ORAL HISTORY TRANSCRIPT

ANTHONY J. CALIO
INTERVIEWED BY CAROL BUTLER
MCLEAN, VIRGINIA – 12 APRIL 2000

BUTLER: Today is April 12, 2000. This oral history with Tony Calio is being conducted for the Johnson Space Center Oral History Project. Carol Butler is the interviewer, and is being assisted by Kevin Rusnak. We are at Mr. Calio's home in McLean, Virginia.

Thank you so much for allowing us to come here today and to talk to you about your experiences in the space program.

CALIO: It's my pleasure to be interviewed, and I think the project you're talking about is a great project, and I wish you success with its implementation and completion.

BUTLER: Thank you.

CALIO: I hope it does keep going, because there's a lot we can still learn.

BUTLER: There is. There's a lot to learn, and a lot of interesting people that made it all happen.

To begin with, if maybe you could tell us about how you became interested in the sciences. You majored in physics, and how your interest then grew and led to you becoming involved in aviation and the space program, aerospace.

CALIO: I was trying to go back in my mind as to what triggered all of this, and you're not going to believe this, I guess, but my father and my mother took me to the World's Fair in 1939 in New York. The General Motors exhibit was talking about the future of the country

and the future of the world, and they talked about being able to travel from New York to London in ninety minutes, and they were going to do this in the year 1999. I kept asking my father if I was still going to be around in 1999 to see that happen, never thinking that I'd be involved in any part of it. But I just thought that was really intriguing, in ninety minutes. It would take us an hour and a half to go to Atlantic City. I lived in Philadelphia then, so I just couldn't imagine how that could happen.

So that got me interested pretty much in what makes things work, and I couldn't make up my mind as to whether I wanted to study physics or chemical engineering, and I finally decided physics was the right field, because for physics you could do all sorts of things. You really had to understand nature and how it operates. So that should be able to help you work your way through all of the sciences and engineering. So that's how I ended up in the physics side. And there was that little thing in the back of my head about going to London in ninety minutes. [Laughter] It kept pushing on me.

But strangely enough, I started out in nuclear physics, and after I served in the Army, I went to work for Westinghouse Atomic Power Division in Pittsburgh, Pennsylvania, where I was part of the team that developed and put in the first land-based nuclear power reactor in Shippingport, Pennsylvania.

But unfortunately, the funding for those kinds of applications programs really went south, and so I was trying to figure out what would be equally as exciting. At that point in time, I was connected with a fellow I worked for when I was in the Army. When I was in the Army, I was in the Chemical Corps. I was assigned to Fort Dietrick [Maryland], which was a biological fort for a center. So I've been in a lot of this crazy stuff for almost all my life. Never by choice, but just by accident.

So we decided to see if we could open up our own company, and that's how we started Mount Vernon Research Corporation. I came out of Westinghouse, and we started here in Alexandria, Virginia. What we did at first was what we used to call buck physics,

which was sort of the beginning of the period when support contractors would help the government solve some of their problems. So there was a myriad of small little start-up companies here, and I guess in a roundabout way, up in 28 in Boston, so we started our company, and what we were doing at the time was building scientific instrumentation for the first rocket programs.

We worked for a fellow by the name of Dr. Chubb, who was an ultraviolet astronomer out of the Naval Research Laboratory, and we were building all of those little ion chambers and sensitive devices to measure the ultraviolet radiation in the upper atmosphere. He [was] associated with [Dr. James A.] Van Allen and how the early belts were discovered. So we were building a lot of that instrumentation.

We sort of got a strange notion in our heads. The other fellow, my partner at the time, he was part of [the] atomic energy program—I'm trying to remember the name of the activity—down in Los Alamos [New Mexico]. So he had a lot of experience in nuclear diffusion, and with my background with Westinghouse and where I went to school at the University of Pennsylvania, I was responsible for developing vacuum chambers and being able to get charged particles through these various tubes. So we thought, hey, we had a skill nobody else had. [Laughter]

The Applied Physic Laboratory up here in Maryland had a contract to build Transit, which was the first navigational satellite. So they wanted a space environmental simulation chamber to be built to test these satellites before they launched them, so we decided to go into that business. Now, here are two physicists who had no practical experience with their hands, trying to go into a manufacturing business. If at the end, you ask me what I've learned: don't do something like that. [Laughter] Because we started out and we were doing fine, and we built these chambers for Applied Physics Laboratory. We were one of the few companies in that kind of business at the time. This was 1958, '59.

At that point in time, the Langley Research Center [Hampton, Vriginia] found out about us, and they wanted us to build a vacuum chamber that would test instruments that would go through a shake table. They still had to have a vacuum, and you had to have a simulation of vacuum and solar radiation. We built that.

Then we—you're going to like this one—we won a major contract with Hughes Aircraft Company to build the environmental simulation facilities for Surveyor, which was one of the first spacecraft to land on the Moon, and we built that chamber. It was the first chamber Hughes Aircraft Company ever had. It was strange that I ended up working for Hughes when it was all over. Hughes was the company that sort of broke our back. We couldn't grow fast enough. At that point in time we had grown, doesn't sound like much now, to a million dollars' worth of business in one year for a handful of us, and that was more than we could handle. So we finally ended up selling out.

At that point in time, I didn't want to get out of the space business. I was hooked. The question of whether you're in engineering or science or what have you, or the instrument side or the spacecraft side, there was a lot of opportunities at that point in time. So I applied to NASA for a job, and I was selected to join NASA.

That was another strange happening, because I was hired by, at that time it was the technology group for NASA here at [NASA] Headquarters [Washington, DC], and they were committed to design and build an electronic research center. So I was put on a task force here to come up with the requirements, the design requirements for such a center, and that was to take place in Boston, Massachusetts. My specific job at that point in time was to try to understand what kind of measurements would be made in such a laboratory, what would you need, and then to translate those into architectural requirements.

So my past experience somehow led me into that and allowed me to do that job. So I joined NASA and we sold the—at that time—gee, I was trying to think of his first name—it was Captain Kelly who was the head of that operation, a naval flier, who was also a graduate

from Draper Laboratories. He had his Ph.D. in guidance and control. So he was our team leader. We defined what would happen, and we sold it to Congress. So then I went up as a task force to start the operation in Boston.

I guess to make a long story short, I guess we got tied up in the wrong situation, because, first, the concept didn't make it. When you really stop and think about it, what they were going to do was take all the electronics research out of all the existing centers and put it in this Boston center. They, in turn, would then provide all the research and development that was necessary for the development centers to do their job. It's like taking a typewriter, or taking these things away from somebody who was really developing the work.

At any rate, they brought in another fellow from the outside, who at the time was an executive for Philco [Philadelphia Storage Battery Company] to run the new center, and cost just got out of hand. We had a budget of about 60 million dollars, in which we had to build these facilities. This is now back in 1963, '62, '63, which at that time was a lot of money. But things got out of hand, where it was something like 150 to 160 million when the architects got finished with the design. So the project, unfortunately, collapsed of its weight.

They built one building, which was another peculiarity. The architect, who at the time was Edward Durrell Stone, he was great for his minarets. So here we are in a back bay of Boston on blue clay, and he was going to build these thirty-story buildings with the guidance and control labs on them for tables that were supposed to be stabilized for inertial guidance systems and the like. I mean, it was a real Donnybrook, because the work they'd have to do to make those platform stable was what cost so much money. It would have been a much better thing if they put that tower on the side and make a slab out of it and that long one-story building. It was still on blue clay, which shifts all the time, was very difficult to set up guidance laboratories.

At any rate, unfortunately, the project collapsed of its own weight, and at that time I then was asked to come back to NASA Headquarters. I was picked up by an interesting

group. It was called the Manned Space Science Division. They talk here about science and engineering. NASA was really bifurcated at the time. There was a manned group and there was the unmanned satellite groups.

The unmanned satellite groups were the ones that did most of the science at the Goddard Space Flight Center [Greenbelt, Maryland], JPL [Jet Propulsion Laboratory, Pasadena, California]. The research centers like Langley and Lewis [Research Center, Cleveland, Ohio] supported both the manned and the unmanned, but more the unmanned activities with most of the science being done there. So some of the early work that was being done was the first atmospheric explorers and all of the work that Jim Van Allen did on developing the understanding of Van Allen Belts. They worked through the Goddard Space Flight Center.

Space science, as it was developing then, was pretty much being developed out of those two organizations, the planetary at JPL and the earth-oriented stuff out of Goddard. There was sort of a schism in the operation. At that time, Homer Newell was the head of space science, was very focused on advancing space science. George [E.] Mueller was the head of the manned space program. There was really quite a dispersement of views as to what was important and how it was to be done. We were put in place to straddle those two operations in trying to get science on the manned program. That was my job at the time.

You asked me about what was I trying to do. In Headquarters in Instrumentation and Systems Integration Branch, that was the whole thing, was to try to get the right kind of science defined into the instrumentation, and then to get that put into the program planning associated with the manned missions. Headquarters is a planning operation.

You asked me the distinction between the field centers and Headquarters. Headquarters, your job is to put the program plans together in concert with the field centers, in concert with, which isn't always the easy thing to do, and then to sell it to the agency, then

to OMB [Office of Management and Budget], and then finally to Congress. It's what the agency wanted to do.

So that was a very difficult task. The reason for it was, and in retrospect it was the right approach, when you went into the manned program, the objective was very clear. Our objective was to get a man on the Moon and bring him back safely before the end of the decade. It had nothing to do with science. It had nothing to do with what do you do when you get there and things of that sort. The manned side was very well focused on that, and mostly engineers, and rightly so. I say that in retrospect, because being in the middle of all that sometimes I had a fight. I don't want to use the word fight; that's the wrong word. I'd negotiate both sides. It was very difficult because people were pretty much locked on to what they thought was most important.

At any rate, we started to do some work in inputting instrumentation on the Gemini Program, right towards the end of the Gemini Program. We were able to get some scientific instruments on board. At the time, I guess the general view by the science community and the National Academy of Sciences—I think at that time they didn't have an Academy of Engineering; I think it was the Academy of Sciences—was looking at Apollo and the money that was being spent on Apollo, and here we were going to go to the Moon, and what were the plans for what we were going to do when we got there? And what was this going to do to contribute to the advance of science? That didn't come down too easily.

I guess right about that time we had the Apollo [1] fire, and, rightly so, a lot of good people were really hurt by that. Not only the astronauts that were killed [Virgil I. "Gus Grissom, Roger Chaffee and Edward H. White II], but also people like Joe [Joseph F.] Shea and others who were so instrumental in the early developments were essentially moved aside. I think Joe took the blame personally for what had happened. So there was a lot of tension as to what we did next on the program.

At that time George [M.] Low was the deputy under Mueller in Headquarters. I think that the timing is right. Just about then he went down to Houston [Manned Spacecraft Center, Houston, Texas] to become deputy director under Bob [Robert R.] Gilruth. Then subsequently he became Apollo program manager. That was a very, very tense time in NASA. They were really trying to understand how all this happened.

