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ORAL HISTORY TRANSCRIPT

OLAV SMISTAD
INTERVIEWED BY CAROL BUTLER
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BUTLER: Today is April 3, 2002. This oral history with Olav Smistad is being conducted for the Johnson Space Center Oral History Project in the offices of the Signal Corporation in Houston, Texas. Carol Butler is the interviewer and is assisted by Jennifer Ross-Nazzal and Kevin Rusnak.

Thank you very much for joining us today.

SMISTAD: You're very welcome.

BUTLER: To begin with, if you could tell us a little bit about your background growing up. In fact, you came from Norway and moved to the United States. Is that correct?

SMISTAD: Yes. I was born in a little town in Norway of fifty people, up around the Arctic Circle, and our family had been there since the time of the Vikings, which was a couple of years before I was born. [Laughter] My father was one of those immigrants that had the courage to come to this country and leave his family and his home, because he wanted something better for his family. So he came over here in the height of the depression, about five months after I was born. I don't think he left because I was born. [Laughter.] I'd like to think not. And settled in Washington state, northwestern Washington state, in a large town of 150 people.

He worked and brought us over about four years later, and I can still recall, as part of the heritage, I think, of being an American, I can still recall my mother holding me up when we came into the Port of New York [and pointing to] the Statue of Liberty. So that's my background of folks that I think that built—not that I did, but my father was one of those that had the courage and helped build this country. He told me, "You never forget where you came from and be very proud of that, but you're now an American, and you learn the language and grow up [American]." So that's the background. It's somewhat of maybe a "rah-rah" patriotic background, but it's one of the significant reasons why I chose NASA, or perhaps NASA chose me.

Other than that, my background is [being raised on] a little farm, went to school at the University of Washington [Seattle, Washington], got an EE [Electrical Engineering] degree and volunteered and spent some time in the service, came back on the Korean [G.I.] Bill and got another engineering degree and then I went to work for [the] Boeing [Company]. Those were the early days of the Bomarc, which [was] a high-altitude interceptor system for [aircraft] defense, at that time.

We were having [our times] with the Russians, [at that time it] was research and development—and then later on, still with Boeing, on [the] development of the Minuteman, the Minuteman weapon system.

BUTLER: What got you interested in engineering in the first place that led to these—

SMISTAD: My graduating class [had 48 students and], we had less than 200 people in [our] our four-year high school. [Well in high school,] I was reasonably good at such things as physics and math, chemistry, things like that. And I had—this is probably a common story—I had some extraordinary teachers that encouraged me. I was fortunate that some of these [had been] college [instructors who] had decided that, "Well, I want to get out of the rat race," and they came to this town of 150 people and taught in the high school.

There was a biology teacher, a math teacher, [a] geometry teacher, [a] chemistry teacher, and actually the coach [that] encouraged me. I also had a qualified college professor who [taught] English, so I learned that if I [was] going to pass that class that I would have to be able to conjugate verbs and express myself [well which really helped in engineering reports].

I spent [my] high school [days] like everybody else—I spent evenings and weekends in the study hall because of some of the things [I did] that I wasn't supposed to do when I was in high school. [Laughter] And we had teachers [in study hall], too, that would [also] help.

Anyway, that's a long story, but that's how I got interested in it, and I think I chose the right profession.

In Bomarc and Minuteman, it was research and development, both airborne systems and ground systems. And that was before the days of the transistors, so these [are] old vehicles [now]….

A little side story there is that the early Bomarcs, which [had] ramjet [engines and] liquid propulsion for the main engine, and [a] 100,000-foot capability. We put the first [test vehicles] in Santa Rosa Island, [and] Eglin Hurlbut Field 7 in Florida. [We lived in] Mary Esther.

At any rate, we fired a couple of them [down range and used] F-86s as targets. And there wasn't anything [on South Rosa Island at that time, about 1958.] There [was not much in] in Fort Walton Beach [either]. I think there was one tavern called the Indian Mound. That's [about] the only thing I can remember of the whole town. We fired [one of the first Bomarcs down range,] the thing rolled, and it [headed] toward Pensacola. And finally the range safety officer detonated it, and I bet we broke hundreds, if not thousands, of windows in Pensacola when the thing blew.

Those were the early days when at launch of those vehicles, [Bomarcs], there wasn't a designated safety area. You would lay in a ditch, maybe seventy-five, eighty yards away from the vehicle and watch it go. Now, not true in the Minuteman. The Minuteman was a—that had a little bit more power.

Okay, then about six or seven years with Boeing—I started with Boeing after the service, so, let's see. I think back. I got out of the service in '56, got married in '57, so in '57 I started working for Boeing, and worked for Boeing until about 1964 in various parts of the United States, putting in [Minuteman] missile wings at the very end there.

And then I read about the Sputnik and about what this country was doing in the Mercury [Project], and I thought, "That's got to be really fun, got to be really rewarding." I said, "And how do you go about getting into an Agency like that? I would bet there's a pretty [tough] selection process."

So, at any rate, I wrote and talked to folks. Eventually one of them came up to Seattle and interviewed me—George [F] McDougall [Jr.], who is long since dead. He was [one of the] historians for the Mercury Program. He was still writing [the final] Mercury [mission] reports when I came in, in 1964.

In 1964, I came into the Agency, into the Gemini Program Office, and worked for one of the finest gentleman that I have ever worked for, Chuck [Charles W.] Mathews [manager of the Gemini Program], who is still alive. I think he's living now someplace in Tennessee. He [had] a cabin on a lake up there, [I believe]. And those were the days of Kenny [Kenneth S.]

Kleinknecht [who followed Chuck as manager]. Jim [James A.] Chamberlin had just left—[other] folks that came down from AVRO [in Canada] were still there in the program office. [Christopher C.] Kraft was head of Flight Operations [Directorate].

The first vehicle I worked on was Gemini II, which was an unmanned vehicle. I came in time for Gemini I and the first two Geminis were unmanned, there being twelve total Geminis, ten manned.

What we started out doing hadn't been done before [on that scale]. Mercury was a vehicle in which there wasn't much time to do anything except go up and come down and check out systems, although there were [handheld] camera systems [for targets of opportunity].… They were handheld Hasselblads. As a matter of fact, Victor Hasselblad, who was the president of Hasselblad, would come over [from Sweden] for every mission. Wonderful man.…

At any rate—I've got to go back. In Gemini it was decided, perhaps we can interest science and scientists in doing experiments, scientific experiments, because there was more crew time. For example in Gemini VII, which was a fourteen-day mission, there's time. Gemini, although it housed two people, was [almost] as cramped as Mercury except that it had more storage space.

So we said, "Okay, we've got to start a [Gemini] experiment program, a scientific experiment program." There were four categories [of experiments]. We [were managed] through NASA Headquarters [Washington, D.C.]. There were four distinct types: there was DOD (Department of Defense); there [was] medical, which generally were crew-related, monitoring a crew; there was technology, which generally was JSC [Johnson Space Center, Houston, Texas] or some of the [other] Centers looking at [future] systems that might be used—it was R&D [research and development] systems, stuff that was used in Gemini and also for following Apollo. One, two, three—oh, medical. No, I just said medical, didn't I? What the heck was the other one?

BUTLER: DOD, medical, and—

SMISTAD: Science. Just pure science. All of those came out of the Office of Space Science in Washington, D.C., I mean NASA Headquarters, but [the experimenters were from] around the United States [and some international].

The toughest people to work with were the medical people, the reason being is that they thought that hardware could be ready the day before launch and they could run up to the vehicle [and stick it in], whereas there was [strict] discipline in DOD, obviously. There was discipline in the technology, but the medical and science folks, that's when we learned that an engineer and a scientist can look at the same thing and see different things. So that after Gemini, or in the middle of Gemini, there was an interpreter between the engineer [and the researcher], and that's when organizations started to build.

Okay. Now, what did we do? The first crew was Gus [Virgil I. Grissom] and John [W. Young] that I worked. I worked Gemini II, Gemini III, Gemini IV and two or three other Geminis. We were called the Mission Manager/Project Manager. There were [six or seven] of us in that entire organization where there are now probably six or seven divisions of people doing the same thing. I mean, it was a different program, don't get me wrong.

But the things we did were the following. You were assigned to a vehicle and you were the mission manager for that vehicle. You worked with the principal investigator [PI] to [help] develop his equipment, to design and develop his equipment, not that you designed it, but you oversaw that and got help from out of the Engineering Division, out of Max [Maxime A.] Faget's directorate, if [we] needed help. Then you followed the equipment through environmental testing.

Let me back up. Initially, the requirements to get an experiment onboard, because it was pretty much ad hoc, [at that time] what can you come up with? [Initially,] the environmental testing [for experiments] was virtually nonexistent. There was the launch [load] criteria, for example. That was important. The EMI [electromagnetic interference], RFI [radio frequency interference], that was important. Thermal properties were important. But that was about as sophisticated as you got.

The first two experiments we flew, Gus was left seat and John was right seat on III, and it was, what, three revs? Something like that. There was a blood experiment, to see what the [Van Allen] radiation field would do to blood.… And the second one was a sea urchin egg, which was what happens in weightlessness to embryo. And I think Gus had the sea urchin. Anyway, they were mounted on the hatches. There was one on Gus' hatch and one on John's hatch.… It had no interface to the vehicle except mechanical, and what you do is you turn a handle and something happened. It activated the systems.

Well, Gus was a very colorful guy, colorful as a man and colorful as in his English language. And he jerked the handle out of the—I can't recall whether it was sea urchin or it was the blood. Probably it was the urchin.

BUTLER: I think the sea urchin. I was looking in my research.

SMISTAD: And he said, "Oh, my goodness, I have jerked the handle out."

Well, when we came back from that mission, [Robert R.] Gilruth said, "Okay. We can't have that happen anymore. Those are crew problems, it could be safety problems, and certainly we don't look real good to the scientific community that want to get the results." So he says, "Okay. Hereafter, we'll do environment testing."

So that started the environmental testing of experiments. I'll give you an example of how we did it on III. I don't know if this should go on tape or not, but I think it's indicative of how the program started. Those two experiments—no, let me go back to II. That was an unmanned vehicle. We flew a plastic vial that had colored solution, and it was mounted right in the middle of the instrument panel. And the reason we did that is that nobody was quite sure how fluids would react in weightlessness, what kind of properties that they would [exhibit] and that was important because Gemini III was to have the first urine disposal system, as you've heard. In Mercury it was a tube down the leg. In Gemini it was an overboard vent—a series of valving and an overboard vent. We were going to fly a dummy vial on II with a camera on it to see what would happen. So we bought three of them and they were seamed and were shaped like a flask that you have in chemistry. So we put two of them into centrifuge [testing], into a centrifuge type of device to see if they could withstand—I can't recall what [ten Gs]. I think launch loads are something like six or seven Gs so this is probably like ten, fourteen Gs. I can't remember. And they both blew up.

