# ORAL HISTORY TRANSCRIPT

HARRISON H. "JACK" SCHMITT
INTERVIEWED BY CAROL BUTLER
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BUTLER: Today is [July] 14th, 1999. This oral history is with Dr. Harrison Schmitt, who was commonly referred to as "Jack" by his colleagues at NASA at the time. Dr. Schmitt is here at the Johnson Space Center in Houston, Texas. This interview is being conducted for the Johnson Space Center Oral History Project by Carol Butler. Thank you for joining us today.

SCHMITT: It's my pleasure. And congratulations on getting this project going finally.

BUTLER: Thank you. Thank you. It's a fascinating project. It's wonderful just to be able to have a chance to talk to you and your colleagues and get your oral histories down. Today we'd like to start with talking about how you first got interested in planetary geology.

SCHMITT: Well, I had no overriding interest other than in the planet we call the Earth until I was looking for a job in 1964; and I sent a letter to Eugene [M.] Shoemaker of the US Geological Survey, one of the foremost planetologists that has ever lived. And he, it turns out, had sent me a letter almost simultaneously [after] looking at the list of people who had taken and done reasonably well on the US Geological Survey exam. And so, since there was a depression in most other parts of geology, I decided to go out and work with Gene.

It was aided by knowing many of the people that Gene had attracted to his group that had, by that time, become known as the Branch of Astrogeology within the US Geological Survey. People like Danny [Daniel] Milton and Newell Trask and Don [E.] Wilhelms and others who I had known for other reasons, and really very, very strong individuals in their science. And it was clearly a credible group to go work for.

I had met Gene some years before in—when I was introduced to him, it was by a hard rock geologist, more in tune with what I was working on at the time, who said, "You have to go down the hall and see what this crazy guy Eugene Shoemaker's working on. He's trying to map the Moon!" Well, indeed Gene did instigate the—that effort—modern effort—of mapping the Moon; and, much to the excitement of the Apollo astronauts and the Apollo managers when they finally decided that we were going to land there, that background was extremely important.

But that really triggered it. I had become interested somewhat in—in spaceflight after leaving Caltech [California Institute of Technology, Pasadena, California] and going to Norway in 1957 as a Fulbright student. And like so many people, when *Sputnik* was launched in the fall of 1957 (October 1957), it caught my attention. But it particularly caught the attention of the other students, from all—from countries all over the world, who were in residence at the University of Oslo. And that's what triggered my interest, more maybe than—even than the technical achievement, because at Caltech we had been around people who were going off to work in the missile industry and things like that. And so, rockets were not an unusual phenomenon to us. But the degree to which the then Soviet Union's success with a artificial satellite of the Earth, *Sputnik 1*, influenced and excited and literally scared the students who were around at that time from all over the world. That really did get my attention about how important space almost certainly was going to be in the future of humankind.

And so, I was more interested in a general sense than I was in actually ever thinking about participating directly myself. And really that thought didn't occur—well, it occurred once in a jocular vein when the National Academy of Sciences put out a report (I believe it was) probably in the very early '60s (I was still at the Harvard Geological Museum as a graduate student). And that report said the—it was done for NASA, and that report said that it—the first person on the Moon should be a hard rock geologist. Well, of course, all the geologists in Harvard and probably everywhere else that had wind of that report cheered and said, "Yeah! That's right." And then we promptly forgot about the whole thing because nobody was—it was sort of a joke.

But in 1964, while I was working in my early time with the US Geological Survey in Flagstaff, Arizona, NASA and the National Academy of Sciences asked for volunteers for the fourth group of astronauts who were to be scientist-astronauts. And I thought about 10 seconds and raised my hand and volunteered. Primarily because I felt—I can remember feeling, at the time, that if I didn't volunteer, no matter what happened to my application, that I'd almost certainly regret it when human beings actually went to the Moon.

BUTLER: Well, it looks like that volunteering turned out pretty good for you in the long run.

SCHMITT: Well, I was very lucky, very fortunate. And I think all the astronauts of that era, whatever they may say now, really were accidents. We happened to be at the right place at the right time. Unlike today, where young people can career-plan to become astronauts and go through several applications and every 2 years or so they can have that opportunity, we—it was presented to us in a very spontaneous way for the most part. And although some people applied two or three times on the pilot side, we were the first group of scientist-astronauts. And it really—we were an accident. And I think most of the astronauts have to say (if not all of them)—have to say that if they really were true to what happened, they were accidents. Not very many people can claim—like Eugene Shoemaker could claim that he had dreamed of going to the Moon from the time he was a child. (There's no question that he did.) Unfortunately, he was not qualified medically in order to volunteer for that group.

BUTLER: It must've been challenging for him to have been—be able to help you all that were applying, and to help you get into the program in that way.

SCHMITT: Well, he seemed to get a big kick out of it. Some regrets, I'm sure, for not being there himself. But he certainly, in those early years, gave heart and soul to trying to organize and stimulate NASA to put together a true field geology experiment for the Apollo Program. And ultimately, we did that. He was disappointed in how sophisticated it was for its time. But nevertheless if he hadn't been pushing, people like George [M.] Low and Gene [Eugene F.] Kranz and even [Gen.] Sam [Samuel C.] Phillips at Headquarters would have had a harder time selling the kinds of things we ultimately did.

BUTLER: As you were going through the selection process to become a scientist-astronaut, as you said, the new group and it was a new realm that NASA hadn't selected anyone for before, what was that process? What did you do? Can you tell us about that?

SCHMITT: Well, roughly, I believe the announcement of opportunity for the scientist-astronaut program, the first selection, came out in November of 1964 (if I remember correctly). And about 1400 young scientists applied—scientists and engineers, essentially none of whom were women, which is interesting when you—later on, we'll talk about the situation today. Or how that changed. Out of that 1400, based on a Federal Aviation Administration flight physical that we all had to submit with our application, NASA took that number down to (as I recall) about 400; and those 400 were asked to send in some additional information. I believe it was published papers and things of that nature. Transcripts.

Stuff to establish their academic credentials. And from that second submission, then 80 (I believe) were selected to be reviewed by a committee of the National Academy of Sciences. And those 80 also were asked to submit additional written material, an essay that I recall about "What would you do if you were suddenly on the Moon?" I don't recall exactly what I said, but I was deeply involved in working on those kinds of problems for Dr. Shoemaker. And so, I suspect that it had a little bit more knowledge and information in it than the average applicant would have had a chance to do. Or to provide.

From that 80, 16 then were selected by the National Academy, submitted to NASA to then take a physical at the Brooks Air Force Base [San Antonio, Texas], where those physicals were being given at that time. It was an 8-day physical; 15 of the 16 showed up. I don't recall the 16th's name or anything at this point—but that would be available somewhere—but he apparently decided that he really wasn't as interested in this program as he thought he was when he put in this application. So, the 15 candidates then went through that physical, came down, and were interviewed by a group of NASA managers here. I know Al [Alan B.] Shepard [Jr.] and Deke [Donald K.] Slayton were in that group. Max [Maxime A.] Faget was there. I would have to, again, refer to the records of all the people that were interviewing us. Oh, Chuck [Charles A.] Berry, the flight surgeon, was obviously in the room. And based on that, six were selected then as astronauts.

At that time, they didn't have any of this idiocy of calling people "candidate astronauts." Once you were selected, you were an astronaut. And I have argued with the local powers that be here and tried to foment rebellion within the astronaut corps not to allow themselves to be called "candidate astronauts." It's ridiculous, this! If somebody's not going to qualify and not going to make it, they're not going to make it. You don't have to call them a candidate to prove that they're on probation. It's a bit demeaning.

At any rate, these six of us were then put in a position where we had to qualify as pilots. As everybody should know, everybody in the Apollo spacecraft really needed to operate as a pilot-astronaut as well as whatever else they might have been in their previous life. And so, four of us who had no previous pilot experience (or at least not jet pilot experience) were sent to Williams Air Force Base [Chandler, Arizona] for jet pilot training. Two, Joe [Joseph P.] Kerwin and Curt [F. Curtis] Michel, had previous flying experience. Joe had just qualified as a Navy carrier pilot, as a flight surgeon-pilot; and Curt Michel had been in the Air Force and flew F-86s (if I remember correctly) during the Korean conflict. So, he just had to get back into thinking about being a pilot again.

The other group—consisting of Owen [K.] Garriott, Ed [Edward G.] Gibson, Duane [F.] Graveline, and myself—then were sent to Williams Air Force Base for what's called—was called their Undergraduate Pilot Training Program. And about 3 weeks after we arrived out there—actually, the news broke even before we were sent. But Duane Graveline ran into a bit of a problem (it turned out for him a great problem) in that his wife sued for divorce. And that, at that time, was not a very acceptable position to be in; and he resigned after about 3 weeks of pilot training. So, the other three (Garriott, Gibson, and myself) remained at Williams Air Force Base and went through the full Pilot Training Program. Except for the military instruction that the Air Force cadet-pilots had to go through.

Most of our group, the class of '[19]67A, were Air Force Academy graduates. So, it was a really remarkable group of young men that were going through pilot training, all of whom were about 10 years younger than we were. So, I would—became terribly impressed, not only with the quality of the people that were becoming pilots in the Air Force, but the unbelievable (in my estimation and experience) professionalism of the Air Force training staff—the Captains and Majors, for the most part (some Lieutenants), that were part of Air Training Command and just did a really remarkable and highly professional job. They really—you can understand why American pilots do as well as they do in combat as well as in peacetime, because of the quality of the training they're getting.

BUTLER: That's very good to hear. While you were down there, how was the environment for you? This was obviously new for them to have scientist-astronauts come train with them, and new for you as well, not having had the pilot training before. What was it like from both sides?

SCHMITT: Well, I, of course, don't know the Air Force side. I'm sure they had some difficulties putting up with these three civilians who appeared on the base. If I remember correctly, we were told we were the first civilians ever to receive Air Force pilot wings and—after our 54 weeks of training. The Air Force seemed to tolerate us. We were probably a bit of a thorn in their side, because we could not be disciplined in the same way that the military pilots. They could just tell NASA, "We don't want them anymore," I guess, and then deal with NASA at that point.

From our perspective, I think we fit in very well. We were spread academically throughout the class in terms of the—well, I believe that Ed Gibson graduated number 2 in our class (if I remember correctly). (He'll have to verify that.) Owen Garriott was a bit higher. I was a bit farther down. I had a harder time learning how to fly these things, particularly on instruments, than the others did—for whatever reason that was. Eventually, I succeeded. But it was a bit traumatic for me to try to work my way through that.

And in the middle of all this, just as we transitioned from T-37s to T-38s, in a basketball game I broke my elbow. So, I had to sit down for a few weeks while that elbow healed, and then try to catch up. (Which I ultimately did.) But it meant an awful lot of flying, awfully fast. Which was fine. That's the best way to learn, I think, is just to get all your flying in at once.

BUTLER: Jump right in there and do it.

SCHMITT: But we, I think, had a good—I think everybody was satisfied with the relationship with the Air Force. They continued to do that for the next group that came on board a couple of years later. And I really, personally, cannot speak too highly of the quality of the training that we received. It really was a remarkable experience.

BUTLER: Good. Going back a little bit, when you did learn of your selection to become an astronaut, what were your thoughts at the time?

SCHMITT: Well, I learned about my selection about 3 days before anybody else. And that was a result of a phone call from a consultant to NASA who was basically the head of aerospace medicine for NASA—Dr. [W.] Randolph Lovelace [II]—who had a clinic in Albuquerque [New Mexico]. I'd never met him before and, unfortunately, did not meet him afterwards. (He was killed as a result—or died as a result of an airplane accident about 6 months after this all happened.) But I was in my office in Flagstaff; and Fern Beeson, my secretary, informed me that a Dr. Randolph Lovelace was on the phone and wished to talk to me. And I sort of vaguely knew who he was because of my New Mexico connections; and so, I talked to him. And his first words were, "You don't have to worry about anything. You're in."

Well, what he was referring to is that both the Air Force doctors and the NASA doctors were a bit taken aback that I had had, in 1960 (and we're talking about 1965 now)—in 1960, I had had what's called a partial colectomy in order to repair a congenital defect in my intestines; namely what's called a malrotation. And as you can imagine, Air Force doctors and NASA doctors, who are used to eliminating anybody with any abnormality, were not too enthusiastic (as I gathered) about my becoming an astronaut. But Dr. Lovelace apparently was asked to review the case. He went to the surgeon—a Dr. Claude Welch, a famous surgeon at Massachusetts General Hospital, who did the surgery back in 1960—and they reviewed it and concluded that I would, as in Dr. Lovelace's words, "would be better off than anybody else because of the geometry of my intestinal tract."

