

ORAL HISTORY TRANSCRIPT

ROBERT O. PILAND
INTERVIEWED BY SUMMER CHICK BERGEN
HOUSTON, TEXAS – 21 AUGUST 1998

BERGEN: This is an interview with Robert Piland on August 21, 1998, in the Signal [Corporation] office in Houston, Texas, interviewed by Summer Chick Bergen and assisted by Ellen Lee and Carol Butler.

Let's begin after you graduated from college. You got a degree in mathematics, correct? And then you went to work for Langley Aeronautical Laboratory in Virginia, which was part of NACA[National Advisory Committee for Aeronautics]. What led you to that position, working in aircraft?

PILAND: It was close to where my girlfriend lived. [Laughter] I had majored in mathematics and minored in physics, ... after I was discharged from service and I came back to school at William and Mary, I asked a counselor what program could I take and graduate soonest, and they said mathematics. So, mathematics.

Later on, the placement bureau told me there was an opening for mathematics majors at NACA [National Advisory Committee for Aeronautics] at Langley. So I went down and went to work there. That's how I happened to be there. I'd never built any model airplanes or dreamed of the Wright Brothers or any of those things. Like I say, my girlfriend lived in Williamsburg [Virginia].

BERGEN: Definitely an incentive.

PILAND: I went to work on my twentieth birthday. That's how I remember when I started work...

BERGEN: So you began working in the Pilotless Aircraft Division [PARD]?

PILAND: No.

BERGEN: You didn't begin there?

PILAND: I began in the Stability Research Division [in] a small section that did theoretical work. That's where the mathematics came from, and I worked there for about a year. Then I transferred to the Pilotless Aircraft Research Division, and the circumstances of the transfer we won't go into. That's another story. But it doesn't have anything to do with this oral history here.

BERGEN: Okay. That's fine. And then you began working in the Pilotless Aircraft Division. What did you do there?

PILAND: Well, for probably the first year I continued the type of analytical or theoretical work I had done in the Stability Division. Then one day a fellow named [Carl A.] Sandahl came in and said, "Hey, would you like to do some experimental work with the rockets?" I said yes, and the fellow I was working for said, "Okay, if you want to," and so I moved from doing theoretical work with my pencil and integral tables and started doing experimental work. It just happened. I was offered the opportunity, and I knew I was not the greatest mathematician in the world, I was not going to make a mark in mathematics, and the experimental work seemed a lot more exciting and interesting. So I went to shoot rockets.

BERGEN: Did you enjoy shooting the rockets?

PILAND: Very much so. It was the most interesting part of my total career, absolutely.

BERGEN: Really.

PILAND: Absolutely.

BERGEN: So what parts of the rocket work did you enjoy specifically?

PILAND: Well, there was a number of projects. At that time the ballistic missile projects were under way, and there was a need for information at very high speeds and very high temperatures, and we were trying to achieve those high speeds by stacking rockets, essentially, staging them. We didn't have huge rockets like we have today. In fact, most of the rockets we got were leftovers from the Army and Navy, and what we would do was not use them as they did, but we would stack them on top of one another, each one adding more speed. So for a number of years there we worked diligently at seeing how fast we could go and therefore make measurements at the higher speeds.

Also, we did other work related to the shapes of ballistic missiles. Ballistic missiles started out—people thought they might be streamlined and long and sharp, and then as people studied and learned, they finally decided that [a] very blunt nose like Apollo was the way you were probably going to have to go in order to handle this heating problem.

But anyway, developing and building these rockets, I think we finally got up to five stages of rockets, and these we launched from up at Wallop's Island off the Virginia coast. We had some considerable success in that area, and there's no question that that certainly contributed or was an underpinning to the work that many of us later did in the space program. That was the basics. So I stayed there for—I was with NACA ten years total.

BERGEN: And while you were there working on rockets, Sputnik launched, right?

PILAND: Yes. What was that, October '57, I believe. Yes. I was there ... and it was pretty interesting. I thought that was great.

BERGEN: So the people you were working with, was that of any special interest?

PILAND: Oh, yes. It was of particular interest, because, you know, the U.S. had a satellite program. Our interest, frankly, as far as our work goes, was more related to supporting the ballistic missile program, which operated in the same speeds and had many of the same type of technical problems, reentering the atmosphere and so forth. So our work was more directed towards the ballistic missile program, but technically there was great overlap. So we had a technical and professional interest in that.

BERGEN: In March, the next year, you were chosen to be an assistant to Dr. James [R.] Killian [Special Assistant to the President for Science and Technology].

PILAND: Yes. ... Dr. Killian requested NACA to provide him with a technical assistant ..., to support him in things related to space and missiles, so I was transferred from NACA to his office in March. Now that you remind me, it was March. Because I got up there, and it was the middle of a snowstorm. That's how I remember. I got to Washington at ten o'clock at night and didn't really know where I was, on a train—you remember trains—and it was snowing like the dickens. You couldn't get a taxicab. So that was the beginning of my career in Washington.

BERGEN: Did you volunteer?

PILAND: That made me right then decide I wanted to get out as soon as I could. [Laughter] I might have even been subpoenaed by now.

BERGEN: So while you were in this assistant position—

PILAND: I wasn't his assistant; I was a technical assistant. He had about ten or fifteen people that worked in different areas, disarmament and other things, and I was the technical assistant for space and missiles. He was also chairman of the President's Science Advisory Committee. That committee had ... panels. For example, it had a space and missiles panel made up of eminent scientists from around the country. I served as secretary to that panel. I was not on the panel, I was secretary to the panel, and overall helped him in anything he had to do. Every time there was going to be a press briefing at the White House in the morning, which I guess there was every day, and something in the news related to missiles, they'd come over and say, "Hey, instantly write some set of information on this missile so we'll have it there in case somebody asks us about it," so you did that. You did lots of things.

BERGEN: Did you make recommendations to Dr. Killian?

PILAND: Oh, sure. You'd make recommendations. And other jobs... They decided to write a space policy, the U.S. government, that is. They'd never had a policy on space, and, frankly, it was a different kind of policy. Most of the policies written had to do with regions or countries. We had a policy on this country or that country, a whole stack of policies, you know. Much of the National Security Council's business was developing policies for national security. So they decided they needed a policy on space.

So they started this by forming an interagency committee, representatives from various government agencies, to draft a policy. So I was somehow made chairman of this interagency working group to write a policy. I didn't really know what a policy meant particularly, not that it mattered. So what we would do, we'd write a draft and then we met with something called [the] Planning Board. The Planning Board was chaired by the President's national security advisor, the head of the National Security Office (not the council the President's head of the council), and he ran something called a planning board. This had the deputies of the various agencies, a deputy from State, a deputy from Defense, and so forth, and what they did was they reviewed and argued and discussed these policies, refined them.

So we'd write a draft, and then we'd get in front of this Planning Board and get torn apart and rewrite it and write it again and rewrite it until they finally got down to where (they discussed this amongst themselves) to where they felt the policy was complete, and there would be two or three places in there where the President, in meeting with the National Security Council, would have to make a decision. It was written so that he could cross out this line and it would make it go one way and he could cross out the other word and make it go the other way.

For example, I guess, when it finally came down to it, it boiled down to whether the United States had a policy to be *the* preeminent leader in space or *a* leader in space. That sounds like a small difference, but budget-wise and in direction to various agencies, it made a big difference. I mean, if you say to an agency, "Hey, I want you to be the leader in space," you assume it's going to cost this much. If you say, "I want you to be a leader, one of several, doing good work," it's another thing, but very crucial. So anyway, that was another thing I did. That was very interesting.

Had lots of interaction with the Department of Defense, not surprisingly, from the missiles side of the business, and then—well, that gets too detailed, but anyway, there were a

number of things. I guess another thing, Dr. Killian played a key role, if not the key role, in deciding what roles and missions NASA would play vis-a-vis the Department of Defense. There were numerous meetings between him and Department of Defense where they went through this process. Dr. Killian was apparently greatly trusted by President [Dwight D.] Eisenhower as far as the space business goes and what decisions to make, and so, frankly, it was interesting that it came down to the last day, the last meeting, was "Who's going to run this man-in-space program?" So that finally was decided. I don't think much is known about the series of discussions that led to that. Even in his book, he doesn't go into the details of that, or any great detail. A very interesting time.

One other thing I did. As part of the first NASA bill or act, something called the National Aeronautics and Space Council was created. I guess it still exists. The administration didn't want it. The President was chairman of this Space Council. Lyndon [B.] Johnson ostensibly created it or proposed it. Supposedly it was to take care of disagreements and arguments between NASA and Department of Defense. Well, the President said, "I'm supposed to do that anyway. What do I need a Space Council for to do that?" But the administration said, "Okay. We'll go with it, and, frankly, we'll handle it as we choose to handle it. We'll create it." Well, they decided that, hey, they're not going to set up another bureaucracy to support this thing, another self-serving staff, etc., etc. Since then I think there has been some sort of staff set up.

So they turned to Dr. Killian, and they said, "We want you to provide the support for this thing." So he said, "I don't have anybody," or any large numbers of people. What this amounted to was, I was made the first secretary of the National Aeronautics and Space Council. I guess while I was there, there were, oh, I don't know, four to six meetings. I'd write up the minutes and Dr. Killian would look at them, decide they were okay, take them over to the President, and he'd sign them. Like I say, they didn't, frankly, believe in the concept or the need for that council.

They used it to make some decisions. Like, for example, it confirmed the transfer of JPL—I don't know if "transfer" is the right word—but the arrangement of JPL [Jet Propulsion Laboratory] leaving the support of the Army and moving to the support of NASA at one of the meetings. That same day they were supposed to arrange to transfer the Marshall, the Redstone unit, the Marshall Center, to NASA, but at the last minute somebody in the Department of Defense started raising a ruckus and appealing that, so they put that off and did that three months later, six months later. So all that time there were many things.

BERGEN: So, having been in that position, what was your view of where the space program was headed at that time?

PILAND: Oh, I don't know. I don't know if mine was any different than anybody else's. It was a little premature to think about the lunar program, although as part of trying to look at the space program and where it might lead, OMB [Office of Management and Budget], which was then BOB, the Bureau of the Budget, made some sort of projection looking at an end point of a lunar program. Very few people, if anybody, would even talk—well, you know, the science fiction people were right there. And they [BOB] made some sort of a projection—I don't remember what it was, some sort of financial projection related to the lunar program, that is the only thing I particularly remember. There was emphasis on the science program, obviously, and communications, obviously.

Right from the beginning, it was very obvious that the commercial people were going to play a major role in the communications business, which they have and did; and the science program; and, I guess, the man-in-space program. I personally was aware that people back at Langley had started studying it very energetically. So in that context, I was interested and aware of that activity, but that probably wasn't projected any more or any less than anything else.

One of the interesting things, you wouldn't believe the schemes that came through that office about "what we ought to do and let me do it" from various organizations, companies, and everything else, just every day. And I got to look at these.