So we said, "Okay. We've only got one left. We can't test that. That's the only one [we've] got left." So we sent that to the Cape [Canaveral, Florida]. A gentleman down at the Cape had it installed on the vehicle.

And I can recall sitting there [near the pad] when the vehicle went up, and I thought, "Oh my god, I hope that doesn't blow up." There would be colored fluid all over the instrument panel. At any rate, it didn't. So we [have come] a long ways from that.

Then the experiments came under the very strict quality control and product assurance, and more organizations got involved. In Gemini, we had a grand total of thirty, forty different experiment numbers, which we flew multiple times. So we flew like 105 experiments total throughout that program. There were visual acuity experiments. We had some on [Gemini] V where we looked down, White Sands [New Mexico] was a site, Australia was a site, and they were selected because they were generally open.

[Gemini] V rolled. They had problems and had to conserve fuel, and so they were doing slow rolls, and we missed those sites, because when we should have been looking down, they were looking up, rolling up.

We had one experiment, [daytime sodium cloud photography], I can recall, I think it was XII. I guess it was XII. From the French Space Agency [Centre National de la Recherche Scientifique], Jacques [Blamont] was the investigator [and also] the head of the [French Space] Agency. He sent a man over, Sol Penn, [who] was with us for [about] two years for training. [The procedure was for the crewman to stand up and take] a couple of pictures out of the [open hatch]. Brought it back, the film's black, totally black. Sol stood there a long time and he looked at us and he said, "Zee next time, I fly Vostok."

So, the interesting thing about those experiments were the people. The reason I'm saying this is that from that time, from that period, from those beginnings to where we are today, there is an entire module [now the Space Station] which is dedicated to experimentation—materials processing, crew health. It has become, as opposed to a corollary—as opposed to and "Oh, by the way, adjunct," it is now an objective and may be [one] reason for [justification of] the [International Space] Station.

Now, they were subjected to the same scrutiny by the time we got through with Gemini, but there weren't [many] organizations. For example, let me tell you. [When we] started—we assisted in the design, oversaw the environmental testing, did the integration into the vehicle, trained the crew—crew training. We got the investigators in and then we would schedule training sessions, [with folks like] Ted [A.] Guillory and [Helmut A. Kuehnel].

Then we would assist in the mission planning and the mission makeup, and those [were] guys like Tommy [W.] Holloway, for example, who was a very young man then, too, like the rest of us. And then we would sit in Mission Control. We sat [at] the booster console. Booster console was vacant after powered flight, after liftoff…. That was the experiments console, so we sat there twenty-four hours a day, throughout all of the [Gemini] missions. Then we would go to recovery, not out on the carrier, but recovery down at the Cape, get the experiments, bring them back, and then the investigator eventually would write a [scientific] report and we would write a mission report.

So I described there about six or seven things, and there are now six or seven divisions that do that same thing [that six or seven] people did [before].

Then from there, in 19—when was Gemini over? Started in about '63, '66—'67. At the end of Gemini—[15 November 1966].

BUTLER: Actually, before we move on from Gemini, if I could go back and ask you a few questions on some of what you covered and then move on?

SMISTAD: Yes. You bet.

BUTLER: Talking about the experience, you just mentioned the stages that you'd walk through, having it assigned to the vehicle, working with the PI on the design and development, going through the testing, integration, crew training. Were you at all involved on the aspect of which experiments were selected for which missions?

SMISTAD: No. There was a selection process that was organized and it was rigid. For example, DOD's were selected by DOD. The priorities were selected by DOD. [They provided] funding as to which ones would [be developed]. And their criteria was probably other criteria than we would use.

The medical experiments were selected by NASA Headquarters. [Dr. Charles A.] Berry, for example, had inputs to it. All of his [were] crew health. [Some] experimenters were [from] here [JSC], like Rita [M.] Rapp and [Dr.] Paul [A.] La Chance, [Dr.] John Billingham, and folks like that. The science experiments were selected through a peer review process and a board selection, which was handled by NASA Headquarters, the Office of Space Science.

The technology experiments were proposed by the various Centers. Most of them were out of JSC [or Langley Research Center, Hampton, Virginia], and they were peer grouped and screened through an engineering selection through the old Code R, which [was] the Office of Advanced Research [and Technology]. I can't recall what the devil it's called now. OART at that time. So there was a process.

BUTLER: When would you find out—I guess early on when you were first assigned to the vehicle, is that when you would find out which experiments were assigned and would you work—

SMISTAD: Generally, you knew [in time]. The first vehicle [we] didn't know [but] just a few months ahead of time. Well, that's not right. Like six, eight months ahead of time, because you had to have an opportunity to develop the equipment, but it didn't take three years to get an experiment on to a vehicle then. It took months, just a few months because of the process, the [flexibility] in the process.

And I would like to say this, that we never lost any astronauts because experiments were not safe. There was also a willingness at that time—an experiment is an experiment, and if the experiment fails, in a large respect, that is success, because you weren't sure how it was going to react or what the results were going to be in zero gravity, in low gravity. Next time, you would know and you'd be able to correct those. And that was acceptable. There has come a period of time when failure is not acceptable. And I will say this, the press was a lot more [a part of the team and] understanding in those days than it is now. It wasn't part of the process.

BUTLER: How challenging was it to integrate the various types of experiments into the spacecraft because some of them would have different power requirements or different weight requirements? Were there any major issues in integrating them?

SMISTAD: Oh yes, yes, there were. There were very different requirements, for example, for medical experiments where the protocol was part of the astronaut's timeline, as opposed to a

DOD experiment which required—sometimes they required extremely fine pointing. If you were going to follow a Minuteman launch out of Vandenberg [AFB, California], that was difficult to do, so [there were] pointing requirements there and the training requirements were a great deal different. [Integrating] a piece of hardware that was a technological experiment into a vehicle, that was different.

For example, we had—I can't recall whether it was one or two that we integrated into the Agena micrometeorite collectors. Matter of fact, [there] was one we worked on with Rusty [Russell L.] Schweickart and a gentleman from the State College, Stony Brook, New York, Curtis Hemingway.

The answer to that question is yes. The mechanical, electrical, human interface, thermal, they were all different, and what was so different was that the organizations that you worked with to integrate them, to test them, these were divisions inside of JSC [and not always structured to support experiments]. [There] were [also] organizations and test labs outside of the Agency. And bless their souls and our souls, nobody had ever done [it] before. So they weren't quite sure how to work that protocol and those requirements into [their routine]. So it was the initiation of a—what is it? I don't have a grasp of the language here. The psychology of how people react and how you [work the various] support [elements].

Now, that's a very good question, an excellent question, because an engineer has a tendency to look at [the] hardware and to kick [it]. You know, that's wonderful, that's really fun, but the human part of the integration was by far the hardest task. And we only had opportunities, for example, to get to the crew very seldom. We probably had maybe a whole day with the crew to brief them on seven or eight experiments that they were going to fly. And then we did the best we could to get training hardware into the system. I don't mean to say that this was a disorganized system. It was organized. It was methodical. It was new, and it became more commonplace as we went along in the program.

As you got into [the] Apollo and Apollo Applications [Programs], the system had been set up that people were now familiar with [experiment protocol]; there was funding available and there was a line of communication. There was a [review and] approval process. For example, the experiments that were left on the Moon were complex. It was a [more] difficult development and test process, and I think they benefited by [our experience in Gemini]. Two or three of the folks that were in our [Gemini] office ended up in the ALSEP [Apollo Lunar Surface Experiment Package] office. So there was a learning curve.

At the end of Gemini and the early Apollos, I had an office that was in charge of these activities that I have [just] described for [the] early-on Apollo experiments.

The first vehicle I worked on in Apollo was the one [Apollo-204] that Gus [Grissom] and Roger [B. Chaffee] and Ed [Edward H. White II crewed], and that particular process changed the Agency. I think it was January of 1967, if I recall, and it was a staggering [loss. Positive] things, [however,] came out of it. Those folks didn't give their lives in vain. It was unfortunate that that happened. Perhaps in retrospect, you can say, well, maybe we should have done this and that.

The vehicle itself was—perhaps the quality controls on the vehicle were not what they should have been. Ethylene glycol had [previously] been [spilled and cleaned up]. Our testing process [at the Cape] was such that they overpressured the inside of the vehicle, such that you'd have a [positive] pressure differential [in the inside of the vehicle]. [The cabin was pressurized with] pure oxygen [as I recall], which is extremely flammable. And that's the way we tested. And we lost that vehicle and we lost those people.

I worked many months [on the accident investigation]. We had some experiment gear [of AS-204]—and the testing on the experiment gear. We methodically tested all systems, all possibilities of fire, what could have happened. So I had the opportunity to [participate] through that from the experiment standpoint, not from a vehicle system standpoint. And that was an extraordinary group of people. George [M.] Low headed that up, [Frank] Borman was the deputy, and George [W. S.] Abbey helped. He helped a lot.

And that was a time of—we didn't look back; we looked ahead. "We've got to learn from this." And we got back up, took us seventeen months, and we flew [Apollo] 7. Is that right? I'm a little confused here. I think it was 7. That's the one that Wally [Walter M. Schirra, Jr., Donn F. Eisele], and Walt [R. Walter Cunningham] were on, I think.

That's an interesting process. We flew 7. [Apollo] 8 was lunar orbit. We flew 9. [Apollo] 7 was a crucial one, but 8 made the program. And that was George Low's idea, excellent idea, because 8 was going to be another Earth orbit [after] 7 [and] 8. [Apollo] 9 was LM [Lunar Module], docking, Earth orbit, [Russell L.] Schweickart, [James A.] McDivitt, [and David R. Scott], I think. And then [Apollo] 10 was—I think that was [Thomas P.] Stafford, [Eugene A. Cernan,] and [John] Young. The LM that they had was the last of that configuration series, and it was too heavy to get [off the surface], so they went down to—I can't even remember what it was—60,000, 70,000 feet from the Moon and came back up. And then, of course, [Apollo] 11.

So, we're talking January 1967 to July of 1969. So that program and that group of people re-engineered the vehicle, upgraded it, made it more safe, [engineered] a new hatch…. Flew [Apollo] 7, 8, 9, 10, flew four missions and landed on the Moon in less time than it took to repair an O-ring on *Challenger*.

One of the things that was really very important was that it was not a media show. As soon as those three folks [died, NASA Administrator James E. Webb phoned] the White House and said, "We will determine what the problems are. We will fix those problems, and we will come back and [report to] you and the nation what happened, etc., but we will do it internally within the Agency…." That chafed the press. They were very irritated because of the information, the lack of information, withholding the information. But [that's how] it was done.