And so, he said, "You're in." So, that—I guess he figured that I had been concerned that I was going to be eliminated on the basis of that operation. It turns out, I think, that everybody in our group had something that potentially could have eliminated them if you applied the strict standards that the Air Force normally would apply to a young pilot. But 10 years is a long time physiologically, as I've learned more about it subsequently; and where in your—when you're 18 or 19 or 20, you will look extremely qualified physically, 10 years later almost everybody has something that you can find out about them that might disqualify them. Whether it's eyesight or a bout of osteomyelitis, which one of the people had and came through okay on, but another didn't. And so, it was a—I didn't envy NASA in trying to make these kind of decisions for people who were in their late 20s and early 30s.

BUTLER: Human body's pretty intricate. Once you got through with the pilot training, what did you move on to next? Is that when you moved down to Houston?

SCHMITT: We immediately came down and took up training, general training—classes primarily—and tours and familiarization with the Saturn Vs and stuff like that with the fifth group of astronauts, who had been selected while we were in pilot training. These were pilots that basically we ended up being in the same training class with them, even though we were senior by one group. Because of the pilot training, we had to go through the classes with them.

And so, the first year probably (maybe even more, again you—all of this tends to merge into a haze with time), but we were training in a wide variety of engineering and spaceflight courses with the fifth group of astronauts. This included people like Fred [W.] Haise [Jr.] and Vance [D.] Brand and Jack [John L.] Swigert. That whole group of people who really became the second wave of the Apollo astronauts. I was the only one of our group that ended up being directly involved in Apollo. But most of the pilots who were in that sixth group did have some direct or indirect role in Apollo.

After that (and during that time)—again, in those days once you were selected as an astronaut, they expected you to perform at an immediately high level and at the old 8-day week/16-hour days that everybody else in the Apollo Program was performing at. And so, we also took on some assignments from the Astronaut Office to oversee certain aspects of the engineering or operations of the Apollo Program as it was developing. That was a very, very important part of the success of Apollo.

And I don't think it's gotten nearly enough credit, of what Al Shepard and Deke Slayton decided to do with the astronauts who weren't directly involved in flights. Weren't assigned to flights. And they were—maybe partly because, "What else do we do with them?" but partly because I think their insight was correct. In that having astronauts who were going to be at the tip of the spear, part of the review teams, part of the Critical Design Reviews, getting very familiar with certain aspects of the spacecraft, the rockets, and the planning and operations of Apollo, was extremely important in making it real to everybody else that people are actually going to use these things that we're building.

It was a, you know—in a way—an adjunct—an important adjunct to the Snoopy Program, which was the Quality That You See Program that was developed, where the astronauts gave Snoopy awards in the factories and assembly plants and—throughout the country for really high-quality work. I really was impressed with how that whole program of quality control and quality management came together and, again, was an extraordinarily important part of the Apollo Program. And the astronaut's role was two-fold: both a visible part of that quality control, but also a working part of it, in that as an astronaut thinking about flying, you bring a different perspective to the engineering, planning, the design of equipment. And we ended up having an office full of experts on different things.

And all of us became experts on a lot of—I mean, not—non-experts on a lot of things. Because each Monday morning there was a Pilots' Meeting (as it was called) (I guess that name comes from the days in the Service) in which the people on the various assignments would be back in Houston and report on any anomalies, any problems, anything of that kind that everybody—they thought everybody should know about. And decisions from particular—big decisions, the Office came to some kind of consensus on how we ought to proceed (if Al Shepard and Deke Slayton's decision can be called a consensus).

But still they had information, coming in from everywhere, on these various spacecraft activities. An extremely important part, I think, to the overall management of the Program. And I never met any of the senior managers that resented it in any way. Maybe some of the younger ones thought that we were in their knickers about some of these things; but still, it was important. For example, what it did was create a knowledgeable base. When the Apollo 1 fire—you could draw a Frank Borman in as chairman of the investigating committee. When we needed a new ascent engine, you could assign Charlie [Charles M.] Duke to lead the Tiger Team to get a new ascent engine for the lunar module. And so, you had people who were extremely knowledgeable about what they had to do.

I, myself, sort of assumed a hardware responsibility. My assignment was to oversee the development of the scientific experiments for Apollo; the so-called Apollo Lunar Surface Experiment Package [ALSEP]. But also in doing so, I realized that nobody in the Office was watching over the development of the descent stage of the lunar module. So, I just sort of assumed that responsibility and began to report back to the crews on how that was going. There wasn't an awful lot of engineering detail that might interest the pilots in the descent stage, even though that was the engine that was going to get them down to the surface. It was—I got interested because it was a—initially interested because it was where we were going to store everything we were going to use on the lunar surface. And so, I began to take that kind of responsibility.

And this was, I think, typical of everybody. If you saw a problem, you began to work the problem; and nobody ever resented the fact that you started to work a problem, whether it had to do with something going on in real time in a flight or whether it was something that you anticipated might be a problem later on. That whole environment, I think, sort of disappeared for a while. And particularly as we led up to the *Challenger* [51-L] accident. It's been my strong impression from things that have been said that having astronauts farmed out around the country, watching over hardware, really wasn't taking place in that time. And the best sign of that is that the—apparently none of the astronauts were aware of the anomalies in the solid rocket booster that had been identified by the Marshall Space Flight Center [Huntsville, Alabama] during some of the earlier pre-*Challenger* launches.

And in the Apollo days, somebody would have known about that. For example, Stu [Stuart A.] Roosa was, for a long time, our man with the boosters. And he would attend all the design reviews, the anomaly reviews, and things like that. And if there was a problem like that, he would have noticed it and he would have brought it back and reported it to the rest of the Office. That didn't seem to be happening in the Shuttle Program during that period of time; and I think it's very, very unfortunate.

BUTLER: Very unfortunate. Your work with the lunar surface experiment package and with the descent module: at what stage did you come in—at what stage were those experiments? How far had they been developed? What was the work being done on them? And then, what did you do to help bring it to the stage where they were actually used?

SCHMITT: Well, when we arrived back from pilot training, the ALSEP (the Apollo Lunar Surface Science Experiment Package) development had, as I recall, just about reached a Preliminary Design Review stage. And Bill [William A.] Anders and I were assigned to go up to a Preliminary Design Review at Ann Arbor, Michigan, where the whole thing was being coordinated with—by the Bendix [Aerospace Systems Division] Company.

When we got there, we were absolutely astounded by what we found! We found that they had designed these experiments—this package—to maximize the workload for the astronaut, not to minimize it. And Bill Anders immediately introduced what he referred to then (I don't know whether he remembers it now) but I call "the Anders big red button concept" in that what he—the ideal was for the astronaut to go over and kick a big red button, and the ALSEP would deploy itself.

Well, by then it was too late. Because they had received guidance (again from astronauts) some time before that—"to give us something to do on the surface" (and these were from non-geological astronauts who apparently felt that they were going to be bored on the surface of the Moon, which is a little bit hard to believe). So, when Anders and I got up there, we had to do an awful lot of work to get this thing down to the point of where it was not going to occupy all the time that the astronauts would have on the lunar surface.

For example: They had a particular type of fastener holding this whole thing together, called a calphax fastener, that required a one-and-three-quarter turn to release; and there were 50-some of these fasteners. And the—and doing that kind of work in a suit was going to be time-consuming and would take a lot out of the muscles of the hand. And we finally got that down to a new kind of fastener—19 new kinds of fasteners—that required only about a quarter turn to release. And just flick your wrist, and it's released. That's the best we could do. We, of course, wanted to have no fasteners, but there was a limit on what we could do.

So, we were making those kinds of inputs. And heaven only knows now how many Requests for Change we introduced from the Astronaut Office at these—this particular

Preliminary Design Review. But by the time the Critical Design Review came along, things were far better control; and the best we could expect, probably. There were almost certainly some other changes that we requested (I don't remember any right now). And that—what the result was, and the dedication that we saw with the people building the ALSEP, which we saw everywhere in the entire program (hundreds—over 400,000 Americans were equally dedicated in all aspects of Apollo), the ALSEPs performed far beyond anything they had been told to design them to do. And that is a credit to the people, of course, not only at Bendix but all of the sub and sub-subcontractors who put together the individual experiments and the principal investigators, who were behind the concept of each of the experiments. And the central station communication systems and so forth.

So, I think overall, we have to be fairly pleased with the way the ALSEP came out. Even though, as a geologist, I would have rather spent the time wandering around, checking on the geological formations and rocks of the Moon.

BUTLER: Understandably. As you were working with the ALSEP, did you also do some work with the tools that you would be using, both to deploy the ALSEP and then geologic tools as well?

SCHMITT: Well, that was a major part of this assignment that I had, is to try to keep track of all the different tools and make sure we were pulling together a suite of tools that would be useful to us on the Moon. There is another place where Eugene Shoemaker made a great contribution.

His initial thought (and one which we had had together, while I still worked at the US Geological Survey) was a concept called the surveying staff, so that we could automate not only the photography but the positions and the orientation of the cameras and everything could all be done in one staff that—and all this data would be automatically recorded. You'd have a range finder, and that would go back on the data link. And so, the astronaut would be free to do the things human beings do best, and that is observe and think about what they're seeing.

Unfortunately, that particular concept fell apart, not because NASA didn't try to do it but because they were—as I understand it, were forced into a small business set-aside contract. And whereas a company—a large company like Kodak, say, could have done this and had demonstrated they could have done these kinds of things very quickly, a small company that was given the contract just never made it. We spent about $1M (in those year dollars) and got nothing for it.

And as it became clear that wasn't going to happen, then more focus came on the tool—the small hand tools—that we could take. Of course, the rock hammer; the tongs for—reverse tongs, in a sense, for—the spring would close them on a sample and you could lift it up; a scoop to get the soil samples. Just very simple geological tools, which have worked well through several hundred years and probably will continue to work several hundred years in the future. But still, it requires a great deal of human action. And the whole idea then—as well as in the future—of putting human beings on the Moon is to minimize the number of routine tasks that they will have to do so you can maximize their contribution for what human beings uniquely do; and that is, look, manipulate in detail, and also think about what they're doing. And what they see.

But still we did the best we could. Shoemaker's and his group, I think, greatest contribution to the science of field geology was from a tool point of view was the what's called the gnomon. It was a device that we would set in each of the pictures (particularly stereo pairs)—a device that had a gimbaled vertical rod that would give us the vertical orientation of the gravity field. It was of a known length (40 cm, if I remember correctly), had a gray scale on the rod, on—it was a three-legged device. Its shadow—the shadow of that vertical rod—would give us azimuth, because we knew where the Sun was, of course, and we could always tell what that was from the shadow on the ground. And there was a color scale as well on one of the legs that can be used in order to print the color pictures more accurately.

Unfortunately, that's a problem that NASA photography (in spite of all their wonderful work) has a hard time believing, some of the colors. And you see it most in the pictures of the orange soil that we found on Apollo 17, in that there's an international red or orange on that leg of that gnomon, and they refused to print to that color. And so, the orange soil never looks as orange to you in a picture as it did to us while we were on the Moon. A recent book by Michael Light, *Full Moon*, has digitized that—some of that picture; and it does show (at least in one part of a panorama)—it does show the best orange for that soil that I've seen yet. But still not quite as bright as it was for us.

BUTLER: You know, that's interesting.

SCHMITT: So, these tools—and in addition, of course, there was the camera. Now the Hasselblad camera has a long history. It was adapted for lunar surface work, but it—as I recall the story, Al Shepard bought the first Hasselblad in a shop down in Florida just before his suborbital flight. NASA was not going to supply him with a camera, which shows how every once in a while you come into little glitches of thinking. (Just like on Apollo 8, we had entirely the wrong television camera on board; and we spent a very hectic one night trying to figure out how many filters and what filters we could put in front of it to actually get a picture of the Earth. It ended up, we put all the filters in front of it to get a picture of the Earth. It was just too sensitive.)

But Al Shepard bought the first Hasselblad, and then after that Viktor Hasselblad, the owner of Hasselblad in Sweden, as I understand it, volunteered to adapt it in various ways for more easy use by the astronauts. He introduced for the first time a motorized winding system, a trigger system so that we could operate it on the surface. A number of things were done to improve the Hasselblad for surface work. Large magazines that they hadn't had before. The rissole plate that gives the little plus marks on the pictures; that was put into it for geometric control of the developed film. All of these things came as a result of Hasselblad's personal effort.

BUTLER: It's great. We'll take a quick break here, and we're going to change the tape out.

SCHMITT: Okay.

BUTLER: We were talking about—you just finished talking about the cameras, and how those had been adapted, and some of the other tools. I ran across in one reference that actually during your mission, Gene Cernan used the hammer more frequently because it was more suited to his size. Was that something that was an actual situation? And how did that develop?