BERGEN: Can you share some of those with us?

PILAND: Well, one of them which wasn't that far out was to put the X-15 on top of an Atlas missile as a way of getting it into space, and I won't say that didn't have some thought to it. I think there would have been some great difficulties with that.

BERGEN: You were in that technical assistant position, and then you went back to Langley as part of the Space Task Group.

PILAND: Yes. I was offered the opportunity to stay in the new NASA headquarters, and I didn't see myself as a headquarters type, and I decided I wanted to go back and, frankly, rejoin the people I had worked with before, particularly Dr. [Robert R.] Gilruth and [Maxime A.] Faget and others, and that seemed like that would be interesting work, so I went back.

BERGEN: So what did you do when you got back to the Space Task Group?

PILAND: The Space Task Group was divided into three organizations at the time. I don't know what it was called, Space Flight Division or something. It was basically the Engineering Division. That was Faget's. Then Operations. I guess Chuck [Charles W.] Mathews might have run it initially. I'm not sure about that. Then there was a third, Support—administration and contracts and other things. A fellow named Charlie [Charles H.] Zimmerman ran that. Gilruth was head of it. Charlie [Charles J.] Donlan was his deputy

assistant. When I came back I became Max's assistant or deputy or whatever word we were using then for helper. [Laughter]

BERGEN: So what was it like working with Mr. Faget?

PILAND: Well, see, I'd worked for him before, and back in the days when we were stacking up those missiles [rockets], we were in competition, and then the competition had gotten kind of bad, so they reorganized the place and combined everybody that had been competing. So I went to work for Max, and somebody else went to work for [Paul E.] Purser, who I had worked for. But, no, I worked for Max, and he mentored me and led me and led me, and he and Dr. Gilruth and Chris [Christopher C.] Kraft [Jr.], the three of them I look upon as the people I worked for in my career totally. There were other people here and there, but they're the people who had an impact on me and helped me and so forth.

Where were we? So anyway, one of the things, the place was expanding very rapidly then. So one of the places I helped Max was in bringing these people in and integrating them into this rapidly expanding division, frankly, to leave him time to think and do more important things. So I did a lot of that initially.

I was involved in one particular thing. We had a small committee, Chris Kraft was the chairman, and its job was to design the concepts, anyway, for something called an abort sensing system. You might have run across that. It was to be placed on the rockets, the early rockets, to essentially sense an impending failure and jettison the capsule and so forth. And I was on Chris's [committee]. There were about three or four of us. I guess that's where I got to know him quite well. He carried the load of that, to be frank with you. He had a knack for—it was a logic system, and he carried the load. I helped it a little bit, but he really carried the load of that. So I did that and, like I say, helped with the day-to-day problems of running a division until a little bit later.

I don't know whether it was six months or a year after I got back, they had begun to think then about what are we going to do after this Mercury Program and what's going to come next? Max had served on a committee, an agency-wide committee called the Goett Committee. If you're into the history of this thing, the Goett Committee was pretty important, because they came out with the specific recommendation for a lunar program, and as far as I know, at least formally within the agency, that was the beginning of that. Max was a member of that committee. That instigated more interest of, what are we going to do next?

Up until then, I guess, the Space Task Group, for practical reasons, if nothing else, so concentrated on this Mercury thing, that's all they could do. But by then we had a few more people, and somebody, I guess Dr. Gilruth, decided we needed to start thinking a little bit about the future. One or two people had spent a little time thinking about some projects, but then they decided to get serious with the thing called Apollo, so they formed an Apollo Projects Office, and although it was the forerunner of the Apollo Program Office, Spacecraft Office at JSC [Johnson Space Center], it was different. It was intended, first of all, for studies, and it was located within this division that Max Faget ran, within the Space Flight Division. It was called the Apollo Projects Office, and the "S" on Projects even had a significance to Dr. Gilruth. I never understood it until ten or fifteen years later.

I was made head of that office to essentially manage and coordinate our efforts in the Space Task Group related to starting to look at a lunar program. Things were very undecided at the time. In fact, the way we looked at the program, the way Dr. Gilruth had us look at it, was that, hey, it might not turn out to be a lunar program, but it might turn out to be a space station program, and, in fact, early concepts with the spacecraft were set up so that that thing we call a service module could be a space station, not a service module.

Anyway, that's kind of when we started the effort related to Apollo. We didn't do all the work in that office. What we did was try to manage and coordinate (whatever program offices do), and there's probably not a good word for it. One, we had an in-house study being

done by the NASA people primarily in that division with some support from the Operations Divisions, a design study for a spacecraft. So that was one element of it.

Then we had three contractual studies that went on simultaneous. For the princely sum of a total of \$750,000, the whole Apollo Spacecraft Program, as far as contractual work, spent that total. That was all that was spent. Lord only knows how much money's been spent on other studies since then, but we spent that sum, \$250,000 to three contractors.

The third and very important part of the effort, then, was to go to the various NASA centers and encourage them to start doing studies in support of the Apollo Program, to turn part of their research work, like at Cleveland, Lewis, Ames in California—I guess Marshall was in a somewhat different category at the time—but to encourage them—and Langley. We were separate from Langley then. To encourage them to direct a certain amount of their effort. And to do that, first of all, we didn't want to, nor could we, direct those centers to do things.

What Dr. Gilruth suggested was that we put together something called "technical guidelines." These were just some very basic parameters to serve as something that they could use in developing their studies. For example, that's where it was decided there would be a three-man crew for spacecraft. That was not done on any exhaustive study. Dr. Gilruth and others sat down and said they'll have three people. They had good reasons. So I think we had about ten guidelines about how long the mission would be, and then what we did was we went to the various centers and gave them these technical guidelines, and with some embellishment as far as technical understanding, like in the navigation area, we had a requirement in that to be able to essentially do manual navigation if necessary with an optical system looking at stars and stuff, and that was one of the requirements that was put in there.

So as a result of that, the centers responded very well, and each of them started many studies which paid off; maybe not in the initial detailed design of spacecraft, but [throughout] the program. There was a cadre of people there that had studied and understood and then

could participate in supporting the program in various ways. For example, Apollo, to the time [of] Shuttle, sponsored the biggest wind tunnel program, probably, there's ever been, most extensive wind tunnel program.

So, anyway, that was the three legs that we worked with during this rather small program office. We tried to manage and coordinate those three things. The way that climaxed or culminated was—well, to back up a bit, industry recognized that, hey, this was big bucks, big money going on for a long time, and they didn't want to be ruled out of it on the basis that they hadn't done one of those little \$250,000 studies. Incidentally, those three studies were done by Martin [Company], General Electric [Company], and Convair, General Dynamics [Corporation].

So the idea was that all the results of our in-house studies, the three contract studies, and the various center studies over, I guess it was a period of a year or something like that, would be brought together and made available to all of industry. I remember we decided to hold a conference to present this information, and it was held in Washington, and I don't know where the place was. It was the biggest auditorium I've ever seen in my life. It was almost frightening. A huge place. So anyway, we assembled up there.

One of the little sidelines of that was, I guess I was the chairman of the program or the agenda-reader or whatever, but we were trying to launch a Redstone down in Florida at the same time. These two things had just by chance come together. So our hierarchy of management, Gilruth, Max, I guess Walt [Walter C.] Williams, none of those were available at this huge thing we were doing in Washington here. It might have been the first attempted [Alan B.] Shepard [Jr.] launch. I'm not sure whether it was that or—it probably was. It might have been that. So anyway, all day long they were supposed to show up any minute, any minute, and I was up there grinding through all this stuff, very interesting stuff, and looking at my watch, wondering when they were going to show up, if ever and so forth.

I guess, 4:30, about the time of the last speaker, here they come up along the line, Gilruth walking along there and the rest of them trailing behind, him in his Florida teeshirt, whatever, and grinning. I don't know whether the others did, but he got up and spoke at the end of the program and said, "I'm sorry."

So then we went home, and the next thing we did in this program office and the rest of the Space Task Group, we got ready to let a contract for a spacecraft, and so what that meant was we had to write a technical specification. I would make the point for some of your readers, maybe, who knows. There's a big difference between a design study and a technical specification. A design study essentially says, "Here's what it is." A specification can say that, or it can say, "Here's what it ought to do, and you figure out what it looks like." Actually, in the specification we were writing, and not unlike other technical specifications, probably had a mixture of both, and there were some things we wanted to dictate, "We want this." There were other things that, "We want it to do this, and you recommend to us the best way to do it." So there was a mixture of those things.

But we had to write up a technical specification and something called a work statement. Again, a technical specification tells something about the spacecraft. That work statement has got to talk—you know what work statements are—have to talk about what work's got to be done and when you've got to do this and all that stuff. So we had to write those things and then integrate with the procurement people, into a procurement package that could be submitted to industry. So we did that.

One of the difficulties during that period was, we didn't know how we were going to land on the moon. You think about the moon program and you think about the lunar rendezvous and the LEM [Lunar Module]. Well, at that time, that wasn't settled. That made it difficult because it certainly had certain effects on the spacecraft and some significant—but anyway, we did the best we could. I guess somewhere in there there was a decision made—this is going to get us off our track here a little bit, but it's kind of important—about how we

would go to the moon. I guess Marshall had proposed the Earth-orbital rendezvous. You know, you took up half the spacecraft on this big rocket from Earth and another half on this big rocket, and they met in Earth orbit, joined up to a spacecraft that would take you to the moon and bring you back.

The lunar-orbit rendezvous, which had been—I don't know if it was invented by, but had certainly been promoted enthusiastically at Langley by John [C.] Houbolt was in the mill then, but I don't think it had quite reached the prominence, if you want to call it that, of the other. So, headquarters made a decision that we would use this Earth-orbital approach here, and what that did was it defined the big Saturn rocket, Saturn V stage.

At the time, there was even thought of a larger rocket, something called a Nova, that would, with one rocket, take the device [spacecraft] and set it down on the moon without lunar-orbit rendezvous, a big enough thing to set on the moon that it could come back to Earth here. The Space Task Group, including Max, I think it's fair to say, who was [our] chief concept thinker, rocket thinker, we'd been so tied up in the spacecraft study and design that we really hadn't—I guess we were thinking still that why not do it just with one rocket and just go straight to the moon and so forth, but we hadn't invested in any large studies in there. Anyway, headquarters made this decision, and I guess Marshall and Dr. [Werner] Von Braun proposed [and] supported it.

I remember the day Gilruth came back from Washington, and he called a group of us together and said a decision had been made. We were all pretty skeptical about that approach, [but] I don't know if we were prepared to say this lunar-orbit rendezvous, which we hadn't studied, was the thing to do, but my impression was, even then, that Dr. Gilruth was intrigued by the idea, by that possibility. He said, "Okay, let's look at it this way. This gets established that the Saturn V is going to be the rocket. It nails that down. There's still room to consider whether there are alternates to this Earth-orbital rendezvous, but it's good to get

that settled so that they can go to work on that rocket, that there won't be a Nova, there will be a Saturn 5, to work on it and get that out of the way." So we said, "Okay."

