity field, materials that once received a velocity boost can go around the Moon. They even get away from the Moon if they have velocities high enough to escape. But they go all around the Moon, and the lunar surface is just covered with debris that's been going around and around. Every impact scatters it around. The objects which come into the Moon are traveling at seventeen, eighteen, twenty, thirty, forty kilometers a second, and they impart an awful lot of energy, and they just have been gardening the surface of the Moon over and over again.

That we were able to find these things and that they were buried, before they'd been completely gardened up away is itself significant, you understand. So there's one hell of a lot to be learned from going back to the Moon with what we know now. Our picture of the Moon is incomplete, and I'm one of those guys that says we haven't really understood as much as we thought, and even though we brought home more than 800 pounds of samples, Harold Urey wasn't right. He couldn't tell you the history of the source from one sample, although we can learn one hell of a lot more about it if we recognize what we know and what we don't know. So I'm an advocate ultimately, given the means, the energy, a fidelity of purpose—I want to use that phrase—for going there to find out about the Moon, for going back to the Moon.

Now, there are other people who have other independent arguments which are probably more convincing. There are some people who want to use the Moon as a staging platform for a Martian launch. That's fine, but if you're going to go to the trouble of using the Moon for a staging platform, let's remember that there are other guys that want to mine the hell of the lunar surface. One of them is Jack Schmitt. I don't think there's any reason for us to do that. I haven't told Jack that. The argument that Jack makes and some physicists that he's working with, has worked with for some time, is that there is so much helium 3 on the Moon, and one of the flowsheets for a successful fusion is to use helium 3 rather than tritium, or hydrogen 3, and my friends the physicists, as opposed to Jack's friends the physicists, say we can do it with tritium. And we've been making tritium for an ignoble purpose, namely, to make hydrogen bombs. I'm not so interested in making hydrogen bombs. I would like controlled nuclear fusion, because we're going to need energy sources with other potential and promise, but it's got to be a thoughtful in a considered way. But, jeez, if Jack ever [gets] to see this, I'm in trouble.

Anyway, right down to the end, Apollo 17, there were revelations. There were other revelations, and we just didn't finish the job from the science point of view. Now, how much money will you spend to get answers to science questions? The estimates for the cost of Apollo were 25 to 30 billion dollars. Those were in 1970 dollars. Now would we spend that money on National Institutes of Health, on fighting HIV/AIDS, in feeding "po' folk"? That's a direct snotty answer to people who say we should be worrying the poor, and a lot of them are black who are worried about the poor, and who are poor. It's a snotty answer, and I shouldn't use accents like that, I know. But it's a matter of the community's values, the U.S. community, the world community, and all the other things, not mine. So I've said what I can say.

Now, there are a couple of topics I want to talk on and then I want to quit, if it's okay with you.

BUTLER: Sure. Absolutely.

SILVER: What did I tell you?

BUTLER: You had mentioned—well, there were several you mentioned, and I'll go ahead and list them all and then we could—you had mentioned the scientific objectives; the success and failure for the various missions; the human performance, which I think we've talked about to some extent; the complexity of the missions; the special people that were involved; the romantic view of Apollo; the best view of Apollo; the unmanned versus manned controversy, and I think we covered that one unless you wanted to say more on that; the quality of the people you worked with being extremely important; the preliminary analysis; and science tragedies which we talked about with John Young and the cable. I think that's most of—that was what I had written down for the list.

SILVER: It may be repetitive. I'd like to touch on each of them just to be sure I've covering the lot. So, ask me.

BUTLER: Okay. Certainly. Okay. You had mentioned for—each mission had a unique science objective, some of which—

SILVER: Yes. A unique series of science objectives.

BUTLER: Unique series.

SILVER: It wasn't one. It was the whole series, and included in each unique series was the general objective, expanding what we know, and that was expanding what we know in terms of the science and also in terms of man's capability. We did upgrade, largely through not in the technical aspects, through things that we thought about as we went [through] the preparation for the early missions, but long before we got [to] the other missions, like the lunar rover, for example.

I think that the engineering performance was outstanding, really outstanding, and many of the flight controllers in the Flight Directorate were engineers, and they were the people, the first people to do an all-cost systems engineering, and that was a great invention. I think it was Apollo who brought that on, more than the military, and so I'll come back to that when I talk about one of the other generalized topics. Between my other remarks, we've kind of covered that topic. Let's go on now to the next topic.

BUTLER: Okay. Next one we have is human performance.

SILVER: The human performance was extraordinary, and I've already given you individual things, but what I want to tell you is about the guys who work all night, year after year, to make sure that their responsibilities are up to snuff. I could not begin to appreciate that until I was down here and participating at some level and seeing what was being done and how between the sessions I would be in and the next day things were done. The guys would not go home, or they'd go home and they'd get four hours of sleep and they'd come back and then to work. So there's a human performance of the actual astronauts. There's a human performance of the scientists working with the astronauts. Those are the ones I got to see a lot of. But most importantly, there's the human performance of all the engineers who worked on the thing, and they were so deeply involved that it's just incredible. You have to ask yourself—it wasn't me—who built up that intense dedication? It was there, everywhere.