SCHMITT: All right. There's no question that he could handle the hammer better. He had a larger hand, and the handle was never—I—we never got the design of the handle so it would universally be compatible for all the astronauts. And Gene just had a very big hand; and he could—he could grip it better and not get as fatigued using it. (I used it every once in a while.)

But on the other hand, for my purposes, the scoop—for geological purposes, I found the scope much more versatile. I could pick up rocks with it—small rocks—and I—but I could also dig trenches and do all sorts of other things with it. So, the scoop was perfectly fine for me; and it gave me something to lean on, to look down. So, I was perfectly happy with the scoop.

BUTLER: A wide variety of tools—

SCHMITT: That's right.

BUTLER: —that all came in useful. As you were becoming more involved (after you had finished your initial training with the pilot training) and then were working, going through with the other class and getting some more of that initial training, and now working on the ALSEP and the tools, when did you become involved with more specifically training on Apollo itself for selecting sites or becoming involved specifically on missions?

SCHMITT: During all this period after I competed pilot training, I was working in a variety of other roles. One was as a member of the Planetology Subcommittee of the Space Science Committee that NASA and the National Academy had put together, chaired by Harry Hess.

The Planetology Subcommittee asked me to be a member, and the Manned Spacecraft

Center approved that I should be a member. And we did a number of things. The most important thing, during that period of time, was to select the principal investigators for the sample analysis program. And strangely enough, Gene Shoemaker was the chairman of that subcommittee—of the Planetology Subcommittee. I should mention here also, he was chairman of the Astronaut Selection Committee for the National Academy of Sciences as well. And whether that had anything to do with me becoming an astronaut, we'll never know. (I think it was more objective than that. I hope it was.)

But Gene did chair that. And it was a very—it was a critical committee for the ultimate success of Apollo in that an announcement of opportunity was sent out for people to send in proposals to work on the lunar samples for whatever specific purposes they thought were important; and we had a very, very broad response to that announcement, including a response—intentionally, a response—from abroad. And Gene Shoemaker should get a lot of credit for having really created the first international science program that NASA was deeply involved in, and it had to do with the analysis of the lunar samples.

And our committee specifically selected people from countries outside the United States, and maybe even biased our quality selection towards—a little bit towards some of those countries, where we knew investigators were of very high quality. Maybe they didn't have yet the equipment that had been funded by NASA in many of the laboratories in the United States. But nevertheless, we knew they would do a good job; and so we internationalized the sample analysis program. I think it was a very important contribution that Gene and that committee made.

In addition, about that time we were also deeply involved—again the US Geological Survey, under Shoemaker's guidance, was deeply involved in analyzing the Apollo orbiter—

(no, excuse me) the lunar orbiter photography, which was flown specifically to try to identify landing sites, particularly within what was called "the Apollo belt of the Moon," the equatorial belt of the Moon, that were as smooth as possible.

Now you may have heard at various times that the scientists and the engineers were at odds about where Apollo should—the first Apollo should land. That's not true. Everybody realized that the primary objective of Apollo was to land safely and return to Earth. And so—and the equator was the best place to land safely, because you could stay on a free-return trajectory back to Earth without any other burns. As you recall, with Apollo 13, they had to make a rocket burn in order to get back on to a free-return trajectory. But in the Apollo 11/Apollo 12 days, we were still trying to stay within the belt that, if you—after launching to the Moon, if you never had any more opportunity to burn a rocket, you would still come back safely to Earth. And so, that's the belt on the Moon where you could do that, near the equator.

And a number of landing sites were evaluated, in great detail, in various ways, in order to select potential sites within that belt; and they were sites roughly that were a 1-day slip apart that—based on lighting considerations. We always wanted to land with the Sun behind us, at an angle lower than the glide slope—than the slope we were on—in order to come down towards the lunar surface so you could see shadows. If your Sun's lower than that, you're going to see shadows and craters and, on the other sides, rocks. And so that, just typical of one of the constraints that we had, was to pick sites that were smooth as we could tell from that resolution and were roughly a 1-day slip, that if you couldn't launch on the first opportunity, you could slip a day and you would still have a landing site to go to. And all across the Moon.

So, we had (I can't remember now) 6- or 7-day slips that we could've taken and still landed on that first opportunity in July of 1969. That was the plan, and that's what we had available to us. That site selection process was something that I was involved in. More on the oversight basis, because there were so many people that could do the job far better than anything I could contribute! It was more on an advisory or oversight basis, and to make sure that they understood what the crew input was going to be and what the crew would be seeing as they came towards the lunar surface.

In 1968, things really started to get intense. A number of things were happening. And by the way, one of the best resources for a lot of this are George Low's archives, which are at RPI (Rensselaer Polytechnic Institute) in Troy, New York. They're available to researchers, and it is a—I've just started to use them myself, and it's just fascinating the material that's in there. He's a meticulous note-taker and dictator and not political dictator but a writing dictator. And so, that's a tremendous resource—

BUTLER: That's good to hear.

SCHMITT: —for historians. Also a more direct resource is now the *Apollo Lunar Surface Journal* that's on the web, that Eric Jones (Dr. Eric Jones of Los Alamos) put together. Most of the astronauts participated in editing and annotating the air-to-ground transcripts as well as the videotape transcripts. So, those are two really important historical resources.

And I hope that somehow or another that *Lunar Surface Journal* will be maintained on the web indefinitely. I—NASA has put a little money into it, but probably not as much as they should in order to ensure its availability over the long haul. We don't live forever. And right now, it's purely the love of Eric Jones and his crew of people from all over the world that are adding checklists and all sorts of resources to that web page.

BUTLER: It is an excellent web page.

SCHMITT: At any rate, in 1968 a number of things were coming together. Number one: the lunar module development was behind, and the Apollo mission that was going to follow Apollo 7 was an Earth orbit test of the lunar module. Unfortunately, the lunar module wasn't getting ready for that; and in mid-year of 1968, George Low had the extraordinary insight to suggest that we take Apollo—what would become Apollo 8 (then I think it was called C Prime [C'], in the arcane vernacular of the day)—but became Apollo 8—we would take that to the Moon.

We'd exercise everything we needed to do in order to go to the Moon, except what we needed to do if we had a lunar module along. And we'd target it to a particular lunar orbiter landing site, evaluate a landing site on the eastern—near the eastern limb of the Moon, prepare the data packages, do everything, and we'd fly it in 4 months. And that mission was conceived and flown in 4 months! It was probably the most remarkable effort that the NASA team down here ever put together. Apollo 11, landing on the Moon, was what we were hired to do. Nobody mentioned that we were going to take a single spacecraft to the Moon in 1968; and so, yeah, believe me, the level of motivation that generated was absolutely unbelievable.

Well, Frank Borman approached me, asked me if I would do the lunar orbit flight planning for their effort. And that meant that I began to interact with [Howard W.] Tindall's group, the Flight Operations Planning group that met weekly that really was the focus of all of the operational planning for a particular mission. They were looking at all the missions, but the one up was the one they were concentrating on. And that's another tremendous resource.

And I'm not sure where there is a complete collection of what were called Tindall-grams. They were his summary of each of those meetings. I have a partial collection at the University of New Mexico in the files there. Whether there would be a complete collection or not, I don't know. But somebody ought to make a very, very specific effort to get a complete collection of the FOP minutes, Tindall-grams, and to get those in some kind of form and bound. Because that is a resource that should not be lost. I can understand it's hard to put together. I hope somebody has been able to do that.

BUTLER: It's a good idea.

SCHMITT: Yeah. Now that is another—in one place, you have an awful lot of what happened in order to put these missions together. It had a lot to do, of course, with computer programming and things like that of that time.

So, probably in about August when the Apollo 8 mission was approved and we began to work on going to the Moon with Apollo 8, I began to fly—and Frank Borman and his crew went to the Kennedy Space Center (to the Cape) in order to use their simulators, which were always a little bit more up-to-date than the simulators we had here. They just transferred most of their training operations to the Cape. And so, I flew back and forth between the Kennedy Space Center and Houston every week, probably several times, coordinating the development of this lunar surface flight plan.

At the same time, Bill Anders asked me if I would start to bring him up to speed on the features of the Moon that would be underneath their orbital path, and so he would have some better understanding of the geology and what he was going to be looking at and things like that, so he didn't sound entirely ignorant of it. So, Bill and I developed a very close working relationship in the evenings down there, actually in the saunas. We would—because Frank wasn't too sure he wanted Bill working on this, as I gathered. And so, Bill and I would go to the sauna—go into the gym and work out, and then go into the sauna and we'd work on the checklists and the things that we had to do while we were sweating away in the sauna at the crew quarters at the Cape.

At the same time, Charlie Towns, who was chairman of the President's Science Advisory Committee (the PSAC), almost exactly the same time requested from NASA a briefing on what we were going to do on the first landing mission. Now you realize, this was all inside of a year when that landing actually took place! And actually, because of the intensity of efforts, NASA had never really sat down and worked out a total flight plan for the Apollo 11 mission. We were taking it one step at a time. And it was a perfectly legitimate request and a very important request, because with that stimulus of the President's Science Advisory Committee we went to work.

And Joe [Joseph P.] Loftus [Jr.] was given the assignment down here at Houston to coordinate the presentations, and three astronaut teams were assigned to make the presentations. I guess that was a little bit of pizzazz; but again the astronauts were deeply involved in everything that was going on. So, it wasn't an illogical thing to ask us to do, and of course, for the lunar surface—the lunar portion of the mission (lunar orbit and lunar surface), Buzz [Edwin E.] Aldrin [Jr.] and I were asked to do that. I was given the lunar surface part of the mission to pull together, and he took the rendezvous and lunar orbit part of it.

So, we went to work to develop actual flight plans and timelines; and it was the first time that was done for the lunar surface. I was working primarily with the Flight Crew Support Division people, Ray [Raymond G.] Zedekar, people like that. George Harrison (I believe) was one of the men who worked on that. And we put together a 2-EVA [extravehicular activity] timeline for the first Apollo landing on the Moon. And it included, at that time, a full ALSEP, the whole 9 yards of an ALSEP deployment, because that was in the plan. That was what—we just took what they had been assigned to the first landing on the Moon and tried to work that into a timeline for that mission.

And in the process of putting that timeline together, it became clear that the chances of us ever flying a full-up ALSEP were very small. It was over 300 lbs. We still didn't know how much margin we were going to have in fuel for landing on the Moon. It was a clear candidate to be offloaded at the last minute in order to increase, by a number of seconds, the amount of time we had of fuel.

And so, I made a proposal that we begin to look at an alternative, which would be a slimmed-down version of an ALSEP, something that would not have a radioisotopic thermoelectric generator, because that was quite heavy, but would have solar cells to power the experiments through 1-day cycles on the Moon. And I began to make phone calls around the country, asking my geological colleagues and geophysical colleagues, "If you thought you would never have more than one mission, what were the priority of experiments that you would want to have on the surface of the Moon?" And it became very clear, very soon, that everybody (geophysicists, geologists) were saying, "We need a seismometer. We need to understand as much as we possibly can about the seismic environment of the interior of the Moon. And we want a corner cube reflector."

And so, that's what we, in this presentation for PSAC—we said, "There are two alternatives now. And in recognition of the primary function is landing on the Moon, and that the ALSEP is vulnerable to being offloaded in order to save weight, we need to begin right away to design this backup science package." Well, when we started to present that here up through the chain before it went to PSAC (the first presentation was to Bob [Robert R.] Gilruth)—and I made this presentation.

And when I flashed up this sign, this view chart that indicated an alternative to the ALSEP, the—Bill [Wilmot N.] Hess, who was then Head of the Science Directorate here at the Manned Spacecraft Center, just came out of his chair. He was livid that anybody would suggest that the ALSEP might not fly. And he was gently reminded that this was always a possibly, and that NASA always thought about the alternatives, and things like that; and ultimately, that's what we had to do. We did design that little package of experiments that—what Neil [A. Armstrong] deployed was a solar cell-powered seismometer and a corner cube reflector that is still up there and still operating, still giving signals back primarily to (I guess it's) Lick Observatory near El Paso—McDonald Observatory near El Paso has been the principal beneficiary of that. But it also became apparent, during those briefings, that the probability of having two EVAs on the first mission (two external excursions on the first mission) was very, very low.

And that the further caused us to think about one more time whether one astronaut or two astronauts got out on that mission. And I had a lot of discussions about this with Deke Slayton. He was adamant (I certainly agreed with him), I think everybody was adamant that we always operate on a buddy system, that both astronauts were getting to get out of that spacecraft, and that we'd better design it so that can happen. Make sure our plans are such that they can happen. There's always going to be a contingency where one of the portable life support systems doesn't work, and that one astronaut goes down, touches the surface, gathers a sample, and comes back.