Unfortunately, I guess you never had the opportunity to talk to George Low. He was a [brilliant] man. He had great skills and was an excellent manager. So was Joe Shea. I don't mean to knock Joe about that either, he was as well. But ultimately George went in there and he ran the program with an iron hand, with a velvet glove.

At that point in time I was asked—they brought a fellow in, they figured they'd better do something about science, and they brought a fellow in to head up the Science Directorate. Initially that was—come on, Tony—and I worked for him for a couple of years. I'll think of his name. Boy, I just blanked out.

BUTLER: Was that Mr. Hess?

CALIO: Yes, okay. Thank you. Wilmut [N.] Hess. Right. Wilmut Hess, another brilliant man. But he was a theoretician, and he was a particles and field scientist, not a geoscientist. But they brought him down here to be the director.

I don't know exactly how it all happened, but at that time I was in the planetary program, and the lunar orbiter was under us as well. So the first planning that we did for the lunar orbiter and the first mapping of the Moon was under this major program. So I was involved in some of it from a Headquarters programmatic standpoint, not from a hands-on doing it at the center. I think because of that, I was recommended to go down there as Hess' deputy. Hess was very science-focused, not project or instrumentation-focused. Hess was not a program manager, but a brilliant scientist. As far as he was concerned, he was doing

science for science sake, and not necessarily within the confines of what the parameters were governing the Apollo Program.

So I went down and went to work for him as his deputy. At that time, the way we set it up is, "I don't want to have anything to do with the project. I don't want to have anything to do with the instrumentation. Calio, that's all yours. You get those things done, and I'll work with the outside scientific community," because he was one who was looked at from the National Academy as being an outstanding scientist. We were in the process of developing requirements for lunar sample analysis. We were building a lunar sample receiving laboratory, etc.

So Bill took the sample receiving lab and the outside scientific community. I took the instrumentation, and I sat on George Low's board for the Configuration Control Board. That's how George managed the program. That board was very powerful. George chaired it, Chris [Christopher C. Kraft Jr.] sat on it for operations, Deke [Donald K. Slayton] sat on it for the flight crews, and Max [Maxime A.] Faget for engineering, myself for science. Who did I miss? Both of the sub-elements, the orbiter and the lander program managers were on it as well. I missed somebody and I don't remember who. Somebody else I'm not seeing. George ran the Apollo Program through those of us who were on the board.

As an aside, we always had board meetings on Friday. They'd always start at eight o'clock in the morning, and they would never finish until everything was done, and it didn't make any difference if it was six o'clock, nine o'clock, midnight or what have you. Once a month we would travel to the contractors and hold the board meetings there. One day at Rockwell [International Corporation] and another day at—

BUTLER: Grumman [Aircraft Engineering Corporation]?

CALIO: Grumman. Thank you. Grumman and Rockwell people were on the board as well. So that's where I got a lot of discipline and learned a lot from George. George used to have eight o'clock stand-up meetings every Monday morning. So you had five minutes to tell him the status of your part of the responsibility of the program, and you'd better get it straight. And I mean that in a very loving way. George was warm and gentle to everybody, but he meant business, and you had to get your job done, and he held you personally accountable for it.

I was thirty-seven years old at the time. That was a heck of a lot of responsibility for a thirty-seven-year-old person. We were all young at the time. We were all kids, so to speak. I'm seventy now, so I guess I look back at that and say if there's anything that's probably bothersome today is we aren't giving young people the opportunities we had when we started.

At any rate, I get off on a track, and I didn't mean to do that.

BUTLER: That's all right.

CALIO: Back to Bill Hess and the beginning of the directorate. So put that in place, and now in the meantime, we had two other factions going on for science within the center. There were a couple of engineers who realizing the importance of science, but weren't connected to the scientific community and were trying to do science. That turned out to be a real fiasco, unfortunately.

That, of course, muddied the waters, because the third faction we had U.S. Geological Survey with the likes of people like—come on, Tony—Gene [Eugene] Shoemaker, brilliant astrophysicist. He had started an astrophysics lab at Flagstaff, Arizona, with the notion that that group would be the group who would call for the science, direct the science, develop the

science requirements and the implementation of the scientific experiments on board the manned missions. So they had been set up and started.

We got into a bureaucratic mess, I guess is the best way to describe it, because there was another, I guess a fourth component. There were the geo types who were more interested in not just the geological exploration as you would in field geology, but with the chemistry and physics for the moon. They wanted to have a say about this as well. So you had all these scientific schisms going on. At the same time, you were trying to figure out who was responsible for really training the astronauts, deciding where we went on the Moon and making sure the instrumentation was in place and working at the right time and for the right amount of money.

Hess took on the responsibility that was his, was within the center. Bob Gilruth supported that. So consequently, we were starting to build a wall between ourselves and [the] U.S. Geological Survey. There were people like Gordon [A.] Swann and Bill [William H.] Muelberger and a few others who worked for the survey, who were isolated at the time. That conflict really grew and was not a good environment. That didn't help the program, took a lot of energy away from the focus of what everybody was doing.

The group that was the engineering group sort of slowly faded away, and it was at Houston at the Johnson Space Center [formerly Manned Spacecraft Center] , slowly faded away into the sunset and we built a new directorate. We also had the responsibility then of making sure that we understood what had to be done with the Lunar Receiving Laboratory, because the concern was back contamination. CDC, you're familiar with CDC [Centers for Disease Control]. On trying to specify how we handle all this and what were the appropriate incubation periods, how long you have to keep the astronauts in the facility, etc, etc. Then also on handling of lunar materials, how do you handle them, there are specifications built for how you manage to take the rocks and put them in these containers, that would be self-

contained and wouldn't be opened until they were back into the laboratory and the seals broken inside the hoods so everything was contained.

In the early days, that caused all sorts of problems. First of all, the crews didn't understand why the hell they had to sit in that place for twenty-one days. That went over like a lead balloon, let me tell you. [Laughter] Especially when you look at all the training they had to go through that really took the majority of your time, and then finally when they get the mission over, and it was all over, those guys sitting in that crazy facility for twenty-one days. Well, but they did it. Of course, we had to administer that. That didn't go over too well.

You asked what was the relationship between science and engineering. It was apprehensive. [Laughter] Those engineers didn't trust us, and the science community, they weren't sure that the requirements [we] were [levying on them] truly needed to be done. But at any rate, Bob Gilruth was very supportive of what we were doing. Chris at the time was on the fence. At that time, when I first went in, of course, Chris was just the head of flight ops and Bob was the head of the center. Then right shortly after that, of course, Chris became the center director.

I grew very close to Chris even before he became the center director. He was very supportive of me. I never, except in one instance, I never had any difficulties with Chris. He was always very supportive of what I was trying to do. He wasn't very happy with Wilmut Hess, because Hess would never try to give him an explanation for what it is he was doing. Understanding Chris, Chris always wanted—"Hey, I'll do whatever you guys want me to do, but I want to understand what I'm doing and why I'm doing it."

As I said earlier, Bill was science for science sake and "Just do it because I told you to do it. I'm the scientist. You're the engineer." So that didn't go over very well. [Laughter] Finally, Bill left in a real uproar, unfortunately.

Then there was a big opening at the time and Chris and Bob Gilruth said, "Well, are you going to try it?"

I said, "Well, I'll give it a shot. I don't know if I can run this operation or not."

Chris said, "You can do it. You can do it." So that's how I was put in as then the head of science. I never looked at myself as a scientist for science sake. I've always looked at myself as somebody who would try to apply science. I guess if I had to do it over again, I should have been an engineer, but I wasn't. I'm more a program manager than anything else. That's how it developed over the years.

George was very supportive of me, George Low, and Chris. So I really had the power base of the center behind me. So what I had to do was see if I could get all these scientists to try to work together. I know I could never reach out to them. I didn't have the credentials that they had. So I established a team called the Four Horsemen. They were made up of Jerry [Gerald] Wasserburg at Caltech; Bob Walker, Washington University at St. Louis; Paul [Gast], Columbia University. Let's see, who was the fourth? Jim [James] Arnold, University of California-San Diego. They were world-renowned geoscientists, geochemists, geophysicists and the like, and they became my advisory council. They were the ones that helped me on the science side to establish a relationship with the scientific community, and sort of became defenders of what I did and helped me put the plans together. I was the guy at the switch, and tried to develop a relationship with the center, and realizing I had to get the center to understand what we're doing and why.

You asked me at the end what was one of the things that I feel that I helped accomplish, and that was to try to bring forward and finally make the remaining Apollo missions useful for science and yet not in conflict with engineering. So I was more of an implementer in all of that. Again, it was incoming from other people, the engineering side doing their thing, and the Four Horsemen in doing the science side.

Now, that created a little bit of conflict, too, because U.S. Geological Survey wasn't very happy with the Four Horsemen. As a matter of fact, there was a competitor at Caltech by the name of Leon [T.] Silver—and all these people, believe me, are great in their own right. I'm not knocking any one of them. But Silver and Wassberg were constantly at odds with each other, and that didn't go over very well, because Lee wanted to train the astronauts. He was more of a field geologist, whereas Jerry was a geochemist. I don't think they ever got things straightened out, but at least we got it to a point where we could ultimately work together.

I guess on the first few crews, Neil [A. Armstrong] was beautiful to work with on Apollo 11. I'll come back to that, because we had a little few difficulties with that, as we were planning it.

Apollo 12. I've got a story to tell you with Apollo 12 that will never quit, with Pete [Charles C.] Conrad. You may have heard the story.

Thirteen, of course, Jim [James A.] Lovell [Jr.], that was very unfortunate. I guess at that point in time is where I got close to Jim [James A.] McDivitt, because Jim had taken over the Apollo Program from George. I think it was at 13. I think it was 13, I think. I don't remember exactly. But Jim took over the program, and there was another outstanding man.

Then Apollo 14 we had the clown. Boy, trying to explain when we were fighting for time on the Moon, a guy pulling out a golf club and trying to hit a Moon rock like a golf ball. Oh, God, did it come down on me. The scientific community was ready to rip me to pieces. [Laughter] But that was [Alan B.] Shepard [Jr.], and Shepard was Shepard.

You asked, make a comment about Apollo 15. That's what I was trying to lead into in that segment. That was Dave [David R.] Scott. Dave Scott had something in his head, by golly, he was going to make science work on this mission, and he really applied himself and the crew. At that point in time, too, I think we were trying to figure out who had what

responsibility, and although we never finally documented it, the center people, for other reasons that I'll come back and tell you why in a minute, divided the activities.

The training of the crew from a field geology standpoint, this really was the best team to do it, Lee Silver and Muelberger and all that, and Dave Scott made that happen. Our group was more the analyst. What do you do with the stuff when you get it? What are the requirements for taking it, and things of that sort. We were the chemists, the physicists and the magnetics guys and all that, trying to understand what do you do with the Moon substances once you got them back. Dave was instrumental in making that happen. He really applied himself very hard, and the crew, to bridge that gap. He did a great job.

Well, let's see, that was 15. Mike [Michael] Collins was his—Mike Collins—what's his name? Not Mike Collins. Mike Collins flew on 11. Who was 15?

BUTLER: Jim [James B.] Irwin?

CALIO: No, the next one. The third one.