BUTLER: I guess fitting in that one is also commenting on the quality of the people that worked on the program then, along with this performance.

SILVER: Yes, and this is again subjective. The people were recruited, and especially people here at the Center, were truly outstanding, but you have to remember that the Center had corporation after corporation contracted for, and those guys were in here working just as hard, whether they were from North American or from Garrett Corporation—that's my brother—or some of these. All these people believed that this was a great activity, and they weren't building pyramids, but they were doing something extraordinary, and whoever recruited them all, and I think that just belongs in the general American science and engineering, and I have to say they extended beyond America, because there were many foreign scientists who contributed in here. I had English friends, Australian friends, Canadian friends, guys from France, from everywhere. They believed this was one of the great undertakings of mankind during this era.

That's one of the most important things about it, was the ability to demonstrate common interests, successful interaction, and, unfortunately, probably not enough reward by recognition that we should have had for all the people who did all the things. But anybody who worked in that program knew how many good people there were in that program.

BUTLER: Certainly took a lot of unique people to make it all happen.

SILVER: Yes, except that it persuaded me that humans are capable of being unique, and they don't have to be picked out from one out of 10,000. The potential of good people is extraordinary.

BUTLER: Absolutely. Looking at the complexity of the Apollo missions, especially as they grew, from the science standpoint, how much did that mission complexity change from when you first became involved, to the ultimate end of the program?

SILVER: Well, the mission complexity did change as NASA's confidence in the ability to do things that they had to do. Their confidence in their ability to send people to the Moon and the performance of things like the rockets and the subsystems and everything else just grew and grew. Unfortunately, to have made the next step along there would have meant a continuous level of investment. The investment of just operating the systems was high, and there just wasn't enough money to continue upgrading. But the intelligence and the capability to have upgraded was there.

Now, maybe there's a natural level-off involved that has to take place, and this may get into something I was going to mention earlier. We were preparing for each mission without having the returns of the previous mission, except that we knew we'd gotten the people there and back, and we knew how that had done. But from the science point of view, we started extending the mission intervals. But that extending wasn't just driven by the fact we were still to learn what we could from the samples we got. It's just that we didn't have the money to go on keeping up the pace.

First shot we were on four-month intervals. Then we went to six-month intervals, and it went longer and longer. That was probably appropriate, but it bears on an issue which is there for NASA and the Center right now. How do you take the time constants for people, training them, educating them, sending them on missions, when the missions become more and more sparse when the activities are more extended?

I happen to be deeply interested right now in the question of what has happened to the women astronauts, because women, more than men, are driven by life cycle, by all the things that are uncontrollable: when do they have a marriage, when do they have a child or children, how do they maintain a home, how do they do all these other things. We haven't really gotten around to that, and we may do extraordinary things sending people on eight-month or one-year-long voyages to Mars and what have you, but they're one of a kind.

How do we get to the point where we can sustain, let's say, the Space Station and its occupation? You know, we've already cut back on the number of people that the Space Station can maintain. How do we make life on the Space Station sustainable? It's one thing to send a woman to live in a Russian Space Station, which they've had, and have her come back and go. That's not steady-state. We've got so many unsolved problems on the social, cultural, what have you. And I'm focusing on women, but it's not to say it isn't there for men. It's there for men but with a different kind of time constants. So I still think one of our toughest of all problems is to recognize how to deal with the human aspects of extended participation in space exploration, and I've been involved in all of it.

Way, way back, right after Apollo, I was involved with establishment of new categories of people, mission specialists, you understand, different classes of specialists, as well as of pilots. And how well have we done with that? And what's happened, as I said earlier, I raised the question, what's happened to all the women? What have we really done in the way of a sociological study base? And I remember talking to this point a little bit. But one reason I'm down here is for a conference at Rice this coming week, and I'm only raising the question because I haven't studied the problem, but I'm concerned about the problem. You can tell I'm an advocate for space exploration, but it's not trivial to just say, "Let's go back."

Okay. Next.

BUTLER: You mentioned earlier two different views for Apollo, the romantic view and the best view.

SILVER: The best view of Apollo, we've already touched on. That's the ability for people, a spectrum of people, from the brightest to the—and the spectrum of people includes talents at various levels and in various disciplines and arts and what have you, for a spectrum of people to do extraordinary things. But the romantic view of Apollo is we just send astronauts up there, and it doesn't deal realistically with what it took to get the astronauts up there. The tragedies, the ultimate tragedy was Apollo 1, and when you think of the kind of people who were taken away on Apollo 1, it just boggles you. That's the fire that killed those guys.

Romantic view, I hate to say this, Al [Alan L.] Bean's pictures, he's a wonderful artist, I'm really impressed, and they capture the romantic view, but talk to the guys who worry about getting each crew up and each crew back. Talk to the guys who worry about the margins. What have we done on the margins? When a congressman stands up and says, "We can only spend so much on space, but I want you to do this, this, and this," that is so detached from reality. So let me lay it on the congressman, but it's not just the congressmen. It's the heads of big corporations, which make a lot of money off the space program. It's congressmen who like to take positions that they feel will appeal to their constituents, get them reelected. And it's boys and girls who have a romantic view. And do I want to take that away from them? No. Do I want them to come down and volunteer to be astronauts? Yes, I kind of do. But what do they know? Who speaks? Who speaks? When we had the *Challenger* accident and a few guys stood up and spoke, they had their heads handed to them.