So anyway, back to preparing our package for our contractors, we put that out probably in the summer of whatever year it was; I'd have to check the dates. I'm sure the chronologists know the dates. Then we got the bids in from quite a number of contractors, and I guess the contract was to be written for something called Phase A, and this was kind of a first part of this thing. I guess we just felt we couldn't define further in the future, the contractor couldn't estimate monies and so forth so far, so there was this Phase A thing. We still had the questions about this ultimate way we were going to the Moon. So anyway, that left a bunch of things open.

But anyway, we released the packages, and I guess the contractors had about six weeks. Today they would probably get six months or six years to prepare a proposal, but I think they had six weeks to prepare a proposal to go to the moon, for a spacecraft to go to the moon. I guess we had a bidders' briefing. Maybe we put them [Requests for Proposals] out; a week or so later [we] had a bidders' briefing. The only trouble was that we had a hurricane instead, and the hurricane—I believe it was Hazel—hit that area, and didn't do too much damage, but it sure tore up airplane schedules and things like that. So we had to postpone the bidders' briefing from eight o'clock in the morning to one o'clock in the afternoon or something like that. I remember these important things. [Laughter]

PILAND: So then we got in these proposals, and we organized a review of these proposals, which was a big job from two standpoints. It was just an awful lot to review, but there was so much riding on it, from an industry standpoint and from the government's standpoint, too, as to who was going to do this work. We didn't have even any facilities. We didn't have any, so we rented two floors of the Chamberlain Hotel. I don't know if you're familiar with Langley and the Virginia area, but it's a famous old hotel down at Fortress Monroe, right on a

tip of land there, sticking out into Hampton Roads. So we rented two floors, and that was where we evaluated these proposals.

This went on for some time, and it came down that two contractors, at least, in the scoring were very close, and things got very sticky, very difficult. Walt Williams, I guess then he was the deputy head of Space Task Group. He and Donlan were co-deputy directors, I guess. He was chairman of the Source Evaluation Board, at least for part of this period, and then he said he had got a—well, I don't know. Anyway, Max Faget became chairman of the Source Board. We struggled with this thing, looking at it six ways from Sunday, trying to come to the best answer. As the Source Evaluation Board, we did not select the contractor; we essentially evaluated the proposals. The administrator then, based on the evaluation and whatever else he wanted to consider, he made the selection. That's why they called it Source Evaluation, not Source Selection. Some places have Source Selection Boards, but this was Source Evaluation.

I remember we struggled with this thing, honestly looking to see what things separated the contractors and had we done everything properly and focused on the most important things and so forth. I remember there was the Source Evaluation Board and Dr. Gilruth, and I guess maybe one or two other people, were meeting. We were meeting all over the country because things were going on down at the Cape [Canaveral, Florida]. So we'd pick up and go meet down there, and then I think we met in Washington one time, working this thing as best we could. We finally decided, hey, we have evaluated all we can, and now it's up to the administrator to make the selection. That's basically what happened.

We took the information and gave our report to the administrator and Mr. [James E.] Webb and Dr. [Hugh L.] Dryden, I guess, and whoever they had with them looked at the information and decided the contractor it would be awarded to was North American-Rockwell [North American Aviation, Inc. (In 1967, North American merged with Rockwell Standard Corporation to become North American Rockwell Corporation)]. So that finished

up the last activity of this first project office. It was the end of the year, just before Christmas. Seemed like we always finished up something in time for Christmas. Anyway, it's late in the year. We by then knew we were going to Houston, and so that project office was disbanded. Well, ... the Space Task Group was reorganized at that time, so that was the last thing that projects office did, was to finish up.

After [North American] Rockwell was selected, we then went through a negotiation with them, and whatever we did, we did. It was going through their proposal and our work statements and specifications and trying to make sure we understood each other and that sort of thing. So then they went off to consolidate.

BERGEN: How did you feel about the choice, personally, about the choice of North American for the contract?

PILAND: I had very mixed emotions, as I think most people did. What it boiled down to, my perception here, is Martin had, you might say, got into the game earlier. They had studied and planned the project very well. [North American] Rockwell came in later—it as our impression that—well, I know they did, they came in later as far as working on this and proposing. They came in later, but they benefited from such things as the fighter aircraft they had successfully developed, going back to the P-51 in World War II, the F-86, the F-100 and so forth. They benefited greatly from that within a lot of people's minds here.

Martin suffered probably a little bit because at that time, if you remember, the Vanguard Program, our first satellite program, was having a very difficult time. At the same time they got positive points from having done it. They met their contractual requirements. Nobody remembers that. They did exactly—they conformed to the contract. They were supposed to put satellites up weighing this much, with allowing for this many vehicle and so

forth, and they did it. They got all those things for the princely sum, I think, of \$25 million. You know what they would buy you today in this business.

So there was that kind of a conflict, and, like I say, I had mixed emotions. I'm trying to honestly remember. I can't say, hey, I really think that this went on or that one did. We had gone through the process and the procedure. We'd set up the best procedure we could. We had gone through that process as honestly as we could, and here it had essentially come out a tie within the accuracy of the numbers and that kind of thing. From then on, we accepted the [North American] Rockwell thing, and we would have accepted the Martin thing, from my perspective, anyway.

BERGEN: So at this time, when did you think we would actually be able to make it to the moon?

PILAND: You mean how long it would take us?

BERGEN: What time line did you have in mind?

PILAND: Well, the time line, I guess, came when President Kennedy said we were going to do it in this decade, so ... that became a given. Because of the unknowns we were working with as the program went further out, I can't remember where we pinned down and said it would take eight years and six months and two days. I don't think we did that. There was so many unknowns. For example, in there, at some points in time, at least in some people's mind, the program should be a lunar-orbit mission and that's all, not a landing, that the program would be a lunar-orbit mission.

There was, as I recall, a study—Washington headquarters [NASA], who were in contact with the White House, President Kennedy, and so forth here, and if my memory

serves me right, George [M.] Low did a study essentially gathering material from the Space Task Group and Marshall and so forth, and put together a study which I think was the basis of communication with the White House. It might have been that they said we can do it in six years, and the President wisely said, "Hey, let's settle for the decade. Decade sounds good anyway." So I'm not real absolutely sure about that.

I can't remember where we had any detailed schedule of going to the moon. I think we scheduled how long it might take us to design the spacecraft and things like that. So the decade thing, and I guess I remember the day—I guess when he made the decade speech or the lunar mission speech, I remember we sat around with Dr. Gilruth, Max, and some others, sat around the conference table. I think it was then for the first time that the scope of this task started to get to us.

I think Dr. Gilruth, in his wisdom, he had some impression of what we really were getting into, the scope of it and difficulties of it. I'm not sure that the younger ... people with less wisdom like myself fully appreciated just what the hell we were getting into here. We were just going from day to day, design a spacecraft, do this, do that. From that day it was pretty clear that he [Dr. Gilruth] was approaching this thing, I think, with a certain level of enthusiasm, but also a better appreciation of just what we were getting into than most people in the country. But anyway, that got us to Houston, I guess.

BERGEN: Got you to Houston. How did you feel about moving to Houston?

PILAND: I didn't know much about it. There had been a Site Selection Committee which had toured the country talking to all the Chambers of Commerce who wanted us to come there. I don't know if we had any strong feelings. As a footnote to history, during this period of time, I'm not sure many people understand we were a part of the Goddard Flight Center. The Space Task Group was integrated and made part of the Goddard Flight Center while we were

at Langley. It still operated completely independently, but it was. Harry [J.] Goett, who was director of Goddard, was our boss, basically. It was only when we decided to move to Houston that we became independent again and became the Manned Spacecraft Center. I don't think that's a very important footnote, but it is a footnote.

As far as coming to Houston, like I say, most of us had never been to Texas, and what we knew about Texas came from cowboy movies and it was hot and dry. That was our impression, but I don't think we felt negatively about it. It was interesting, I don't want to speak for Dr. Gilruth, but I believe his feeling was it ought to be located in Florida. You know, "why separate this thing?" But I'm not sure he appreciated the politics of the situation. Anyway, just before the final decision—and I guess the Site Selection Committee, which was a nice bunch of guys, they'd been looking around the country, and they'd been evaluating things on the basis of available facilities, universities in the area, closeness to water, and this kind of thing, and just before the selection was made, here they pop up with a real winner. It was in California. I want to say Ventura or Vallejo, but I'm not really sure, somewhere in the San Francisco area. What it was was a naval facility that had been built—a brand-new naval ordnance facility that had been built at the end of World War II and basically never used because World War II ended. And evidently it was great facility, all sorts of things, built right on the water, big harbor, San Francisco, lots of universities. Well, with certain people ... their recommendation of that—I don't know whether it got to a formal recommendation—but they did decide that was right; [then] more "careful selection" indicated that maybe we should go to Texas.

Incidentally, one thing, I would mention (another footnote). I don't know if you're interested in footnotes, but I'll footnote you again. Lyndon Johnson is a name associated with many things in the space program. You know, the center's been named after him, and it coming to Texas. There was another person that played a significant role, maybe somewhat quieter. I'm sure you're familiar with the little post office, the Albert Thomas Post Office.

He was a congressman from this area here, and, more importantly, back in the days of NACA, he had run a committee, the congressional committee, something called Independent Agencies or whatever, that NACA came under. He ran NACA from a congressional, political standpoint.

Evidently, the Senate, for whatever reason, bowed to the House of Representatives in certain matters, and vice versa, I'm sure, and so he ran it. He decided what the budget was going to be. He had the interest in it. I guess his committee had something to do with it, but he ran it, Albert Thomas, and he played some role in this matter. But Johnson, who I guess was Majority Leader in the Senate or something, he was obviously a much bigger name.

Incidentally, I'll go back and footnote again, some of these footnotes aren't directly related to the Johnson Center, so stop me if you want. But the business of the act that created NASA, where did it come from? Johnson is given credit for that. He did add on this council thing that I mentioned, but the act that created NASA was actually written by a gentleman in the Bureau of the Budget named Al Dean—not John Dean; Al Dean—and with help from a fellow from Commerce. I forget his name. Those two were the personal architects of writing that bill as it went to Congress. I'm not sure who introduced the bill into Congress. I know we had interaction with a congressman from Illinois, whose name I can't remember. But that's where that bill was written. Al Dean wrote it, Al Dean in the Bureau of the Budget.

I think [Willis H.] Shapley played a role. Shapley subsequently became a deputy administrator of NASA. Paul [G.] Dembling, who was NASA legal counsel at that time, or NACA legal counsel, I guess; he participated to a certain extent. He sat on the Drafting Committee. I got to sit and watch for a couple of times, but they were well along before I got there. I didn't make any contributions to it except to listen and learn. That's another footnote.