And what I have just described to you, those four missions happened in that limited period of time, in that short period of time, in contrast to the *Challenger*, in which there were Nobel literature folks, folks from across the [public] spectrum [on the investigation committee]. Every day they met, there was information [was leaked] that hadn't been looked at and studied. You really need to make a long evaluation of what the heck the problems are and how you're going to fix [them]. And that is not a day-by-day thing; that's a long period of time so you can make an intelligent assessment.

So the press came out every single day, and it seemed to be one bad thing after another, [a media show]. And what that did, in my personal estimation, is it tore the top off of NASA. [We were unfairly] exposed to the media. It was not quite the same look [and was not] the same Agency for a good number of years. I think that [is] over, but I thought [that] investigation lacked [professionalism]. Okay. Well, that's a personal opinion and is not necessarily [NASA's or] anybody else's opinion.

Okay. What happened then? After the first two missions in Apollo, or three, I was reassigned to an Earth Observations Program.

BUTLER: Before we move on to that, then, could you comment on some of the experiments that you did work on for those early especially Apollo 7, 8, the ones that you were involved with?

SMISTAD: The Apollo—oh my lord, I wonder if I can. Apollo had an airlock, an experiment airlock, that was attached to a hatch where you could put the airlock in and you could put experiments and expose them to the vacuum without having to get out of the vehicle. So there were up-looking experiments like looking at stars or whatever it was in the various spectrums like UV [ultraviolet]. That was one. There were medical experiments for [bone] calcium loss.

[Pauses] Huh. Well, I flunked that test.

BUTLER: That's all right.

SMISTAD: I've got to tell you one story. I have to go back, like old guys do. It was Gemini IV. I worked Gemini IV and we had several experiments in Gemini IV, and they were all stowed up behind the crew, up behind Ed [White]. I wish I could tell you which ones those are. I can go back [and find] it. At any rate, the day before launch one of the managers [came to me] and said, "We have taken off all your experiments that you had on the vehicle."

I said, "You have what? I worked on those damn things for a year. Why did you take them off?"

He said, "Well, Ed is going to go EVA [Extravehicular Activity] tomorrow," which was the first I'd heard of it. They said, "We've got to make room for that handheld gun and the hose and all that, so we took your experiments out."

And I said, "Well, that sounds like a reasonable thing to do.…"

BUTLER: So you weren't involved then on planning for any of the EVA activities?

SMISTAD: No. That was held very close to the vest. That was, I think, after one of our Russian friends had [done the first] EVA. That was just almost immediately after that.

BUTLER: How did the principal investigators on those experiments react to that action, or did they understand it?

SMISTAD: The same way I did. Yes, yes. And we told them, "Well, we'll try to get you back on another flight." No, it was a very proud moment for the country.

Gemini was such a critical program. I remember reading, [President John F.] Kennedy [Jr.] stood up in Rice [Stadium]—[when] was it, 1961, '62, something like that, and said, "We're going to go to the Moon."

And Max [Faget] was telling me afterward, "What in the hell is he talking about? We haven't even flown the first Mercury, and in eight years or nine years, we're supposed to land on the Moon?"

So you designed and flew all of the Mercury flights, all twelve of the Gemini flights, and, what, five Apollo flights in that period of time and landed on the Moon in, what, July 20, 1969. Gemini even though I worked the experiments which I am the most familiar with—it came up with critical, very critical, technologies on systems to do rendezvous, docking, EVA. Those were absolutely critical—things that the Russians were [then] unable to do—which enabled the Apollo Program to do the same, the docking between the LM and the command module, and the rendezvous and the EVAs, and the ability to get—all that technology was demonstrated in just, I think, about two short years. Gemini wasn't very long, was it?

BUTLER: No.

SMISTAD: A couple of years, plus or minus. I got waylaid.

BUTLER: No. Actually, while we are back on Gemini—you had mentioned—and Gemini IV is what reminded me—you had mentioned how the interface between the engineers and the scientists sometimes needed a little interpretation, coming from the different angles. How were you able to work that relationship between all these different groups, the managers who would make a decision. "Okay. We've got to pull these experiments because of this," and the scientists who are focused on their experiment, the engineers who are integrating all of the systems together, and even the astronauts who had to be familiar with everything? How did that relationship grow and—

SMISTAD: You try to get a scientist to help, not necessarily a principal investigator, but a staff scientist or a [card carrying] program scientist or somebody that had a doctor's degree, that were able to talk one-on-one to an investigator. But even more so than that, you [listened and] learned. Experience taught you how to handle these folks and how you could get them to meet schedules and how you could understand [their] requirements. We learned sometimes through some tough experiences, that you wanted to be very certain that you brought that principal investigator along with you every step of the way, such that he was part of the process. He then became educated as to what went on that was not science.

So you put together a team, you learned, they learned, and then you also had an interpreter who was an in-between scientist [and could communicate with engineers]. We had scientists, for example, at JSC that could help us. That was generally the way it worked.

BUTLER: It's for the whole program, because everyone had to work together as a team to make the entire program happen—

SMISTAD: It was very important that the investigator understood what his product would be, because initially what you [did was] build the hardware, integrate it, and [sometimes] he wouldn't [participate in all of the testing]. Then the result would come out and he said, "This isn't what I was looking for." So, you revamp that process. We learned. It's now down to a very fine science.

BUTLER: How did that work with the DOD experiments that were more on the classified? Were you able to have them as involved or were you even able to know as much details about the experiments?

SMISTAD: We didn't know much about the hardware from the respect of—the exterior interfaces, yes. The power requirements, the thermal requirements, electrical requirements, mechanical, things like that, yes, we did. I don't think the experiments were classified, but some of the equipment might have been, because to some extent they were surveillance and [technology]. We didn't know much about them. They appeared. There would be engineering drawings and then the DOD would assign an experiment officer to each one.

There was also an AFLC [Air Force Liaison] field office established in what is now Building 1, and they had about seven or eight folks in there, all officers, headed up by a guy by the name of Colonel Dan [Daniel D.] McKee. Dan was the one of the youngest P-51 fighter pilots in World War II, flew out of England. The deputy was a guy by the name of Colonel Bill [Wilbur A.] Ballentine. He was from Seabrook, as a matter of fact—Seabrook, Texas. [One] of the guys that came out of that office [was] Ed [Edwin E. "Buzz"] Aldrin [Jr.].

[Also] Ed [Edward G.] Givens [Jr. who had been] a fighter pilot in Korea. He was the guy that was going to fly the first backpack [Astronaut Maneuvering Unit (AMU)]. He was killed in an automobile accident over by Pearland [Texas]. After fighting his way through Korea and all that, he was killed in a Volkswagen accident. Those [were] two people [who] came out of that office, and there was another one, too, I can't recall. So it was an excellent group of people, very technically capable. They had the knowledge of what was on the other side of that experiment. And the coordination was through NASA Headquarters [Washington, D.C.].

There—an interagency experiment—this isn't the right name; I don't know what the heck it is—[a] board which was chaired by NASA [Manned Space Flight Experiments Board (MSFEB)]. [They] looked at all the experiments and [then made] decisions as to which ones were going to fly. And that's [where we got our direction and] that's what we integrated.

BUTLER: We talked a little bit about Apollo and you mentioned that after some of the early Apollo missions, you were moving over into the Earth research area.

SMISTAD: Yes. I've got to take time out here and try to remember when that was. Let's see. I think it was probably, I would say, 1968, I went over to the Earth Observations area. Just a minute. I will look. I'm curious myself. I'm sure I came to work every day, so there must be some record of it.

BUTLER: Well, you were certainly doing enough things. You had a lot going on at the time.

SMISTAD: It was fun to do. A great group of people. Okay. About mid-'68 is when I moved over to become the Manager of the Earth Observations Aircraft Program Office. And I stayed around in Earth Observations, in one capacity or another, until about mid-'82, so that's like fourteen years. Wow. I didn't realize I was in there that long.

Okay. What was this? This was, again, a fledgling program. There [was] a determination made—and I can't recall when that was, that it would be of great value to the nation and to the world to be able to observe our environment—crops, weather, changes due to man. Those are generally the three.

So [that was] the early beginnings of looking down, getting up and looking down. Space has three unique primary attributes. One of them is microgravity. Another is [an infinite] vacuum. There is [a high] vacuum, like 10^{12} or something like that, and the other one is vantage point. You get up pretty high and look down. Those are the three attributes of space that are different [from] a laboratory here on Earth.

We were looking at the one of [the] vantage point. So we said, "Well, how in the world do you do that?"

So, there were assembled—the folks that started this program, at least at this Center and perhaps in the Agency. There was a guy by the name of Leo [F.] Childs, who is now deceased. There was a gentleman by the name of Harold [D.] Toy, who—I don't know where he is. He was over in Aircraft Operations. Leo was in the Science Directorate at Johnson. And there was a gentleman in NASA Headquarters by the name of Ted George, who subsequently went over to the department of EPA [Environmental Protection Agency].

And there were visionaries in NASA Headquarters. I am ashamed to say I can't remember their names. John [E.] Naugle was one of them.

One of the first things you have to do is to have some kind of a platform where you can get up and do this [down] looking—aircraft. So, initially, what we did is—there's probably a couple dozen people that did this and I certainly didn't have the [initial] vision. I just [happened] to be in charge of the aircraft program. This is why they brought me over.

[We] developed sensor systems. Okay. Now, what does that mean? The electromagnetic spectrum is split from way down into the—I can't even recall it anymore. You know, like—oh lord, where does it go. It goes from X-ray to microwave. You and I sitting here looking at each other, we've got just a little teeny sliver of that which is the visible.

[We worked with] science labs like Purdue [University, West Lafayette, Indiana] and Northwestern [University, Evanston, Illinois] and [the University of] California and places like that. The military in World War II [developed some technologies. Some of] this probably came out of England. I'm not too sure. There are [at least] two things that came out of [World War II] England. One of them was infrared film. I think it was the English. It might have been the American, but I think it was the English. [It was used for] camouflage [detection]. So if you throw branches over an ME-109, or over a Stuka, what you've done is that you have killed a living branch, which is [full of] chlorophyll, and that chlorophyll, it's under stress, it's dead, you ripped the limb off. So you fly over that sucker with this film which looks in [that] particular spectrum, not the thermal IR, but down to the lower reaches of IR, and you see this outline that looks like an ME-109, except you can't see it visibly because it's full of branches. So that was the start of infrared film [remote sensing].

We said, "Okay. We can use this film to look at stress in crops, vegetations of all types, living things in a chlorophyll range." That was one thing.

[Our] military [was] doing surveillance in the thermal IR. What's thermal IR? It's about eight to fourteen microns. There [is] some down in three or five microns and some eight to fourteen. Now, what does that [mean]? Well, what that means is that living things generate heat, for example, [thermal radiation]. The military could fly over targets or areas of interest with this scanner. It wasn't a calibrated scanner. In other words, it didn't say "Okay, this has a thermal temperature of *x* nor did it have] the thermal [property calibrations]. All [they] cared about was whether or not things were hot or cold. If they were cold, it was probably a tank. If they were warm, it was probably personnel movement.