BUTLER: [Alfred M.] Worden. Al Worden.

CALIO: Al Worden. That's 15? Who's 16?

BUTLER: Sixteen was [John W.] Young, [Thomas K.] Mattingly, and [Charles M.] Duke [Jr.].

CALIO: Okay. There's where it is. Okay, Charlie Duke. Charlie Duke picked up the grail on 16. Young was an engineer. But Duke thought, "Hey, I'm going to follow in the footsteps of

Dave Scott," and he did. He took up the mantle in that respect and became very well trained in what he had to do.

Of course, 17 was Jack [Harrison H.] Schmitt. That was another side issue, I guess. Jack and I used to really go at it, both of us being very pragmatic, but it all worked out in the end.

So what happened on 15, 16 and 17 was really the responsibility of Dave Scott, I think, in turning that around and getting the crews to pay more attention to what they were doing on the Moon for science sake. U.S. Geological Survey was brought in to train them, so they got good field training, and our people did the analysis and talked about the samples and the instrumentation and all that stuff. So it separated itself out, not necessarily in a quiet friendly way, but it did finally work its way through.

I guess we had a few disasters in the meantime. Paul Gast was one of the Four Horsemen. I really wanted somebody at the center who would be there all the time and could understand the problems of the center from a scientific standpoint, and not to be just like advisory groups, because they would tell me some things sometimes. They couldn't understand why I couldn't get them done, but the politics of the situation wouldn't allow it to happen. So Paul came to work for us from Columbia as the head of lunar science, and, very sadly, got cancer in a couple of years and died. That was traumatic for all of us. We never could replace him.

Mike [Michael B.] Duke, who was then the curator under Paul, took over and did a masterful job. You asked me a question, who did I look to on the science side. There were people like Mike Duke, Paul Gast, people who worked in the receiving laboratory, who had their head screwed on right and did the right job at the right time. I guess Mike's still there, I think. Isn't he, Mike Duke?

BUTLER: He's working through Lunar and Planetary Institute right now. But still associated, yes.

CALIO: Yes. Well, he was the curator at the time. Other people, Jack [W.] Small. You asked about ALSEP [Apollo Lunar Surface Experiment Package]. Jack Small left the center at a very early time period, but he was one of the people who was on the project management side and was responsible for bring ALSEP together. That couldn't have happened without Jack and people like Dick Mock. Dick Mock isn't there anymore, I don't think. John [G.] Zacaro, I don't think he's there anymore. All of those folks, the last three fellows I talked about were the fellows that were the program managers. They got the instrumentation together and got it on the spacecraft and met all the spacecraft requirements and the like, which was no easy task.

As you have probably heard by now, or understand by now, getting stuff on the spacecraft you have to really consider weight and cost and time, etc., and time and weight are very precious. We used to have a rule of thumb that it would take you 10 dollars to put something on and 10,000 to take it off. [Laughter] So we had to be very careful of what we put on. Time and space were limited on Apollo and there were a lot of unknowns. We tried to do as many engineering tests as we could, but there's still a lot of unknowns. A lot of the work was done, I think, not just from an engineering standpoint of testing everything, but on the belief within the system that the individuals who stood up and said they're ready to fly, they knew they were ready to fly. George was responsible for that. So when you went through final review for whether we go or not, you had to stand up and he held you personally responsible for making sure what you delivered would work right, not jeopardize the crew.

So that's sort of an overview of the situation at the time. I guess I got crossed a little wise with Bob Gilruth, the fact that the scientific community wanted to continue to fly

Apollo, and we had more spacecraft and launch vehicles in the pipeline. I took on the role of trying to get those to stay alive. I think that's the first time, first and only time, I ever got Bob Gilruth upset with me, because I can remember him telling me, "What's the matter with you? [Do] you want to continue to fly this program until you kill some more astronauts?" I didn't realize until then how heavy Apollo 4 [Apollo 1 fire] rested on his shoulders. He never wanted another incident like that. So we stopped at 17. We could have gone to 20. There are a lot of little anecdotes that fit into all that.

Let me come back to the beginning again. I guess Bill Hess left. Then there was a question about who should take over the Lunar Receiving Laboratory. At that time life sciences and physical sciences were separated. I don't know how they are now. But Chris put them together, because Chuck [Charles A.] Berry, I think, left at that point in time, and a fellow by the name of Dick [Richard S.] Johnston took over the Life Sciences Directorate and he became a part of ours. Then Dick took over the final development of the Lunar Receiving Laboratory.

The Lunar Receiving Laboratory turned out, in some cases, to be a spur under some people's saddle from the standpoint of the costs and restrictions that were associated with it. The original head of the laboratory was a fellow by the name of P. R. [Persa] Bell. Oak Ridge Y12, excuse me. That's where my partner was, too, Oak Ridge Y12, that developed the diffusion processes and the like. P. R. Bell was there, too. P. R. Bell helped build the Oak Ridge facilities. A brilliant man, too, but a very strange fellow in how he'd go about doing things. So the general notion was more requirements were getting in than really had to be done to meet the criteria for satisfying the CDC, and that created a lot of problems.

Bill was a great supporter of—well, he brought P. R. in, as a matter of fact, to build the facility. So when Bill left, P. R. was there for a little while longer, and then finally Dick Johnston took over to manage the facility and bring it home and make it work the way it

finally worked. That ties off that end. The Receiving Laboratory had its ups and downs. I think in many respects it did a good job.

I guess with Apollo 12, that was our first spill, when all the doors closed, etc., the crew was inside. As a matter of fact, we had a couple of scientists inside. Golly, I can't remember his name. We had a world-renowned geochemist in there from the University of California-San Diego [Chris Frondell]. I said, "Oh, my God, he's going to be locked up there for twenty-one days." So I got on the telephone. This was Apollo 12, Pete Conrad's mission. The scientists went in to debrief the astronauts on what they saw on the Moon and how they saw it. I can't remember his name. So I picked up the phone and talked to him, and I said, "Gee, I'm sorry, what happened."

He said, "Don't be sorry about what happened, just get us a case of Scotch in here and everything will be all right." [Laughter] As it turned out, we got the case of Scotch in there. He and the astronauts had a great time. He had quite an experience with those days until we could recover from the spill.

Of course, Pete Conrad was a character to begin with. There was another little anecdote with Pete that caused me a great deal of anguish, as well, from the scientific community. When the astronauts were on the surface and they were going to pick up a rock, before they did that, they were supposed to take pictures of it from various angles to be able to understand how the filets were up against the rocks, because of the question of the solar wind and how the solar wind would affect the materials on the lunar surface. So they could separate out the charged particles as to what was causing what to happen. Then they were to pick up the rock, take a picture of the rock, take a picture of the hole they left.

Pete came ding-donging along and he kicks the rock, takes a picture of it. [Laughter] He did this a couple of times. He was taught not to do that, but Pete didn't do that. Oh, when the scientific community heard about that, they came down on me like a ton of bricks. I had people come down from the National Academy, looking at how badly that the science was

being run on the Apollo. Oh, jeez, it was unbelievable. That's when the notion that—Wilmut had started the concept of a consortium. Who [was] next door? Who's Duke working for now?

BUTLER: Lunar Planetary Institute.

CALIO: Lunar Planetary Institute. Bill Hess had started discussions at that time with Dean Gordon at Rice University on the establishment of this institute right next door, and the fact that this would be a place where visiting scientists could come and have a repository of all this information without necessarily coming on the center, and that institute would also help in developing requirements in the analysis of what we would do on the Moon and how we did it. That has a [whole set of] stories of its own.

So at that point in time, after Pete did this, there was a great push then for this institute to be established, because these yo-yos in Houston didn't know what they were doing. We had this committee come down, and then they finally backed off. Then they put in a fellow by the name of Joe Chamberlain to head up the institute. Joe and our people didn't get along too well, nor did I with him. So I took him on, unfortunately, which caused another investigation as far as the academy was concerned, because Joe Chamberlain was a world-renowned scientist, and who's this turkey down here in Houston who's going to tell him what to do. So we were investigated again by the academy.

So these things used to go on and off. You asked the question about my relationship with people. I'm a very pragmatic, was very pragmatic at the time. I was focused on getting the job done. You're either going to help me to do it, or you're going to step aside and I'll get it done anyway. So that wasn't very bright, in retrospect. I did not necessarily try to develop satisfactory relations with people, because I had this notion in my head that there are those who help and those who won't. I don't want to waste my time with those who won't. So I

was a Quick Draw McGraw, in a sense, and I finally developed a relationship with people. They either loved me or hated me; there was nobody in the middle. So that's the reputation I developed. "Don't get Calio angry with you, because he'll take you out of the system." And that was not necessarily a very smart way to handle things. I guess I was not very diplomatic, nor did I see at the time the importance of stopping and training the folks or getting the folks to understand what we were trying to do. Instead, I pushed them aside. That was a mistake.

Yes, that sort of ended up with the institute situation. They changed out Joe Chamberlain. They finally put in somebody else. There was a fellow by the name of Gene Simmons. They brought him down from MIT [Massachusetts Institute of Technology]. He was supposed to be with the staff of Gilruth to watch this guy Calio to make sure I didn't do anything crazy, or Paul Gast to do anything absurd, as far as science was concerned. That turned out to be nothing, because Gene really never really engaged or made an impact as far as what we did.

I'm trying to figure who then went over to the Lunar Science Institute after Chamberlin left. I can't remember it. I can't remember who that was. At any rate, that ties off another little aspect of it.

Let's go on back and talk about the instrumentation. Our job was to determine where we [went] on the Moon, what we did when we got there, development of the instrumentation that went on board, training the astronauts, and the subsequent analysis of the data to see that it was analyzed and published. Three, that was my charter. Now, my charter, that was the charter of the directorate that had to be done in concert along with other people.

At the time, there was a group in Washington that supported Headquarters, it was called Bellcomm [Inc.]. It was like the SI, the systems integrator concept. They sat on the side, did all the analysis, etc., etc., and advised—let's see, when George left, General [Samuel C.] Phillips, Phillips, came in as the Headquarters program director, George being the one in

the field center. They used to advise Phillips. I guess after Phillips was Rocco [A.] Petrone, one of the program directors from Headquarters.

So in concert with Bellcomm we did a lot of the analysis to determine where we [went] on the Moon, picking of the sites, what was most interesting, what was most interesting from a scientific standpoint based on a whole set of criteria. The Four Horsemen helped me a lot on that. So did the survey weigh in on that? I guess—he's dead now too, Hal Masursky was one of the major people in the survey who really got involved in the lunar planning activity. But ultimately Bellcomm would develop the final papers and submit them to Headquarters for approval. The head of that operation at the time was Noel [W.] Hinners, who ultimately went to work for NASA, became head of science there. I ended up as his deputy later.

At any rate, so that's how we decided where we went on the Moon. The astronauts played a little part in it. Dave Scott played a lot in his part of why [we]—I guess went to the Apennine on [Apollo] 15, and David was very active with that. So that's how that part of the job was done.