Where are we? I guess we got to Houston.

BERGEN: We got to Houston. You basically were put in charge of the Apollo project?

PILAND: No, it was decided—first of all, they said, "Hey, we need a much bigger, stronger, more powerful program office to run the implementation of this thing as opposed to studying it," because letting that first contract got beyond, really, our original scope of activity, very much so. So we're going to have a new program office, ASPO, Apollo Spacecraft Program Office. It will not be under Max. His division became a directorate, the Engineering Development Directorate, which still exists, and we created this new independent ASPO. I was the deputy director of it. Charlie [W.] Frick, who was working for Convair, I think, at the time, had previously been an NACA employee at Ames, was selected to be the manager, I guess it was, of the office.

But anyway, we came to Houston. I guess what I remember was the first time I came. It was snowing in Virginia. It was lousy weather. And I got here to Houston, and I guess we were down to Dickinson. I think I was looking for a house or something. It was delightful spring weather here, like you sometimes get in early January, just beautiful, and, "Gosh, what a wonderful place." [Laughter] That was one of my first impressions.

We were first located in something called the Farnsworth Chambers Building. Farnsworth Chambers had been a construction company. I couldn't even find the place today. It was a beautiful place made of stone and so forth, and it had been a construction company. I think it had gone bankrupt or due to some strange happenings in the management, which my memory doesn't straighten out, it had gone bankrupt. Anyway, it was out of business. Its buildings were available, and we rented those, and so we went into Farnsworth Chambers.

I don't know when Charlie [Frick] showed up on the scene, but I ran the office until he got there, and we were hiring people like mad and developing the office—I guess it went up to something like 300 people—so, processing and integrating those people. Fixing to get started was taking a lot of time, but I guess I spent much of my time on that, somewhat similar to the way I'd helped Max in putting together that division. Charlie, when he showed

up, and I can't remember when he showed. He never did move his home and family from San Diego, and he'd come and he'd be here Monday till Saturday or whatever, but he never did move his family.

After he got here ... he took over as his primary emphasis the management of the North American contract, and he focused on that. He did other things, you know, running the office, but he focused on that. The first thing I got involved in, we were letting a contract for the navigation and guidance system. That had been pulled out of the spacecraft, and we had a contract with MIT [Massachusetts Institute of Technology] not to build it, but to be an advisor, consultant, analyst, help us check it and analyze it, whatever, which contract continued on forever during the Apollo Program. But this was getting into hardware, and we had to have somebody do a detailed design of the hardware system. It got pulled out of the [North American] Rockwell contract, and that was another story as to why, but others can talk about that better than I.

Anyway, letting that contract became very troublesome and difficult and complex, I guess, is the word. So I got involved in that midway along in its career and spent a goodly amount of time on that, and finally got the three contractors, I guess: A.C. Sparkplug and Raytheon and whoever did the optics system. Was that Colesman? Anyway, there was a third one.

One of the interesting things, one of the things I found most interesting, was during this period of time of Mercury, Gemini, and Apollo, one of the things that I admired most about Dr. Gilruth was, frankly, almost like a military tactician, he had inadequate forces, and he was having to deploy these forces very carefully and delicately. It was one of the reasons, for example, we had not gotten deeply involved up until that time in this business of how we go to the moon, this rocket controversy. Just didn't have the wherewithal here. So when we got in and established a program office, he essentially said, "You take care of [North

American] Rockwell thing here, and within Max's directorate we now are going to do a study, a detailed study, in house, of this business of how best to go—"

Basically, by then, I think, from a judgment standpoint, Max and Gilruth had decided that the lunar-orbit rendezvous was probably a better way to go. So they put together a study to essentially look at that scheme or that concept to see if it would work, to look at as many aspects of it as you could, to establish its feasibility, if you want to call it that. Chuck Mathews was head of that study under Max within this engineering directorate. But as they got into their study, Charlie gave me the job of, doing again, putting together the technical specification and a work statement, because we were going to have to let a contract for this LEM thing.

So, I guess, for the rest of that year, whatever year that was, the first year we were down here, my efforts were devoted almost 100 percent, after that navigation and guidance thing got through, so I guess this was about March or April, to the end of the year I was taken up with getting a technical specification and a statement of work and working with the procurement people to put together the whole process of selecting a contractor for that activity here.

Can we take a break?

BERGEN: Sure. [Tape recorder turned off.]

PILAND: We're talking about a study of the LEM, Lunar Excursion Module, as it was known then, I guess lunar module is more politically correct now, and the preparations for selecting a contractor and so forth. During that time, as the Chuck Mathews study was going on, he essentially finished it, and the center by then had come to the conclusion very much that it thought this lunar orbital rendezvous was the way to go.

At the same time, there was a battle going on in Washington between NASA headquarters and a staff person [for] the President's Science Advisor (the job I had had before) and Joe [Joseph F.] Shea was carrying the load for NASA. They were, frankly, in a debate as to which of these techniques was the best way to go, and Joe and NASA headquarters at the time was, I guess, oriented towards the lunar-orbit rendezvous. This staff person, he was a reliability expert, the people that do the very worthwhile job of calculating the .999 percent thing. But he was working it strictly from that standpoint, and that was the field he had selected to play the ball game on. In doing that, at that time, you had to make an awful lot of assumptions and things.

Anyway, Joe was fighting the battle with him, and as best I understand, it was a battle not with the President's Science Advisor or with the President's Science Advisory Committee, but with this staff person. That's my personal impression. [Laughter] And I'm pretty sure that's right. He may have had the President's Science Advisor [say], "Okay. Fight the battle." Well, it was probably a worthwhile battle fought.

Anyway, that was going on, and actually we got to the point down here where we had our—what do you call it, that thing you send out to the contractors? Request for Proposal just sitting there, and we couldn't release it because this battle was still going on in Washington here, and headquarters was reluctant to let us go ahead until they had gotten some sort of clearance from this White House office.

Incidentally, intermediate to that, toward the end of Matthews' study, we all went to Marshall and presented the results of that study. Chuck and the people that have helped him presented the results of that study to the Marshall people. It was some time after that, that Werner Von Braun very graciously decided he would support the lunar-orbit rendezvous and withdrew any opposition to it, a very, I considered, statesmanlike gesture. Technically I don't know what was in his mind, but [it] had become a real bone of contention and probably was the basis for subsequent advertisement of differences between MSC [Manned Spacecraft

Center] or JSC and Marshall, which I think were always overstated. But anyway, that was in there.

But even after that, that got NASA all on the same playing field, and they still had to do this battle with what's-his-name. So that was holding up our proposal, and we considered things time-critical, always considered things time-critical.

Now, remind me to go back to—maybe on one of your questions. That reminded me of something.

Finally the President's science advisor gave his consent in some form or another, and we released the proposals, and we got in quite a number of proposals from companies. [Some] that don't even exist anymore. Very good proposals.

We did something different. You're always doing something different in these contractor-selection things. Anyway, we limited the technical proposal to, I think, fifty pages. In the command module, in the technical proposal, we at least had some sort of limitation. It might have been 300 pages or 500 pages, something like that. The business or administrative proposal didn't have any, which, frankly, created certain problems here, anything they had in their files from some companies, literally. But we limited the technical proposals to fifty pages, and, frankly, they were some of the best proposals I ever saw in my whole career because the people had to make choices, they had to limit that proposal to what was important, they had to make priorities and not spend umpteen pages on things that everybody knew anyway and wouldn't make any difference in the selection, and so forth and so on. So that proposal evaluation went much more smoothly.

We evaluated Grumman [Aircraft Engineering Corporation] to be the whatever you call them, the best proposal-writer or best contractor to do the job, whatever your feelings are on that matter, and took it up to headquarters, and the administrator concurred or made his selection based on that, and it was consistent with our results. We thought there was a

difference between it and second place. I don't even remember who was in second place at the time, which, I guess, is some indication.

Incidentally, Grumman had focused in on the LEM and that mode early on, earlier than [we had]. It's interesting as a little footnote, if you go back to the proposal that Grumman put in for the command module, the first proposal, they had an appendix to that proposal which described the whole lunar-orbit scheme, rendezvous, and so forth. So they early on were focused on it, which I'm sure aided them in writing their proposal. Grumman had very good credentials, if you want to call them that, amongst all the people involved, and they were a long-time builder and made the airplanes and so forth. So we negotiated with them and finished up a negotiation again. Just before Christmas, we finished up our negotiations and sent them home to go get started, and that was the end of—I guess that was our first year, or my first year, in Houston.

Like I say, my time had been mostly taken up with working administratively in the office and this LEM contract. I was the chairman of the Source Evaluation Board up to this time, and we had learned a lot from the previous one and, I think, benefited from that. Of course, Grumman, during the development, they had their troubles, too, but, at the same time, they managed to build a product and do the job.

So it's very hard to evaluate the whole contractor-selection business. I know somewhere along the line some story came out of Washington that Grumman had been given the contract because of some political connection or something. I can say from personal experience that that is absolutely not so, that they won that thing fair and square. Now, maybe if they'd lost it, somebody would have reversed us. [Laughter] I won't give in to that aspect, but they won it fair and square. Anyway, that was the end of that year.

I guess I continued on for the next few months as manager of the lunar module. I was deputy of the office, but my focus was there. Charlie Frick brought in another deputy manager, and he wanted this other deputy manager—what was his name? Jim—

BERGEN: Decker?

PILAND: Yes. Decker. He wanted Decker to run the North American activity or monitoring, whatever we did, and I'd run the Grumman lunar thing, and [that would] let him work both. We worked on like that for the first few months. Grumman at that time was gearing up, planning, studying, and all the things you do the first few months of a contract year. And then Charlie Frick resigned. I think maybe that was in April or something. So Gilruth called me over and he said two things: he wanted me to be acting manager with an unknown future, but he also wanted me to manage the office [to] also ... take on the direct management of [the] North American contract, shift Jim over to take care of the lunar module.

Well, I had some misgivings about this, but I understood, I think, what he was saying about how he wanted to do that. So I started managing the Rockwell contract. I guess by now, I guess [we were] a year or so into the contract with North American, and I guess we'd got to the point where people were beginning to understand just how tough things were and difficult things were, and there were a lot of problems and difficulties. I guess if you're going for the moon, there ought to be some difficulties.

So anyway, I guess I, frankly, struggled and worked very hard with that contract for the next few months. I guess if I did anything that I look back upon with a degree of satisfaction, it was, we had never had a mock-up inspection for the lunar thing and essentially letting the world look at it, critique it and so forth, and from Manned Spacecraft Center, particularly the operations people who were at some distance from the contract, and the engineering people, too, they were clamoring, I guess is the word, for that mock-up and their chance to impact it and so on, particularly the astronauts, because the mock-up, they've got to live in it. It's of a particular interest to them how things are arranged, where things are, that whole human factors aspect of it here.