So you knew that you could determine that [it had] applications for the identification of [rock] type [and] structure. There was the active microwave, which is radar, which the British did [develop] in World War II. Theirs was a [detection] system. [Our] radar systems [were L-band], X-band, P-band, K-band, out in those frequencies. And what that would do for you is that [they were] impervious to the water [vapor] band, which some sensors are. There's a [spectral] range where you can't see through the atmosphere because of the water vapor. I'm trying, in fact, to remember what [spectral range that was]. Anyway, active microwave/radar will penetrate that.

The radar systems are such that you've got an emitter [which sends a beam to Earth], and a receiver [which records and profiles the return Doppler signal and you get a 3-dimensional image. You also use] polarization [modes]. For example, you [transmit] at it in the horizontal [polarization] and you receive it in the vertical [polarization], and what all that mishmash means is that when you get it back, you can actually see structure [in stereo]. You can see three-dimensional. Okay. So that was very helpful.

There was [also] passive microwave. Well, what does that do? Passive microwave will not penetrate the clouds, but what it will do is, it's excellent at looking at such things as ice [and soil moisture for crop survey production]. It'll tell you age of ice, it [won't] penetrate ice but it will give you some idea of the [structure] characteristics of ice, and it will [help predict] breakup and things like that. Then there was a UV system, which we looked down into the UV, and I can't even remember why we did that. I think we finally gave up on UV.

Anyway, we had an assortment of these instruments [in various spectral ranges and frequencies]. To start with, they were [old] DOD [systems]. And also camera systems, of course, Zeiss cameras and Wild Heerbrugg cameras. They were six [and] twelve-inch focal lengths and nine-inch [film] format, which is the stuff you've probably seen—those [classic Earth] photographs, of course. And then you had the [capability for stereo] overlap, where you could overlap frames like 40 percent or so and you could actually get some sort of a [stereo out of it]. So we had all those systems and said, "Okay. Now what do we do?"

So we enlisted—actually paid for—help, smarts from people in the universities. And we must have had—let me read you some of these here. I'm going to have to read them myself. These were people who had expertise in various areas. The guy that I remember vividly is Bob

Caldwell, at the University of California in Berkeley. He was the father of [U.S.] infrared photography in World War II, and I think he's still alive. He must be in his nineties.

Here's some of the participants that we had in the Earth Resources Aircraft Program [reading from a document listing projects and principal investigators]: [U.S.] Navy Oceanographic Office [John C. Stennis Space Center, Mississippi], NRL [Naval Research Laboratory, Washington, D.C.], Scripps Institute of Oceanography [La Jolla, California], [University of] Michigan [Ann Arbor, Michigan], Oregon State [University, Corvallis, Oregon], New York University [New York, New York], [University of] Kansas [Lawrence, Kansas], University of Puget Sound [Tacoma, Washington], Ag [Department of Agriculture], we had the Forest Service, the Agricultural Research Service, Purdue [University], [University of] California, [University of] South Dakota [Vermillion, South Dakota], [University of] Minnesota [Minneapolis-St. Paul, Minnesota], Department of the Interior, [U.S] Geological Survey, [Bureau of] Commercial Fisheries, Bureau of Land Management, Northwestern [University], Stanford [University, Stanford, California], University of Denver [Denver, Colorado], American University [Washington, D.C.], Dartmouth [College, Hanover, New Hampshire], [University of] Georgia [Athens, Georgia], Florida Atlantic [University], and South Dakota State [University, Brookings, South Dakota, to name some of the participants. They all had their individual technical/scientific expertise and conducted research and development in those areas.]

We [also] had an international participation program, which I want to describe to you, because it started remote sensing programs with some of our friends. That was Mexico, Brazil, Argentina, and Canada. We [also] worked with NOAA [National Oceanic and Atmospheric Administration]. NOAA was interested in the passive microwave systems and the sounder systems and the satellite systems you see up there today, where you turn on your TV and look at the weather, [they contributed to the development of this capability from] the results of these early investigations. And then we had a jillion research contracts. So it was a pretty sizable program.

BUTLER: How did that program grow to be—as you said when you started into it, it was kind of a fledgling, and again you were learning as you went along.

SMISTAD: Well, there were government agencies [and academic institutions] that had a need. There were universities that had the [scientific and sensor] expertise in the areas. For example, [the passive] microwave [expertise came from] was a guy by the name of Doug Peek at Ohio State [University, Columbus, Ohio]. Color infrared photography was Bob Caldwell at University of California, Berkeley. Active microwave was a guy by the name of Dick Moore at [University of] Kansas. Geology was a guy by the name of Ron Lyon at Stanford [University]. Purdue [University] had the software folks: Bob McDonald, [Dale] Landgrebe, Dave Larson and those folks. [They helped analyze data by generation of a software program,] for example. [In] those early days it was [mostly] analog stuff or photography. They would develop algorithms and [perhaps a] Monte Carlo [software] approach [for feature identification] as to what [we were] looking at; [i.e. what is] the probability [that what we think we are looking at is really accurate] stressed [vegetation] maturing wheat, stressed corn, those kind of [determinations].

There were [many] universities [and] a combination of universities. Some of them had sensor technology smarts, others had science smarts. There were government agencies. For example, [U. S.] Geological Survey [which] has been around forever. They were still in the black and white photography era. They hadn't come into the color IR yet.

So the program started out—we [eventually] had [a total of] four aircraft that I recall. [Initially] we had a Convair 240 [that was used for Apollo systems development testing]. The first system that went on the Convair 240 was an [X-band] scatterometer system. What does a scatterometer do? Well, it's an active microwave system [which will produce] profile. It was used for sea state [investigations], looking at wave height and fetch [and agricultural investigations]. We got that off the LM. That was the Apollo Lunar Landing Radar System, and [we] called [it] a scatterometer. We got camera systems from DOD [as well as uncalibrated] IR systems.

As the program went along [and we got smarter], we started [developing] our own equipment, because we found that the equipment we had, although it enabled the scientists to get fundamental data [didn't provide the specialized data and information needed. For example,] it was determined that we needed to break the [magnetic] spectrum up into very small pieces, like ten, fifteen angstroms.

One of the systems we developed and brought in was called a twenty-four-channel scanner. It was a scanner that [recorded data: the spectrum up from] the visible on up through the thermal IR into twenty-four distinct [narrow bands]. You could look at data from each one of those [bands] to determine if there was [important] information content in that narrow band that had been hidden because you had a sensor, for example, that [gathered data over a broader] spectrum and just integrated it.

That [24 channel scanner] system was built by Bendix [Corporation] in Ann Arbor, Michigan, and the program manager of that was a guy by the name of Gene Zeitzeff who's probably still around. That system enabled us to [conduct expanded applications from our aircraft]. I'll go back to systems research.

The Convair 240 was a two-engine reciprocating [engine] aircraft that Leo [Childs] found somewhere. We had a P-3A, and that P-3 was serial number 003. It was a prototype Electra that they sawed off and made an ASW aircraft out of it. It was an as-built. What's an as-built? An as-built means there are no [fabrication] drawings. [There were] redlines, because it's one of a kind.… [Most, if not all, of our aircraft were one of a kind.

We had a C-130. A C-130 enabled us to put the ramp down in the back, and look out the back without having to cut a hole through the airplane [to collect data]. That aircraft we got from Langley, and it [had been] used as a boundary layer test aircraft. That is one of the characteristics of this aircraft. [It had been used for] aerodynamic testing [and was a low hour aircraft].

Well, the wing they had on was really a thick wing, which was something we couldn't use. So we [looked] around the world, and in Zambia we found an aircraft that had [the right] wings. [It] had been shot up by the rebels, and the folks inside of it [had been] wiped out. So we bought that thing, and it came over on a ship from Africa. That was put on by either LTV [Ling-Temco-Vought, Inc.] in Dallas [Texas] or GD [General Dynamics] in Dallas, I can't remember which, in the Greenville area….

Then we got an RB-57 [from the Air Force]. We got two RB-57s. What's an RB-57? Okay. It is a two-man [crewed aircraft with full] pressure suits. It is a 65,000-foot [altitude] aircraft. NASA still has the only one in the world sitting out here at Ellington [Field, Houston, Texas], if you've ever seen it. It's looks like a bat. It's a beautiful aircraft. It's 122-foot wing span, 22 foot at the wing root. When it's fully fueled, you've got to [put skids under] the wingtips, because they sag down to the ground. It'll lift two people and 6,000 pounds of equipment, up to 60,000 feet, and fly for about six hours [to eight hours]. [We called them WB-57Fs.]

Then we had a couple helicopters that [conducted low level] ground truth [data collection]. In other words, you looked [from] 60,000 feet [with the WB-57F], 25,000 feet [with the P3/C-130], and you looked at about [100] feet [with helicopters]. That way you could [obtain various synoptics data sets and sort] out the [atmospheric effects] and then [calculate the] atmospheric aberrations.

So we started that program and [performed] basic [discipline science and sensor technology] research. There must have been a hundred investigators. We'd have annual [planning] meetings. [In which] we would review the results from the previous year. We would [grade ourselves and] plan the next year. Investigators would [also attend]. Reports were put out and published.

Then there was need—we said, "Okay. What this country needs to do is to be able to do [national and worldwide] surveillance on crops, for both [tactical and] strategic reasons, and we need [to develop] satellite systems [with sensors in the correct spectral ranges to enable this to occur]. We probably need a satellite system that'll come over [the same region] every two or three days," so you could look at the maturing of the crops, whether under stress [and so forth]. The twenty-four-channel scanner and other [experiment] data [was utilized to determine appropriate spectral bands and] was [ultimately] fed into a program called LACIE [Large Area Crop Inventory Experiment, which had global data acquisition capability].

Let me start over again. University of Michigan had [developed] a double-ended scanner, like 36-channel or 30-channel, whatever it was, [for] spectral data [collection and analysis]. Purdue [University developed the test] algorithms. Northwestern [University also] did algorithms. So you had a way of analyzing [spectral] data. From that [program] came the ERTS [Earth Resources Technology] Satellite. The spectral bands for the ERTS Satellite, which was the Earth Resource Technology Satellite, which you folks know today as Landsat, [were determined from our research program]. That was how that group of people and that data and [these] aircraft [and that] "hockey-puck," started the Earth Observations Program. At that time, nobody in the world had that [technology], and now everybody has it, [like] the French have it [in their Spot-Image system].

We went from that to say, "Okay. Let's look at the crops in other countries." There were various agencies in the U.S. Government that were interested in [world wheat production in places like Russia,] and we were interested in it from the respect of [also] being able to monitor our own crops, and the way to do that was a satellite system.