BUTLER: You talked, when you were talking about Apollo 16 in particular, you mentioned how the preliminary analysis and the geological understanding was different than what ended up coming back in results. Could you tell us a little bit about what went into preliminary analysis and how that—

SILVER: Well, a hell of a lot of work. Remember that people had not been to the highlands. The closest they'd been to the highlands is when they got to the foot of the Apennines in 15. But the ideas about 16 had been the result of years and years of study by very good geologists. I knew them all. I knew them not because I was involved in the Apollo Program; I just knew them as good geologists.

And they, I think, [had] what I'll call a geocentric view. That is, they're using Earth systems and Earth analogies a little bit too far, and they hadn't married the things we were starting to learn in the first year of Apollo, that gases were extraordinarily rare, and a lot of the materials that they thought might have been produced by certain kinds of volcanic phenomenon required gases, and if all those gases were really down at 200 or 300 kilometers in the Moon, it was unlikely we'd have at that time. And that's my subjective, "Silver-centric" point of view. But I was in a position to critique and criticize. And I can't say that I did.

What's my excuse? I was so overwhelmed by all the things I was trying to do, and that's when you push the people to limits, where you know they're good, but they can't possibly do it at the rate that everybody had to do it. We see that in a variety of ways. Arlana works for a high-powered institution, and we see this. People are pushed too hard, even the very best. So in this case, there hadn't been enough external critical review of the science models.

BUTLER: That sort of ties in with—or could kind of fall into the category of the science tragedies that you mentioned earlier with John Young and the cable on Apollo 16. Were there others that you wanted to mention?

SILVER: Oh, there were a number of others. The most difficult jobs we had was putting priorities on various classes of proposed science experiments. The heat-flow experiments were the general domain of a very fine scientist from Columbia University by the name of Marcus [G.] Langseth, and that poor guy failed to get the information he wanted, which was, in fact, first-order information needed for our science models. So many times, and I was there in the science back room with him, when John backed off. But it wasn't John's just backing off; it's the extent to which the experiments he proposed were, in fact, selected to be carried on some of the other missions and that they weren't made, and that happens over and over again.

And then there were the guys who wanted to carry heavy equipment, to have the astronauts do seismic experiments. One of the things we'd hoped, that we'd have a flux of objects coming to the Moon, that when we deployed seismometers, we'd get signals as we do on the Earth all the time from earthquakes, you understand. But the signals would be energy-generated by the impact, and the flux of impacting objects is little, so little that in the end we were slamming the residual rockets into the lunar surface to get the energy. There are many things we didn't get done.

We, over and over again, thought we saw classes of things we thought were volcanic, and they were on the surface, and we thought, therefore, they'd be among the youngest features built on the lunar surface. But the most volcanic things we saw were the lava flows in the maria and these glasses, and they were all three-plus billion years old. So they were old.

So what do we know about current volcanic activity of the Moon? There still are some candidate things, but we haven't gotten those.

BUTLER: I think that covers most of the topics that you had mentioned.

SILVER: What have you got?

BUTLER: One question that I'd like to ask you that sort of ties in here with talking about the volcanics is, going into the Apollo Program, what was your perception and the general science perception of what would be found on the Moon and what is would tell us? And then how did that perception change as the results came back?

SILVER: Well, I had this very fine student, Mike Duke. Mike and I had written a paper. We had been studying a class of meteorites called basaltic achondrites. These are different from the most common forms of meteorites, irons and chondrule-[bearing] meteorites, which have a totally different chondrule. The basaltic achondrites have the chemistry of basaltic rocks. I expected that the mare might have—and we said in the paper that one of the candidate sources of basaltic achondrites might be the lunar mare. That was before anything had—this was done back in this sixties, so it's way back.

But I didn't have that much. I didn't know what the highlands were. Anorthosites were a reasonable thing, but there wasn't much emphasis on anorthosite. The highlands were mysterious. Maybe they were all going to be something we all called rhyolites, which are very light-colored silica-rich rocks. We have found almost no silica-rich rocks. So my expectations for the lunar stuff were low, and they started to build up as we got through Apollo 11 and Apollo 12.

Apollo 12 threw a curve at us. Apollo 12 brought back a little piece of rock about that long, postage-stamp-sized. It was sample number 12013; 12013 was not a basalt. It wasn't anorthosite; 12013 was enriched in silica—it had free silica in it—enriched in uranium and thorium, and other things. It had all the ingredients that were what we would call continental on Earth, not oceanic, but continental. Apollo 14 had more of the same, and by the time we had flown remote-sensing radiometric analysis material around the Moon, we built up a strip map—it wasn't a complete lunar surface map—of a lot of things that were concentrated by an order of magnitude or more so that they represented continent-like materials, and this stuff that particularly showed it was 12 and 14 and a little bit of 15.