For various reasons there was trouble in getting this mock-up inspection scheduled. There were certain people that wanted to start off with having Rockwell inspect it first, which, as far as I was concerned, that was all right, they could do what they wanted as they were doing it, whatever, but then have a rather limited inspection, and these were some people in our own organization, have a limited inspection by, say, the program office and maybe a few people from MSC, and let Rockwell fix all of that, go back and redesign and fix all of that, before we let, you might say, the broader community, broader groups, stakeholders, get into this thing.

That was kind of difficult, and I guess I was able to say, "Hey, we've got to go ahead with this mock-up. We've got to let these people in and let them start making their inputs to this thing." And so, over some considerable objections, I was able to accomplish that. You had to have been there to appreciate that that had some significance. Probably from this distance in history, it's of very little interest.

I guess some time after that they decided that Joe Shea would become manager. I won't go into why I was not manager. It was my choice not to have a shot at that. I had misgivings about a number of things, and I, frankly, was also pretty well burned out at the time. This had been going on for some number of years then. But anyway, Joe Shea was selected as manager and he transitioned down. I forget how long it took him to come down here, and we struggled on. And there were struggles in those days, daily struggles, decisions, and schedules. I mean, you could not keep a schedule. You'd make a new schedule on Monday morning and by Friday it was completely out of whack.

BERGEN: Why did you think that was?

PILAND: We couldn't do things as fast as the optimistic schedule-writers. We'd write, "We need to do this," yet people were backing up from the end of the decade, saying, "We need

this by here, we need this by here and this by here," and they were allowing times for this and that, very legitimately, "And so therefore, you need to finish the structural testing by this date." Well, they gave us a week. [Laughter] And what's more, we [did] structural testing and the things broke in half. People remember all the victories; I remember all the defeats. It was that kind of a thing. The schedules weren't realistic. People were trying, but not having workable schedules, we did things out of whack, because people were doing things that they shouldn't in some respects because the schedule was leading us. We through these—I forget what they were called—MDSs, Master Development Schedules, something like that. I think we got up to MDS-32 or something.

Anyway, Joe came in and he took over. Joe and I got along very well. I respected him greatly, and he was extremely nice to me, as all my bosses were nice to me. I never had any trouble.

Incidentally, my relations with Rockwell—some people had very difficult relations with Rockwell. My relations with Rockwell were extremely good. I knew their people. I respected many of them, not all of them, but many of them, and we got along extremely well. Anything I asked them to do, they tried to do.

I guess one other, as far as my contributions during that time, I did play a significant role in instigating a form of a test program or a way of defining the test program to assure that we had tested everything properly for every condition we might go in, and I took some pride in that. I notice Aaron Cohen still used it on Shuttle. Aaron was working in the program office then, and he took that same scheme and used it the Shuttle orbital program.

With Joe, I did a couple of things that stand out. There was the usual helping around the office type of thing. I did one study for him. That's probably not important. We were having trouble trying to get the first unmanned spacecraft, unpersoned spacecraft, prepared and built to launch, unmanned. With Spacecraft 009, I guess it became Apollo 1 or something later on. So I volunteered to—Joe was getting established in the office. I

volunteered to essentially be a project engineer for that spacecraft and work with Rockwell to make sure that we in no way were holding that up, that if there was any problem or any lack of approvals or anything from the government side, that I would work on it to see we were not holding up the game.

So that year—I guess we'll say that's next year—I went to Rockwell and/or Washington, I think. I was on the road thirty-five out of the fifty-two weeks of that year, I guess thirty at Rockwell. I just would go Monday and come back Thursday night or Friday and spend one day back here working on problems related to that spacecraft. A lot of things came up in the program early on, sometimes legitimately, sometimes not, where the contractor felt that NASA was doing things that were holding up the process. I could see how that was probably true in some cases. In other cases, if we'd looked into them, we'd find that these problems were arising within the Rockwell system itself. Anyway, I guess I was helping to work those problems, and I guess I worked with that until the thing got shifted to the Cape, and I felt that I had done my project engineering job. It ended when I'd done it.

I guess then the next thing that loomed on the horizon or started to come up about that time had to do with the lunar experiments, lunar science, and that sort of thing. The Space Program Office had not worked this at all, and frankly, simply, it was a matter of priorities. Early on—this relates to one of your questions a little bit—but early on, we were very focused on that presidential objective, and that presidential objective said "Put a man on the moon and bring him back safely to Earth." It didn't say do all of the science along the way; it said do that. So that was our priority, and we tried to focus on it.

The problem where sometimes the scientific people got somewhat upset with us was basically priorities and resources. The other thing was the spacecraft weighed too much. It just literally weighed too much, and we fought that battle day in and day out down to the ounces, trying to save the weight so that the booster could carry the thing. So you come along and say, "Hey, I want to put 500 pounds of experiments in there," good God, where are

you going to put them? How are you going to carry them? That's oversimplifying, but that created, with some people in the scientific community, a feeling that we had a poor attitude towards the science, which we didn't. It was strictly a matter of priorities and weight.

The weight thing never got solved. The only way the weight thing got solved was Von Braun, as his booster development came on, finally decided he could give us another thousand pounds to put in that spacecraft, and if that hadn't happened, I don't think we could have ever taken a thousand pounds out of that spacecraft.

Here's a little footnote, somewhat embarrassing, but I'd like to give a guy credit for something. Back at Langley in the early studies of this thing, we were so focused on doing what we had to do and nothing else, we didn't even have a place to bring back any rocks from the moon in that spacecraft, I'm ashamed to say. "Hell, no, those boxes weigh stuff. Nobody told us to bring back any rocks, you know." We were at lunch one day in the cafeteria, and there was a fellow there that ate lunch with us who worked in the administrative part of the organization, I think personnel. Maybe it was the fiscal office or something. He was a pretty smart guy. I don't mean to say he was a smart guy anyway; he was a smart guy, yes. Ray [Raymond L.] Zavasky. He was an old-timer and he'd been around a bit. We were at lunch one day, and I guess this subject came up. Incidentally, I hadn't thought too much about this subject one way or the other then. We really were focused on what we had to do, and the science thing hadn't even come up too much. We started discussing it, and he brought it up and talked about it in a way that you could not go to the moon and come back without bringing some of the moon. I and, I think, Caldwell [C.] Johnson was there. We said, "Yes, that's right. We can't." That afternoon we went back, and I and Caldwell decided we'd put those boxes in there to carry the rocks in. They would have got in sooner or later.

We had another battle about the TV camera. Today, I or anyone in this world cannot think of doing such a thing without a TV camera. If you go back in history, everything wasn't being recorded on TV the instant it happened then. I think we had a camera, but I don't think

we had a TV camera. The PAO [Public Affairs Office] people were after us, and we finally put the camera in somewhere along the line. We recognized it.

I really say this—maybe we were wrong at the time—but to emphasize that the focus and determination of a people to do what it was we had to do without—I mean, to us it had to do with whether we could do the mission in safety. Those two things were involved. So anyway, they're somewhat embarrassing footnotes.

Anyway, after the 009 or Spacecraft One or whatever you want to call it, after I finished up there, the experiments business was looming, and we hadn't worked on it at all. Over in Max's Engineering Department, they were working with this lunar surface package called ALSEP [Apollo Lunar Surface Experiments Package]. Well, that's an interesting thing to know—Apollo Lunar Science Experiment Package. They had been working there. Max, in his wisdom, in his Engineering Department, Directorate, he had a Science Division in there or branches. There was one that was geosciences and cartography and another one that was radiation, atmospheric, and meteorites, things related to that. Now, this was applied science. It was science oriented towards—like what interested geologists, geophysicists having to do with the design of the lunar landing [module] legs. What's the surface like that we've got to hit into? Nevertheless, there were geologists, geophysicists, those kinds of people in there, much to his credit, being able to see what was needed.

There was an experiments program going on in Gemini also. Well, George Low decided that we needed to organize, focus, or whatever, this experiments business, science business, more, so he decided he wanted to form an Experiments Project Office or Program Office, whatever, to handle all the experiments, manage all the experiments, and pull them out of the program offices so that they would have a focus. So I was selected to head that office. That's how I happened to leave the Apollo office. Although I was still working on Apollo, it was just another facet of it, which relates to my career. It looked like certain things

changed, but actually many of them were just evolutions. The ALSEP, at least according to schedules, was behind schedule and it was a problem.

The Gemini experiments program was in very good shape. Chuck Mathews was running the Gemini Program. A fellow named Norm [Norman G.] Foster was managing the experiments part of the program, and they were well organized and doing a good job, no real problems, day-to-day problems, no real problems there. The Apollo thing was essentially ground zero.

So we had to organize a new office. Looking back, I'm not sure if I would have done it that way. I think maybe I would have left Gemini alone, because it was being well handled, and maybe, in the Apollo Program, made them set up a distinct activity in there. But anyway, George thought this was best. There was a reason for it. Actually, some of the Gemini investigators were getting involved in Apollo and so forth, and I think George wanted them not to have to work with this group. So they had a new office to organize. This was called the Experiments Program Office. It was in the Engineering Directorate. It was one level lower in the hierarchy than the Apollo spacecraft and the Gemini offices. So I was back reporting to Max, which was no problem. He let me do whatever I wanted, run the program the way I thought. He would help me in any way that I asked for or that he could help me. So we organized that. I guess we got up to about 100 people in there. It was a pretty good-sized operation.

Our big concentration was on this—we had three focuses: Continue the Gemini Program, which Norm pretty well took care of; The ALSEP—four things, I guess—the ALSEP, which was the lunar package to be left on the moon; there was [also] a group of Apollo Earth-orbital experiments having nothing to do with the moon. We were just taking advantage of the Apollo being in Earth orbit to [do] more Gemini-like experiments. Then there was a fourth, which had to do with doing the geology of the moon, in other words, what the astronauts would do there in a geological way, which involved cameras, where they went,

traverses, picking up the rocks, what kind, that whole planning of that kind of activity, which was very geologically oriented. And then there was another thing that came up this time: quarantine. The lunar rocks and the astronauts and everything that came back from the moon and protecting those rocks in what is now ... the lunar receiving laboratory. I'm sure it's called something else now.

But anyway, for the time being, our focus was on another procurement activity. We needed to get a contractor to manage and integrate these experiments into this lunar surface package and get that package integrated into the LEM. So we went through another big procurement activity and finally selected Bendix [Corporation] of Ann Arbor—I'm not even sure they're a division of Bendix anymore—as a contractor. Then we had to let contracts with the individual contractors who were going to provide particular experiments to go into the package. They [Bendix] were creating the data system, the structure, all the support, kind of like a little spacecraft, and then there were eight or ten, maybe fifteen, experimenters from all over the country, each, ... we had to get a contract with. Some of them [investigators] built their own equipment. Some of them had Bendix build the equipment or [had] Bendix ... contract to build the equipment. Some of them, we let the contractor build the equipment. In some ways it was more complex than the spacecraft contractual arrangement.