So the LACIE Program was started. [A guy was] brought down from the University of Michigan, his name was [Dr.] Mike Holter. A guy [came] in from Purdue [University], his name was [Dr.] Bob McDonald, who's still around here somewhere. He'd be an interesting guy to interview.… [Robert O. Piland managed the entire program at JSC.] Those are [some of] the folks that started it.

There was a whole pile of people over in [our] Earth Observations Division [at JSC]. The aircraft program managed to have like fifty or sixty civil servants and 350 contractors, and there were probably, I would guess [a total of] seventy-five [other] civil servants. That's not right. Had to be more [like] a hundred civil servants. It was an appreciable drain on the [Center] resources. By then, [Chris] Kraft was the Center Director.

The LACIE Program, Large Area Crop Inventory Experiment. God, how did I remember that? [Anyway, it] successfully proved that you could monitor production from space.

Production is the product of area times yield. In other words, how many acres you've got and how many bushels per acre [are produced]. So you can make a determination from the areas that you monitored throughout the world as to whether or not they're going to have [food and economic] problems next year or not.

It [also] looked at [such things as crop] stress. The Department of Agriculture was very heavily involved in this. So from that [spawned] the Landsat systems. And then, as I've described earlier, there was also [environmental] data that went into the NOAA systems. That was some of the data [we collected]. NOAA had their own research programs.

BUTLER: So is LACIE a satellite program or was it based on the aircraft?

SMISTAD: Well, it was both. Yes. It was [both] a satellite [and aircraft] program in that data was gathered from the ERTS satellite, Landsat satellite [with underflights] of aircraft where you [gather] higher resolution [data]. Because I think ERTS was like I can't remember—30 meter, 60 meter [resolution]? And then there [were] also [low altitude] helicopters, so you got layered data [to] analyze. You could [determine crop] signatures [from] space based [data and layered] ground truth [data collection] under you. In other words, [spacecraft, aircraft, helicopters,] and also people on the ground to [make insitu data collection and] verify the validity of the data.

BUTLER: If we could pause for a moment and change out our tape before you go to the next one. Hold that thought. [Tape change.]

You just finished talking about the Project LACIE experiment and you were mentioning that there was another one.

SMISTAD: Yes. And I can't remember what the devil it was called. It was the follow-on to LACIE and it was—well, I'll have to remember that later on.

Okay. The Earth Observations Program. The determination [had been made] many years ago was that there will come a day—this is what the Department of Agriculture told us [anyway]—there will come a day when the Earth is no longer able to sustain the folks that live on it. There was a concern with birth control, concern with agriculture, feeding, [and all that. And one day] food would [become] the strategic commodity. This is what [we were] told [anyway] and [so] could become a source of people [fighting] other people.

So we said, "Well, one way that this could be helped is that if we could initiate remote sensing [Earth Resources] programs in other countries that didn't have them, [especially] where there [were heavy] population growths, where this could help [them in their planning work with] countries that were receptive [and had a need]. So that cut off Asia, and besides that, we didn't have the logistics to get there.

So it was called an International Participation Program, and the folks that were brought into those programs were Brazil, Argentina, Mexico. There's a commonality there of large nations with large population, and at that time, not too large an economy. That was a long time ago. And Canada [in specialized ice dynamics investigations]….

There were a group of people from the U.S. that went to each one of these countries. I think the State Department was heading it up. And there [were] agreements signed [for] international participation. At that time, by the way, Bob Piland was my boss in Earth Observations Program Office.

So there was a group of scientists from each country that came up [to visit us at JSC]. They were taught the fundamentals and the theories of remote sensing, both from a scientific and technical standpoint. They were then led through data that we had acquired. They were assisted in [the] determination [and] analysis of that data. We then [took] them [to] test sites throughout our country, where they actually did work on the ground, in ground-truth-gathering, spectrometry and stuff like that, and photography, on the ground. They [also] flew on the aircraft, to see what that was like. All of our aircraft [and people] were involved.

Then we went to each one of [their] countries on an individual basis. [We performed] remote sensing at their sites and sent our scientists down there to [assist] with the [data] analysis. They did their own ground truth on their sites. They had agricultural sites, hydrology sites, geology sites, oceanography sites, [and whatever]. [This] initiated the remote sensing programs in those countries. And today, they all have remote sensing programs, which have [been] developed throughout the years. I would hope that it has been of assistance to their countries and to their governments in planning of acreage utilization, [food production,] disease, [detection and] how they might counter that.… I would think it would [have been very] helpful to the people of that country. So [we shared] the U.S.'s technology, which at that time, was—I'm pretty sure it was very unique in the world.

Okay. That's the end of that one.

One of the interesting things was that when we had completed Apollo, we [went on to Skylab]—I haven't hit Skylab. On Skylab, there was an Earth Resources Experiment Package, EREP package, and that EREP package [consisted of] down-looking cameras, down-looking thermal systems. I think there might have been a microwave system on there [also]. I can't remember anymore. Data was acquired [during] the entire duration of Skylab [and] utilized [for] analysis.

As a matter of fact, we did one complete map, from Texas up to New England, [over] this country. John [S.] Llewellyn [Jr.] was one of the flight controllers on EREP. At that time, the program office was called [the] Earth Resources Program Office, headed up by Cliff [Clifford E.] Charlesworth, who was one of the early flight controllers on Gemini and Apollo, [and] who is [now] deceased. John [G.] Zarcaro [and] Bill [William R.] Pogue [were] in that program office. [Also] Al [Allen J.] Louviere. Al Watkins, others.

At the end of [the] Skylab Program, before Shuttle, we [sponsored] a symposium, an International Earth Resources Symposium. It was held in 1975 at the Shamrock [Hotel, Houston, Texas]. There was a [big scientific] contingent [which presented] 200 to 250 technical [and] scientific papers over about a three [to] four-day period. It was attended by 1,500 people from [many] countries and [presented] the results of these experiments that I have been talking about in all the manned [and] satellite programs.

By that time we had ERTS data and also [extensive] aircraft program [data]. The featured speakers [were Wernher] von Braun and Caspar [W.] Weinberger. He was Secretary of [Health, Education and Welfare at that time I believe]. That was a very successful international affair.

Charles Mathews was the [NASA] Headquarters [Associate Administrator for the] Earth Observations Program, and that [symposium period] was the only time that there had ever been a ball game rained out in the Astrodome. It rained like thirty inches [or so]. It was a massive flood down there. You couldn't get to the ballpark to play [because of severe flooding]. That was the year that there was maybe a couple three thousand cars stranded up on the Gulf Freeway

[I-45, and] people wading home. They just left their cars there for days at a time. I think that was 1975, I believe. That was before your time, I realize that. Yes, it was June of 1975. That's when it was.

BUTLER: At least the symposium wasn't rained out.

SMISTAD: No. The people at the Shamrock [were marooned and] stayed. The people that came from home couldn't make it [back home and] the people that were there had to stay. They did a land office business at Trader Vic's, which was the bar in the Shamrock. That was a beautiful place. It's a shame [it] had to come down.

BUTLER: Was the LACIE Program after Skylab, before Skylab, or was it kind of during?

SMISTAD: The LACIE Program was before Skylab. Can you tell me when Skylab was?

BUTLER: Skylab was in '73 and '74 when it actually launched. It launched in May of '73 and went through the early '74.

SMISTAD: I can't remember. I think [LACIE] was before [that].

BUTLER: I believe the Earth satellites were launched in the early seventies, so that would make sense.

SMISTAD: I think so.

BUTLER: Well, I'm sure we can look that up.

SMISTAD: I forgot.

BUTLER: That's all right. When Skylab was originally planned, Earth Resources hadn't been planned as part of the package. Do you recall any of the discussion around that? Integrating it into Skylab, when that came about and how?

SMISTAD: No. I wish I could. I don't recall that. But you're right, it was not—primarily Skylab was observation instrumentation that looked at sun corona and the galaxy, NRL-type experiments, the Dick Towsey type. No, I do not remember how that came about.

BUTLER: That's quite okay. What was the difference between integrating the whole process of experiments from Gemini, compared to the aircraft, compared to Skylab? Could you contrast some of the differences or how the process grew from one program to another?

SMISTAD: Yes. It grew from ad hoc in Gemini, as an afterthought in Gemini. The organizations that were involved, like training and [mission integrated] and [such] that was more or less ad hoc, because the experiments were more or less [added in an] ad hoc [manner]. There was a growing recognition of other things that were possible to do beneficially on the mission.

By the time we got to Apollo, there had been organizations developed or that had been started to specifically handle experiments, as [compared] to Gemini. Mercury was totally—it was just handheld cameras, so that wasn't a big deal.

In Gemini, there was, like I say, there [were] forty [or so] experiments, forty different pieces of equipment flown at least a couple of times each. Initially that was done by people within the Gemini Program Office, [six or seven of us], going out to those [engineering] organizations where we needed engineering help, [also] scientific help and, saying, "Hey, we're doing this [experiment] and we need help." And as that grew, there was a recognition that there would have to be a structure, a recognized structure, [and] elements or organizations formed that [performed that] particular function as opposed to doing that function as a part of their other job.

Now, that was not true necessarily in engineering, because engineering was a matrix organization anyway. You could [always] go to those organizations [for help]. It was [also] true to a [lesser] extent in flight operations, in mission planning and mission operations. And then as you [progress] into today's [time], for example, there are [now] stand alone organizations that [support those experiments and their utilization]….

That doesn't answer your question, but I think that if you look at it this way, is that initially experiments were add-ons and adjuncts, afterthoughts. They were looked upon as corollary types, as opposed to today where there is a United States laboratory that does nothing but experiments, [such as] materials processing and human [research]—a health facility, whatever it's called. Anyway, today, it's a program all in itself, it's structured all in itself. The evolution was because there was a need [for it].

Are you through with those questions? Do you have any more?

BUTLER: I have some on some other—let's see. Well, one on the aircraft, in particular. Did you have a chance to fly on any of the aircraft as the experiments were going?

SMISTAD: Yes. I [believed] that in order to understand and to appreciate the tasks, [problems] [and] complexities that the folks in the aircraft who [deployed] to remote sites [experienced] and who really [were] NASA's voice to those experimenters, I felt it was necessary to understand how that went on such that [when] decisions were made, back at the ranch, [they] would be made in an informed manner. So I did. I went on as many missions as I had the opportunity to [be a participant]. I did not fly in the WB-57F. That was specialized, very specialized training.

BUTLER: And the individuals that flew on the various aircraft, obviously the pilots were trained as pilots and they weren't involved in the experiment-side of things other than—

SMISTAD: That's right.

BUTLER: The other people that flew on board, were they generally NASA employees like yourself or scientists?