And as a matter of fact, that PSAC briefing also brought to the fore an idea just called a contingency sample. And Neil had a little bag and scoop that he would use in order—if that—if it came to that—they couldn't walk around or do anything other than get down and touch the surface—he would have something to grab a contingency sample and bring it back into the spacecraft. So, those two things going on simultaneously, the planning for Apollo 8 and the preparations for briefing the President's Science Advisory Committee really kept me humping in addition to trying to keep track of what the lunar module descent stage was doing in its development.

And at the same—and also at that time, I had—was having to try to organize a brand new training program for the Apollo missions, particularly the post-Apollo 11 missions. That had started, really, late in 1966 when I, after having thought about what was going on in the science training of the astronauts, having participated in some of the lectures, participated in—or had attended some of the lectures (not given them), participated in several of the field trips that the astronauts had been given as grouped—as a group, it seemed to me that we were really off track.

Number one, we were boring the astronauts. They were not interesting. The intent, apparently, was of the people then in charge to create—in their words, "create astronauts with Master's degrees in geology." Well, that's not what we needed. What we needed were focused, relatively narrowly trained field geologists who primarily were pilots, but people who could select—who could observe and select the widest variety of rocks, could tell us about the context in which each of those samples came from, and so forth. And since simulation training was working so well and all other aspects of our training, I went to Al Shepard with a proposal that we, number one, begin to focus our science training on an actual simulation of lunar traverses in areas on Earth where we could learn something about the kinds of problems that we would encounter on the Moon. (There are no real analogs on Earth, but we could focus on concepts and actually run short traverse exercises during this time—during the time they had for science training.)

And number two, is to go out and recruit the best teachers that we could possibly find to do this, who would not only—were very good scientists but very good teachers, and who could understand what we were trying to do and not be trying to change our emphasis, but to live within that. And in so doing, I called people like Richard Johns, who was a old teacher of mine at Caltech, one of the best field geologists that ever lived. I got a hold of Bob [Robert P.] Sharp, also a Caltech professor of mine who was the best observer of detail and what he called "belly geology" that I had ever encountered. Lee [Leon T.] Silver was invited to participate. And I called a number of lecturers, particularly people I knew, from Harvard such as Jim Thompson and Jim Hayes, Gene Simmons, and others to prepare lectures that were stripped of the vocabulary of geology and used the vocabulary of general science (of physics and chemistry) rather than the vocabulary of geology.

And so, the lecture program was reoriented much more towards what would be useful to them when they got to the Moon. And the field program, about a once-a-month training plan, was reoriented towards simulations, where the field geology was learned and geological concepts were learned, but in the context of actually performing as if you were on the Moon in everything but a pressure suit. That made all the difference in the world, I think. To Al

Shepard's credit, he agreed that the Astronaut Office should take over the management of the science training. He said, "Go ahead and put it together;" and so, we did. And the first crew that we really worked with in detail was the Apollo 13 crew.

But before I get into that, I should mention that we were standing on a foundation of work already under way related to training, related to equipment, to plans, and objectives of field geology on the Moon that the US Geological Survey people (under Gene Shoemaker) had already been working on for a long time. And they were really the technical core of our training program. We always depended on them to organize the logistics of the trips, to recommend the sites that we would go to, to lay out the traverses that we would want the crew to work on (obviously in conjunction with everybody else). But they were really the hard rock core (pardon the expression) of the training program throughout the Apollo Program. They don't get nearly the credit they should. But they were. Without them, we couldn't have done it.

It began with Gene Shoemaker as a principal investigator for Apollo 11 and 12. Gordon Swann then became principal investigator for Apollo 13, 14, and then 15. And then Bill [William R.] Muehlberger from the University of Texas (but also he was employed by the USGS in this context) became the principal investigator for Apollo 16 and 17. And on top of that, we had the assistance of Lee Silver on Apollo 13 and 15. We had Dick Johns on Apollo 14. Lee also helped out a bit with Apollo 16 and 17, but Muehlberger was sort of the lead guy on all of that.

So, it—we really brought in a national program of training that, I think, resulted in what we have today. And that is a first-order understanding of the Moon as a planet; and, as a consequence, a first-order understanding of the early history of the Earth, of Mars, and the other terrestrial planets. And I'm not sure we could have gotten all of that without this reorientation of the training on top of this really fantastic sample analysis program that had been put together in about the same timeframe.

One thing that is implicit in all of this is that we were allowed to do all of this by people like George Low and Bob Gilruth and Sam Phillips and others who were the senior managers, primarily because (I think) right from the beginning almost (certainly from the late-1960s—the mid- to late-1960s) they realized they were going to be successful. They had all the confidence in the world they were going to land people on the Moon and return them safely to the Earth, [President] John [F.] Kennedy's challenge.

But once we did that, we were going to have a capability to explore the Moon. To do things far more than just that; that's what the lunar module looked like. And so, the various modification programs were put in place well before Apollo 11 that were leading us towards a Block II lunar module that could be—ultimately was used on 15, 16, and 17, to have a—some kind of mobility on the surface (for a while there was a competition between a lunar flyer and a lunar rover; the lunar rover obviously won out), and that was all done.

But that was done—to the credit of Low and others—in anticipation of success on Apollo 11. It wouldn't have been ready had you not anticipated that success. So, they were as much a part of the science experiment as anybody else, because they encouraged us to go ahead and—us—I mean the scientific community—to go ahead with these kinds of plans. And indeed, put the money into the redesign of the—and upgrading of the lunar module and of the lunar roving vehicle.

BUTLER: Very forward thinking.

SCHMITT: There's no question. And again, they don't get the credit they should, particularly from the scientific community. And that was an unfortunate aspect of Gene Shoemaker's later years. He became disillusioned, and he—I don't think he ever realized how much influence he actually had on the program and how much the NASA management (that he on occasion criticized publicly) did in order to make these science programs achievable.

BUTLER: You mention the support from the NASA managers to help this program go. What did the other astronauts, the pilots that had for so long not done this detailed geology work—how did they receive these changes? Were they eager to accept it?

SCHMITT: As near as I could tell, they became eager. But I focused on one team, as I indicated earlier: the Apollo 13 crew, Jim [James A.] Lovell and Fred Haise, with John [W.] Young and Charlie [Charles M.] Duke as backup. And I persuaded Jim Lovell to take a chance on us. I told him, it would be fun; it'd be a nice weekend. "But let us have you for 3 days out in California with Lee Silver, and let us demonstrate this kind of training." And it was in the Orocopia Mountains of Southern California. We went out there and camped way away from everything. There was no press or anything around. And we—the people—the Caltech people put together the meals and had some staff assistants. (They didn't get paid for it. They just did it.) And we went out for 3 days and ran traverses like we talked about. And then we'd go back over them so they could see what an experienced geologist would have seen and the critique was done in near real time (as we say).

And Jim and Fred came back very enthusiastic about this, as did John and Charlie; and that enthusiasm was such that they said, "Yes, this is what we want for our training program." And once they had gotten into it, they persuaded Dave [David R.] Scott, who was going to be the Apollo 15 crew, to undertake that same kind of intensive simulation-based training. It was integrated to a lesser extent into Al Shepard's mission, Apollo 14, primarily I think just because of the nature of the people.

People are different. That mission came together a little bit late. Its focus changed somewhat, because you had to demonstrate that we'd solved the problems that had caused the near-fatal accident in the Apollo 13. So, it was more difficult to work with that crew at this level of effort. I, also, had, by that time, been assigned as a backup crewman for Apollo 15. I couldn't put as much—any, really, effort. (Once you're on a crew, you've got to focus on that crew.) And so, 14 did a wonderful job. It was an extremely important site, and they did a great job! But they didn't have the focused training that the Apollo 13 crew went into.

I should mention, though, that the Apollo 12 crew, in spite of the fact that they had to be prepared to duplicate the Apollo 11 mission, and then once Apollo 11 landed with somewhat uncertain accuracy at their landing site, they had to get into the business of pinpoint landing at a *Surveyor* site, which was a very important effort by NASA and not just a "gee whiz" demonstration that we can do it, but it meant that we were going to open up a whole bunch of more complex areas for science landings later on (science-oriented landings).

And so, in spite of all that, Pete [Charles] Conrad [Jr.] and Al [Alan L.] Bean worked very hard at their geology, primarily run by the US Geology—USGS astronaut—(excuse me) the would-be astronauts, the USGS geologists. Gordon Swann was leading that effort for Gene Shoemaker and just did a wonderful job. He deserves, again, a lot of credit for not only that effort of getting the—Conrad and his crew up to speed in geology, but increasing the acceptability of that kind of intense science training as part and parcel of an Apollo cycle.

Swann also (as a little sidebar)—he was the one that I introduced to Gene Kranz; and

Gordon brought to Kranz the concept of a science support room that we had worked on even back when I was still at the US Geological Survey. We had thought about how would a group of scientists interact with a whole bunch of engineers in support of a particular mission in real time. And he had followed up on that, with some other of the people there (Ed [Edward] Wolfe and Bob [Robert] Sutton and others) at the—at USGS in Flagstaff; and they were able to give Kranz a demonstration as well as a full-up briefing on what a science support room could do to enhance a mission.

And Kranz bought it (to his credit). And they began to work out, "How do you put together that kind of support and science that we knew we had in all other spacecraft systems and operations?" So, that was an introduction that I'm very proud of that really ended up contributing a great deal to what we were able to do. And if nothing else, it contributed to the enthusiasm of the scientist to work inside Apollo rather than outside.

BUTLER: That was a role that sort of evolved for you, it seems, that you were an interaction between the scientists and NASA and the astronauts particularly, but also engineers and Mission Control. Did that just kind of evolve as you went along or—?

SCHMITT: Well, it evolved as we went along. But I think it was probably a primary reason that the National Academy (some people, Harry Hess and others of the National Academy) kept pushing NASA to bring scientist-astronauts into the program. So, even though we—it was our responsibility to make it happen, and we never talked to them about it, that's what we tried to do.

And, of course, Owen Garriott and Ed Gibson and Joe Kerwin did the same thing for the Skylab in the Earth orbit flight. They brought their science (solar astronomy in particular, but also biomedicine) into—and their colleagues, and they acted as an interface during that period of the early spaceflight days.

And so, yes, I think that's really the role we played. And we passed it on to the next team of scientist-astronauts that came in after we did and became ultimately payload specialists, mission specialists, in the Shuttle Program.

BUTLER: You talked earlier about how you worked with Apollo 8 and—even in the sauna, to work on some of the orbital geology work for Bill Anders. How then for example, Apollo 10, looking forward to Apollo 11, how did you work with those astronauts? Or what role did that play?

SCHMITT: Well, when John Young, who was on Apollo 10 as Bill Anders' role (the command module pilot)—well, actually, no, that's not true. Erase that.

BUTLER: Okay.

SCHMITT: Gene Cernan was the lunar module pilot, which was Bill Anders' role. But John Young decided that he wanted to learn—wanted the same kind of briefing that Bill Anders got about features on the Moon, since Apollo 10 now was going to go around the Moon with the lunar module and Apollo 9 had proven it out in Earth orbit as a follow-on to Apollo 8. So, we finally got back on schedule, even though the lunar module was a little bit late, with Apollo 9. And so, Apollo 10 was going to go around the Moon. And the initial request that I had from John was to provide him with that kind of briefing.

So, I was flying back and forth. But I was starting to run out of time. I was limited in what to do. So, I invited Jim [James W.] Head [III], who's now at Brown University but was then working with Bellcomm in Washington (one of the contractors to NASA Headquarters), and Farouk El-Baz, who was a geologist also working at Bellcomm. I asked them if they would begin to transition and come down and help give these orbital science briefings to the crews—the command module pilots that were going to be flying around the Moon and see if we could gain some significant science from those kinds of observations. And they were quite enthusiastic to do that, and they began to fill that role gradually. (We overlapped a bit, and then gradually I just turned it over to them and let them do it.) Because they got along well with the crews and nobody seemed to have a particular problem with that.

There were others that came in. Again, the US Geological Survey played a major role with people like Hal [Harold] Masursky and Don Wilhelms, who were really the gurus in orbital geology anyway. (They knew far more than anybody else.) And so, they not only had materials but they had knowledge that we brought them into a lot of briefings for the command module pilots and the backup command module pilots.

But with Apollo 10, of course, every crew was always looking for a hook to make their mission stand out a little bit more than the last one. Number one, Tom [Thomas P.] Stafford, who was commander of Apollo 10, decided he didn't want to go to the Moon with a camera like we had on Apollo 8, which was the old RCA Vidicon camera that was initially designed in order to get the first step on the Moon. It's the same camera—it was the same camera in the command module that we eventually had on Apollo 11, looking down at the ladder and the front landing pad so that one—that there would be a video recording of Neil Armstrong's first step on the Moon.