Going back to this geological stuff, we had to get tools, lunar tools. We had a contractual arrangement you wouldn't believe. We had to have a power supply for this ALSEP. Well, the power supply was a nuclear device. In today's world, I'm not even sure you could fly it. We called it RTG, Radioactive Thermal Generator or something. That was buried so deep in the Atomic Energy Agency that I didn't know what was going on. We had one person at JSC that kind of looked after it, and I don't know how close he ever got to it, but it showed up [and worked].

Oak Ridge, Tennessee, the laboratory there, Union Carbide, built the boxes, I believe, the lunar boxes. I think JSC itself designed and built the tools, Jack [A.] Kinzler and those

people. Or somebody was designing them and Jack built them. However. Then we had all these scientists and Bendix. This was great fun. I can remember a chart now that showed all these connections and arrangements on it, and it was something else. So anyway, that went on, and we worked at these things.

BERGEN: Were you responsible for choosing the experiments?

PILAND: No. No, no, no. Headquarters, in its infinite wisdom, had committees and so forth that received, reviewed proposals. We played one role in the evaluation of those proposals. We evaluated what it meant to the spacecraft, whether we could do it or not, whether we could carry it, how much it weighed, whether it stayed within limits we had, and we provided that information to headquarters. Headquarters selected the [investigators]. Probably they might have held the contract on a couple of them, just to make things a little more interesting.

Gemini was a little interesting. Gemini, there was a certain amount of feelings in the early parts of Gemini that the experimenters, individuals, felt like they weren't getting enough time. I can remember one occasion where an experiment was supposed to go on a flight, and the people loading the spacecraft said, "It won't fit." And fitting stuff was significant. Well, before I could turn around, that investigator had learned about this, because everybody had their contacts everywhere, and he had run off to Washington and gotten hold of, I guess, George, the science guy up there, and gotten in touch with George [E.] Mueller, who was running the manned program, and the manned program jumps on Gilruth, and I [had] failed to tell Gilruth that we were being bumped.

All of a sudden, he gets hit broadside from headquarters about this thing's got to go on it. And it did. They repackaged it or something, got it on there. I had made the mistake, I guess, of more or less accepting it when they said they couldn't get it in. I said, "Well, if you can't get it in, you can't get it in." I learned then what might happen in such situations.

[Laughter] I think Gilruth was a little unhappy with me for letting him get caught sideways, and I was, too. I felt badly about that.

The Gemini Program, the science program, like I say, there were some little difficulties like that, and as far as during a mission, time was of the essence. An experiment would get thirty minutes ... and the experimenter would feel that it deserved more time and so forth. But the people doing the work were weighing lots of things and trying to get it done.

What came along was the long mission, the fourteen-day mission, Borman and Lovell. I remember preparing for that mission. Because it was fourteen days, it was a lot of time. They didn't have enough operational stuff to do. So tons of experiments were on this thing. I remember, as a footnote, an anecdote, a Monday morning, [Frank] Borman and [James A.] Lovell [Jr.] come wandering into the office—not wandering—and said, "Okay, here we are. What do you want us to do with all that stuff? We're prepared, we're enthusiastic about it, we want to do it," and I would say I don't think all astronauts were equally enthusiastic about doing experiments. Most of them were very good. "We want to do it. We want to do it right." To us, this was like a breath of fresh air because Experiments did feel they were fighting an uphill battle, and they did. From then on, I guess I was a little closer to Lovell than most of the astronauts. I wasn't particularly close. There are a few of them I was. Lovell was a great guy; Borman, too.

BERGEN: Was your office responsible for all the training of the astronauts on the experiments?

PILAND: No. I use the word very loosely, we "managed." Flight Operations people were responsible for the detailed training of astronauts. They had set up a training program. All we would have to do is, in letting these contracts and so forth, we'd have to, first of all, lay in

time for training. We would have to get training devices, make sure the contracts got training devices, which were then furnished to the—I guess it was an organization called training, astronaut training. They managed the specific training of it.

Many things were done like that under the office. Like this fellow I was talking about that monitored the radioisotope generator for us, Jerry—I forget his last name. He would work with Thibodaux in the Propulsion and Power Division. He was a power specialist. So we contracted over in the center, and that's how the program offices were supposed to operate. They weren't supposed to do everything themselves. They were supposed to, whatever specialized work—so that sort of thing was done by the—and the planning of the detailed time lines was done within the Operations Directorate, and they would have occasion to place requirements on us to go into these contracts and things and to have the investigators there. We kind of, to some extent, looked after their [investigators'] time. As time went on, there would be arrangements amongst people set up and worked out and so forth. So we struggled along.

I'm going to take another break. [Tape recorder turned off.]

PILAND: As the experiments program continued, the development of the lunar experiments and the other field experiments planned, we [the center] recognized a continuing need to place more emphasis on science. As the program got to a point where it could do science, where it was getting other things developed and could spend more time on priorities, on science, I guess I thought that science deserved a bigger organizational role at JSC than it had up until then, and I and/or George Low, we proposed to form a Science Directorate, in other words, at the highest level equivalent to Engineering and Operations and so forth, and we did form the Science and Applications Directorate, which you might run across.

We incorporated into that these groups out of Engineering which I had mentioned earlier, the geo groups, the cartography groups, the radiation, atmospheric, meteorites, and

the experiments office. It was a program office. We pulled [these] together to make a directorate out of it, and I was named deputy director. I understood my limitations in science, but somebody had to kind of operate it, get it started. So I was named deputy director, and I started pulling these groups together while we looked for a notable scientist to be director of this operation. There were several names—well, I'm sure there were a lot of names we considered, but we considered Gene [Eugene M.] Shoemaker, who was with the U.S. Geological Survey and who played a role in the program in the field experiments. He almost took the job and decided not to. I'm not sure how well that's known.

PILAND: Others on the list being considered at the same time was Bill [Wilmot N.] Hess. He was up at Goddard. I remember George Low and I flew up to Washington, to Goddard, to see him, talk to him, and he agreed to take over that directorate and make science more of a visible part of the center and put more emphasis on it, which everybody was in agreement should be done and ought to be done. So Bill accepted it. I was deputy director at the time, waiting for him to get there, basically, and getting these groups pulled together, which we did. He came on board in, I don't know, a month or so, a couple months, whatever.

I had become interested in Earth observations from space, personally interested, and what people think about primarily pictures from Gemini, for example. Actually, we had another program going on less well known, an airplane program, which was funded out of another place in headquarters, an applications place at headquarters. It was a flying test bed for various instruments that can make measurements of the Earth, not just cameras. Cameras were in there, but all sorts of microwave, radars, infrareds, ultraviolet, all sorts of devices, which, frankly, many of them, I suspect, came out of the intelligence community, or the concepts, not necessarily the devices.

This program, which I had nothing to do with starting, but which we absorbed into this experiments program, had these instruments on this airplane. The airplane would fly

these missions around the world, literally, and there were investigators selected by headquarters, and the airplane would basically gather data with these various instruments. These investigators would go home and analyze it, report.

Through it and through the Gemini photography, I became interested in Earth observations, and I suggested within this Science and Applications Directorate we [have] an Earth Resources or Earth Observations Division. So I kind of separated myself from the directorate. Actually, I think that I was transferred to Gilruth's staff and went back and became an acting division chief to form this division, this Earth Resources Division. What was bothering me, frankly, was that JSC was flying this airplane and handing data to people. [We] didn't understand what they were doing, except pushing buttons and gathering data. [We] had to make sure the machinery ran, but as far as the application and use of it, [we] didn't understand it, and I didn't think that was right. Just like I think it's very appropriate that JSC has a contract with you for doing history work. I don't know if they still do, but I think there ought to be at least one historian in the center. Is there one still?

BERGEN: Just started last week.

PILAND: That there should be at least an historian in that center to communicate, to understand to a greater degree, and I felt that same way about this program. Here we were spending the government's money, and there wasn't anybody in the government that really understood what was going on. I didn't mean to stop doing the contract or anything, you understand. So we kept the airplane program in there, but with the intent of building a cadre of people who lived in that world, that scientific world, and understood something about it.

After I left—I'm going to tell you where I went in a minute, but after I left, that division grew into a huge thing under Bob McDonald, who came down from Purdue [University], and they had this program called LACIE. Have you run across that? LACIE

was a very large program, and its intent was to monitor the Earth and essentially predict wheat crops and a prediction of what the wheat crop was going to be, huge financial implications, what people are assuming it's going to be. They started on this very ambitious program, got deeply into it, to essentially do worldwide wheat surveys and other crops, too, but wheat was the focus, the most important. Anyway, that program, later on, for reasons I won't go into, got chopped off at its knees.

After this Earth Resources Division got started and went along, I had been acting division chief, and I left and gave that up. We got a division chief in from the University of Michigan to run it. He preceded McDonald, I guess, in running it, and I continued on as technical assistant to Gilruth. I'm not sure just what I did there.

Somewhere in here, another thing came in from the side. At the end of Apollo, or maybe not the end of the missions, but the end of the development program, headquarters decided to close up the Mississippi Test Facility [MTF] where engines had been tested. I don't know if you all are familiar with it, but a rather large facility, test facility, had been built over there, a beautiful facility with all sorts of stuff. It was a part of Marshall Center as a test station just like, I guess, the test station in New Mexico is to JSC. I guess it still is.

They decided to basically mothball that facility. Well, this didn't go over too well with certain people, certain rather powerful people, that here they had gone out of their way with their constituents to give up land, to move people, to put all the little towns and cities into debt to build new schools and water supplies and whatever. So NASA had a problem, let's say. So what NASA did was turn to its various departments and said, "Hey, can you put a program at MTF where, if we get enough programs here, we can keep this place open, but with other programs?" They went to the applications people in headquarters—Leonard Jaffe [phonetic] was running that program—and they said, "Can you put an applications program down there, Earth Resource, Earth Observations, or something down there, or do you have any ideas?"

So Jaffe called me at JSC and asked me did I have any ideas, so I said I'd work on it. So I put together a plan to start an Earth Resources Laboratory at MTF. In the meantime, MTF had been recruiting various government agencies to locate, hopefully, technical groups there. They were trying to make themselves into an interagency technical conglomerate or something and were having some success at it. So I put together a plan to form an Earth Resources Laboratory over there, starting from scratch, and focusing on the region, not flying airplanes all over the world, not trying to solve problems in Iceland or something, but really to focus on this regional Louisiana, Mississippi. I mean, you could work there for the rest of history with the variations and topographies and crops and all that kind of stuff.

So I got intrigued with it, working up the plan, and I volunteered to go over there and do it. So Gilruth said, "If you want to do it, it's all right. Go make your own mistakes." [Laughter] So he approved it. So headquarters decided that they'd do it, but it was a part of JSC, this organization, not a part of Mississippi Test Facility or Marshall. We were a part of Manned Spacecraft Center. I'm not sure I would have gone under any other conditions than that. You always keep open your road of retreat and things like that. Well, I knew where my support was. Also, [it] made a number of things easier. I went over there, and I was able to recruit, with the center's approval, a number of people to take over with me, go with me to help start the thing. The center supported me in recruiting people from all over the world, it seems like.