SMISTAD: That's a good question. No, on the medium-altitude [and] low-altitude aircrafts, there was a contractor that furnished the equipment operators and trained them, and this was Lockheed [Missiles and Space Company].

In regard to the [high altitude] WB-57F, those were NASA people, and there were five or six folks that were trained to fly in the back seat. For a couple of missions, we had Kathy

[Kathryn D.] Sullivan and [George D.] "Pinky" Nelson. Now, they were not part of our group, but they were qualified operators, and George felt that [there] would be a great advantage if they could become familiar with pressurized suits and operating equipment in a low-gravity situation. But those were the only two that I can recall, other than our civil servants, that flew in the back seat.

In the front seat, they were [initially] Air Force, the 57th Weather Reconnaissance Squadron out of Albuquerque [New Mexico]. Initially, [the] WB-57Fs were called RB-57Fs. The country had two [types of] reconnaissance aircrafts. There [was] the U-2, [NASA had 2 U2-Cs] which was a one-man aircraft and were built in the [Lockheed] skunk works. [The first U-2s] took about ninety days to design and build and [fly] because there was a great need for [their] reconnaissance [capability during those times].

[The WB-57s were originally named] RB-57Fs. [General Dynamics (GD) modified some RB-57s], which was a medium-altitude aircraft, about 35,000-40,000 [foot capability]. They took seventeen of those out of the [Air Force] fleet, put them into GD [who] jacked up the fuselage and they ran [new] wings in under them. They built seventeen of those reconnaissance aircraft. As you can see, [it was a two place aircraft] that hauled 6,000 pounds where a U-2 would probably haul 600 pounds [of] surveillance gear. [The single place] U-2 could get to 68,000 [feet] in a WB probably, comfortably, about 62, something like that, 63. A huge aircraft, though.

One of those [seventeen] aircraft went in [at] Kamchatka, one of them went in [at] Albuquerque, [I think] in a thermal, so there [were] like fifteen left. One of them was dedicated to the Department of Energy [DOE], [and] was transferred, and they did atomic sampling when there was the A-test program. When the Russians were doing testing, and the French were doing nuclear testing, it flew a route from the tip of Alaska down to the tip of South America. We inherited that aircraft when the Air Force went out of that business. NASA had two of their own, which [were] bailed from the Air Force, and DOE had the other one, so we flew three aircraft.

DOE furnished the back-seat operator in their aircraft, a guy by the name of Paul Guthals out of Los Alamos [New Mexico]. The main engines [were] fans off the B-52s. [There] were four engines. They were the two large engines, the turbines, and then there were a couple of smaller engines on the wingtips. When you lit all [four of] those, at the end of the Ellington runway, they were probably at 10,000 to 15,000 feet [at the end of the runway]. They [could go] straight up.

The Air Force then phased them out. They were expensive to maintain, and they put them down at [the Airplane Bone Yard at] Davis-Monthan [AFB] at Tucson [Arizona]. They took the engines off of them and [grounded the rest]. So there are no more left, except NASA's. [There is] one out here at Ellington. I think there are two of them, because I think the DOE aircraft is sitting out there in that big hangar, too, I think. Last time I talked to Joe [Joseph S.] Algranti, which was many years ago, he said that there was one in mothballs in El Paso [Texas]. So that's a very unique aircraft. That's a marvelous aircraft.

The U-2s operated out of Moffett [Field], out of [NASA] Ames [Research Center, California]. The folks that flew those were the same guys that [had flown those] U-2s from Turkey to Bodo, Norway [during the Cold War]. [At that time] they were [in] the same group [with Francis] Gary Powers. Marty Knutson and Chuckie Brown and Bob Carlson, I think, those guys. We had an opportunity here [at JSC] to add the U-2s to our fleet, but the Center management said no. They don't want to do that.

When the Center needed [more] engineers and people, because the Shuttle was tooling up and there was a great need for manpower, and there's *x* number of [total] billets, regardless of what they're filled by, the decision was made to transfer the aircraft program to [NASA] Ames, out to Moffett [Field]. So we transferred the C-130 and a bunch of our sensor systems—most of our sensor systems.

The P-3 went to Wallops for test purposes. I think they also had a remote sensing program of sorts, but I think they used it as a range aircraft. The two WB-57Fs stayed here. What does that leave? There [were] a couple of helicopters, and they were phased out.

So there is no longer a remote sensing program here, an aircraft remote sensing program, although there probably are still some discipline folks that still work at the Center, but I would think they would be in the minority [or phased out by now].

BUTLER: When you were planning these flights for the aircraft, for the remote sensing Earth Observations missions, how often would you fly one particular mission or with one goal in mind and how would you go about planning these flights, and what would be the goals, the objectives?

SMISTAD: That's also a very good question. What you did is looked at the investigations. There would be several hundred candidates for any one year, [for example], and those several hundred were sent down in a basket from NASA Headquarters [who said], "Here, sort these out."

So our planning guys, [there] were a [whole] total of two structured missions based on deployments to a [specific] geographical area of the United States. You could base, say, out of Peterson [AFB] up in Colorado, or Fairchild [AFB] in Washington state, or wherever [and perform measurements at various sites within] the range of the aircraft. Also that aircraft had to have the capability of—it had to have the sensor complement on board that it took to satisfy the spectra ranges of [those] particular experiments.

So you sorted all those out, you had four aircraft to work with, and so you [staged planned] missions. In one mission, maybe you had six sites, seven sites, you [might] have a pre-dawn, and you'd have day, maybe evening flights, depending upon the diurnal requirements. And in that manner you loaded up all of the flights, and you probably flew a couple hundred missions a year total, at least [over] a hundred. And then, of course, you had the inevitable [weather and sensor] screw-ups [and] you'd have to go back and replan [those].

But there was such things as, I can't recall what year it was [1970], that there was an earthquake in Peru and at that time the Peruvian Government—I think it was military, but it was turning Communist. Anyway, the Russians sent their aircraft in to [perform damage] surveillance [for the Peruvian Government and the U.S. State Department commandeered our presence to assist and counter the Soviets].

Let me describe the earthquake. I wish I could [remember] the year. I'll think of it. [It occurred in the Rio Santa Valley,] north of Lima. On one side are the Blancas, and the other Negras, [mountain ranges and] they're maybe 14,000 feet high. The valley floor was like 7,000 feet, 6,000 feet [high]. One of the peaks was Nevado Huascaran, and it [had] cracked several years before under [the] stress of another earthquake. There happened to be a lot of rain that year, [which softened the ground,] and [then] a tsunami [occurred from an] earthquake off shore. The energy in it transmitted through that large cap cracked off Nevado Huascaran and it fell about 5,000 or 6,000 feet. It was gravel and snow and rock and whatnot. By the time it hit, it hit on a cushion of air. It compressed the air underneath it. So it moved virtually frictionless, and the wall was probably, I'm guessing, like twenty [to thirty] meters high, sixty, [eighty] feet high, and it traveled [between] fifty to [one hundred] miles an hour.

It was on a Sunday morning. There was a [town] called Yungay. There happened to be a World Cup in which Peru was involved, so there were a lot of [people] in there [to listen to the radio broadcasts]. They don't know how many. Anyway, [the slide] completely covered that town. So there's like 25,000 people [or so] buried [in their sleep]. The only things [remaining above the slide] were four palm trees in the Center of the town, and the graveyard. The statue of Christ [was] looking out over this complete devastation, and that [picture] appeared on the cover of *Life* magazine. One of [our] guys took a picture of it as we went by.

We stayed there for about four or five days, flew [several sites], and supplied that [photographic] data to the Peruvian Government. We [also] had our aircraft out there on display. People would come in and look [at our gear]. There was about four Russian aircraft there [also], and you couldn't get any closer to them than about thirty yards, because there were folks there with automatic weapons guarding it.

It was really a pathetic situation, because the people were living in tents in that [high] valley. It turned out—I read about it afterward—it turned out that that was the third layer [of people] that had been buried. They just leave people buried, [and] there had been earthquake after earthquake in which there had been this debris.

Other missions we flew that were of interest was a sea state mission [based in Ireland]. We flew [in conjunction] with [the] Naval Oceanographic Office, and with some folks from Ames [Research Center] in their [Convair] 990. We encountered sixty-seven-foot waves, something like that. We flew right down [so] close to the deck [that we] took [salt] water spray into the engines. That data was pumped into NOAA for their system designs [on weather satellites].

What other interesting missions? We had several international missions [and extensive mapping missions like the state of Alaska].

I wanted to go on to Space Station. NASA started looking at space stations in about the 1960s. They'd started looking at space stations [really] before we looked at shuttles. President [Richard M.] Nixon said we can't have both. So OMB [Office of Management and Budget] and Nixon, I guess, opted that decision—and NASA said, "Okay. Let's go with the Shuttle."

We then started looking at the next program after Shuttle while Shuttle was still going on. Well, it still is now today. And the Space Station started with a skunk works operation, which [was in] a couple of buildings [near us]. It wasn't the Agena building, but it was over there where Ed Muniz is now—where Muniz Engineering is. And that skunk works was to determine the size, shape, requirements, [and] the programmatics for Station. The first manager was Neil [B.] Hutchinson, who has since left [for] SAIC. John [W.] Aaron was [the] deputy (flight controller out of the Apollo Program) and guys like Carl [B.] Shelley, Al Louviere, Mark [K.] Craig—oh lord, who else?

John [D.] Hodge was the Headquarters program manager by that time. He came back from [the] Department of Transportation. He had left NASA after Apollo, I think. In that particular program, what I did was commercial advocacy. And what does that mean? It is to generate [the] interest in the industrial complex in this country for the utilization of space, in this case the Space Station, and the mechanisms to integrate them, both [venture]-wise and hardware-wise, into the Station.

At that time, it was determined that NASA's role in the commercialization of space would be effected through the Shuttle and through the Space Station Program. So there were a few of us, here from Johnson, a guy by the name of Bill [William J.] Huffstetler, who is now deceased, myself, and [others]. There were [also] people from other Centers, from NASA Headquarters, [and] the White House. This was during [the Ronald W.] Reagan [Administration]. Bush [senior] was the vice president. He had a staffer by the name of Craig Fuller who came over and helped us. Our NASA leader at that time was a guy by the name of Bud Evans. Bud was the son of Lew [Llewellyn J.] Evans, who [had been] the president, CEO of Grumman [Aerospace Corporation]. Lew subsequently died and Bob Gilruth [who was a widower], married Lou's widow. Anyway, that was Bud's father and mother.

What we did is we set up [commercial support] structures within NASA. [One element] was called the Centers for the Commercial Development of Space, CCDS. They are today called CSCs, Commercial Space Centers. The idea was to [form] consortiums headed by academics in a particular discipline. There were many disciplines. There was power, horticulture, materials processing, metals, alloys, [and] communications [among others]. These Centers were set up throughout the United States at [various] universities.