Well, it was a very low light level camera. It had to look in the shadow because

Neil's first step would be in shadow on the Moon. So, they had designed a very low light level camera—what's called a Vidicon—and had not ever thought about that you might want to use it to look back at the Earth. And with Apollo 8, we had this problem of the Earth being too bright for that camera. It just bloomed in the picture. And as a side point: as soon as that happened, well, I gathered a bunch of people over in the optics lab and we went to work simulating the brightness of the Earth and had that camera and tried to figure out how we were going to reduce that light level with what we had on board.

We had a number of filters. Strangely enough, I don't even remember why the filters were on board—the camera filters were on board. But we just ended up taping every filter we had together with gray duct tape (it holds the world together) and when we finally got a recognizable picture of the Earth, it was because Bill Anders was holding that taped-together set of filters in front of the Vidicon. And we finally treated the world to a black-and-white picture of the Earth!

Now Tom didn't want to go through that. (I don't blame him.) And he asked if there was any way to find a color television camera that would be able to be adjusted, so you could get a good picture of the Earth as well as pictures of the crew inside and things like that. Well, the loser in the competition for the first camera on the Moon had been a Westinghouse color wheel camera, and when I began to investigate this, I found this out. And so, it had already had some design consideration for spaceflight. And we eventually were able to persuade the engineering side of NASA and the—most importantly George Low, who ran the CCB [Configuration Control Board] for the Apollo Spacecraft Program Office at that time (as Director of that Office), that we ought to fly a good camera; and this color wheel Westinghouse camera was really outstanding camera.

And because we got it onboard of Apollo 10, it eventually became the lunar surface camera for the Apollo 15, 16, and 17 missions. I can't remember exactly when it first flew. I think it—I don't think we had it—no, we did not have it on Apollo 14. It was 15 the first time it flew on the surface, but it flew on Apollo 10 to give us some good pictures. And there are some good tapes of the crew and of the Earth from Apollo 10 and that kind of thing.

Also, Apollo 10 was originally going to be targeted to the same landing point (even though it wasn't going to land)—but to go through all the motions of targeting, it was going to be targeted to the Apollo 8 landing site—what we called the Apollo 8 landing site (even though it never landed). And it occurred to me that we had already seen that. That the crew had gone within 60 miles of the surface (60 nautical miles of the surface). They looked at it. They had some good photography of it. They felt very confident that, from that distance at least, it looked like it was good terrain to land in.

And it occurred to me, "Why don't we get the same amount of confidence for two more landing sites?" Namely, "Why don't we target for the—next a lunar orbiter landing site to the west, and then if we—since Apollo 10 was going to be a longer orbit mission, we would have a chance to see and look at a third site." So, that meant for Apollo 11, we potentially would have three sites already looked at by the crews, in which we had significant confidence, and had already gone through targeting activities here at—in the old Mission Planning and Analysis group (MPAD) that would, I think, increase everybody's confidence in the—in flying at least Apollo 11.

Well, that was a little more difficult to persuade people on because it entailed a 1-day slip in the Apollo 10 launch date to go to the next landing site. And they already had—they just had to make some slight revisions of all the planning documents (the data package, as they called it) in order to go to the Apollo 8 site. (That was all pretty well taken care of.) And if—here’s Jack Schmitt coming along, saying, “Let’s go one more site downstream,” and that meant all that work had to be redone.

But we put together our arguments. A whole bunch of advantages. Number one, you’d be a little bit farther west, and therefore you had more time for updates to the lunar module after the spacecraft came around the Moon and you had acquisition of signal (AOS) again. We had all the advantages again of Apollo 11 having two sites to go to. Or three sites, actually. A whole bunch of things. And and we started briefing this up through the system.

And we finally got to Chris [Christopher C.] Kraft [Jr.] with this briefing. And he was obviously skeptical. (He was then Head of Flight Control Directorate.) And we made this presentation. And one of the last things we had on our list was that instead of landing before sunrise in the South Pacific on recovery day, you would land just after it—sunrise with a daylight landing. And when I said that, Jerry [Jerome B.] Hammack (who was recovery officer at the time) said, “All of that and a daylight landing, too?” And that sold it really to Chris.

Now we hadn’t sold it to anybody else. But Chris became an ally and began to—(I think, probably internally) to talk about it some. And we finally, though, late one night (and in those days, everything happened late at night; it seemed to be significant)—it must’ve been 9:00 when George Low called Tom and me over to his office, and Sam Phillips was there and—in order to talk about this one last time. And George was not too enthusiastic about the whole thing. But we gave him the briefing on why we thought we ought to slip one day, and he thanked us and we left.

Tom and I left Building 1, saying, you know, "That's it," you know, "George—he's not very interested in this. Let's go off and worry about other things." And the next morning (I believe it was the next morning), well, Tom called me in said, "Well, George bought it." And what I think may have happened is Sam Phillips bought it; and Sam and George talked about it, but I have no inside knowledge. I think that somehow that evening, it happened. And it meant launching—slipping from the 17th of May to the 18th of May for the launch of Apollo 10.

And by doing that then, it became absolutely clear that Apollo 11 was not going to redo this again for a different landing site, and so they used the Apollo 10 data package. And that's what determined us going to Tranquility—what became Tranquility Base on Apollo 11 was that little song-and-dance about slipping the launch day of Apollo 10 one day.

BUTLER: Now that's something interesting that I don't think many people are aware of. It's very interesting, all the intricacies of the planning that you all did.

SCHMITT: But it's illustrative of the way Apollo worked. And if somebody had a good idea and you could convince a few other people you had a good idea, you could work it up through the system and eventually somebody would listen to you. It was never stopped. There were really no barriers to good ideas. And it worked in the planning stages, and it worked in real time during a mission. If a problem developed during a mission and somebody had a good idea on how to solve that problem, you had as much chance of getting that idea pushed forward as anybody else.

And it—so much of Apollo's success depended on that. And I think the reason was, it was a very young Agency. One of the biggest flaws in an otherwise very (I think) credible movie, and that's *Apollo 13*, was the representation of the ages of the young men and women who were in Mission Control during that—dealing with that problem that occurred on Apollo 13.

The movie would lead you to think they were in their forties, their late thirties, early forties. In reality, they were in their twenties. They just came out of engineering school. They had just been hired from Rensselaer and Tulane [University, New Orleans, Louisiana] and LSU [Louisiana State University, Baton Rouge, Louisiana] and VPI [Virginia Polytechnic Institute & State University, Blacksburg, Virginia] and places like that to work, you know, for Chris Kraft. And it was true of the whole organization. They were people in their twenties who really believed that they were doing the most important thing they were ever going to do with their lives. And it wasn't to beat—initially, it may have been to beat the Russians to the Moon. But that was—everybody knew that race was over in '65 (probably) with the successful launches of the Saturn V launch vehicle. Even the Russian émigrés will tell you that, that they basically, even though they tried to launch the N-1 rocket later and it blew up, they really had pretty much given up (in their own minds) when we successfully launched the Saturn V.

And we sort of sensed that over here. And that everybody believed (450,000 Americans believed) that this was something we ought to do. We ought to go to the Moon. Ought to return safely. It's important to whatever. Some of them patriotism; some of them humankind. Whatever it might have been, it was the most important thing they could do in their lives at that time. And that's why they worked those 16-hour days and 8-day weeks. That's why you didn't do it for $20M—$20B. You did it because these people believed it ought to happen.

And that kind of environment is just something that we see too seldom. Too often it has to be war. In this case it isn't war. It was competition, but it certainly was not war. And those people were the reason that you could get almost anything done. There was never a paucity of ideas. Imagination was rampant, and most of it very good imagination on how to solve problems. And a group of people could get around a table, work together, and in a noncompetitive—it seemed noncompetitive, at least at the time—and the sum of the output of that table was far greater than just the individual parts that were there.

It was really an exciting time to be involved. And that's why Apollo 13 was saved. That's why Apollo 11 landed at the time it did. It's really why any of the in-flight emergencies were dealt with successfully, is because people could get together and figure out how to solve the problem.

BUTLER: A very motivated and dedicated group of people.

SCHMITT: It was—it was really a remarkable time.

BUTLER: As a remarkable time, you mentioned Apollo 11; and now we're coming up on the 30th anniversary. What was your involvement (if at all) with planning for that mission? And then, where were you when they did land on the Moon?

SCHMITT: Well, I was very closely involved with the crew because—on the science training and the descent stage activities. Also, I just gradually got into the role of being a *de facto* mission scientist to act as liaison with the principal investigators. The crew just didn't have time to see them, other than in very formal sessions—training sessions—which really did not amount to a lot. It just gave the principal investigator a little bit of access to the crew, because they had other things that had to be done besides learning the experiments were.

And so, actually a month before launch (I think it was about a month before launch), Deke Slayton formally designated me as a mission scientist so that I had a little more clout (I guess). I don't know why he did it. It was unnecessary, because I was doing that anyway. So, I was with the crew at the Cape most of the time when—and helping them in their various lunar surface simulations that they did; helping to plan the actual timeline, which began with the PSAC briefings and then it matured into what actually they were going to do. We had a lot of interaction in trying to fight off the people that wanted to get rid of the portable life support systems and just have Neil go down to the surface on an umbilical and grab a sample and come home. I mean, there were a lot of these kind of things. And nobody believed that we were going to be that desperate for weight and that desperate for time that we needed to go to those lengths. And, indeed, obviously we didn't.

I have lots of recollections about being with the crew at breakfast or dinner down there and various things. Neil was quoted as saying and it was, I think, an accurate quote, is that he felt that he only had a finite number of heartbeats and he wasn't going to waste any of them on exercise. That was, I think, in a *Life* magazine article. And I'm pretty sure I heard say Neil say that in person. And so, one—but Buzz Aldrin was just the opposite. (Buzz is a very much an exercise freak, if you will.)

And one time at about two weeks before launch, Neil came into breakfast with a handgrip—a rubber handgrip—and he just sat down at the table and was gripping this. And Buzz Aldrin was sitting on the table just getting redder and redder because here it was two weeks before launch and Neil knew exactly what he was doing, you know. He was just teasing Buzz by gripping this.

The thing, though, that most people don't realize is that in training in pressure suits, you get an awful lot of exercise. It's very, very difficult to go through training for a mission and not get in pretty fantastic shape for the kinds of things that you have to do in a pressure suit. So, Neil hadn't really skimped on anything. But he was just giving Buzz a hard time.

BUTLER: I think it's good that everyone was able to keep their spirits and their sense of humor, it sounds like.

SCHMITT: Well, I'm not sure Buzz kept his on that occasion. But most of the time he did.

BUTLER: And where were you when they actually landed on the Moon?

SCHMITT: I was back in the Mission Control Center. I was helping out at the Capcom [capsule communicator] console. I was not Capcom, but I was helping out there.

BUTLER: It must've been quite a time to see the goal actually achieved for the first time.

SCHMITT: It was. But, you know, we all—I think everybody—I was very close to Mission Control. One of the places I found you could learn—you could do a little simulation training before I was ever assigned to a mission and you could learn an awful lot about spacecraft systems, was to go over to Mission Control and work with the various console teams because they were the ones that developed the so-called schematics of each of the spacecraft systems. And I learned an awful lot from studying those and working with those people.

So, I had a real good relationship with the Mission Control folks. We went out to the

Singing Wheel together often—and—the late Singing Wheel, I guess we have to refer to it since it's burned down—for barbecue and shuffleboard. And it was a good crew. I really enjoyed my time with them. The—Sy [Seymour A.] Liebergot and the—Steve [Stephen G.] Bales and the booster guy whose name I—. I feel bad but I can't remember all their names now.

But we spent a lot of time together. I learned a lot from them. And I think, with the AOS (the Acquisition of Signal) on Apollo 8, was probably for that group and for me and a few other people the highlight of the Apollo Program. We hadn't planned to do that. We'd gone around the Moon and we were bringing these guys safely home. And the place just erupted when we got that first signal from the Apollo 8 crew, "Headed back home." And that didn't happen again.

Apollo 10? Same thing. Ho-hum. You know, the viewing room was almost empty (as I recall) for the AOS on Apollo 10. It was just the way we were. You know, we'd done that. Now let's go on to the next thing.

And certainly, the landing—the successful landing of Apollo 11, particularly with the suspense that came with that landing and the problems with the executive overload in the computer because it was monitoring the rendezvous radar signals it didn't need to monitor. And Steve Bales' call on that to say, "Oh, you're go for that," and then finally Neil had a chance to look outside. And he was headed towards a bunch of boulders, so they overflew West Crater in order to find a good landing point. All of that was terribly exciting. And, you know, it was certainly what we had signed on to do.

But I—an awful lot of people would go back and I think they'd say, "What was the mission? Apollo 8 was The Mission. It's the one we didn't expect to do and the one we did remarkably well."