To come back to Apollo 11, the instrumentation that was defined at the time was done by an outside scientific group that helped define what should go on ALSEP, the geophysical measurements and making sure that we did all that properly. Poor David and that drill. We had to drill a core, and, God bless him, I thought we were going to lose him. He tried to drill on 15. I remember at the time they were losing their electrolytes because of what was happening, and at night their heart rates went way down. Boy, he worked so hard with those suits trying to make that drill work on the surface of the Moon.

Back to 11. I'm sorry. I'm drifting out of some of these other things.

BUTLER: That's all right.

CALIO: So the Apollo Lunar Sample Analysis Planning Group put together the concept for ALSEP. We went out on a bid for that. I guess the contract was awarded to—God, come on, I can't remember the name. It'll pop into my head later on [Bendix]. Who went off to develop all the instrumentation and then put it all together. Jack Small was the program manager on the government side and did a great job in trying to bring it all together.

Now, we were supposed to put ALSEP out on Apollo 11. However, at that point in time, and as a matter of fact, [we had] only been training Neil and—excuse me for the names slipping out.

BUTLER: That's all right. Buzz [Edwin E.] Aldrin [Jr.]?

CALIO: Buzz Aldrin. Buzz, on what had to be done there, and also training Mike Collins as to what would be done on the orbital stuff, because the other thing we were talking about doing was actually putting instruments in the command module. Well, that didn't happen until very late. A lot of it happened in Skylab.

You were asking about the Skylab. Skylab we ended up putting some instruments that should have flown on Apollo, on Skylab, which turned out to be pretty good as far as some of experiments were concerned. We did some good work there.

At any rate, there was concern about the life support system, the inability of the life support system to regenerate itself in the time available for the crews, and weren't sure of the workload and how much BTUs [British Thermal Units] they'd be chewing up when they were actually doing their things on the Moon. So George finally made a decision that we were going to cut the stay time on the Moon shorter to make sure we had sufficient margin in the life support systems, the portable life support systems that the astronauts were carrying, which said ALSEP would not deploy, and that decision was made about a year before we were going to fly.

So George said to me, "Well, figure out what you can do with" I forget how much time we had on the Moon. That was the highest priority was time on the surface, surface time. That led the whole thing. So we came up with a notion of putting these lunar laser reflectors out, so that once those reflectors were put on the Moon then with their radio, with the laser beaming from the Earth, we could do some very important things relative to the Earth. As a matter of fact, we learned how much the San Andreas Fault was moving and things of that sort, because we had a good point out there we could laze off of.

That turned out to be a very successful experiment, very simple. The principal investigator for that was a fellow by the name of Carl Alley from the University of Maryland, and a fellow by the name of Fowler. Both, they were just so difficult to get along with. They were as eccentric as could be. But at any rate, we persevered and got it done.

Then there was a question of a few samples that Neil and Buzz could collect. That was going to become very important, because we never knew if we were ever going to get back again or not. So that sample mission was planned.

So the whole first mission was completely revamped, in that ALSEP was not put on board. Now, of course, that caused a lot of rumble in the outside community, because they were counting on getting this station in place. As a matter of fact, we were finally going to put a whole bunch of these out, so we could do a lot of geodynamic work on the Moon.

Am I doing what you wanted to do?

BUTLER: Absolutely. Doing well.

CALIO: We finally got over that, and that caused another investigation. I don't know how many investigations there were from the National Academy figuring out whether we were doing the right thing or not. Especially after Wilmut left, there was this great tension, because why did this outstanding scientist leave this center? He used to take the notion that

people in the center didn't even know how to spell science, let along carry it out. So, of course, that created all sorts of problems, too. So, at any rate, it cascaded.

We got ALSEP on board, I guess, on Apollo 12. Everything went pretty well there, except for Pete kicking the rocks. [Laughter]

Thirteen was the sad mission. As a matter of fact, when 13 was over, [Jim Lovell] came in as my deputy for space science in the directorate. He stayed there for about a year or so. Talk about a fish out of water. [Laughter] Poor Jim didn't quite know what to do with that job, but again, he was there to try to ameliorate some of the issues between the science and the flight crews and the like.

But Deke was always very supportive, tried to do what he could do, so Deke was never negative about the science aspects. Just different astronauts had different views of science and the like.

Let's see, 14, I don't think we had any difficulties, other than them playing golf on the Moon.

Fifteen got very, very much improved by Dave and what they did. As I say, he did an outstanding job on the Moon, and we were really concerned about his health at the time.

Sixteen, 17, Charlie Duke did a good job. He was well-trained. Of course, Senator [Harrison] Schmitt did his thing as well, being a trained geologist.

So that's about pretty much what happened on the missions. What am I missing?

Also brought up the point when McDivitt took over, things were just as good as ever. Jim was a very dedicated fellow, very bright, very capable, had the same discipline that George had, and kept it within the program, and he really continued the program. I guess there was one funny thing that happened in Apollo 11. Right before it happened, I mean, God, right before it happened, it was a nightmare. So many things had to be done, and we weren't sure they were done. Somebody brought up the notion that the pyrophoricity of the lunar surface once you expose it an atmosphere of oxygen. George called me in one day, and

I guess it was about three months before launch, and said, "This guy is bringing this up. You got to go do analysis with your community and tell me whether we're safe or not, okay? I can't ignore what this person is saying."

So off we went. [Laughter] Boy, I tell you, again people helped me tremendously, and so we did the final analysis to show that—we got some the best scientists in the community to do the analysis. We got it done and submitted it to George, which we said we didn't think there was a problem associated with it. So that took care of that. But, boy, try to get all that done while we were trying to change around the mission at the last minute and not screw up deal Neil and Buzz with what we were trying to do. Boy, they were two dedicated guys, all three, Mike Collins, too. I mean, they were really working all the time and really had their hearts in it.

Let's see, as far as Apollo is concerned.

BUTLER: Were the results from the missions what you expected?

CALIO: Yes. As a matter of fact, what we were concerned about—well, of course, understanding the age and the constituency of the Moon was one of the biggest issues, not that it was going to be made out of blue cheese, but try to figure out what all the structures were, what was granitic, what wasn't, what was volcanic, what wasn't. There were some of these concepts about how the impacts were formed and created the rays and indeed, the way they thought they were. They were, and we were able to date the Moon somewhere about four and a half million years. Things of that sort.

The question of lifeforms on the Moon. We had a team, Al Burlingame out of the University of California-Berkeley, were doing some of the experiments and microbiology associated with it, geomicrobiology, to determine whether there was any lifeforms that could be existing on the Moon, because everybody said there could be lifeforms there. At that

point in time, we didn't find any. Of course, the soil was very rich, so if you ever got it into an Earth atmosphere, it would flourish as far as the nutrients were concerned.

We had people from Zurich. Johannes Guiss from Berne, Switzerland, an outstanding solar physicist, he got to understand what the solar particle distribution was on the surface of the Moon and the impact of that. The sun and the Moon surface, let's see. Jerry [Wasserburg] and his geochemistry team were able to put together a structure of how the Moon was really built up. That was beyond my comprehension in some of this material. But a lot of good papers came out of it.

Of course, everybody was looking for a Nobel Prize. [Laughter] I think that was one of the driving forces in some of this contention. We had a thousand scientists on the program, outside scientists, over a hundred different universities worldwide. Trying to manage that was—well, that's where I got an honorary degree from Washington University at St. Louis, an honorary doctorate of science degree, because I was able to keep all these balls [in the air] at one time. I wouldn't take any nonsense from anybody, no matter whether they were a scientist or they were an engineer. I had a mission. As I said, I was very pragmatic. I wasn't as polished as George. George could do it with gentleness. I wasn't that way, unfortunately. So, but we got it done, and that was pretty important.

Let's see, what else? What other questions?

BUTLER: Were there any surprises in the results that you found?

CALIO: I'm trying to remember. The age was one of the things that was surprising, I think, and finding some of the rock types, the breccias and the orthocites, anorthocites on the Moon was interesting from a geological standpoint, because they're here on the Earth. So the question is how was the Moon formed and all that stuff. Brought up all those questions, and I don't think anybody's ever really answered that, except the way it's been explained in the

past. No lifeforms. That was a big thing, too. I guess the question of water on the Moon, was there any water on the Moon, and I guess they're convinced there is some, but it's a question of how do you mine it, because it's transformed in other things.

People talk about going back there, and what's the back side of the Moon look like. Probably no different than the front side of the Moon, but scientists, of course, want to know what's on the back side. The notion of putting a station up there or putting people up there to live on the Moon, I wouldn't want to do it. I don't say that as—I mean, I would love to have been an astronaut. I mean, I lived vicariously through them. But the notion of going up there, and I'm a pretty gutsy guy, to go up there and try something like that, live on the Moon, I don't know why you'd want to go do that. I guess there are people who would like to do that. It's a very hostile environment. The temperature changes from day to night are extreme. So it's nice to look at. Let's go someplace else.

Of course, when I came back and finished the—when Apollo and Spacelab was over, I was a little burned out, and I asked Chris if I could go to school. I always wanted to go to business school. So he said, "Go ahead and apply." So I applied to Stanford, [for] a [Sloan] fellowship, and they accepted me, and Chris sent me. So I went to school for a year after the Skylab. That was during the Apollo-Soyuz [Test Project] mission and the start-up of Shuttle. So I was very fortunate.

I thought I'd go back to the center when I got out of Stanford, I didn't. As a matter of fact, when I left—oh, gosh, what was his name, fellow that took over the directorate? He was an astronaut. He flew Skylab. Owen [K.] Garriott. Owen took over the directorate at the time. So I missed that interval U.S. Space Station. The Space Station, I wasn't involved. I was off at school.

As I say, Chris was very good to me. So was Bob Gilruth. But when Chris became center director, he was very good, very supportive. Of course, George [W. S. Abbey] was his aide at the time. At times George would get pretty unpopular with some of us in the

directorates, because George would come down. I guess one time, I don't know, he gave one of my people a hard time and chewed him out. I went up there and chewed George out, and five minutes later I was in Chris Kraft's office, and Chris sat me down in a chair and he says, "Young man, you do that one more time and you'll be out of this center." For one year—one year—it took me to win back the graces of Chris Kraft. [Laughter]

BUTLER: Well, at least you were able to.

CALIO: Well, yes, Chris was more forgiving, I think, than most people. …I don't think George has ever forgotten me about that, because George and I were always at it because of the positions we were in. As I said, I never tried to become diplomatic about how to work some of these problems. I was a bull in a china shop; I just put my head down and charged. George at the time may have been just about the same way. I'm sure George has mellowed in time, and certainly so have I.

But that was the only time that I can ever think of ever getting quite crosswise with the center management for the whole eight or nine years that I was there. They were always very, very supportive of what I was doing, realized the difficulties I was having with trying to balance the science and the engineering side. All of them were very helpful, Deke and George Low and Chris Kraft, the whole crowd.