We came up with a new nomenclature and, doggone it, the question is, was the Moon in the process back three billion, three and a half, four billion years ago, of starting to differentiate, as this planet had done? I think the answer is unequivocally yes. But in what way? And if we really understood those concentrations, which are all seen in surface materials, would we really understand how continents originated on the Earth? We never got there. That didn't get the attention I would have liked, but I was patient, because what Marcus Langseth was doing, trying to take the temperature of the Moon, would have a very strong bearing on this. So we never quite got to the end of that, and that was a very important thing.

I can't say I was disappointed by what we learned. We learned so much, and what we learn has raised so many questions in my own mind, and everybody who considers them—and there are a lot of people considering them—will have a different evaluation. What it tells me is, we haven't begun to milk the Moon for what we can learn about planetary formation. Now we're rushing off to Mars, you understand, and we won't do as good a job of sampling Mars as we've done on the Moon, and the argument is, well, we've been there, done that, and if I have a disappointment, it's that "We've been there, done that" kind of attitude.

BUTLER: I think at this point, before we go on with any of the questions I have, I'd like to ask Sandra and Rebecca if they had any, and then we kind of evaluate where we are.

WRIGHT: I was going to throw it up to you, did you have some questions, Arlana? I know you were taking some notes or something that—

ARLANA SILVER: Oh, just some bits I was going to ask him later.

SILVER: She has special access.

REBECCA WRIGHT: My only question for you is, when you're ready, you know, we're very interested in hearing about those other individuals that you wanted to make sure that you did not leave those out.

SILVER: Yes. And I probably won't do justice to them all, but I've got to do as much as I can.

REBECCA WRIGHT: Well, we will certainly listen to that.

BUTLER: If you'd like to go ahead and mention them now—

SILVER: Let's do that. Well, I've already mentioned, and I hope adequately, the important role of Gene Shoemaker, who's had a profound influence on our whole space exploration, and he was a good personal friend and certainly educated me over and over again.

There were so many geologists. I want to talk about the geologists first, while I've got them on my mind. After Gene decided to pull out, the guy who was left to carry the responsibilities which he had established and who knew much about his style of work while he was working with him is this man Gordon Swann. Gordon Swann has never gotten the appreciation. Every time I see Gordon, whom I consider to be a very good friend, I feel guilty because I've been more visible.

Flagstaff, Arizona, is where Gene Shoemaker established a branch of geology called the Astrogeology Branch, and a lot of these people still live there. And I just feel that I've gotten more attention than I deserve, and they haven't got enough attention. Just recently, one of the important guys there, a guy by the name of David [J.] Roddy [a Caltech alumnus], he was killed—well, he died of Parkinson's disease. Dave was mostly a student of Gene Shoemaker's, but I was his academic advisor. We'd divide up roles. I had known him for a long time. He became an expert on craters created by chemical explosives, worked on them all over the world, took advantage of mining explosions, took advantage of accidental explosions. We learned a great deal about the way in which explosions affect the surface geology of the Earth, and this was built into all kinds of other data we were gathering on what impact explosions would have done on the Moon and certainly on Mars and any other body.

So there's Dave Roddy. Now, he was an especially good friend because we had known each other while he was a student. As a matter of fact, when I was training astronauts in Flagstaff, Dave would fly into Phoenix, pick me up, fly me back to Flagstaff, because there wasn't good air service. Flagstaff's a little town, and then he would fly me back out of there. And he did this because, (A), he loved to fly, and he had that nifty little plane called a Mooney, and, (B), because he thought we were doing some of the most important things.

So how do I bring Dave Roddy's name into this? Now I have to go back to Gordon Swann, right? I'd mentioned him. I was totally ignorant of the Apollo scene, and I was working with Gordon. When I started this training down in the desert, what I called the Orocopia Mountains, Gordon wasn't visible. But once it was decided that we were going to continue this, then NASA decided that the way for us to do it was to work through the U.S. Geological Survey, because they had the contract for doing the surface studies, and I was talking about the things that they would do. But Gene and Gordon and innumerable other people—Dale Jackson I've mentioned already, I know.

I didn't mention Ray [Raymond] Batson, who was a superb camera man. I didn't mention Bob [Robert L.] Sutton. Bob Sutton could take meaningful notes of everything going on on the lunar surface faster and more accurately than a tape recorder. So the science back room, which was acquiring data on what was going on by taping and by all the other things, Bob Sutton was pulling out the essence of it.

Now, why was this important? Well, we had an EVA team, and we had another team which was called the tiger team. The tiger team worked when the EVA team, went to bed and ate, and the tiger team stayed up and tried to digest what had happened and how it bears on the following, on the rest of the experiments, and what have you. So the stuff that he did, which is super.

Anyway, old-timers and young guys. Old-timers, Dale Jackson I mentioned. Let me mention another guy, Howard [G.] Wilshire, a terrific microscope specialist. Dale Jackson and Howard Wilshire and Lee Silver would look at the first things we can see from the [returned] samples. Then we'd published some things on them. They helped me over and over again.