BUTLER: We found that a lot of people do agree with that, which is interesting. And Apollo 8 did achieve Kennedy's goal of getting to the Moon.

SCHMITT: Yeah. But, you know, obviously we were not about to give up on landing. But it just was the emotions of Apollo 8 I think were very, very different than—it was a much more professional matter of fact for Apollo 11 than it was for Apollo 8.

BUTLER: As you began to move on through the (now)—the landing missions with Apollo 11 and Apollo 12, when did you begin to get a chance to actually look at the samples that were being brought back? And—.

SCHMITT: Well, I was working on the Apollo 11 samples immediately upon their—getting them back. I, indeed, participated a great deal and authored the first paper in the lunar science conference—the Apollo 11 science—(what did they call it?) the Apollo 11 Science Conference Volume that was published in 1970. The first paper, I and Gary Lofgren and a few others—Gene Simmons authored as an introduction to that volume, and included a section where I summarized the work that I had done on the lunar samples. It was—what we call hand specimen analysis. But still, I think was—it certainly would have been helpful to me in thinking about these rocks through the years.

So, I started working on them right away. And I stayed pretty current on the samples as they came back.

BUTLER: Did they meet your expectations?

SCHMITT: All I had as expectations: that they'd be exciting, and they certainly were! They—all the—not only just visually and some of the things we saw in them were very, very unusual—relative to terrestrial rocks.

One of the first things that catches your eye, there's no evidence at all of any water activity; and there isn't any water in these rocks. So, that absence of water or signs of water catches your eye right away. And because of that, there are mineral assemblages that are very, very clearly identified just in hand specimen, and—which you'd normally have trouble doing here on Earth with the volcanic rocks. So, it was exciting to be able to sit and work on the actual rocks that you had dedicated a small part of your life to.

BUTLER: As the other missions progressed and as you did begin your training for backup mission for Apollo 15 and continued to look at these samples as they came in, what were some of the differences between the different sites? I know, obviously, there were several; but in general.

SCHMITT: Most of the differences come out in the chemistry of the rocks, and that of course took a good while—a few months to come out because the samples had to be distributed to the investigators and they had to do their work. And then the results would be reported on. But each site is distinctive.

For example, the Apollo 11 rocks are very high in titanium, and so you see a lot of the mineral ilmenite in them. And you see it in hand specimen in the vesicles—the holes in the rocks—and things like that. Where Apollo 12, you did not see as much. It was very little—there was relatively low titanium, but there we had a better chance to look at what's called differentiation of flows in that these were olivine-rich lavas. And the olivine, as it crystallized out, sank to the bottom of the flows; and this determined the kind of rocks that Pete Conrad and Al Bean picked up around the various craters that they looked at.

So, they're quite distinctive there. The Apollo 14 site was on an ejecta blanket. It wasn't on a volcanic rock at all; it was on an ejecta blanket from the Imbrium Basin. And we knew we were going to see a lot of the rocks composed of fragments of other rocks which we called breccias, and we did. And the sample suite there is very, very distinctive from what we had seen. And even though we saw those—saw breccias again as a dominant rock type in Apollo 16, they were a very different kind of breccia, one which has resulted from the long-term gardening of the lunar Highlands at the Apollo 16 site, where the Apollo 14 rocks were primarily those ejected from one basin.

Apollo 15 got us back into the basalts again, but low-titanium basalts. And although we didn't fully recognize it and its significance, the crew also picked up some unusual volcanic glass (in that case, green glass in contrast to the orange glass that we found in abundance at the Apollo 17 site). Also, Apollo 15 was the first time we saw big chunks of crystalline rocks, coarsely crystalline rocks such as rocks composed primarily of feldspar (calcium feldspar, called anorthosites), that we had suspected we might see there and elsewhere, but we had not really been able to collect large fragments of it.

Again at Apollo 17, we saw these kind of rocks in abundance in the breccias or fragmental rocks that we sampled there. Apollo 17 had got us also back into high-titanium rocks, so ilmenite was again a very dominant mineral phase. The orange soil, though, is really what attracted most of the scientific attention, initially at least, to the Apollo 17 site, although we found that there's a lot of other things of interest there as well.

BUTLER: It shows that the Moon is a very diverse place.

SCHMITT: It is diverse. Not nearly as diverse as the Earth. The absence of water and water-driven processes on the Moon reduce that diversity significantly. But still in its own way, it's not only very different than the Earth but it has a diversity as a result of that. A diversity we don't see here on Earth, because that part of Earth history has been obscured or destroyed.

I mean just—Earth history beyond 3 billion years ago is very difficult for us to look at here on Earth. In fact, Earth history beyond half a billion years ago is somewhat difficult to look at. You have to go to very special places. On the Moon, we just start looking at history at 3 billion years and go back from there. So, it's a pitted and dusty window into our own past. No question about that.

BUTLER: Absolutely. As you began training on the backup crew for Apollo 15, you mentioned that now you got pretty involved in that aspect of things and so less so in some of the other areas that you had been in before. How did your role kind of evolve, and what did you begin to focus on specifically?

SCHMITT: Al Shepard called me into the office in January of 1970 and said that, "We're going to assign you to a backup crew and you'd better start stealing simulator time." I didn't say, "I already have been," but—because that's what everybody did and he knew it. So, he gave me sort of a formal permission, if you will, to work myself into the simulator training schedules. Which I did, of course, and very rapidly dropped off everything else I was doing.

Because once you're on a mission, everything else you do needs to be focused entirely on the mission. That doesn't mean I didn't work on the science, work on the experiments—but they were the ones we were going to fly with on Apollo 15. So, I became purely an integrated part of that crew rather than trying to deal with Apollo 16 and any future missions that might come along.

Dick [Richard F.] Gordon [Jr.] and Vance [D.] Brand were my crewmates. Dick was the commander. Vance was a command module pilot. On the backup crew we, of course, were backing up Dave Scott, Al [Alfred M.] Worden, and—and Jim [James B.] Irwin on that Apollo mission. Now I think also, though, because Dick and I are competitive people (when we're teamed together or not), as most of the astronauts are—I think we were able to push the prime crew pretty hard on their science training as well as—I probably pushed them pretty hard on their simulator training as well.

Because you don't do much cross training. You do a little. At least—every once in a while (very rarely)—but every once in a while, Dave Scott and I would fly a simulation; because there's always a possibility that one or the other's going to fly rather than the prime crew. So Dick Gordon and Jim Irwin were doing some cross training, but not very much. That was very rare. The probability of a backup crew ever flying was pretty low, in spite of the experience of Jack Swigert replacing Ken [Thomas K.] Mattingly [II] on Apollo 13.

We pushed them as hard as we could in the simulations and everything. And Dick Gordon and I became a pretty good spacecraft crew, too. Dick would never let me try to land it from the right side; but other than that, we worked together very closely and I think probably flew those simulators as well as the prime crew. (Of course, that's what any astronaut will tell you.)

BUTLER: From a somewhat of a social aspect, did interactions between the crews—both the prime and backup—were those at a pretty good level?

SCHMITT: On Apollo 15 and 17, my two experiences? Yeah, I think so. We didn't see an awful lot of our backup crew on Apollo 17, because they knew that they were just sort of filling a square unless something really serious happened. But still, when they were around, we did a lot of socializing with the support team. We tried to with contractor teams and stuff like that.

The astronauts were generally pretty good at socializing with people who were important to the success of the various missions. It was very important to do. Both formally, we had formal competitive baseball games and barbecue activities and things like that; and informally, we would golf and do things. I did a lot. And as I indicated before, I did particularly a lot with the Flight Control Division people.

BUTLER: At this point, did you do even more with the Control people?

SCHMITT: Yes, we did. At least, as a team. As a crew, we did. We did a lot more. We had more formal interactions with them. Our formal—informal interactions. Because everything has to be scheduled. You just—when you're training for a mission, it's hard to just, you know, break off and say, "Okay, let's go do something else tonight." I mean—"or this afternoon." That, you pretty well have to work those into a formal schedule.

BUTLER: You mentioned the contractors. Did your role with them—did you have a big role with them over the course of your career? And did that change as you became involved on the missions?

SCHMITT: Before being assigned to any mission, the astronauts dealt primarily with contractors, because they were the vast majority of the—90% of the people who were involved in Apollo were contractor personnel. And so, one reason why we traveled so much and got so many hours in T-38s on cross-country flights was that we were flying to the various contractors around the country.

And it changed with time. In the very early days, of course, an awful lot of travel was to—in the early days of Apollo was to Los Angeles and the [North American] Rockwell facilities there and to Grumman [Aircraft Engineering Corp.] Bethpage [New York] for the Grumman facilities that were there. I went into Michigan quite a bit because of the ALSEP. And there were occasionally other particular things that we would do around the country. In the early—even earlier days, Gemini and Mercury, of course, an awful lot of travel went into St. Louis where those spacecraft were being built by McDonnell Douglas.

As you began to focus on a particular mission then that contractor interaction became more with those people who were supporting the actual training, supporting the testing of your spacecraft and your rockets and things like that. People who were stationed formally at the Kennedy Space Center, for example, would be the ones we would see more than we would see people at the plants. We didn't—I didn't normally go to the plants. I don't remember actually going to plants at all while I was in a formal training cycle.

But we did see an awful lot of them. And your scheduled training briefing, say, on the landing radar or rendezvous radar or something like that would involve the contractor that had built it and people from that contractor, who specifically were assigned to a training exercise or a training effort—either lecture or a demonstration.

BUTLER: We talked earlier about the landing site selection; and you then talked about some of the differences, geologically, in the sites from Apollo. Did those differences, as you were going on with the missions, as the results were coming in, and as you were training for the next ones—did that—how much did that affect the selection of landing sites for the later missions? Or had those been pinned down pretty well earlier?

SCHMITT: We had a fairly large set of potential candidates for landing sites for each mission; and as the results from a completed mission began to be known, it would bias interest in the sites one way or the other. Particularly as we got near the last couple of missions—it became apparent that they were going to be the last couple of missions—the competition among ideas for what landing sites should be selected became more and more intense. And for example, Apollo 17 involved a number of different possible candidates, and the Taurus-Littrow Valley was not one of them initially because everybody had assumed that we couldn't land in there—it was too narrow—and that the landing error ellipse was too large to permit a 3-sigma error ellipse was too large to permit us to go in there.

Fortunately, I think, a number of people (particularly some of the Bellcomm folks back—Jim Head and others back in Washington) didn't give up on it. And they were able to persuade, through various calculations and operational initiatives of—new initiatives on how you track spacecraft and stuff like that, they were able to persuade the Flight Control Division, which was the important player down here, that we could indeed get that error ellipse down to a size that would fit in the Valley of Taurus-Littrow. And so, ultimately everybody agreed, "Yeah, we can go do that."

Part of it had to do with coming up with a set of procedures that will allow us to get a better update of the lunar module's state vector on the partial orbit just before we ignited the descent engine to go into the valley. That was a very important part of being able to reduce the size of that error. So, once that agreement came along, it didn't take very long for the consensus to say, "Yeah, Taurus-Littrow is probably the best place to go because of the variety of geologic structures that we know will be there and the apparent range of ages of processes that we would be able to sample while we're there." And then, on top of all that, unknown to everybody, it's just a magnificent place to be! It's a beautiful site.

But Alphonsus was considered a candidate. There was some site, which I don't really remember much about that was—I think it was called "southwest of Crisium" that had been proposed. (Someday I have to go back and find out what that was. I just don't remember that much about it.) Copernicus was a candidate to go into the floor. Copernicus. Tycho to some degree was—the rim of Tycho was considered a candidate. And most of these had been considered for other missions.

After Apollo 13, as a little sidelight, I had the thought that once we had verified our systems again with Apollo 14 and gone to the site—the so-called Fra Mauro site that had been planned for 13, which was a very important site on the ejecta blanket of the Imbrium—I began to talk up a plan that would take us to those places on the Moon that almost everybody had always wanted to go throughout history, independent—or had always attracted great interest, even before there ever was an Apollo Program.

And one was Tycho, the Crater Tycho; another were the polar regions where there's permanently shadowed areas; another would be the west rim of the Moon, which would mean a big basin and young basin—relatively young basin, Orientale; and then the final mission, if—at that time we thought we had four missions left after 14 (through 18), we'd go to the far side of the Moon. And I had a lot of fun with that in trying to develop the arguments and work—and, again, work them up through the system and see if anybody was interested in a different kind of a program.