BUTLER: Do you think that from the experiences you had in Apollo, trying, as you said, in balancing the science, the engineering, the operations, and that, of course, was throughout the Apollo Program, and Skylab was more science-oriented, do you think that future programs might experience similar difficulties, or did you kind of see NASA moving toward more of the—

CALIO: I saw a significant change. But as I say, I don't take credit for that. I think Dave Scott, if anybody, should be credited for that in that he, the position that he was in, was very influential in how he could have taken on that task. Okay? This is not being disrespectful to either one of them, as far as Pete Conrad and Alan Shepard were concerned, they were flight jocks. Okay? Their attitude was entirely different. "I'll fly this thing. We're going to make it work," and all that stuff, but Dave really took on the mantle, and he was instrumental in making that change take place with the flight crews, and I think later on with the Skylab and the Shuttle. Of course, where you have the mission specialist and the like, where you have an integrated scientist with the team you have a better interaction of science and engineering and it works much better. Yes, so the change was significant, very significant, by the end of Apollo and the beginning of Skylab. Very significant.

I don't want this to be published, but I'll make a comment about Apollo-Soyuz. That was a disaster as far as the science was concerned. I don't know how the science was even picked, but that wasn't the objective of the mission anyway. The objective of the mission was to have a U.S.-Soviet space venture, and I think from that standpoint it was successful. I guess General [Thomas] Stafford flew that. I guess Deke flew that too, didn't he? Yes.

BUTLER: Yes.

CALIO: So they didn't consider Apollo-Soyuz as far as science is concerned.

Now, the Shuttle is a horse of a different color. I didn't work on the Shuttle. I was away at school when it was being developed. When I was finished school, I thought I was going to go back to Houston at that point in time.

When Paul Gast died, I tried to hire Noel Hinners from Bellcomm to take him down to replace him [Gast], because my relationship with that team was very, very good, and I thought Noel was outstanding. It turned out Noel wanted to come down, but he finally said

no. Well, the reason he said no is because John Nagel, who was then the associate administrator for space science, told Noel he was going to bring him in as the head of space science. I didn't know that at the time. So Noel then became the Associate Administrator for Space Science at Headquarters.

So when I was finished with school and ready to go back to Houston, Noel said, "No, come on to Washington and be my deputy." So we switched the relationship we were originally going to have, and I did, and I came back to Headquarters, which I thought I would never do. I'm not a Headquarters type, didn't think I was. At any rate, I did come back as Noel's deputy for about a year, year and a half.

At that time Bob [Robert A.] Frosch was the administrator of NASA, and this was during the [James E.] Carter administration. Bob was very keen on expanding the science and applications part of NASA. So when Chuck [Charles W.] Matthews left, he asked me if I would become the Associate Administrator for Space Applications in Headquarters. That's when I took over that job, and that was remote-sensing communications, etc. That got my link to NOAA [National Oceanic and Atmospheric Administration], because in space applications, the director of that operation was responsible for making sure that all the spacecraft that were built for NOAA were managed in an appropriate way. My job was managing Tiros and the geostationary satellites that were being built through Goddard to NOAA. So that's how we finally slide into that.

So I became the Associate Administrator for Space Applications. Then when [Ronald R.] Reagan won, and Bob Frosch left, let's see, at the time, [Alan M.] Lovelace was the Deputy Administrator. He became the Acting Administrator, and I guess I was one of the most traveled [and] senior people in the general schedule. So I became the Acting Deputy Administrator of NASA during that period of time when the Shuttle was first launched, but I had very little to do with it.

Then with the changing of administrations, Lovelace was out, and Al really wanted to become Administrator of NASA, but, unfortunately, that didn't happen, because Jim [James M.] Beggs got it. I wanted to become the same thing, and I didn't get it either, as the deputy. Hans [M.] Mark came back and became the Deputy Administrator of NASA at the time.

Let's see, before I get into the rest of it. What else didn't I cover on the—oh, yes, there is an important aspect. You asked me what else we did besides lunar science. I inherited an Air Force when I was at Johnson [Space Center]. [Laughter] A ragtag group of people. Unbelievable. We ended up with a—excuse me—I guess there were two rifts I had with Gilruth. The second one was flying a U-2. Bob considered it a very unsafe aircraft. I had my little Air Force, I had a U-2, I had a WB-57 [*Canberra*] in that operation. We started a remote-sensing group. I was convinced that there could be some good mining of technology in the application of remote-sensing arena. How can we use space to remove the required data about the Earth and do something about Earth management and environmental sciences and the like?

So I got a lot of support from the center to do that. We used it as a ruse to train the astronauts. [Laughter] I'd take pictures of an aircraft, which they did, but I then had my own flying team, and so we developed all sorts of instruments that we flew on the WB-57, pretty high-altitude aircraft, and flew [on] the U-2. We had a [Lockheed] Constellation as well.

So we were really coming along with remote sensing, and as a matter of fact, we not only got into the instrument side, but we also got into the analysis side. I brought in the guy from Purdue. Golly, my memory's going. I can't remember his last name. His first name was Bob. He ran the remote-sensing group there for us, and he was trained at Purdue, and was a pretty smart guy.

We used to do the analysis of the data. What we were trying to do, the first experiments were trying to see if we could assess the agricultural production of wheat

worldwide, first building models of wheat production in the United States, and started to build computer models to be able to do this, and built up a whole remote-sensing team.

The CIA [Central Intelligence Agency] got intrigued by what we were doing, because they were doing it, too. They were using different techniques than we were using. I was convinced at the time that digital techniques were the only way to do it, and that this notion of photo and imagery analysis was okay for now, but that was going to be a technology that was going to pass us by. We really had to go digital. We had to build charged-coupled arrays, we had to be able to really separate out the different frequencies. If you're going to try to recognize something, there were only four ways you can recognize it: what's it look like now, what's its size, what's its color, and what's its polarization. If you can measure all those, then you should be able to tell what's on the surface of the Earth.

So I brought in another guy, from Willow Run, the University of Michigan, to work in that operation as well, and we were doing great. We were doing so well that NASA decided—well, it wasn't NASA again, it was the U.S. Geological Survey. U.S. Geological Survey thought they wanted to put up a remote-sensing satellite, an Earth remote-sensing satellite. So they went forward in one of their budgets to do that and OMB said, "No way. There's only one space agency and that's NASA. If you want one of those satellites, you're going to have to go to NASA to get it done." So that, of course, helped the U.S. Geological Survey again.

So they did, and then Chris got involved in this, son of a gun, and it was the right decision, but at the time I was really emotionally attached to it as well. The agency was going to decide who would build the first, it was called ERTS [Earth Resources Technology Satellite] at the time—George Low was here as a deputy, as the Deputy Administrator—which subsequently became LANDSAT.

I wanted to build it in Houston, and Chris said, "No deal. We're not going to focus on anything else but manned space flight down here."

I said, "Come on, we established this whole thing. We know what we're doing. We've got a good team. Let's do it."

"Nope."

They put it in Goddard Space Flight Center. Oh God, I was crushed. I really wanted to build that satellite and fly that mission. At any rate, that didn't happen. Consequently, Goddard, of course, has built up a great remote-sensing capability today. I think that—I don't know what's happening in Houston. I believe they've probably atrophied at this point in time, because there was no other support for it. But we established a very vital program for the agency I think that was important. We did a lot of the pioneering work. Talk about a ragtag team, flying these airplanes with these instruments hanging all over, it was really funny. God, and Gilruth used to say, "One of these days, Calio, you're going to kill those people." [Laughter]

BUTLER: Well, luckily nothing went wrong.

CALIO: No, we were very fortunate. Touch wood. We were very fortunate. So that was another thing I did there, and I enjoyed it as well. That's what really also got me into the applications bit when I came up here. Bob Frosch was familiar with what I had done in Houston, and knew I had a background in communications, remote sensing and the like, and so when the applications job came open, then he offered me that opportunity.

BUTLER: Before we go any further, if we could pause a moment to change our tape.

CALIO: Sure. Sure. Okay. [Brief Interruption]

Anything else to really talk about before I go into the Headquarters business? I was a thorn in Headquarters' side. In communicating with the scientific community, I had

developed a good relationship with them through the use of the Four Horsemen. So when we were trying to go out with notifications to the scientific community, people at Headquarters always screwed it up. [Laughter] I started in Headquarters and then went to Houston, and I think Noel did this as my penance, but I'd come back in Headquarters, because what I used to do is, I said, "Just do me a favor. We'll prepare all the documentation down here and get this situation straight and right. We'll leave the signature titles open, and we'll just send you all the letters you can send to the scientific community. We're not usurping your authority, but you can't keep screwing things up."

So Noel says, "Okay, you're that smart, you come on up here and straighten it out." So that's sort of another thing that was stuck under Noel's craw as far as he was tired of some of the people in the lunar program that worked for him doing battle with Calio, and so he got rid of that situation.

BUTLER: How was working at Headquarters in a science arena different than working, for example, at Johnson, at the Manned Spacecraft Center?

CALIO: Well, your role in Headquarters [is not] an implementing role, in a sense. The way it's structured has always been, I think, a difficult problem for NASA. What is the role of a program manager in Headquarters vis-a-vis a program manager in a field center? When I was up here as Deputy Administrator for a while, I tried to straighten that out. I tried to get rid of the role of program manager in Headquarters.

In Headquarters, the responsibility is to do the planning, in concert—in concert—with the folks in the field center, so that there is good integrity, whether it's scientific or engineering, integrity in the mission that's being proposed, and what we're asking the taxpayers to—what we want to build and why we want to build it. Then you've got to sell

that the agency, and you've got to sell it to OMB, and you've got to sell it to Congress. So that's the role of Headquarters.

The role of the field center then is to implement what has been approved. Now, what went in the front may not necessarily be what came out of the back, because of OMB's twisting and turning, and the agency, and Congress. So finally what you get, and the role of program manager in Headquarters, is to make sure the people in the field center understand what was approved in the budget, and what we were permitted to do and what we weren't permitted to do.

Now, you got different responses from different places. As far as Goddard Space Flight Center was concerned, what they proposed they were going to implement, the hell with what happened in the process. [Laughter] Okay? So that always became a very difficult task, what was the role of the manager in Headquarters vis-a-vis the manager in the program. That was tough.

At Johnson, it was quite different. They were pretty much aligned with our manned space flight operation here in Headquarters, so they understood that what was approved by Congress is what they implemented. So you had that little difference that was occurring between the science organizations and the engineering organizations.

But I pretty much defined the role of Headquarters, and sometimes they aren't responsible for the implementation of the program. They're responsible for making sure that the program is carried out according to the specifications approved by the Congress and implemented into the program plans. It's up to the center to make sure that happens. And there's where the conflict comes, if there's a deviation from that, who's responsible and where does it lie ultimately. So that's really the difference. So in Headquarters, you paper-push. I mean this in the right way. And at the field center and you contractor-push.

Now, that brings up another interesting point. In Houston, I've always felt, when I was on the inside—I don't know if that's true on the outside or not—we were an integrated

team between the civil service and the contractors. It wasn't, "I'm telling you what to do and I'm responsible," etc. The arms and legs of the implementation of whatever was done was done by both civil service and contractor employees.

I think the greatest advocate of that was Chris Kraft. Flight ops at the time was you couldn't tell, we all wore the same badges. I don't know what they do now. As a matter of fact, George Low, when he was down there, got rid of the color code. We used to have yellows and greens and blues and reds to tell everybody—now everybody has the same red badge. But at different centers it's different, it's handled differently.