So I spent the next four years in Mississippi, developing and running this Earth Resources Laboratory, and I say with some pride that it still operates today. I think we accomplished some things. Like I say, we focused on that area, experiments. I had more of a balance of scientists and engineers, which was what I thought it should be. We were supported ably by Lockheed, with our P_____, was a support contractor for us, and they did outstanding work with us in forests and bayous, however you want to put it. We had a small airplane we used just in that area—we didn't fly over the world—in response to anything.

We had a boat. These things aren't very important, but they also aren't what you think of when you think of Apollo. We got involved in all sorts of things. I'll go back to another footnote in a minute.

So I spent four years over there, '70 to '74, very enjoyably running this laboratory. Government and contractor personnel together were about 125 people, I guess. Like I say, we had a boat and a truck. We had a truck with an array of instruments on it which we would send all over.

I would mention one thing, again, which is another footnote. We worked in the region generally. One day Jaffe called me and said, "We've got a proposal here." I don't know whether it originated with NASA or—what's the drug agency? DEA, Drug Enforcement Agency. "The proposal is that we will go map opium in Mexico, and we need somebody to do it. You guys do it." So we get into a very different kind of project. There was a particular, specialized kind of camera, a multifrequency camera with particular wavelengths it operated at, and these were certain wavelengths that would make those poppies really stand out.

So we entered into this program with DEA and with the Mexican Government, and we surveyed chunks, sent a team down there under heavy guard, literally, and surveyed big chunks of Mexico. There wasn't any problem locating these things. Probably the Mexican Government could do it anyway, locate them, but what do you do with them? All they could do was send Army troops to slash and burn these things, which was a huge effort, I mean, cutting down acres and acres. But that was a little footnote, which, for obvious reasons, didn't get any advertisement at the time.

We had a couple of scares in there. We thought we had a threat to one of our people, and to this day we're not sure if it was a real threat or somebody being funny, but it was not funny at the time. It caused considerable concern. But we did that.

We did other programs which we considered of interest to the area, land-use programs. We worked with groups at LSU [Louisiana State University] in Louisiana and Mississippi and surveyed things and did various experiments. Like I say, it's still going on over there, but ... it's Stennis [Space Center] now. It's since become a center. Shortly after I left, it was transferred from JSC, which we had envisioned that if it stayed there forever, sooner or later it should happen. At the time, [initially] there was too much turmoil, and personally I was reluctant, for myself anyway, to think the lab should get involved in some of the problems that were going on, normal kind of problems that organizations have. So I guess I stayed over there four years.

Then I came back to JSC. I guess Kraft had become director by then. He encouraged me to come back. He wanted me to go back in engineering, working for Max as one of two deputy directors to work advanced programs. This I did. We worked various programs. One of the things we spent a lot of time on which did not come to fruition, but was a most interesting project, it was a solar power station. I don't know if you've run across that concept, but back in the days when we were all upset about running out of energy, a fellow up at A.D. Little in Boston, Peter—what was Peter's last name—came up with this concept of essentially, in space, capturing solar radiation and transmitting it to Earth where you wanted it with microwave transmission.

This was a huge project. It was so huge, I guess that's what put people off, but it was interesting and it was like Apollo, it was very well defined what you were trying to do. You were trying to bring power from the sun and put it into that grid down on Earth. You knew exactly what you were trying to do here, and that made it very appealing to engineering people, as compared to some projects we have. We worked on that for a couple of years. I think then a combination of lack of interest by the Department of Energy and just the scope of it, it was so large, and it had a lot of questions in it, too. It stopped. We finished it up, finished up the studies. Anyway, we spent a large amount of time on that. It was extremely

interesting. Numerous concepts in there, you will see if you look closely, are in the space station, various concepts that came about in that program.

I guess the other thing we studied was the Space Station. We did a number of Space Station studies. One particular concept was to move somewhat away—not neglect, but move somewhat away [from] the idea of just sitting up there doing science experiments, something called a Space Operation Center, where the purpose of the device would be not only to do some experiments but also to serve as a refueling station and various things, a way station for other missions and so forth. So we developed that concept and called it Space Operations Center to change the focus a little bit. Of course, we were criticized instantly because they said, "Oh, well, you're trying to neglect science again," which wasn't the idea. We were trying to see if there were other useful objectives and things to do. So that was another major effort during this period of time.

I helped Max run the directorate there. I guess during this time also I served on the Astronaut Selection Committee. George [W. S.] Abbey was the chairman of it. I forget now, we [JSC] had the original group of astronauts and a second class, maybe a third class. I'm not sure. Then we stopped for, I don't know, ten years, ten or fifteen years, because this would have been '75, '76, '77, and we hadn't had any astronauts during that period of time, partly because there was a question, was there going to be a Shuttle, was there going to be this, was there going to be that. So this was the first big one. The first one turned out to be big. I don't know, I think we had 9,000 applications or some such number. So we were all tucked away for months, it seems like, evaluating the candidates. That resulted in the selection of goodly sized numbers of astronauts and mission specialists here. So that, Space Station, and solar-power satellite and other kind of things, we worked on it during that period of time.

Then I guess the next thing that came up was that the director of Space and Applications Directorate, which we talked about here, had become and changed its name to Space and Life Sciences, and I guess that's when they had integrated the Medical Division,

Medical Research Division, into it and changed its name to Space and Life Sciences. I don't know who was director of that. Anyway, it didn't have a director who [had] left or something, and Kraft asked me to go run a directorate. [Laughter] Guys were going around in circles about that, because a lot of those people I had worked with, for, and whatever, off and on for years.

So I guess, I don't know, a year or two, a couple of years, I directed that group. A lot of our effort then was devoted towards the development of life science experiments in the life sciences module, which was flown on Shuttle. That was a big part of our effort there. That group, incidentally, wasn't called that then, but its genesis of people was back in the experiments program office, some connection there.

There were other things that happened during that period of time. It was kind of interesting, I became closer to the medical people, medical research, than I had been previously. We used to have life sciences experiments in the general experiments program, so I had bumped shoulders with it, but I became closer to the medical people and their interests and problems and attitudes and values and so forth. That was very interesting. We had some things that we worked through. Anyway, I was there a year or so. It's getting pretty hazy now.

Then, I guess, Max retired. So Kraft and Max, I think, before that, decided that I should go back and [be] Director of Engineering. So I went back to Engineering and was director of Engineering for a year or so under those conditions. Max stayed on until the first Shuttle mission, so I'm over there during the first couple of years of the Shuttle flights. We supported those flights. There were problems with tiles and the auxiliary [power] generator, always problems with it. We worked problems and operated the directorate. There were not a whole lot of new projects coming. There'd be variations and new designs, improvements to this, that, and the other. We were already looking at improvements we'd like to make to the

Shuttle, for example, which I don't know if they've accomplished them or not, but, you know, second time around you always think of a lot of things to do better.

So I was kind of just—not just, but devoted towards the directorate, devoted towards supporting the Shuttle Program and, frankly, trying desperately to reduce the engineering effort in support. We had a huge amount of effort that geared up every mission to support that. We were trying to get it more into more of an operational mode and start shifting those people away, off the Shuttle Program. These were basically development people, which was perfectly appropriate for them to support the early part of it, but we were trying to wean them away, and that was very difficult to do.

During that period of time a very difficult task I had was to—Kraft, the center couldn't hire more people, and he felt that the way things were going, he needed more people in operations. We went through a very difficult exercise of taking a number of people out of Engineering and putting them into Operations. Like I said, it was very difficult and a lot of people were unhappy. Some were happy. Some made the change easily, but many were unhappy, they wanted to stay where they were. That was one of the more difficult things I had to contend with. He decided that was what we needed to do, so we did it.

Somewhere about then, I guess a year or so after that, the Space Station began to look like it was for real. We had space station studies back in Mercury, if you can believe that. Marshall, for example, had a big study on a space station. We decided it was probably for real and that the center had better form a Space Station Project Office. So he asked me or directed me or however he did things, just said them and I saluted. So there I was again to form a program office, a project office. So I went back, and we put together what was the beginnings of the current Space Station Program, project, office. I don't know what it's called now.

I guess I finished up my career there, with a couple of years there, and then I had reached earliest retirement time. I didn't have to retire, but I had decided I'd like to do some other things. I wanted to teach, which I [have] done. So, after two or three years I retired.

BERGEN: What do you think about the current Space Station situation and it being an international project?

PILAND: Very mixed emotions. I go back to our studies of it and our work with it. Incidentally, it's grown considerably since then. I really do have mixed emotions about it. I don't know the answer. I'm sure you have some people who will give you the exact answer. I'm not sure. I don't know. I think if a decision for a space station is not a technical decision as such, it's a different kind, almost a political decision. I guess I had been a little discouraged—and maybe something's happened that I don't know about—in seeing what you might call large benefits, significant benefits that you can identify as potentials. No question that there are numerous specific and detailed experiments, and you could say, well, on a cost-benefit basis, you don't do it for that reason, or maybe some people would say you do.

There's the other reason, do we want to just opt out of space? And that's kind of a significant decision. You might say, hey, I'm not sure about this thing, but we don't want to opt out of people operations in space, so we do it for that reason. That's okay. So it's difficult.

I guess the international stuff, I don't know how to assess that. That's completely beyond my realm of knowledge. I can't assess what the people in Japan, Germany—their assessment. I can see a group in Japan wanting to do this, wanting to do their particular experiments, and wanting to be a participant in this thing and working with their government to convince their government that they should, if you're going to be a major role player, you've got to do this, that, and the other. All of that, I guess, has some significance. I think

the Russian thing is a problem. I don't think that's a very useful bit of information. [Laughter] So I have very mixed emotions about the Space Station.

One thing I'd say is in our studies and at the time I was disappointed, I guess is a way of coming up with things you really could grab hold of. Electrophoresis, I'm off electrophoresis and so forth and so on, making crystals and so forth and so on. It's great. But trying to tie that to this investment, I don't think that's—well, you might find a parallel in the lunar business. Lunar science is great, very interesting, I'm sure, to the scientist. I'm not sure we would have gone to the moon just for lunar science. In just the same way, I'm not sure we would build a space station for orbital science. So that says that there's another reason. And just like the first reason, going to the moon had a lot to do with politics and things. A space station has other reasons, and you have to make your decision whether you think they're good reasons or not. I don't know.

BERGEN: Before we end, I wonder if it'd be okay if I see if Ellen or Carol have any questions they might want to ask you.

PILAND: Sure. I don't have to have answers; they just have to have questions. [Laughter]

BUTLER: I guess I just have one question. You had worked so much with North American getting the contract for Apollo and then working with the lunar module. When we actually landed on the moon, do you remember where you were and what was going through your mind at the time?