[They] were to bring industry in on a cooperative basis. NASA would fund seed money, but the joint research would be funded by the industrial groups. The purpose of this was [to conduct] research, commercial research, industrial research in space. [It] was not to [produce] products. People may live long enough to see products come out of space. [It was specifically for industrial] research. This happened to be—scientific research, research that could be applied on the ground [to produce a] product line to make money.

Okay. There were like twelve of these formed initially. We helped form [that group], and we utilized [them as the baseline for] the commercialization of Space Station. Okay. There's one over here at the University of Houston [Houston, Texas]. A guy by the name of Alex Ignatiel, Dr. Alex Ignatiel [heads that up now]. They utilize the vacuum of space for epitaxial [crystal] research.

BUTLER: So each of these Centers focus on one particular—

SMISTAD: One particular discipline. One specific discipline. The guy who started it all [at the University of Houston] was [Dr.] Paul Chu, who [was the] superconductivity guru. And let's see. There are two at [Texas] A&M [University, College Station, Texas]. There is one power and one engineering. There's [one] in Wisconsin, Colorado, [and] Florida. And they still exist. And that was a [starting point for] commercial advocacy [for] Space Station.

So, what did we do? [How did we effect commercial advocacy for the station?] Well, we [started by forming] a committee [assisted by] NASA Headquarters. [It was called the Commercial Advocacy Group (CAG).] Turned out to be old guys from [each] of the [NASA] Centers. One attribute of [our group], by that time we were all bulletproof, so it didn't matter what we did.

We were dedicated to the [fostering of] industrial research [and participation for the Station]. This is something that, as an individual, I did.… One of many things. We [also] determined that there was a requirement for a [research] laboratory in space, much as there are laboratories here on Earth. We [formulated the] requirements for it. What are the requirements? Well, requirements are how much power you need, the volume, the systems, what sort of experiments would be [performed]. [We] put all that together [and got a technical requirements study funded for the] MMPF, the Microgravity Materials Processing Facility, or something like that. We got authorization to do this from John Aaron. The study [was awarded] to Teledyne Brown [Engineering] in Huntsville [Alabama], and they came up with the characteristics and attributes and requirements for that lab, instrumentation, and all that sort of stuff, and which you now see as [the] U.S. lab in Space Station.

We met a lot of people, went to lots of small-, medium-, and large-size [commercial/industrial] companies in this country to interest them in research [participation]. There weren't any that I recall that did it on their own in that NASA had to provide seed money.

We were greatly assisted by the structure that had been put in place through the CCDSs, through these Commercial Space Centers. The gentleman that I worked for in doing that was Carl Shelley. Carl is [still around]. He's working for JAMSS America [Inc.], and he would be a very interesting man to talk to.

BUTLER: We just talked with him, I think late last year it was.

SMISTAD: You did? Carl?

BUTLER: We sure did.

SMISTAD: He's a first-class man, first-class guy. Okay. What else happened in Space Station? That was Level B. I worked in Level B, which [now] is, I guess, Level Two.

In about 1986, whenever it was, there was a determination made that [the Level B] function was going to go to [NASA] Headquarters, and eventually it went to Reston, Virginia. I guess there was, in that whole program office [at JSC], there must have been like 150 of us. There weren't many people. Gemini was seventy-five, by the way, in the whole program office. And I think by the time, '86 or so, it was maybe 150, maybe 200 people in the program office, supported by E&D [Engineering and Development] and stuff like that, [in] a matrix [manner]. So the determination was that the whole function was going to go up to Headquarters [and] be merged.

I decided, anyway, that I was too late in my career to check that block. It turned out that I went to talk to the Center director, and one of the guys that helped guide me was also Roy [S.] Estes. He's a good friend and a good man.

I think of that 150 to 200 people, however many there were, I think there were less than ten that went to Headquarters. So Headquarters, when they started out in Reston, had pretty inexperienced people, and it showed. It has shown, unfortunately, in the early phases of the Station.

About the same instant that that happened, I got a call from Max Faget. Max, who I'd worked for before, had started a company called Space Industries [Inc.]. Space Industries was really a spinoff of Eagle Engineering [Inc.], Eagle Engineering [had] guys like Owen [G.] Morris, [Joseph Guy] Thibodaux was over there—no, not Thibodaux, Bass Redd, [some] of the old-timers from NASA were there. And he said, "How'd you like to come over here and work for us?"

I said, "I would be really interested," because that was a commercial venture [along the lines of my CAG experience and] not funded by the government. It was all venture capital stockholders that had put money into it like—oh good gosh, I think one of the principals was David [H.] Langstaff, who is now the president and CEO of [Veridian]. Is that right? [Veridian]. What's the telephone company?

BUTLER: Verizon?

SMISTAD: Verizon? Okay. It's [Veridian]. It's the other one. And Joe [Joseph P.] Allen [is] there. I think he was President [of Space Industries at the time]. There [were] probably [ten to twenty] of us [old NASA types there to start].

The idea here was to build with private money, a private initiative, a man-tended facility. The man-tended facility was called the Industrial Space Facility, the ISF. It was launched by and serviced by the Shuttle. It had a thirty-year lifetime and it would be manned during periods that the Shuttle was attached to it [through] a docking tunnel. It had its own propulsion system, com system. It had a propulsion system, for example, that when the Shuttle wasn't there, it would pump it up thirty or forty or fifty nautical miles and then it could decay [and wait for the next Shuttle visit].

During the time that it was occupied by the crew, the life support system would be provided through the Shuttle, and then during the time it wasn't there, it would have lithium hydroxide canisters and stuff like that to get the crap out of the air so it wouldn't be moldy.

Westinghouse Space became a partner, so it became the Space Industries Partnership, SIP. They provided engineering and also invested money.

David [negotiated] a SSDA, a Space Systems Development Agreement, [with] NASA. Jim [James M.] Beggs was the Administrator of NASA at that time. In [the] case [of a SSDA], you have to be developing a system that [was] of benefit to [the nation and] you're going to make money with it, one or the other, and we fell in both those two categories. Actually, I have an idea I'm making some of this up, because I can't remember all the specifics.

What you did [was repaid] NASA back for the [launch costs] out of your revenue stream. [The] vehicle would accommodate industrial research, NASA research, government research, in other words, users. The [users and] utilization would be broad.

The Space Station was just starting at that time, and we got to the point where we got through [preliminary] design, through the Phase One safety reviews here at Johnson and at the Cape, and at that time we became a threat. We became a threat to the Space Station. There was a NASA review of [our industrial] program in which there was—oh, by that time it had become so involved with NASA, in that NASA was integral in the decision process. I can't recall exactly how that happened, but that was an important step, and NASA was going—oh, I know what they did. They said, "We can't allow you to do this anymore on a sole-source. What we have to do is we have to compete this, because we're looking for NASA business." That's what happened.

When that was to be done, there were two committees [formed by NASA]: there was a Cost Committee and there was a Science Committee. The Cost Committee was headed up by a gentleman from NASA Headquarters, Bill [Lilly]—the comptroller of NASA at that time. And Joe [Joseph P.] Shea, who was then at Raytheon, past manager of Apollo, [headed] the other one.

The determination of that committee was such that (we figured we could do that whole program, including [several space facilities], ground equipment and spares and logistics modules, for right under 900 million) and a number that NASA came up with was something like 4 billion. Also the scientific assessment [was that the capability was] premature, and it would be better if it came after the Space Station, [instead of] before the Space Station. [These decisions were not surprising, indeed predictable, and the Space Station was no longer threatened.]

So at that time, the Industrial Space Facility became a nonentity. It did get up to Congress for a vote for funding. Ironically enough, at the last minute, what was thrown in to that same bill was SPACEHAB. They came along as a rider, and today's SPACEHAB is alive and—I don't know if it's well, but it's alive. What we did at that time is we said, "Okay. We either have to go out of business or we've got to go find other business."

So based upon the contacts that I had made earlier in the Commercial Space Centers, we did manage to secure three programs. One was called the Wake Shield, which was an ultra-high vacuum facility, a shield really, launched on Shuttle, [for] the University of Houston [CCDS]. That was Alex Ignatiev.

The second program was called COMET, which is an acronym for Commercial Experiment Transporter. It was an unmanned scaled-down version of Gemini. The program had a vehicle [and] a launch system. [It was] a NASA-funded program through the CCDSs. The launch system was the Conestoga, which was Deke [Donald K. Slayton]. He was [heading] that program. I worked for Max and Joe [at Space Industries].

We had [vehicle design and fabrication, payload integration], mission operations; we had all the attributes, all the functions that you have in a normal program. We were going to launch out of Wallops [Island, Virginia] and recover outside of Hill Air Force Base, Utah, on the other side of Salt Lake City in Skull Valley, out in those flats.

That went along well until there were technical [and funding] problems in the [overall] program and NASA has to cancel it. And that's about the end of my story on that. See if I missed anything here that might be of interest where I might have to go back.

BUTLER: You certainly had quite an interesting career, that's for sure.

SMISTAD: Well, I'm working very diligently today, personally. I have a real personal interest in commercialization [of space and the Space] Station. If we go back to an hour and a half ago when I said that my dad came over and he said, "You owe a lot to [this country]."

Will Rogers said it very well. He says, "It's a great country, but you can't live in it for nothing. You've got to give something back…."

So I still have a very personal interest, and I'm going to continue in the pursuit of that. I've been working now part-time for one of the aerospace contractors here, and then also I do some of this on my own nickel.

This is hit and miss. This is stuff I got out of the attic last night. I took the trouble to get it so, by golly, I'm going to read it.

BUTLER: Absolutely.

SMISTAD: In 1970, in the Earth Resources Program, we had a visit from the U.N. Committee on the Peaceful Uses of Outer Space, must have been thirty, thirty-five countries that came at that time to listen to where we [were] in remote sensing and what [benefits] there might be for their countries. That was in 1970.

BUTLER: That must have been interesting.

SMISTAD: Yes, it was. They were really nice people, very interested and very high up in their governments. They were just ordinary people. I think that that's a characteristic of people like that.

Let's see. Let's see here. [Referring to documentation] That's my life story.

BUTLER: Okay. You've mentioned, as we've been talking, several of the people that you worked with, and I was wondering if you could tell us a little bit about some of the people that had a personal impact on your career or that you think were key contributors to the space program? Maybe you can mention some of them.

SMISTAD: Oh, there are so many key contributors. I certainly was not in a position where I met or knew all of those folks, so I've told you those that I came in contact with, but I certainly, if I tried to do that, I would slight somebody. People that have had an influence on my career I can add—family, father, mother, high school educators contributed, in that you can do what you set out to do.

Within NASA, I guess Max, for sure, Carl Shelley, Chuck Mathews, Kenny Kleinknecht, [folks like that]. I worked for a gentleman by the name of Major Dick Henry, who later became Lieutenant General Dick Henry, who headed up Space Command eventually. He was an influence. Roy Estes was really a personal influence.