An interesting aspect about it when I got into industry, I went back and worked for NASA. We won a major program there, and it's impossible to work with these people. I mean, they're constantly expecting you to do things that's not in the contract. "We've changed the requirements. We want you to jump in and fix them," etc. I used to ask the people that I worked with, "My God, was I that bad when I was in the government? [Laughter] These people should be taken out in the back lot and shot."

But it's just interesting, I just never felt that way in Houston. Now, maybe I was kidding myself. Philco was the name of the company, I think, that built—Bendix, I'm sorry. Bendix [Corporation] built ALSEP. I always felt we were in this together, you know, we sink or swim together. It was not a question of—but you never were in there doing it. I mean, the boulder was not on your back, in one respect. Now, when I worked for industry, the boulder's there. I mean, you have to deliver the product. You're not shuffling papers, pal, you've got to make sure something happens at the end.

I don't think in some places in NASA that is respected with the contractors. I think George understood it: with Rockwell and Grumman, we were a team. I think Chris did with IBM and some of the other folks there, PHILCO. You know, they're the people that taught me, so I had that instilled in my head. I think it's different [all] over NASA, how that's handled, and the contractor relationships.

The difference between NOAA and NASA, NASA provides the tools in which to do the job. NOAA does the actual application of those tools to develop the finished product, helpful to the citizens, to the country, i.e., weather forecasts, nautical maps, fishery management and the like. In NOAA there is not the relationship with the contractors that I saw at the Johnson Space Center. It's more like what you have at Goddard. That's not very satisfying in some respects. The contractor's there to do the bidding, and the government can change its mind.

NOAA had the attitude, I think Jim [Dr. D. James Baker], the current administrator, has changed it somewhat, that they pretty much worked in an ivory tower. They were in the university complex. They weren't working for the government, when you look at some of the people who were developing some of the early forecast models for weather and for the early ocean circulation models for the ocean. So, to me, those are the differences.

The agencies and within the agencies, the difference is between the various components of the civil servants and the contract employees. None of this would work if it wasn't for the contract employees. I'm not saying that because you're a contract employee. I mean, that's the way it is. You don't feel the burden until you have to—you, responsible for delivering that product. I talked about George Low holding me responsible for standing up as to whether we go or not. I knew it wasn't me; it was that contractor team, had they done their job and we worked collectively to get that job done. Now, when I went to industry, boy, that monkey's right on your back. Nobody's going to take it off. So, to me, those are the differences that exist.

I don't know why I went off on that tangent, but at any rate—

BUTLER: How did the opportunity arise for you to move over to NOAA? You had mentioned that at Headquarters you were working on meteorological—

CALIO: When I was made the Associate Administrator for Applications, there my responsibility then was to develop remote-sensing space communications. At that time we were doing also processing in space, what kind of experiments are you uniquely qualified to do in space, like growing crystals and some of the other things, not life sciences, but geophysical and geochemical processes that could take place that would be enhanced because you are in a zero-G environment or the environment of space.

I was responsible for the technology transfer program, too, to be sure we documented what was applicable in space. In those days, boy, we were really, really struggling to try to get a brass ring. Everybody was looking for the follow-on to an equivalent of the COMSAT, the notion of communication, space communication satellites. We were looking for it in remote sensing and it wasn't there, and I still don't think it's there. People think it is, but it isn't anymore.

So I was responsible for providing, through NASA to NOAA, the weather satellites, both the geostationary satellites and the polar orbiters. When I got in there, we were just in a process of a block change on design. It was RCA [Radio Corporation of America] who was building it. Okay? We went through that block change fairly effectively. Tiros and the Air Force meteorological satellites were both built by RCA at the time. So there was a slight difference in those two spacecraft, and up until three years ago, there were certainly two separate programs, DMSP [Defense Meteorological Satellite Program] and NOAA's program. They finally pulled them together. I guess they worked on about three different task groups as to whether they should be pulled together or not. I guess I was the rebel in the outfit. I couldn't see, because I figured once NOAA had agreed that it could be done, the Air Force would take over, and that's exactly what has happened, and that's where they are as far as the satellite development is concerned, because there's a new program called NPOESS [National Polar-orbiting Operational Environmental Satellite System]. It's a joint program between the Air Force and NOAA and NASA.

As it stands now, the program manager sits in NOAA. Boy, it's a small world. I ended up working for him as a contractor when he was in the Air Force. He's a retired colonel from the Air Force. So he's now in NOAA running the development of that new polar orbiting satellite systems for both the military and civil use.

Let's see. I told you I wanted to become the Administrator in NASA. I really wanted that in the worst way. It was good that I didn't get it, because I couldn't have done some of the things that the Reagan administration wanted done in the reduction of government costs. I did believe in that very strongly. But I had worked in every element of NASA you could think of in the eighteen years I was there, and so I knew an awful lot of people. I don't think I could have done what had to be done at the time. I think they went overboard, but I mean, we really had to focus on what was absolutely necessary and what wasn't, and take out the things that were not necessary, because there was a lot of pork in some of that stuff, as well, and the same is true for NOAA…

…But at any rate, we had a joint program between Navy, NOAA, and NASA, which I was really gung-ho about. We got it into the first year. Admiral Williams, who was a three-star, was the guy in the Navy for us. Johnson, Dave Johnson, was the NOAA guy, and I was the NASA guy. We got it started and I thought it's exactly what the Navy needs. When they're going out on a field maneuver or things of this sort, you want to know what the weather's going to do, because you may run into some pretty difficult situations you hadn't anticipated unless you had some of these measurements.

Well, it got curtailed in the Reagan administration. The first year out, it got bumped because of the fellow who became the Secretary of the Navy [John Lehman], was trying to build a 600-ship Navy, and money he was spending on his base, as far as he was concerned, wasn't necessary, so he took it out.

But during that process for the year or two before that, I mean, those three agencies became very close. So when the administrator of NOAA was then picked, Dave Johnson

suggested why didn't he get me with my space background into NOAA as the deputy to the administrator. I didn't know Secretary [Malcom] Baldrige at the time at all. I've always been a Republican all my life, and I had been striking for a job in the Reagan administration, but I was going after NASA.

But at that time I'd also worked a lot with Hughes. Bud Wheelon, who was then the president and chairman of Hughes Aircraft Company, who sat on the Defense Science Board, put in my name for C^3I [Command, Control, Communications, and Intelligence] because of all the stuff I was doing in remote sensing. I never finished that story. That was an interesting story. When I turned him down, the White House actually asked me if I would consider the job, and I said, "No, I don't want to go there. I want to stay with NASA." Well, of course, that didn't happen.

But then when NOAA came along, and I said, well, I wasn't going to get C^3I, I turned down C^3 I. I had gotten the White House angry with me. So, yes, okay, I'd like to become deputy, which was a political appointment at the time. So I went in and talked to John Byrne, who was the administrator, and he said, "Yes, you're the kind of guy we need," and so he hired me. I didn't know Secretary Baldrige at all. As it turned out over the years I developed a very close relationship with him, to the point we were like two brothers. His death just blew me away.

So I went to work as a deputy, and again it was back to the Wilmut Hess-Tony Calio relationship in NASA. John Byrne was an oceanographer from the University of Oregon. So he had all the scientific connections. I didn't have any scientific connections. I was the implementer. So that's the way we started off. After three years, John didn't want to stay anymore, and I did, and the Secretary did want me to stay, so he asked me to stay on.

I wanted to quit after the first term, but there was a big conflict going on at the time on the commercialization of LANDSAT. Unfortunately, the Deputy Secretary of Commerce had some discussions with COMSAT management about where this program should go, and

it was supposed to go on a competitive basis. That got into the hands of some people in Congress, and so Secretary Baldrige really got under the gun about what was going on here, what was this all about, because Secretary Baldrige was a businessman all of his life, in manufacturing. He was taught as a schoolteacher. He started out as a schoolteacher, and so he knew nothing about satellites or things of that sort.

So he asked me to stay on beyond the second term of Reagan, because we were on the Hill [Capitol Hill, Washington DC] and we were under fire from the Congress on what was happening here, what was the shady business going on within the department. So I stayed with the Secretary and that was one of the things, of course, that bonded us, was I was there when he needed me, and had the kind of knowledge that was needed at that point in time. We got through that very well. As a result, we established some very good relationships with some people on the Hill.

Of course, the Secretary was a master in presentation. I mean, he was slow, deliberate, conscious, and a very capable salesman. He was very good. And he was also a great teacher. So [I] ended up staying for seven years until he died, or was killed in a rodeo. He was a commercial rodeo rider.

That's another funny anecdote. To be a commercial rodeo rider, you have to win prize money every month. You can't just go along like a—and so he was a calf roper, and every Wednesday afternoon he had a friend that had a ranch over here in Middleburg, and so he used to call me Wednesday morning and said—when I first got there, I didn't know what to do. He used to call me on Wednesday morning and says, "Well, you're running the weather service. What the hell's the weather going to be in Middleburg this afternoon?" [Laughter]

"I don't know what the weather's going to be."

"Well, find out." [Laughter]

So finally we got that straightened out, too. I had to report every Wednesday morning to tell him what the weather—what was going to happen in Middleburg. So he used to go over there and practice. He was a real cowboy. He used to smoke Marlboro cigarettes. He had a basket behind him, and he used to have these long matches. So he used to take out his Marlboro and strike his match and throw it behind him, hit that basket every time. [Laughter] He used to wear hand-tooled belts, and on formal occasions he had silver-tipped alligator boots he'd wear. [Laughter] Oh, he was something.

Actually, he threw a, for the President—excuse me. Before he came in as Secretary of Commerce, he was the chairman and CEO of Scoville Manufacturing Company. So he was a man who had made his fortune in industry, and he loved it. He loved coming in. At that point he used to ask me, "Calio, you did it wrong. Why are you working for a living in the government? You can't do that. You've got to make your money first, and then come into the government and enjoy it."

At any rate, he threw a big rodeo for everybody in the department and the White House, out here at the Capital Center one day, which he paid for, and it was funny to watch him out there roping and stuff. He was great.

The reason it was so devastating to me was the relationship that we had built. It was a very strange one, in a sense that we never really spoke much to each other, but we communicated immediately. I mean, when he just said a few words to me as to what had to be done, I knew what had to be done, and the same way when I responded to him. He liked that, because he had a very interesting policy. If you couldn't write a letter on one page on what had to be done, he didn't want it. He didn't want a volume. "Tell me what the problem is, tell me what you're doing to fix it, and then if I want anything more information and background, I'll ask you." And when you spoke to him, you had to do the same thing. In the three minutes you had to tell him what the whole situation was and what you were going to

do about it. It was magnificent. It was unbelievable. He really did a great job in making that really work.

So we had developed a very, very good relationship. We didn't have to spend much time with each other, but when we did, it was very productive. The thing that was so devastating to me is, we had a meeting on the Hill, we had to go talk to a couple of congressmen—a couple of senators rather, the day before he was—well, it was a Friday he was leaving for San Francisco, where he was going to go into a rodeo contest. So we drove up together and we spoke to the senators and we came out. It was a very successful meeting. He says, "Well, just go back and write up a short note about it, and then when I come back on Tuesday, we'll talk about what the next steps ought to be."