There were two guys, Ed [Edward W.] Wolfe and George [E.] Ulrich, who when we first put together these little—when we were out doing exercises in the boonies and we aren't very well along in the evolution of our preparation, these guys would be the guys at the radios, and we'd have the crews talking to them, and they'd be taking notes, and I would be debriefing the crews and them about how well the communications worked. And then we finally got people like the capsule communicators to come out in the field with us, and we'd all take part in the debriefing, and it was a preparation. This is now for just for the geological education.

I look at all kinds of names here. There's no use in my giving them to you. Those guys had worked there, but—oh, there was another guy, Tim [Mortimer H.] Hait. I was training the Apollo 15 crews in the high San Juans. One of the things I wanted them to appreciate was that since we didn't have fliers, we might have rovers, that you're going to learn about what's up on the flanks of mountains if you can find what's come down on from the flanks of the mountains. So I took them up into the high San Juans to show them what I'll call talus slopes, debris flows, all these other things. And Tim Hait was the guy who helped me do all that. Now, the thing about Tim was, he had the same size hat I did, and I'd burnt to a crisp in fifteen minutes, especially at 12,000 feet or 13,000 feet. So there's wonderful picture of me taken by Joe Allen, wearing Tim Hait's hat, makes me look very dashing. But the point was, we supported each other.

Another guy, Gerry [Gerald G.] Schaber. In the world of scientists, the guy who might have really challenged some of our preconceptions or preliminary models was a guy was by the name of Gerry Schaber, who always started from first principles. He was very good.

Then there are two guys here, George Ulrich and Ed Wolfe. Those two guys were instrumental in putting out the big finished products. On the Apollo 16 mission and the Apollo 17 mission, Ed Wolfe took the lead, George Ulrich followed. They also worked on volcanic features all over the country. They're U.S. Geological Survey people.

George Ulrich, his professional career ended in an unhappy way. Wouldn't have happened on the Moon. He was working out of the Hawaiian Volcano Observatory [HVO], and he was working among some new flows, and he was walking across one too soon, and he broke through the crust, and his leg was immersed in the—I thought I showed you [Addressing his wife]. His boots are on exhibit just outside out the HVO at the [Jaggar] museum there. So these are guys who I'd worked with and had rough times with.

I'm just talking now about the people in the—there are outside geologists and scientists with who I discussed. One thing that I think helped me be more effective, I could do geochemistry. I was doing geochemistry. I knew the geochemistry community as opposed to the geological community. I had worked with geophysicists and seismologists at Caltech, and I could talk to the geophysicists. And so I could probably talk to a larger spectrum of scientists about their science, and I could weave that into the discussions with crews about the complexity of the experiments, and there were so many different experiments, you understand. And I've given you one or two names. I haven't begun to mention people like M. Talwani. Oh, I'll just run down the list of names of who were good, to whom I can talk and who, in fact, are, in turn, talking to me. Now, there's another guy whose name I don't remember—there's several.

At the Geological Survey, after Gene left and came to work at Caltech, one of the guys who stayed there and worked very hard on building the big picture about what we knew and didn't know about the Moon and what each site might be able to provide, if we could make a successful mission there, was a guy by the name of Harold Masursky. Hal Masursky had been a disciple of Gene's, but he was also an independent thinker. He's the guy whose slides I took to Australia to the Sydney Geological [Congress]. I was telling Arlana and George about this last night. He [kept] the big picture. He was prepared to talk about the geology of any larger site. He was encyclopedic about what he knew from the work of others, and he was a great synthesis man. Small guy, suffered from Type I diabetes, killed himself, in effect, by going all out working on wherever. He was not only working on the lunar problems, but he was a key figure in the Viking series on Mars and what have you. And he was one of my educators, one of my mentors, you see, all the way through.

Two other guys, one was a guy who now heads up a very effective Lunar and Martian Planetary Science Group at Brown University, Jim Head. Another was a guy who was working for BellCom. Bell Communications was a subsidiary of AT&T, and they had the contracts for providing consulting services to Washington [NASA] Headquarters. One of the guys who was very important there was a guy who had been a student at Caltech, took a master's degree, left, and went off to Princeton [University, Princeton, New Jersey]. [Noel W. Hinners]. I'm having troubles with the names right now. It's not in here.

BUTLER: Farouk El-baz, was it?

SILVER: No, I'll talk about Farouk in a minute. He's on this list. [Noel Hinners.] No, he worked for BellCom, and he was a guy, along with Jim Head, who reported to Rocco Petrone and the other guys, and to George Low back in Washington, and other things. He eventually was the Director of Goddard….

Then I have to go back and mention another guy, who had been a professor of Gene Shoemaker's. His name is Harry [H.] Hess. Harry Hess, a member of the National Academy of Sciences, was important in being selected to head up an advisory committee to NASA on personnel, on approaches, and all the other things.