PILAND: I was at home. I was into the experiments business, and on that first one we didn't really have any. I could have been at the center, I guess, but, I think, as best I recall, I was home.

I certainly was happy, satisfied, whatever, but I couldn't help but reflect, in all the enthusiasm and cheering and so forth, I couldn't help but reflect back on years of difficulties and pain and those parts of the program also, that our people went through, that had culminated in success, that had contributed to success.

There were tragedies in addition to the Apollo fire. I mean, we lost two astronauts in St. Louis when a jet crashed into the corner of a building. We lost a whole string of parachutists very early in the program. I forget the details now. It was not well publicized, but it had something to do with one of our test operations. We had, off Bermuda, Puerto Rico, a group of people that we lost them. I guess I had, again, mixed emotions—not mixed emotions, I was absolutely thrilled that it happened successfully, but I couldn't help but think back on other parts that weren't so joyous, and the difficult periods.

I can't help but remember problems that—I can remember when we were worried about the capsule floating upside down. I mean, we fought with this problem. It was just a dirty, detailed engineering problem. Remember those flotation bags, and how that was finally the solution? We had something called—I forget what it was—that indicated the possibility of this thing coming in nose first or tipping over in the water and not righting itself. Those bags put on the picture righted itself. It was just a dirty job.

One of the other things I remember that had to do with engineering is how many decisions were made on the basis of statistics. Early on in the program, when it first came up about going to the moon, a leading scientist in the country with great headlines, or at least modest headlines, said that if you go to the moon, to protect the person you'd have to have a lead shield ten feet thick. Well, you know, this was—you'd see people read that, "Gee, what are those clowns trying to do?" The answer to that was we had to go—what he was thinking about was if that thing encountered the worst solar storm that existed. Well, what we had to do was work around the solar cycle and to make measurements and to go at a time when it

was statistically—we still couldn't prove it, but it was statistically very unlikely you'd run into a significant storm, in addition doing what we could within the weight limits we had here.

I'll just give you one more example. Think of the spacecraft coming down in the western Pacific, and whether the structure would survive or not would depend on such things as the way the parachute was swinging, height of the waves, wind velocity, and there were combinations that you could conjure up that we couldn't build a structure to stand, but we could say 99 percent of the time we're not going to run into that combination of conditions, and a decision was made on that basis.

That gets me back into another thing I found interesting about the program, was the things we got into was, where do you find out what the wave heights are in some parts of the Pacific where we were supposed to land? There aren't any meteorological stations there taking pictures every hour. Guys go off—girls, too—searching how do you do these—how do you get this information. Half the time it wasn't available.

I remember another thing. I was coming out of the Space Task Group up in Langley, and I had this friend in the Operations Directorate, and I saw him in the parking lot some time at night, and he was smiling. He said, "Hey, we had a breakthrough today." We'd exchanged good days and bad days, and what he was working on was trying to establish hard-line communications around the world so we wouldn't be dependent strictly on radio between the tracking sites and back in the mission center. He said, "Do you know, we've located a cable. It runs from Australia to Vancouver, British Columbia. It's been there since 1895 or something." Look at your globe and look at the Pacific Ocean, and here's this cable that the Brits or Canadians or Australians, some combination, they laid that cable back, I don't know, fifty or seventy-five years ago all the way across. I always was just so flabbergasted about these kinds of things.

One other thing I might mention. I mentioned a little bit about the poppies in Mexico. This Earth Observation Program, in addition to that, talking about this radiation.

PILAND: You wanted to launch sounding rockets prior to missions down off Brazil to get this southern Atlantic anomaly or whatever it was. Well, I said fine, and our guys arranged for rockets to make radiation measurements. But there again, there was a little *quid pro quo*. So off we were, flying the Amazon and places like Bella Horizonte and other names I can't remember. It was extremely interesting, those programs, and it offered us experiences that we could never have expected otherwise. They were very enjoyable. I always enjoyed doing different things.

LEE: On my way moving down here to Houston, I stopped at southern Mississippi at this hotel, and this man was listening to me and my parents talk about what I was coming down here for. And he said, "Oh, well, you're going to go be working at NASA. I used to work there." I told him I was going to be working on a history project, and he said, "Well, if you ever run into the name Roy [S.] Estess, he'd be really nice." Turns out this guy was Roy Estess, and I ran into [unclear] with you.

PILAND: Oh, it was Roy.

LEE: Yes. I wrote the book that they looked at to do the interview on you. I did the research on you for this project, and I saw his name in conjunction with you.

PILAND: Oh, yes. I think Roy is director of Stennis now.

LEE: Yes. Actually, I think he told me that he had retired.

PILAND: He is an outstanding gentleman.

Butler: He seemed really nice.

PILAND: He is really nice, and when we were over there, they had instigated a little science program. It wasn't Earth Observations, but it was kind of applications of space stuff. I guess one part of that which you might remember that one of the people was working on, was this business of insects or amoebas or something that would eat up oil spills. You might have heard that, whatever it was. But they had started that little organization, and subsequently, that little organization, which he ran at the time, was integrated with our Earth Resources Laboratory. I think they're one and the same today. I don't know what they call it now, but he was probably one of the more outstanding people I met over there, very nice and smart.

If you ever see him again, ask him about a fellow named Balch, B-A-L-C-H. I don't imagine you'll run across him. We won't go into Mr. Balch. That's another story. We don't have enough tape for that.

BERGEN: I have one final question. You worked so many years in the space industry, what would you like to see in the future?

PILAND: Oh, gee. If I'd known that, I wouldn't have retired. [Laughter] It's very difficult. I hear all the conversation about Mars. I'm sure some people have, but I don't think superficially we've faced up to just how big a project that is. I think we're thinking, well, it's just a little bit further than the moon or something. And I guess I'm not sure we've thought through why we want to go there. Maybe you did. I don't know. I think it takes—again, I think the scientists would have to produce an awfully good case that you would undertake that kind of effort for certain kind of science experiments. It'd surely be interesting, but on a tradeoff here.

I guess one of the things—first of all, NASA does this in the manned program particularly, the manned program's involvement, but what they call it, Mission to Earth or something? It's the Earth Observations Program. I don't believe we've exploited the Earth observations to the extent they could be, for various reasons. I think there are still things to be mined there. I still find the Earth the most interesting of planets, mainly because I plan to live here, hopefully, for a few more years, and we've got six billion people we've got to look after. I would hope that program receives support. I would hope the people involved can justify such support.

The manned program—I'd like to see another Shuttle. The Shuttle is, what, twenty-five, thirty years old, whatever. In some ways, a new Shuttle—of course, it's too late now—but versus a space station, a new Shuttle in itself which can do lots of things. It can do many of the things a space station can, except it can't do them but for so long. I always thought—well, I don't know about always—but the business of a smaller—I'll use the term "smaller"—space station that you visited and put things there and left things there and left them there to get your time in space there. Of course, people will say, "What we want to do is stay manned—or ladied—in space for long periods of time." Okay. If you put that requirement in there, you've obviously got to have a station to keep people there. I don't know.

You know, we've spent an awful lot of time putting people in space. That doesn't mean you can't learn things about them, but the Russians send people up there and leave them and forget about them, it seems like. They might or might not come back and get them if things are working right. I don't know. I don't think my thoughts are very useful in this arena.

I think a new Shuttle—I do think a new Shuttle is—and I think there are some useful things, many useful things you can do with a Shuttle, and I think if you built a new Shuttle, it would have more capabilities and bells and whistles and things. I guess I'm worrying about the Space Station having trouble under its own weight. When I say weight, I don't mean that,

but its overhead, its continuing cost of operations. Maybe it won't. Maybe it'll work out fine. So I'm not against the Space Station, but I have mixed emotions. I would like to see a new Shuttle. I mean, you know, we've got to be able to build a better airplane twenty-five years later.

Max [Faget] for many years had a concept about a space station called a "can." I'm sure you've run across this can. In retrospect, at the time I didn't have much feelings one way or the other. In retrospect, I think it probably would have been a good idea, as most of his ideas are.

One thing about Max, whom I've greatly admired always and not just for his engineering talents, but in other ways, I guess it was right after the Russians launched Sputnik, we went to Ames from Langley—this was before NASA—on a DC-3, which took us a week, it seems like, to get to California, and we took a group of people to a conference. I remember the day before we left—and what was this? This was '57, maybe, and the Shuttle came along, what, thirteen years later. Max was in our office and he was throwing gliders around that looked very much like a Shuttle. I don't know if he remembers that.

See, even back at that time, the Air Force had made studies that were kind of Shuttle-like. I don't know if you've ever heard the words "dinosaur," which how you would ever sell something you'd name "dinosaur" I never figured out, when you took it apart, and "Brass Bell" and "Robo." These were kind of Shuttle-like things. I remember Max—we hadn't even got up the Mercury yet, and he was flying these little models that, as I recall, looked like Shuttles. When the Shuttle came to life and I was doing something else, I remembered that. I think he finally got what he wanted.

I don't know why I was talking about that. Did you ask me a question? [Laughter]

BERGEN: That's wonderful. We like to hear stories like that.

PILAND: Well, they're interesting. I guess there's an awful lot more of little things that—I don't know whether it's good or bad. I don't know where you stop in these things, but you've got to be somewhat reasonable.

Good Lord, I'm keeping you from your lunch.

BERGEN: That's perfectly okay.

PILAND: Anything else?

BERGEN: Well, I think that we've made you talk long enough for today. We appreciate it. It's been wonderful. It's very interesting.

PILAND: I appreciate it. I enjoyed it. It's kind of interesting, reminds me of things, like I say, both good and bad. I'll be interested to see the Space Station develop. I've been out of the program. I retired in '83, so that's almost fifteen years. I go over there and there's nobody there I know except George and one or two others. I read the little paper they put out, and I never see any names I recognize. It's sad, but it's not sad. Get the old people out of there and let the other people do it. They'll have some new ideas and such.

One other thing. Back at Langley, before we came down here, we needed people, a lot of people, and there was this fellow from Langley who was—well, he came down here, I guess he was serving as our personnel director. He was our administrative director, I guess, but he was doing everything. We were having trouble recruiting enough people. I'm sure you're familiar with the bunch we got from Canada, thirty or forty, but that more publicized than what I'm going to tell you. But they hired 100 new graduates—I think it was 100—right out of school. I remember him coming in one day. He said, "Hey, here I've got a good kid here. He's from East Tennessee State University."

I said, "Where? Where is East Tennessee?"

He said, "Trust me. He's a good boy." And he was. And those young people—there was 100 of them—if you go back and check, they contributed greatly to this program in very significant ways. They were given responsibility early on, much more responsibility than they normally would. They responded very well to it. Like I say, they didn't get as much publicity as the Canadians, but there were more of them, and they played a very significant role in our programs.

Enough of this yakking.

BERGEN: Thank you.

PILAND: You're most welcome.

[End of Interview]