When he was out there in San Francisco, a horse bucked on him. He used to wear these silver buckles, and the horse came back and the horn of the saddle went into his stomach and crushed his internals, and he died. [Calio sighs.] At any rate—

BUTLER: Well, it's fortunate that you were able to have such a good relationship with him.

CALIO: Oh, yes. He was super. I mean, here's a good example. I was running roughly half the agency. NOAA was about half the size of the Department of Commerce. When I finally got in there as Administrator and he brought me up to Under Secretary, as a matter of fact, made that an Under Secretary post, I asked him why he'd done it. He said, "I don't know anything about your business. You take care of that half of the department, and I'll take care of my half." That's the way we operated.

A good example is there was something that came up that Senator [Robert William] Packwood wanted, and so he sent me up to cut this deal, cut a deal with Packwood, which I did. Then I came back and wrote up what was I was doing and gave him a few-minute pitch of what was happening, and he was okay. Then I had to go off—in NOAA, you're involved

in a lot of environmental things. Did an awful lot of traveling in NOAA and these ocean dumping and just a whole bunch of things. I was off in Paris on one of these things shortly thereafter. In the middle of the night the phone's ringing, and it's the Secretary. He says, "Well, Packwood wants to renege on the deal. This is what he wants to do. Let's talk this through." So we talked it through and what we ought to do, and he went ahead and did it. Now, here's the Secretary of a Department. I'm just one of the weenies down there. To reach down and to get my views before he made a decision, I mean, how many guys would do that? Their egos would get in the way. He was superb. Absolutely superb. An unbelievable man. And he was a schoolteacher.

I finally got him to sail on boats, too. It was an interesting thing. He served in World War II, he served in the Pacific, and he was a shave-tail second lieutenant. When they shipped him over to the Pacific, he was in the hull of a boat with his company of men, and they had some terrible seas and he got so sick, it was unbelievable. So when they had the big deal with the Statute of Liberty [200 year celebration] up there, the Navy had asked me if we knew the depths of the water around there, and so if we would organize how all the big ships would come in, because the *Kennedy* came in and the equivalent French carrier came in and all of that. So I was involved.

So we had one of our biggest ships out there as well, and so I said to the Secretary, "Why don't you come on board and spend a couple days with us." "Not me. I'm never going back on another ship." [Laughter] Well, he did it. I got to his wife, and she wanted to do it, and so she talked him into it and he did it. He had a ball. He had an absolute ball. But he didn't go anywhere without that life vest. [Laughter] It was funny to watch him.

We had these little water taxis that would take you from boat to boat, which he loved. He was asked to go over to the *Kennedy*, and we got him over there, and so that was a lot of fun.

Okay. Let me come on back. Went way afield on that one. Where are we?

BUTLER: Oh, no, it's good to hear about the different people, and it's interesting how so many people have such different backgrounds.

CALIO: Oh, I've been an awfully lucky fellow. I've had great bosses. It sure makes life a lot different.

BUTLER: It certainly does.

CALIO: They have treated me well, and I've developed good relationships with them, even though I've been, as you probably have heard, a pretty rough character to some of my people. When I told you about that, nobody is in the middle of the road, [they] either love me or hate me. Okay.

So we got in to NOAA because of my background in the past in developing weather satellites. Then once I was in NOAA, then I worked there, worked in the flight projects mostly. Then when John left, John Byrne left, the administrator, he became president of Oregon State University, and I was moved up to become the administrator.

I loved working for NOAA in the sense that they cover everything. They go from the bottom of the sea to the stars in the skies and the weather. It was a very fascinating job. The Weather Service was fun to work with. The Weather Service suffers from a very unique situation with respect to them and contractors. [They're going to] do everything themselves. Everything. One of the things that I did in concert with the Director of the Weather Service was to get the Weather Service to move to a position where their job was to define what it is they had to measure and developed predictive tools to be able to give the country the assessments of what the weather was going to do. They didn't have to be communication experts. They didn't have to be satellite experts. They didn't have to be all this in the chain.

So I finally got them to the point where we should be hiring meteorologists and nothing more. We don't need communication technicians. We don't need communication. Buy that as a service from contractors.

It took a while, but we finally did that. So now they are focused on meteorologists, with one exception, and that exception is a reasonable one. They have a geodynamics laboratory at Princeton University where they develop the new models for weather forecasting. So they're right on the forefront of computer technology and how you build these seventeen-layer models. [Laughter]

But at any rate, I think weather forecasting has improved in the last decade. I think in some respects with Dick Halgren, who then the Director of the Weather Service, and some of his people and ourselves, we got the Weather Service turned inside out to they're doing what they're supposed to be doing. They're developing predictive tools and predicting weather, and not worried about how the product gets to them or gets out of there or anything of that sort. So we have contractors do that. So that was a major step forward.

We also developed new techniques like the NEXRAD radars. Some of the radar systems the Weather Service had were archaic. They were 1957 radar systems and they were coming apart at the seams. So getting new radars in place was important, and the other thing that was important, [although] a little late, how do you integrate all this together so that all this information can be digested by the meteorologists and productive and put into productive use? Satellite data, ground data, ocean data, and all that stuff. So we developed the what was called AWIPS 90, late, but it's there. Now it's working, and you can see the results of it.

Another thing I was struck with was, why do we only have a three-day forecast? Why couldn't we have a five-day forecast? Why couldn't we understand climatology and what was happening over the long haul with respect to the Earth? I mean, everybody's talking about the greenhouse effects and all these things. Well, what's their fundamental basis for all this? I mean, some guys in our research labs say, hey, the Nile overflowed every

three or four hundred [years], I forget what it was, you know, so you always had some of this stuff reoccurring. The difficulty is, we didn't have good scientific data that spanned the time that we were looking at with respect to climatic forecasts.

So I got involved in that and trying to push the climatic research program, which I did. We had excellent people both inside the government and outside, contractors and in the universities, which got extended into a large oceans program, because the ocean is the storehouse of a battery for all this energy. So how does this transfer take place between the atmosphere and the oceans? Why does the El Niño take place and all of that? All that was just being uncovered at that point in time. What was happening to the ozone layers and Antarctica and things of that sort?

So I guess one of the things that I looked to [is], I was instrumental, just a catalyst for some of these things happening, like being able to go for longer-range forecasts and better understanding of what's happening in the air-sea interaction, what's happening in the oceans, etc.

So that was pretty fascinating. I got involved in fisheries. Didn't know a doggone thing about them. Fishery management is a very difficult task in this country with respect to the laws that are in place, and how do we protect the fish that is a renewable resource? How do we protect the U.S. fishermen? How do we manage the relationships internationally with some of the people who like to fish in U.S. waters? The richest waters, as far as fishery is concerned, is off of the Alaska coast.

Consequently, I got very close to Senator [Theodore F. "Ted"] Stevens, because the Japanese and the Russians and a few other countries were fishing up there. Their techniques are a little different than some of the things we do in the United States. They have long-line techniques, which means those lines that are a mile long or so, get broken, all these hooks, of course, get caught by the fish in a nonproductive way, etc., etc., etc., and so you have all sorts

of things happening with respect to fisheries management that takes place that the general public doesn't really know much about, but the fishing industry does.

We rely very heavily on the fishing industry, and it's a tough business. A tough business. We've fished out the North Atlantic, not just [due to] U.S. fisheries. It's very sad what has happened [there] with respect to swordfish and a few other of large game fish that we used to be able to catch there.

The tuna industry. I used to keep a lot of lawyers busy when I was in NOAA, because I took a real interest in the fisheries activity. I guess one of the reasons was when I was a little boy, I actually used to go out on the fishing boats out of Sea Isle City, New Jersey and fished on those boats as a commercial fisherman. The local fishermen—my uncle took care of me. So I took an interest in the fisheries.

When tuna fishermen, they sight on the spinner dolphin. Spinner dolphin are on the surface. Right underneath them are the tuna. So there's restriction about taking dolphin. So when I was in there, a couple times we closed up a tuna fishery, just because they were taking too many dolphin. And how did I know that? We put observers on the ships, etc. So in some respects we had to strike a balance between protecting the environment, protecting the dolphin, as well as catching tuna.

And the fishing in Alaska is a nightmare, because the Japanese and the Russians at the time would fish in U.S. waters without permits. I actually used to go out on some of the flights to oversee that, and that was an exciting part. The Coast Guard flies the Aleutian Chain all the time and patrols that for the fishery management activities, and so I used to go out on a couple of those flights.

NOAA also provides landing charts for the aircraft. They shoot all of the airports for clearances and things of that sort. So that was another thing that was a lot of fun. We used to go out with them while they were taking the data for putting together the aeronautical

charts, and actually went up to Alaska and landed on the icebergs and everything else up there.

NOAA was a much more rewarding job for the administrator than I think NASA is for the administrator, because you have to live vicariously through everybody else. This way you could go experience the things yourself. [Laughter]

Then the satellite business in NOAA was changing, too. The big problem—you asked what was the challenges. The challenges were the battle of the budget, making sure what you do is an important thing to do and the right thing to do, and then to make sure you get the right amount of the funds to do it.

I think [Daniel S.] Goldin [NASA Headquarter's Administrator] has cut back too far. I was really pretty happy to see him go into that job, because finally here was somebody who was coming in who had business experience and had the monkey on their back, had to deliver spacecraft, etc., and he would know and understand what that part of the business was like. But, boy, he's made a mess of the place. You know this cheaper, faster, better, etc., has really come back to haunt him, because I think he really cut in too deep on what had to be done, and cut back too sharply. That's unfortunate, because I think that NASA has suffered because of it.

I'm sure when you finally get to Chris, he could wax most philosophically on that. Because I had dinner with him, oh, a couple years ago, and he said then, "Jeez, Tony, don't—when NASA was formed, it was a reincarnation of the aviation industry from NACA, and out of it sprung a new agency called NASA. It had a new vitality." And he thinks the same thing has to happen to NASA today; it has to be reborn into something else. I guess he has some very unique notions about that. I was not in at the very, very beginning, from the '58 to '63 time frame. Chris was, so he saw that formation. So when you get to talk to him, see if you can get him to give you some of his ideas and philosophy.

BUTLER: We'll do that.

CALIO: Okay. What else did we not cover?

BUTLER: Well, looking back over your time with NASA and with NOAA, was there one thing in particular that sticks out in mind, like a most significant moment or discovery or challenge in the space science area for you during that time?

CALIO: No, I think I pretty much talked [to it:] the notion of bringing engineers and scientists together was a very vital part. I think NASA's gone a long way to bridge that gap. I think in the future, at least on the manned side, I think we shouldn't see those problems develop. You just reminded me of something. I don't know whether I want to talk about it or not.

CALIO: I may edit this out.

BUTLER: That's fine.