My student Mike Duke, who I've mentioned, his role here was extremely important. I went to a meeting at the University of California, and Mike delivered an important paper in which we proposed that these basaltic achondrites, where they'd come from on the Moon, and Mike did a beautiful job. We left, and he and I were walking the building. The Berkeley campus is a bit on a hill. You can go on the basement floor or sub-basement floor, and you work your way up [through] the building. The auditorium was up on the basement floor, and we walked in, and we were going to take an elevator, right up to the top. Pushed the button, the elevator came, the door opened up, and here was Harry Hess, leaning against the wall, smoking a cigarette. He's famous for this—he was one of these guys who smoked and smoked and smoked, very distinguished guy, wound up as a commodore in the U.S. Navy, and he directed the first submarines to map magnetic anomalies on the ocean floor. He was really top-pole, and he had some great ideas about global tectonics.

Here was Mike, and I, and Harry with a cigarette hanging out of his mouth. He said, "Mr. Duke." He said, "Dr. Duke? That was a great talk." Now, here's a guy who was at the pinnacle, you understand, picking up a young guy. But he didn't know that Mike Duke would then become the caretaker, the keeper, and, of course, subsequently he got to know Mike very well. But I'd known him for other reasons, because I'd done work that overlapped some of his work, and we'd shared common interests and what have you. And I knew a lot of students that went from Caltech to Princeton, and some from Princeton who came here, so there was a lot of exchanges. Very good.

But Harry Hess was instrumental in, first of all, helping Gene get to do what he started to do, and in sending a lot of good people to Houston, and in keeping the level of the science high by setting standards. Now, weren't there a lot of other guys who did that? I can't remember then all right now, but there were, lots of other guys, and Harry Hess was, for me, one of those influential figures.

For me, another extremely influential figure was a professor at Caltech, Harrison Brown, that you heard me speak of. He hired any number of people that left later to become members of the National Academy, and that was Sam [Samuel] Epstein, Clair Patterson, myself, and others. He had a deep interest in the origins of the solar system, but he had started out doing a dissertation at Johns Hopkins [University, Baltimore, Maryland] on the separation of U^{235} from U^{238}, in the late thirties. That was his Ph.D. And there was a certain timeliness in that. He wound becoming the director of [one of] the major labs, and then he went to the Enrico Fermi

Institute in Chicago [Illinois], which is one of the centers where many good people were thinking about the solar system, including Harold Urey, of whom I've spoken, but many other people as well. After all, it's a community that interacts because all these people had influence one way or another.

I want to turn to the Flight Directorate. When Gilruth was the Director of Johnson, Chris Kraft headed the Flight Directorate. His special assistant was George Abbey, and they worked exceedingly well together, as did the two of them with Gilruth. George has a quality that I hope I share, because I respect him so much. He respects the people that go in front of him. He's very good about being generous and acknowledging his indebtedness to the people. Well, when I say Gilruth and I say Chris Kraft and I say Rocco Petrone and I say George Abbey, and don't you think for a minute that George Abbey hasn't been influential for thirty-some-odd years in the program, and never claimed to be a scientist, never pushing, you understand.

The only thing I ever saw George do that I always found striking, we had our first Shuttle flights and people were landing at Edwards Air Force Base [California], George was always the first guy to meet them. Have you noticed that? I don't think George was doing that just to share in the publicity or the glare. They were his people. So George is another one of the guys I'd put in there.

I have to talk about another guy. After Apollo 1, NASA needed to find somebody to bring the Apollo Program back, and they selected a guy by the name of George Low. I didn't know George Low at that time, but I got to know him in the program, and George Low was paying attention to the training. He was paying attention to Jack Schmitt. He was paying attention to everything. He was doing one of these superhuman performances under pressure and what have you. And so I got to know him, and he was very important to me, and it turned out this is a small world kind of thing. After Apollo was over, George left NASA and went to Rensselaer Polytechnic [Institute, Albany, New York] and became the president of Rensselaer. National Academy of Sciences selected a committee comprised of members of the academies, all three academies, called the Committee on Science, Engineering, and Public Policy. They took on tough projects, and George Low was the first chair. I was picked to serve on that committee, so I worked with him for a while. After a while, I got to be picked to be vice chair, and then George died abruptly, and I succeeded him.

But my years of overlap with him and my knowledge of him before, let me say, assured me that he was one of the great men of the time, and he did bring the whole program back. He had the quality he could listen. He had the independence of mind. He was willing to take the responsibility for what he was doing. So he's one of my heroes. I saw him working for other elements of the national enterprise, and I know how good he was.

I can keep on going down this list. There are all kinds of other people that I know.

BUTLER: You had mentioned before the oral history, Jack [John R.] Sevier?

SILVER: I want to mention Jack Sevier. Jack Sevier is a case in point. Jack Sevier was involved in a committee [called] the [Lunar Surface] Traverse Planning Committee. He did many other things, too. He was part of the Flight Directorate, Flight Controllers Directorate. But in the science community there was always a technical argument for this and a technical argument for that. All these things that we might have wanted to do were taking five times as much time as there was. Again, we had to coordinate with all the other requirements of the mission. Scientists, most of the scientists, were advocates for their experiments. That's what you'd expect. But Jack was the man who could run a meeting and bring the various advocates into a common purpose—I served on this committee—and he was able to get a rational solution. Was it always the best? No, it wasn't always the best. Who knows what was always the best? He was able to do that, and the tug and pull was enormous.