It turned out that very few people were. And so, that idea disappeared. Of course, we lost Apollo 18. That was cancelled, it turns out, at about that same time (as I understand George Low's archives). But when Apollo 17 came along, I once again surfaced the possibility that we might land on the far side of the Moon with communications satellites placed in what's called a pseudo libration point about (if I remember correctly) 30,000 km behind the Moon. We had two satellites on the shelf (I think they were Tyros satellite buses, as a matter of fact) that TRW [Thompson-Ramo-Wooldridge] had that could be placed back there; and we got the cost figures from TRW and others. But again, Chris Kraft finally stopped me in the hall and said, "Let's don't talk about going to the far side of the Moon on the last mission." So, I stopped talking about it! I thought it was a pretty good idea, though, myself.

BUTLER: We'll take a break here…

Talking about some of the landing sites—and you were talking about the possibility of a mission to the far side of the Moon—are these areas that, hopefully for future missions—that will be considered? The polar regions and Tycho?

SCHMITT: I think they will be of interest over the next century. But I don't see that we're going to get back to the Moon for any scientific purposes until the infrastructure for going into deep space has been redeveloped for some other reason and, with respect to the Moon, probably a commercial reason; namely, access to its resources. Once you have the developed the infrastructure—capitalized, if you will, the infrastructure—on the backs of some crass, profit-making enterprise, then you can start to do all sorts of things. You've lowered the cost of access to space by a major factor because of these other ongoing activities.

And so, science can piggyback on that back to the Moon just paying whatever a launch cost would be by that time. And I think, in order to make these other commercial enterprises successful (if we're talking about the Moon), you're going to talk about a cost of putting a payload on a trajectory to the Moon that would be in the range of $1000 to $2000 a kilogram (in today's dollars), and that's very low. That compares to a cost of about $70,000 per kilogram for Apollo. Say, a marginal cost of another mission at the end of Apollo would have been about that cost. And that's fully capitalized. That includes interest on debt if you had to borrow the money, which the government does but never pays interest on (it doesn't seem to).

And so, it is only in that way that I think science is going to go back to the Moon. But I think it's the best way that it's going to get back there. Whether it's to continue the exploration of the Moon or establish a astronomical base or some other purpose, many of which have been suggested by others.

BUTLER: Well, hopefully, we'll see some progress toward that at some point in the future.

SCHMITT: Well, it's going to be difficult. It's going to take somebody with a business plan that can compete, at least on the surface, with other uses of capital. It's going to take investors. The government, I don't think, is going to be a player other than a regulator and a treaty enforcer.

The government's always a player, and unfortunately, in our lives, and—but it won't—I don't think it'll be providing at least the initial funding. Now it may buy the rides to the Moon in order to establish a scientific base or something like that. The government may sponsor that, and I suspect it would; but I don't see it initiating it. There's just too many—going to be too many other demands on the budget in the foreseeable future—with retirement security and health security and almost certainly increasing national defense requirements.

So, I think those of us who are interested in going back into deep space have to concentrate on finding a commercial reason to do it; and I don't really know of one that's better or even feasible, other than the idea of bringing fusion fuel—namely, helium 3—back from the Moon for use here on Earth in electrical power plants.

BUTLER: What were—while you bring it up, let's talk a little bit about helium 3 and the possibilities there and your work in that area.

SCHMITT: Well, helium 3 is a product of the solar wind that is embedded in the surface materials of the Moon. And over 4 billion years and turnover in the regolith or the debris layer that covers the Moon, it has been mixed in; and there is a certain constant steady-state concentration of helium 3—particularly in areas that are rich in titanium. It turns out that the mineral ilmenite, an iron titanium oxide, has a crystal structure that retains helium and helium 3 better than do other minerals. So, we've been trying to investigate what that is at the University of Wisconsin, where I now do teaching and some research. But we don't have a final answer. We've been able to duplicate some of the results with experimental bombardment of ilmenite, but we still don't have a good answer of how it's actually being retained.

But the empirical evidence is quite clear. It is retained in higher concentrations in the high-titanium regolith of the Moon. Apollo—the Tranquility Mare—Mare Tranquillitatis, the border regions of Mare Serenitatis in particular. Helium 3 can be extracted very easily from the regolith. It's embedded and retained. But if you heat the regolith to 700°/800°C, the helium as well as hydrogen, which is the dominant solar wind component in the regolith, are released; and these can be then fractionally cooled, particularly at night when you have a very, very clear, cold sink to deep space. (You can actually do it during the day, as long as you can reflect into that cold sink of 4 K [Kelvin]—that's pretty cold—which is deep space.) And ultimately, you would separate the helium from the hydrogen, and then the helium 3 from the helium 4, which is the normal hydrogen, using a process already known and used here on Earth to do the very same thing, and bring liquid helium 3 back to Earth in relatively small amounts.

Because it's so valuable—its energy equivalent value today, relative to coal, would be about $3B a metric ton! And so you only need about 60 to 100 kg to operate a large power plant (that's a—say a gigawatt power plant—a thousand megawatt power plant) for a year. So, there's a—helium 3's chucky jam full of—as Dick Johns used to say, "chucky jam full" of energy. And it's extremely valuable. So, at $3B a metric ton with a long-term potential marketplace of about $90B (just in the United States alone)—that's long term, you wouldn't do that very fast—you can see why there's some of us feel that there's a economic potential.

Unfortunately, it's very difficult in most circumstances (there are a few imagined circumstances where this may not be true)—but in most circumstances, you can't tie up investor capital for 10 years without a return. And that—it'll take you about 10 years to, you know, from start to get back to the Moon with your first shipment of helium 3 back to Earth, and your first power plant operating and ready to use it. So, that makes it difficult to imagine right now how you're going to attract investors without some kind of near-term bridge business that is consistent with your long-term aims but begins to bring investors in that can, indeed, realize some return on the near-term business.

And in the case of this particular initiative, that near-term business is probably using very low levels of fusion to produce medical isotopes at very low cost at the site where they're going to be used. And this is particularly valuable for positron emission isotopes, which have very short half-lives and a need to be produced basically where they're going to be used. (It's hard to produce a positron-emitting isotope more than 4 hours away from where it needs to be used and have anything left by the time you get ready to use it.) So, the near-term business will probably be somehow based on medical isotope production using low levels of fusion power of various kinds rather than an actual sustained power plant.

But that's one way in which you develop a bridging business. Now there are a lot of people talking about different other kinds as we sit here today, talking about different other kinds of commercial businesses in space. Most of them require that the infrastructure already exists for it ever to be profit-making or requires the government to be a customer or a partner or, indeed, a development partner. And again, I don't think any of those are really feasible in this day and age. It's going to take something more like a large-scale operation, more like helium 3, in order to build that infrastructure and also produce a return on investment.

BUTLER: Sound like some very interesting possibilities for the future.

SCHMITT: Well, it is. And it's important, because we're going to double our population within the next 50 years on Earth. That means in itself, just to maintain the status quo, a doubling in our use of energy. If you expect to—for the aspirations of the developing world to be met at all in that period of time—rather than just staying at extremely low levels of energy use and, therefore, very low levels of development—you're going to have to increase the energy use by even more. You won't even get to where we are today in this country if you quadruple their energy use! So, you've got a big step forward to take if there is such a thing as global climate change (and there will be, geologically speaking; whether it's warming or cooling, I don't think we know) in the long run, but that—either one will take more energy to mitigate the adverse consequences.

So, you're looking—as the primary issue, I think—technical issue of the 21st century is energy supply. Are we going to use our ace in the hole continuously, and that's fossil fuels? There's plenty of fossil fuels. The price will go up, but there's plenty as I said, but I'm not sure you want to do that. Not only is it uncertain what its effect might be on climate over that period of time, but it is probably so valuable as a chemical for the future generations that you don't want to burn it. And so, helium 3 starts to look to me like to have a very, very strong philosophical as well as a economic reason to be considered.

But all of this is going to be done in a competitive world. And if we're going to—if we hope to have helium 3 power on Earth for all of these good reasons—environmentally benign, a high-efficiency substitute for fossil fuels, and so forth—then I think it's going to have to be done in a pure business environment, and we're going to have to consider fossil fuels and fission nuclear power and solar power and others as our competitors, and show that we can provide a better product, at a better price, with less consequence. And I think we can do that, but a lot of us have a lot of work to do before that happens.

BUTLER: And it certainly is a different world from the Apollo days in that, like you mentioned—that take's business motivation to give—.

SCHMITT: Yeah. The window for government or taxpayer-funded initiative back to the Moon probably was open in the 1970s for a little while. Clearly, we were not looking in that direction. But it was probably open. But I think it's pretty well closed now. I just don't see—even though I think it's only going to take $10B to $15B to get us back to the Moon, to give the initial capital cost, I think that's beyond what we could expect the Congress to appropriate or a President to sign in this day and age with all the other demands on the budget. Discretionary monies are decreasing, not increasing in spite of all the talk of big surpluses and things like that.

BUTLER: Going back, when—again to site selection times. In one of the references I came across—in fact it might have been Wilhelm's *Rocky Moon*—it mentioned that some consideration was given to not selecting sites that might be potential for Soviet probe return missions. Were you aware of any of that? Or was that a big factor that you knew of?

SCHMITT: I am not aware of that ever being a significant factor when I finally got involved at any point. I'm sure it probably was discussed among the scientists, but I don't remember it ever coming up. It doesn't mean it didn't. I just don't remember it ever coming up.

BUTLER: Okay.

SCHMITT: The Soviet probes provided such a small sample and no contextual information such as the Apollo crews provided that you could go to a place where a *Luna* mission may have returned a sample and I don't think you would duplicate very much at all by having a full-fledged mission there. So, I think calmer heads, cooler heads probably prevailed and said, "That's a non-issue." But I don't know that. I'm just guessing.

BUTLER: A well-informed guess sounds like, thank you. Going into—as you trained for Apollo 15 as the backup, but during—at some point during that training the decision was made not to have the Apollo 18, 19, and 20 missions. At what point did you learn of that? And what did you then think of the possibilities of you actually flying on a mission?

SCHMITT: Well, it turns out, having just gone through some of George Low's archival material at Rensselaer, that on one hand the Apollo 19 and 20 missions had been lost a long time ago. In fact, if I remember correctly, early in 1970 it was clear to them, to inside of management, that there was not going to be a 19 or 20. And 18 was also very much in jeopardy and was lost much earlier than I thought originally.

I thought that 18 had really been—and maybe it just officially was cancelled by NASA in the middle of the Apollo 15 training mission. That's when we heard about it. But if I interpret George Low's notes correctly, the Apollo 18 had been lost long before that. And that 17 was already known, within NASA management, as the last mission to the Moon.

Now, we didn't know that. But—at least I didn't. And I don't know anybody on our crews that knew it. And we assumed that Dick Gordon and Vance Brand and Jack Schmitt had a real shot at being the crew of Apollo 18 until we heard it had been cancelled in the middle of the Apollo 15 training. But in fact, I think we were living in a fool's paradise with respect to that assumption in that, that mission had disappeared long before that in the budgetary fights that NASA was having with the Office of Management and Budget.

BUTLER: Quite a— that's interesting.

SCHMITT: In fact, one of George's set of notes describes a December 31st/January 1st set of meetings with the—(I think it was) Frank Saar, who was then Director of the Office of Management and Budget, in which Apollo 17 had been cancelled in the proposal that had come back—the final budget that had come back from the Office of Management and Budget; and George and company were able to get it restored but at the cost of having it slip beyond the 1972 elections. The clear statement being that, if it were—that the White House did not want to have a possibility of a accident in space before the 1972 elections.

So, we launched in December of 1972 rather than earlier because of that particular desire by the White House. And I understand that! I've been there! I didn't spend a few years in politics not to understand those kind of considerations. Whether you like them or not or agree with them or not, I think that that's, you know—it's foolish to kowtow to that kind of baser instincts in politics. That's something else. But that's—I'm sure that's true. I'm sure that's how that happened.

Now how they finally decided that Apollo—that I was going to be on Apollo 17 is another story. And again, in looking at those archives, it looks to me like that decision at least had been made at Headquarters long before it was finally agreed to here at the Johnson Space Center. But unfortunately, the one person that probably knows all of that is gone; that's Deke Slayton.

BUTLER: And that is very unfortunate. He would have a lot to share with us, I'm sure.

SCHMITT: Well, I'm not sure he'd share it.

BUTLER: That's true.

SCHMITT: You know, Deke was very—he kept those cards very close to his chest. He never—I don't know—he and Al probably shared confidence (Al Shepard), but I'm not sure that they would—that those were things that they felt anybody really needed to know. We certainly didn't know how crews were selected. At least I didn't. And I don't know anybody that did! And the value of that was that it meant everybody pretty much assumed that politics was not going to be the way they were decided. You just had to do the best job you could; and if you did it well, you'd probably get a flight. If you didn't, you may lose a flight. And the people—and there were enough examples of that going one way or the other that made you a believer.