It's an interesting question. And, of course, the family I have now. You didn't spend a great deal of time at home in Gemini or Apollo, kind of went somewhere and slept on your desk. It wasn't quite that bad, but weeks on end, and the families at home had to adjust to that. The wife had to adjust to that. I made a decided effort to be involved with my kids in the formative years. So I coached. I had two boys and a girl, and it was fortunate they [were] all good athletes. They don't make their living as athletes, but it beats the hell out of any other peer group that you could get them into.

So I coached football and basketball, Little League sports, stuff like that, became involved with them. And then I coached soccer, and I was one page ahead of the team in the textbook. I was a total spastic. My daughter went on and played collegiate ball in soccer, so apparently it came out all right. But she had more talent than I had coaching ability.

I think all of us know that we came along in a period of time in which we had the opportunity to do—it was fun engineering, but we had the opportunity to give, I think, the nation some pride at a time when we were struggling, pride in a program [and] pride in this nation. I mean you were able to look at something besides your own problems. The nation was really behind the program, and the program will become exciting again. NASA will again become a very exciting place to work. They will be doing research and development, I think, a great deal more than they have been doing. It's a great Agency and it's a very good group of young people in it, dedicated. If I had it to do all over again, I'd do the same thing.

BUTLER: Well, looking back over your career at NASA with the space program, what would you personally say was your biggest challenge and then also your greatest accomplishment?

SMISTAD: Oh, I don't think I have any great accomplishments. Biggest challenge? I think in [helping to] start the remote sensing field, starting with a program that was bits and pieces, nothing, and helping to mold people and organizations together with results and products. Again, I think products that are assisting people to help with their resources, to help control their resources and manage them, throughout the world. I think that's an accomplishment. That was difficult at the time. I mean, I wasn't the only one that did it, but it was an appreciable effort and it was a rewarding effort.

As far as, really, the most fun part, I think Gemini was the most fun. It was really a neat bunch of people, and we learned as we went along. If somebody couldn't or didn't get the job done, you went over and helped him to do his job. I know Kraft said that one time, somebody brought up to him in the meeting, "Well, so-and-so is doing my job."

Kraft says, "Hell, you've got enough to do anyway. I mean if he's going to do your job, great. Don't worry about who's responsible for it."

And when it's all said and done, like today, is that they have a lot of fun. We have what we call—[I'll] use another word—the old-timers. I'm going to use that word—the old-timers' breakfast. About once a month we get together over at Kelly's, you know, swap stories. At this period of time, it's, "Did you hear that so-and-so died last week?" That's about where we're getting. But it's fun. Those are the folks that were around in the sixties and seventies.

BUTLER: Good that you're still able to stay in contact.

SMISTAD: Yes, it is. It's neat. You forget the organization and perhaps the bureaucracy, and what you remember are the people. Like John [S.] Llewellyn, you know, how can you forget John? [Laughter]

BUTLER: I'd like to take this point to ask Kevin and Jennifer if they had any questions before we finish up, if that's all right.

SMISTAD: Be glad to try to answer them.

BUTLER: Okay.

ROSS-NAZZAL: You had mentioned that you did some work on the Apollo 1 fire, specifically related to the experiments. Can you expand upon that?

SMISTAD: Yes. There was a system in there called the Medical Data Acquisition System, the MDAS, and [it recorded data from our medical experiment] instrumentation [placed] on the astronauts and then there was also some other experiment hardware that was putting out data. That data was recorded on a Medical Data Acquisition System. At that time I think it was just tape [recorded]. It was in 1967, I guess. To analyze afterward.

When the vehicle burned, it was not [immediately] known why. There [were] some theories which [later] turned out to be accurate. So there was an [investigative] test program, [testing] every piece of equipment that was in that vehicle [and] foreign to the vehicle, foreign being it was not shipped out of Downey, [California], with it. The vehicle was tested separately in the vacuum chamber over here at Johnson.

I was the experiment project engineer on that vehicle, and it was my responsibility to test that MDAS to see whether or not it could be the source of the ignition and the fire. The [gentleman] that headed up [the] board was Floyd [L.] Thompson. He was the director at Langley, at Langley Research Center, a very highly respected man and engineer.

My assignments were given to me by Max and Bob Piland, and I participated in testing [our] system. We ran shorting tests on it to see what happened if there was a dead short across it. We ran characteristic tests on it—were any of the spurious signals that were coming over into the vehicle data? Could that have come from an ignition source? And was that characteristic of the signals that we were [recording]? And there was a guy by the name of Don [L.] Teegarden, who was in charge of that phase of the testing. And Don [Donald D.] Arabian was another one. And we ran extensive tests on that.

I went to the Cape to pick up the backup flight system, because the other one burned. I had that system handcuffed to me, and we bought a seat for it. So I flew [with it handcuffed to me]—it was that kind of an investigation. As it turned out, I can't recall specifically, but I think what happened was that wire bundles on the floor of the vehicle or in the side of the vehicle had been damaged through either personnel traffic through there or whatever, and there was also [an] ignition—I don't know the cause of it. I can't remember exactly. So what I'm saying might be 15 percent wrong, which is dangerous, but I'll go on with it anyway.

The ethylene glycol, which was a coolant that went through the vehicle cooling system, there had been leaks in the fitting, and that ethylene glycol had come out on the deck. What was in there I don't recall, whether there was a rug or whether there was fabric of some type. You clean it up, but it's still residue. You can't get it out of there. So the source of the—there was a short of some type. There was electrical spark of some type. I think they determined that it came out of the wire bundles, but then again, I wasn't in that part of the investigation, which promulgated itself into the ethylene glycol, there was Velcro, and various [other flammable stuff plastered inside the] vehicle.

And [a very significant] thing was the [over cabin] pressurization [with] pure oxygen [to replicate the pressure differential encountered in space]. [It was] like a bomb. The hatch at that time was designed so that you could only [open] it off from the inside, and [that took some time]. There was a complete redesign made of that hatch, such that it was much easier to operate.

That's the, I guess, three or four months. That was part of that seventeen-month down time. There was a lot done in seventeen months. That was a time that you didn't spend much time at home. That was a dedicated group that says, 'We're going to do it. We're going to go from here." And those three guys didn't [perish] in vain.

ROSS-NAZZAL: Thank you.

SMISTAD: You bet.

BUTLER: Kevin?

RUSNAK: I did have a couple. The first relates to a comment you had made earlier how in the Gemini Program it generally only took a few months to get an experiment on board versus on the Shuttle or later programs where it took a longer time. What was the role of, and the attitude of, the astronauts in terms of this process, in terms of learning about experiments and getting them integrated into machines that they're going to be operating and performing the experiments in?

SMISTAD: Oh, in a large respect, they [felt they] were a pain in the ass. Is that what you're asking?

RUSNAK: Essentially, I think so.

SMISTAD: Yes. In that the experiments were not initially—when you started out with a mission plan from Gemini, for example, it was not part of that mission plan. You know, Gemini was invented back here during the Mercury days and it was invented as an engineering R&D vehicle to do things with for long duration, fourteen days, which is roughly Moon, and rendezvous and docking and EVA and stuff like that … [technical stepping stones that had to be accomplished in preparation for the lunar program.]

Then there came experiments [from] out [of] somewhere [to be performed on top of the other work]. Here comes experiments. And it was an added burden. It took time. It took time to train. It took time to do it. There was a recognition, though, that when you have a seven-day mission, which is what [Gemini] V was, I think—V was seven [days] and [Gemini] VII was fourteen, I think that's how it went. That's a lot of time. What are you going to do for that [all] time? What can you beneficially do? Experiments.

So I'm not answering your question directly, but it depended on the personality of the guy that was flying left seat or right seat. Some were really neat to work with. I mean, these are professional people, and they're dedicated people, and they're damn good at what they do. And here comes the guy with an experiment. I know Kraft felt that way initially. But then I think, in reading his book, I was heartened to see about the time of the middle of Gemini, he looked at it a little differently.

I talked around it, but I think that's about what it is, as close as I can come.

RUSNAK: The second question was related to your Earth Resources work, and perhaps this was something above your level, but maybe you can comment on it anyway. Why is it that JSC ended up with this responsibility and then why does it end up going away, I think, like Stennis Space Center for instance, takes over a lot of these responsibilities?

SMISTAD: That's a good question. It started at JSC because of the people there that were very interested in doing it and took the initiative to go to Headquarters [to sell it]. People [like] were Leo [F.] Childs. Bob Piland was one of them. I mentioned Harold Toy. People. People did it.… There're some interim answers of questions that you didn't ask that I'm going to get back to. When it went away from JSC, it was a question of priorities. The billets that were doing that work were needed [for other programs like] Shuttle. I think so. Kraft needed help, needed more help. So that's what those billets went into.

You asked about Stennis, and that's interesting. That's very interesting. Senator John [C.] Stennis—I certainly was not in it on this level, but, in effect, he said, "Look. All this [space] work's going to other states. I'd sure like to see [some NASA work] down here in Mississippi." And he didn't say, "Well, I'm on the Appropriations Committee," or on these other committees. He just said, "I really think it would be a good idea if you thought about doing something down here," with nothing specifically in mind.

Bob Piland was given the task by Bob Gilruth. I think it was Gilruth at the time. Sure, it was. Had to be. "You go over there and figure out what to do, what we can do over there, what NASA can do, what we can put in there."

Bob was Earth Resources oriented, among other things—by the way, he [started as a] presidential intern. He was in the White House for a time. He's a superior man. The determination was Earth Resources, Earth Observations, and it was on a small [scale]. JSC had a large program, and Stennis had a small program. They had a jet of their own, a small jet, I think a Learjet. And they had instrumentation and they did remote sensing, and what they generally did was they worked with small commercial firms or medium commercial firms, to get the Earth Resources technology out into the commercial sector, whereas we in the Earth Observations Program here at Johnson generally worked with government organizations, international organizations, [large scale research, many investigators, and larger program].

So that's how it started, and then Bob came back here, I don't [remember] where, but he came back to the Center. He sent me over there a couple of times to be a chairman of SEBs [Source Evaluation Boards], and I enjoyed working with those people over there. It's a good Center. They have an excellent Center director, Estes, Roy Estes.

RUSNAK: Was it through that work that you met him?

SMISTAD: It was. Well, yes. It was through that work and then later when he became Center director, too. He was a deputy for a good number of years for a guy by the name of [I.] Jerry Hlass who was, I think, the second [or third] Center director out there. I'd forgotten about that.

RUSNAK: Those are all the questions I had.

SMISTAD: Thank you.

BUTLER: Well, thank you very much for coming and talking with us today, sharing your experiences. You had some very unique ones, very interesting.

[End of interview]