I didn't talk about this, but I have to now insert something. There was continuous competition. Various people were building up advocacies for running the science in different ways. For surface science, only the U.S. Geological Survey had the real technical competence to work with the photos, the maps, create, prepare all the things for that side. But as we did more and more work on the samples, there was a growing group of sample analysts right here. They were led by a brilliant guy, Paul [W.] Gast, who was here. Paul Gast was one of the finest geochemists we produced. Well, Paul Gast thought like a geochemist, and a number of other people thought like geologists. A number of other people thought like geophysicists. These people were all smart, articulate, could make their case.

It couldn't all be done. Jack Sevier helped us come up with a rational basis. But there were points in the evolution of the programs where there was controversy between, let's say, the geochemists and the geologists. That controversy came out of Building 31. I remember that number. And it wasn't flattering. It redounded in a funny way in my own work, and that was that I'd spent so much time working with the Apollo 15 crew, but some of the geochemists, including Harold Urey, were convinced that the geologists were biasing the astronauts, that is, they were building in preconceptions, notions that the geologists had, and that this would, in fact, detract from the ability to perform the best experiments on the lunar surface. This included one of my colleagues who advocated this position.

So during the Apollo 15 mission, there was a small team that came into the science back room where the geological team was working, to watch us. At the same time, operating out of Building 31 was another science back room, created by Paul Gast and the doubters, because they were going to try to compete with us. This is kind of difficult because they wanted to send messages to the control room in competition with what was coming out of the science back room we had. That happened in Apollo 14, and that's one of the things that turned Dave Scott off.

When we got through with 15, we were patting ourselves on the back. The competence, the effective execution, the implementation by the crews was obvious to everybody. So the three guys who had come in said so. So we got off that hook. But it was typical. And from then on, my preparing the crews wasn't in doubt or anything like that, but it was typical of a cabal or a series of cabals who didn't see eye to eye, and there's no way in science you can point the finger and say, "We're going to do our science the same way. We're going to be a team." It just simply can't be done. So that was probably the least attractive moment in there.

Paul Gast was my friend and always had been, and I admire his work. His contributions to terrestrial science and to planetary science are fantastic. And there were a lot of names I could name, but this was an issue, and it showed up in something that I did not relate in *From the Earth to the Moon,* and it must have come from somebody else. But if you've seen that series, and you saw the hour ten and the hour twelve [episodes], it appears as if there are two people who had very different positions, and in the end, the character who played Silver, as it was written for that story—I didn't have a thing to do with it—triumphed. That's bad. But there are still people, including a former director of a national lab, of a national center, asking, "Who was that guy?" I don't get named in there. But it did reflect some of this turmoil, and in the end it came out all right.

I keep talking.

BUTLER: Well, it's an oral history. You're supposed to.

SILVER: It's oral history. It's only one man's version.

BUTLER: Yes.

SILVER: And I can't get anybody to corroborate what I said.

BUTLER: Well, you had mentioned you wanted to go back and mention Farouk El-baz's role.

SILVER: I do. While I was training the ground crews, Farouk was training the circumlunar crews, one-man crews. But he was also training—his training extended into all the crews. Farouk is a master, an articulate master of educating and maintaining interest. The guy who played him in *From the Earth to the Moon* isn't as good as Farouk is. He's just a master. We were never in competition. I didn't hear most of Farouk's training because I wasn't there at that time. We each had our shots at the crews, but that was among the things the crews had to learn, you understand. And the crews all had their opportunities to go around the Moon a number of orbits, so they got to see things which they were all lit up about. They just loved it. And Farouk did a fantastic job on that.

But Jim Head contributed to that, and this name I'm struggling with right now.

BUTLER: We'll definitely be able to look it up.

SILVER: Oh, you can find it, and I'll know it the minute I leave. What else?

BUTLER: Sandra, did you have any questions?

[To Dr. Silver] Well, was there anything else that you in particular wanted to talk about for your experiences during the Apollo Program?

SILVER: Well, I know that sooner or later we have to regenerate an Apollo operating record and the spirit and all the things that made it successful. There have to be people who are drivers. That was a driven program, driven by President [John F.] Kennedy's remarks, among other things. But it was driven by all of us. I doubt if we'll ever again be able to dedicate that much money to space science. We just spending as much money in the health sciences and things like that, but it's current dollars; it's not 1962 to '70 dollars.

But the mission dedication is what's going to be important. The people who have to be selected have to be selected for their ability to motivate, make it go. There were masters, and I've mentioned how much I respected George Low, Rocco Petrone, Gilruth, Kraft, George Abbey. You have to know that some great work was done in NASA. I want to come back and say John Young has been a key man in the success of the astronaut aspect of all this work, by demonstration, by performance. But also you could speak to him at any time you chose to.

There are so many good people who I've said good things about: Tony England, Joe Allen, CapCom for 16, CapCom for 17. They're all just great. Bob [Robert A. R.] Parker. Do you know Bob Parker?

BUTLER: Yes.