BUTLER: Whatever it takes to have a good, strong group of people working toward something they all really want to do. There was some pressure to have you fly on a mission. Were you aware of it to a great extent while you were working on Apollo 15 and then following that, to actually have you put on Apollo 17?

SCHMITT: Oh, yes. I was aware of various press and informal activities, trying to convince NASA that I should fly on on a mission, either 18 or, when that wasn't available, then 17. But it was not something that affected what we were doing on Apollo 15. It was just something we were aware of; and we—Dick Gordon and Vance Brand and I knew that we were sort of in competition with Gene Cernan's crew for Apollo 17 once 18 had been cancelled. But we did not—the only thing we could do is keep doing the best job we could in order to be in a position to compete.

And there were the options were obvious. You could substitute—you—since Cernan's crew was in line for Apollo 17, and by the way, there was no—there were precedents for breaking that (what appeared to be a) tradition. In fact, Al Shepard is a classic example of where, you know, he stepped in,—got Apollo 14 once he was cleared of his Ménière's disease problem. And he had not been backup crew for any preceding mission. So, there was—it was clear that you couldn't be absolutely certain you were going to get the mission that you would feel that you would cycle to.

That was one option, was just that we would immediately go into training for Apollo 17 after Apollo 15, but as a coherent crew. The other was that I would replace Joe [H.] Engle on the Apollo 17—on the Apollo 14 backup crew, which became the Apollo—would—would become the Apollo 17 prime crew. And that's indeed what happened. But there were still a number of ways in which that might have happened.

BUTLER: As a backup crewmember, what was your role during the actual mission time? Once they actually launched and were on their way?

SCHMITT: The backup crew was primarily in support of Capcom so that—because we should know best how the crew trained and what—because we trained as they trained. Dave Scott—or the prime crew sets the training philosophy, the training cycle, and obviously, we trained for the same mission, the same parameters, and everything. And so, we would know that best on whether the crew would be familiar with a particular procedure or not, with a particular part of the mission, how well they—how much time they'd spent on it or something like that. Because we just duplicated what they did.

That's what backup was all about. And we might spend a little more time because we were—at least I felt I needed more time, always, in the simulators to train. I never let up on that. And so, I might have put more hours in. But the whole purpose of what we were doing was to—in the mission, during the actual mission, was to support—to continue to support the prime crew by advising the Capcom and the flight director on what the crew knew and when they knew it.

BUTLER: As Apollo 15 came to a close, that mission, at what point did you actually realize—were you notified that you would—one of those conditions that you had mentioned earlier—the possibilities—that you would actually fly Apollo 17 and replace Joe Engle? And it must've been with some mixed feelings that you heard about it and realized that you would have this chance.

SCHMITT: Well, I didn't have any mixed feelings at all. We were competing for a slot. And, you know, it's like running a race. And you're happy to win. Not everybody—only one person can win. And I felt very, very fortunate. I understood how Joe would feel, and—I hope I understood. But I wasn't about to give up the position because he might have felt disappointed. It I guess must have been—now see, when did Apollo 15 fly? It was July—was that July of '71?

BUTLER: I believe so. I didn't write that date down, but that sounds about right.

SCHMITT: Well, anyway, I believe it was August then. It was that month after the flight that—I don't remember exactly what I was doing during that month (probably just sitting around, waiting for the phone to ring); but that I was at home in my apartment over here in the Nassau Bay area. And some people came by and some phone calls started coming in saying that the news media were announcing that I had been selected as the lunar module pilot for Apollo 17, replacing Joe Engle. And on Gene Cernan's crew.

And I, of course, said, "I have no knowledge of this," because I didn't. And I had not heard from either Al Shepard or Deke Slayton. But that—and a couple of people, Jim Head being one, came by the apartment in order to announce this fact. And I even got a call from my sister in Tucson, Arizona, saying that she had heard it. But it wasn't until a little bit after that, that evening, that Deke Slayton called and asked me if I was willing to—or wanted to have that position. And I, of course, said, "Yes." And then we had a couple of drinks.

BUTLER: Were you—what thoughts went through your mind at the time?

SCHMITT: Oh, I don't know that they were anything very profound. It was just something you had worked for and trained for; and I had already, some years before, made the decision that if I wasn't assigned to a crew (and I didn't think the probability was very high at that time) that I thought that, like everybody else in Apollo, it was well worth dedicating that part of my life to it without any question. And so, in a sense, getting assigned to a mission was frosting on the cake. I had already done more in a fantastic effort than I had ever expected to have a chance to do. And having a chance to actually now fly a mission was something that was far more than I had originally expected. Always hoped for, but never expected.

BUTLER: Were you able to settle in pretty easily with the crew and into the training for the mission?

SCHMITT: Well, I thought things settled in pretty fast. I guess we'll have to ask Gene Cernan—because we can't ask Ron [Ronald E.] Evans, unfortunately. But ask Gene. And I have not had a chance to read his book, so I don't know what he said about it. But Gene and I are both pretty headstrong people. But I think we began, like all the crews—you know, all the astronauts are headstrong and individualistic. But once assigned to a crew, I know of no good example of where someone acted unprofessionally and didn't begin to work as a crew and make every effort to see that the team gelled and that you were able to do things together.

And I think Gene and I were able to do that. I hope very soon he realized I knew the spacecraft. I could fly, and I could do the simulations as well as, maybe, anybody. (The possible exception being Fred Haise. Fred probably knew more about the lunar module than anybody alive or ever will.) But I spent an awful lot of time at it. And Gene obviously had a lot of experience in other flights; and he brought that to the crew. So, we, I think, began to get along pretty well; and he seemed to be willing to work very hard at the science side of it, because that's obviously what we were going there for, and did an outstanding job.

BUTLER: Was the training for Apollo 17 different to any great extent than for Apollo 15? Other than the site that you were training for?

SCHMITT: Oh, it was very close. The equipment was pretty much the same. Some differences in the science payload, but nothing major. The science was quite different because of the difference in the site. But still, the planning concepts and the flight concepts all were pretty much the same. So, in that regard, it was very familiar territory to go through.

BUTLER: As you were getting ready for the mission and you'd actually been assigned, but yet it was also the last mission, were there was some discussion about people would emphasize that this is the last mission. And did that affect what you were doing at all in your training or your planning ahead for the mission?

SCHMITT: The only thing I think it may have—the only thing it may have affected was the words that Gene Cernan and to a lesser degree myself thought about saying on the Moon. But you can't help but be aware that it was the final visit for some time. Nobody that I know of expected it to be a quarter of a century or more. But nevertheless, we knew that that was all; the way it was going to be for a while. And that affected maybe the more philosophical thoughts you had once you were there and once you left; but I don't recall it affecting the tenor of the training in any way.

The training is something that has a life of its own, and you live within that life, and make sure that it gets done. The one thing that you don't want to do is have a aborted launch on a launch pad and have to recycle and come back a month later and go through another month of simulator training. You're ready to go when you're ready to go. And I think everybody felt that way.

BUTLER: Did anything in the training for either 15 or 17—did anything surprise you? Anything that wasn't what you had expected it to be?

SCHMITT: One thing that comes to mind immediately was the value of the helicopter training, which was always a bit under fire and Deke Slayton always supported it and—as he did the T-38 training. There were people in Congress and in NASA Headquarters who thought that was a waste of money and it would unnecessarily expose the astronauts to the risk of accidents. And Deke's answer always was that, "These airplanes and helicopters are our only dynamic simulators" that, you know, in the simulators that we use for spacecraft, if you make a mistake you reset and tried again. You don't do that in an airplane or a helicopter. You have to get out of that situation yourself.

And so, these flying airplanes and helicopters kept us intellectually and professionally honest about the kind of work we were in. And I really felt that was an excellent argument, a very persuasive one to me. Plus one he didn't make, and that had to do (at least in my experience with flying helicopters)—is the more I was flying helicopters, the better I performed in certain really strange off—what we call off-nominal, off-normal spacecraft maneuvering situations; such as, what they call lunar orbit insertion aborts (LOI aborts), where you end up having to burn every engine you have with a weird configurations of joined spacecraft, the lunar module joined to the command module, and in some of those your—the commander and the lunar module pilot are both using all four controls in order to keep the spacecraft firing through the center of gravity of the combined vehicle. And it is not an easy task.

It has tremendous hand/eye coordination as you look at the various gauges and work your hands in different directions. And that's exactly what you have to do flying an H-13 helicopter without any governing control of any kind, is: you use your hands and eyes and your feet (in the case of the helicopter) in order to maintain that control. And I found the more I flew a helicopter, the better I performed in those particular kinds of simulations.

BUTLER: That's an interesting connection.

SCHMITT: It's a minor technical point, but it's certainly one that, to me, helped justify flying the helicopter. Now fortunately, we never had to fly those kind of aborts in real life. But still, that's part of the training and it's part of what builds the confidence (not only your confidence but the confidence of—in flight control team) that we can actually do this. And confidence is very important in these kinds of endeavors.

BUTLER: Well, and it paid off well on Apollo 13 at least, that, even though it was different—

SCHMITT: It did. And, no, that's exactly the kind of maneuvers that they were doing in order to get back on to a free-return trajectory. They were having to burn the lunar module engine with an offset center of gravity, one which the computer didn't know about, particularly if you didn't have time to bring up the computer and you were having to fly it hands-on. So, yes, it made a lot of difference for Fred and Jim as well.

By the way, when Apollo 13 happened, I was down here stealing simulator time. I knew that—not down here; I was at the Cape (lost track of where I am today). Because I knew as soon as the crew launched everybody would be back here and there'd be free simulators. And so, I stayed at the Cape after the launch and was working out in those simulators all day; and when the accident happened, the simulator team and I just transitioned into trying to work out some of the procedures we anticipated they would need, such as being able to rough align the platform and use something other than the normal guidance system in order to fire the engines, and things like that.

And we were working on that. And then as the checklists were developed here for particular procedures, they would ship them down to us and we would run the procedures, because we had the more up-to-date simulators. And so, my contribution to Apollo 13 was down at the Cape in verifying procedures and helping to develop ones, particularly the ones where they used the Earth as a guide to burning back on a safe trajectory.

BUTLER: All that extra simulator time that you were trying to get in paid off.

SCHMITT: Oh, yeah. Yeah. It really did.

BUTLER: It must've—that was a—certainly a very intense time and luckily it did.

SCHMITT: It was. And it was the, you know, classic example of how a whole bunch of problems cascade to create a problem, and then how a whole bunch of solutions cascade in order to work your way out of it.

And the crew certainly deserves all the credit in the world for their stamina and their abilities in actually implementing procedures under extremely adverse conditions. But of course, the heroes were the people down here. Not just Mission Control but all the engineers and the contractors and everybody that was working together around those tables, trying to—as each crisis developed, not only solving that crisis but trying to anticipate, "How do we solve the next one we're going to hit?" Because we knew what most of them were.

Probably the best example of that is the one which almost nobody ever dramatizes in any of the discussions that we've had recently about Apollo 13. And that is the, "How do we charge the ascent batteries?" Because when all of this happened to the command module, not only did you drain and lose the fuel cells but you drained the batteries necessary for power to control the spacecraft during reentry. And there were a team of fellows working—a small team—working to try to figure out how—"Is there any way we can charge those batteries?"

Because initially, nobody knew how to do it!

And if you couldn't do it, all of this other stuff was just make work; I mean, it was work you would do, but because you're always anticipating somebody's going to figure out how to charge those batteries, and so let's get these earlier problems solved. Now, well, indeed, a couple of fellows who knew the schematics and the systems of the lunar module and the command module extremely well did figure out a way.

There's only one power—there was only one power line between the command module and the lunar module. It was a sensor line. Very, very low current. In my side of the command module (the right side of the command module), I could monitor the temperature on a—on one ascent battery—ascent or descent battery? Probably a descent battery—in the lunar module. And that was a line that came over; and every time you docked, you connected these sensor lines. The command module pilot would do that when he went up and took the docking probe out and we stored that away. Then the sensor lines would be connected.

Well, these guys—these two guys figured out that—and we need to find out who they are. It's a little research project for somebody. Tell you who would know, and that's any of those Mission Control guys who are still around would know who these fellows are. Gene Kranz would probably know them, too. Anyway, they figured out a path by which they could take—they could reverse that—take that current coming through that sensor line and run it through the battery chargers, the inverters, and trickle charge those ascent batteries.

And for hours during that drifting flight back to Earth, that's what was happening, is they were trickle charging—very low current, so it took a long time—but they were trickle charging those ascent batteries until they were up at a level where they could actually reenter.

BUTLER: That's very interesting. And something that hasn't been covered.

SCHMITT: No, you don't hear about that. And those fellows are as much heroes as anybody else. We need to find out who they are.

BUTLER: Absolutely.

VOICE OFF CAMERA: We need to stop tape.

SCHMITT: Okay. All right.

[End of Interview]