SILVER: Bob Parker's a Caltech alumnus. He finally got to fly. He now heads the NASA office at JPL [Jet Propulsion Laboratory, Pasadena, California]. But he had a great touch, good humor, dedication. He was a wonderful guy. These guys are my friends, so I don't want you showing this around. I tried to be friends to everybody I'd meet. Some of them are gone, so that's the way it goes. Who was the Apollo 16 CapCom? I don't know.

REBECCA WRIGHT: One of them was Fullerton.

SILVER: [F.] Gorden Fullerton. Haven't mentioned him. Gorden served and served and served in every capacity. I mention him to you, and I asked George about him last night, and he's now out at Edwards, or it's Dryden [Flight Research Center, Edwards, California], I guess, now. A good man.

The people you need, you need people in ranks, and you have to recognize that if you don't have the first one, then the second guy will step in, the third guy will step in, and that's the way it goes, like that. It takes it in ranks. It was a community, a fantastic community. So if you ask me, was it one of the highlights of my life? You'd better believe it.

BUTLER: Looking back on your career with Apollo and the Apollo Program, you've talked a lot about the team and you've talked about your contributions. What would you consider, for you personally and then for the program as a whole, to be the biggest challenge?

SILVER: The biggest challenge was to subdue your natural human responses and remember what your mission was. Even when I was fighting like hell, and that fight extended to my own campus, it was important not to lose sight of the mission. All kinds of people that I didn't agree with, nevertheless, made contributions. That's the way it is.

BUTLER: And what would you consider Apollo's biggest accomplishment?

SILVER: Well, I've kind of touched on it, but I ought to restate it. I would like to find the words that would say it very well. Apollo's biggest accomplishment for me was marshalling human intellect, human energy, human organizing abilities for a common purpose. We've done it before in various ways, usually in times of national emergencies, wars, what have you, things like that. But this was peaceful and somehow—then again, I don't know. I wasn't party to what was going on in Washington very much. I strongly suspect a guy like George Low played a major role. I have to recognize Jim [James E.] Webb way, way back, when he did all that. I have to recognize the national science community, which at first did not bitch about putting so much money into Apollo, because they were interested in the big-picture questions. But it was the community of human intellect, you understand, and its ability to achieve something very, very big.

And we have to do that. It's clear we're going to do that more and more in the future, and I don't know who the natural leaders are, who could pull it all together, and be persuasive. I do know that the young people now can get excited about things, not necessarily space exploration. But a lot of them [unclear], but it isn't until you see the ranks form and a great mass of people move forward and that they're all the people who have to manage and organize, make decisions, and they're all the people who couldn't give a damn about being a manager or an organizer and wanted to simply do their thing. It was an inspirational demonstration of the human community doing something very big and very special.

Now, you'll say, what about the Moon? Well, we learned a lot about the Moon. We learned things about the solar system. And what we learned from the Moon and the solar system was not [only] hard fact, but [we posed] the next set of questions, and that's what we do in science. We pose the next set of questions.

You know, I'm running out of steam.

BUTLER: Okay.

SILVER: Unless you've got something special—

BUTLER: Just one last question for you. Is there anything that you would consider personally to be your most significant contribution to Apollo?

SILVER: No. I was one of those guys, like many who served in many ways, who was at the right place to be able to do something. I'm so glad I did it well. I know how perilous it was. You don't know how perilous it was for me, and I'm not going to tell anybody how perilous it was. But I was there, and I was able to do it, and I carried it off. I was so relieved I did it well.

I did one thing that you won't know about. At the end of Apollo, I organized a Center-wide party and it was massive, dinner outdoors, what have you. It was a great celebration, and all the troops came, and I was [not] known by everybody as the guy who stimulated it, and I organized a large part of it, and it was just the sense that we had done something very important. I know that Chris Kraft and Gilruth remember that. I know that George Low and Rocco Petrone [remembered] that. So it was a nice flashy little bit of "Here's the end of it."

Last week we were in Washington, and we were meeting with an old friend [Dallas L. Peck], who is the retired Director of the U.S. Geological Survey. During the Apollo Program he was on the [USGS] team. When Apollo 17 [crew] came back and splashed down, recovery was complete, Gordon Swann and Dallas Peck and the guy whose name I don't remember [Noel Hinners] finally got back to our rooms, and I had a fifth of Wild Turkey. We sat on the steps of one of our hotel rooms, motel rooms—the name of a motel I see is still there, but it's got a new name on the last one. We were breaking out whiskey, and we knew we had done something. That was a wonderful experience, too. Dallas remembers that. He says for him—this is a guy who was director of the survey—he said, "It was the finest moment in my life." So I had mine there, too.

The name of this guy is gnawing at me. He now is living, I believe, in Denver because that's where the head of Lockheed Martin's space section is. A very big man in space exploration.

BUTLER: I know we'll definitely be able to find that.

SILVER: I'm sure you will.

BUTLER: Well, you all certainly had done something, and we certainly appreciate you sharing that with us today from your perspective.

SILVER: Well, I was trying to do it right.

BUTLER: I think you definitely accomplished that.

[End of Interview]