CALIO: I guess I have a philosophical difference about the development of the Space Station. I guess this was developed between myself and Secretary Baldrige. We were convinced—we convinced ourselves, put it that way, that if this country were willing to go forward with the Space Station, that it could be supported by private industry, as long as there was an anchor attendant. If that anchor attendant was NASA, then we could make it happen. We looked forward to industry stepping up and doing it in consortium, several companies, with the understanding that in time, this anchor attendant would be buying space to do the kinds of things—the development that's necessary. NASA's a development organization, okay, and it

should stay there, and it should pioneer new ways of doing things from an engineering standpoint and manned missions, as well. I mean, the astronauts ought to be—the guys who fly it ought to be a mixture of government and private contractor people.

When the notion of the Space Station was brought forward by Jim Beggs to the president at the Cabinet meeting, I was there with Secretary Baldrige, and we disagreed with Jim Beggs. We thought putting it forward as a NASA program would—NASA wasn't really building the Space Station anyway—was really diverting NASA's thoughts and direction and energy, and that we really ought to sell it as a combined consortium and let industry step forward and build it, and we'd buy time and space on it.

We actually made that pitch to President Reagan. I did it for Secretary Baldrige. It wasn't my idea. It was Secretary Baldrige's, because he was also convinced that space is here to stay. Man in space is here to stay. We ought to look at space as a commercial venture and not the government venture. He convinced me that that was the right thing to do. [Laughter] So I put the pitch together and made the pitch to the domestic council. I went down in flames.

At the time, the President's science advisor was—I can't remember his name [Jay Keyworth]. He was also at the same place with us. But people had already gotten to the President, as far as Jim Beggs was concerned. The President approved. We move forward on the Space Station. Because I was also concerned that what the Space Station was, from an internal NASA standpoint, what the Space Station was going to do is what it did. It's taken all the money and more, than what the original budgets were.

I guess I learned that on Apollo. I mean, George [Low] was one of the most tight-fisted fellows I ever met in my life. He got blood out of a stone. To try to keep those costs on Apollo within range was a very difficult task. The Space Station, the way it was structured, with all the people, who's responsible, what belly button do you push and say this guy is responsible for making the thing work, wasn't going to happen.

At any rate, it [the Space Station] got approved. I think that was a major mistake, and it has taken a lot money from the other parts of the agency. I'm not saying manned space flights should be done away with in that process, not at all. They should be a part of that consortium. But [we're] not there, and I don't know what's going to happen at this point in time. I really don't.

I guess I was still in Houston at the time when Apollo was coming to an end, and I guess it was Chris that said it to me, we were talking about going to Mars, and he said, "We're not going to have any technical problems in going to Mars. The question is putting it all together and make it happen as a concept that this is what we're going to do. The technology is not going to stop us."

We used to have a saying that we used to overestimate how fast we could do things in the short term, but underestimate how quickly we could get to something in the long term. I think Chris was absolutely right. I mean, we can do all these things. The question is, we've got to have the philosophical background and the political clout to make some of these things happen. Technology is not the stumbling block. So not that we went down in flames. That doesn't bother me so much as I think it was the wrong thing as far as the country was concerned, and the way it was put together. Boy, we're probably going to spend a hell of a lot more money this way than if we had given it to industry to develop, and NASA become a major tenant. There would have been other government agencies who would also become tenants.

Talk about our part in the agricultural assessments. When I was down at Houston, we had our techniques for doing agricultural assessments. CIA had theirs. I was convinced we could do it with computers and with physical techniques, and not through just photo interpretation, etc., that ultimately we would be better with man out of the loop as far as an interpreter, but more in the loop as far as an analyst is concerned.

Now, what is all this mean? We did our work in concert with the CIA. At the time we stumbled a lot, because the computing power that we had was terrible. I mean, we flew Apollo. That computer in there has more power than what we flew in Apollo. So at the time it didn't work out that well. But now with telecommunications and computer capabilities that exist today, it's a cinch to do it digitally. Everything is done digitally. That's the right way to go. But at the time, we stubbed our toe because we were not as good at using computer techniques for doing crop forecasts from the remote-sensing data than the CIA was in using their photo interpretation techniques.

So there's another little anecdote that goes into all this, because I think if a space station ultimately—CIA would become a very, or the other parts, the intelligence department, the intelligence people would become a very important part of a space station. That gets us into a whole other arena that I didn't want to talk about. But I think it's vital, it's important to get some of this stuff out on the table, so that we know when somebody's doing something to us, rather than doing it in a spooky way.

Okay. Let's see. You asked me a question and I went off on that tangent.

BUTLER: That's all right. I think we've covered most everything I have. I was going to ask Kevin if he had any questions.

RUSNAK: Yes, I guess I did have a couple, just real quick ones.

CALIO: Sure.

RUSNAK: Going back to the beginning, I was curious to hear a little bit more about your work on the first nuclear reactor when you were working for Westinghouse.

CALIO: Let's see. When I got out of the Army, I applied for a job at Westinghouse, and I was put in a team that had just finished the development of the second core for the Nautilus submarine. Okay? So these were a bunch of engineers, and my job with the land-based station was with a very specific group. We had the engineers who designed the land-based power station, but there were no rules in engineering to tell them how to design one. We had the theoreticians over here on the other side who could tell us all about nuclear physics and how chain reactions work, etc., but we had no link between the two. And our team was set up to be that link.

We would go to the scientific group—that's where I got started in all this crazy stuff. We would go to the scientific people, and they would tell us what parameters we should be measuring, what we should be looking for, etc. Then we would go and work with the engineering group. My first tasks were with a critical facility, which was a full-scale mock-up of a nuclear power station. We used to map the neutron densities that would take place through the reactor and where the hot spots were, where they weren't, where did we have to cool down the nuclear reactions and the like, and how do we do that? We used to put these wands in the core. They were cadmium or boron or things of that sort, so they would become activated. When the nuclear reactor would run, this was a power-type full mock-up, but we never took it to power, we just took it to zero power.

Then after the run was over, we'd go in with Scott air packs and all those crazy things. I've had four children since I worked there, and they're beautiful children. We had no idea what we were going to do to ourselves, but we used to climb into these things. They used to keep good tabs on us as far as blood counts and all those things are concerned. They had some notions as to what the medical plateaus were that we could reach, as far as radiation doses were concerned.

So we pulled these wands, these foils out, and measured them and so we were able to map the neutron density in the cores, so we could then take this data back to the theoreticians

and ask them whether this was the right thing or not. So we would analyze the performance of the reactors based upon the theory that was being generated by the scientific group. We were constantly bringing new information to the engineering people as they were developing, they continued developing this station.

So we were writing what we called the PRW cookbook, pressurized water reactor cookbook. So we were writing a book on how you build the nuclear reactor and [what] all the criteria and the parameters were associated with it. So it was a great team to work with. I mean, we got to work both sides.

As a matter of fact, we finally got the configuration that we were going to ultimately use. Duquesne Power and Light was the electric company that was going to operate this. The overseer of the whole operation was General [Hyman] Rickover on the Naval Reactors Board. We were responsible for the start-up of that nuclear reactor and the training of all the Duquesne Power and Light people.

I was selected to ride shotgun when we brought the fuel elements from—we built them in West Mifflin, part of Pittsburgh which is just south of—this is in Allegheny County. But to get up to Shippingport it was about 150 miles, so we had to transport all these fuel elements up there, and the scientists had to go with every convoy, because if there were an accident and a truck would fall into a lake or a pond, you could set off a reaction, because these were water-cooler reactors, and they slowed the neutrons down by the water. So I was selected as riding shotgun on these convoys, was one of my first tasks after we were finishing the analysis.

Then I was actually on the start-up team. I was just one of the team members that worked with the Naval Reactor Boards who were the "Rickover Rockets," we used to call them. They were Navy officers who were going into the nuclear sub groups. They would oversee the whole government operation, when we were training Duquesne Power and Light Company.

So the interesting part about it is, we had worked all the critical facilities. I mean, we worked the full-scale mock-ups, so how you pull the rods out of the core, and how you get the core started up, and what happens when you get a hot spot, and things of that sort. We knew what to do. Not by any other means than by empirical techniques. We learned the bad way, unfortunately.

So we trained these crews. It was interesting, because at that point in time then we had to sit behind the operator and, boy, that was one of the most difficult things we ever did, was sit behind the operator as they were doing something. We had to tell the Naval Reactor Board person to tell the Duquesne Light and Power person what to do. [Laughter] That what they were doing was wrong. Oh, God, that was frustrating, but we had to work through that chain.

It was a lot of fun. As a matter of fact, I was in graduate school at the time at Carnegie Tech. I had to drop out of graduate school, because up in Shippingport in the wintertime it snows a lot, so there were a lot of times we never got out of the facility. So I ended up staying there six or seven, eight days at a time. But I was young and fearless and it was great.

We brought it up the power. As a matter of fact, they've had three or four cores put in it. I still believe it was the right thing to do. I think we have to conserve our—find new ways for generating electricity and power. We are a great consumer of electricity. Nuclear power is one of the ways, not the only, for augmenting fossil fuels and the like. I know it's a great controversy. I was on the other end of it when I got into NOAA, what do you do with the waste material, deep-sea bed disposal and things of that sort. I was on a U.N. [United Nations] commission to figure out how we handle ocean dumping of nuclear materials and the like. It was interesting to be on that side of it.

In the early days, we also had to write the safeguard manuals for how do you safely operate a reactor. Westinghouse was very responsible. I take my hat off to them at that time for how they handled it. We did everything very conservatively.

Oh, God, this brings back memories. [Laughter] One day Rickover was coming through, and I was working at my desk. I forget what I was calculating. At those times we didn't have computers yet, and so I was doing all these transport calculations using a Freidan calculator, and sticking a number of coefficients into my equations and stuff. I was busy working. I didn't even see the entourage walk in. Rickover walked over to me and bent over in my ear and said, "Young man, do you know what you're doing?" I think there was a wet spot on the seat when I stood up and answered him. [Laughter] Jeez. Oh, God. [Laughter] That was unbelievable. So that was a lot of fun.

Our first computer was a 650, IBM 650, and, we thought. manna from heaven. By golly, get rid of these Marchands and Freidans. Freidans made calculators. We actually had a machine that would do all the computations for us. Well, that was another whole learning experience, because there'd be nothing worse than you work all day getting the run ready that you'd submit to the batch processing that they run at night, and you come back and find out the next morning it was dumped in five minutes because of an input error. Oh, God. There was a whole day's worth of work that was lost because we screwed up on how to use the computer. So then we learned. I learned about computers.

We had the 650 behind that. The 750, rather; 650, 704. Then we got into all the 9600s and everything else. They sent me off to computer school for a while to learn how to program these things. So that was another interesting experience.

Unfortunately, that whole area went south because of the concern that people had about nuclear power. Because the next project that I was supposed to work on was called the A1W, and that was the first nuclear aircraft-powered carrier that we were going to develop. That was a different kind of a reactor. I was really enthusiastic about it.

What we developed were the pressurized water reactors. We used to work with super-heated steam to go into the turbines and the like, but we used to super-heat the water in a nuclear reactor, okay, through the neutron interactions. That was a little dangerous in a sense that not only did you have your nuclear problem, but you also had eye-pressure problems. We used to work at 500 psi [pounds per square inch], so folks were pretty nervous about that, especially in a submarine.

So we were going to what was called a pebble bed, where we'd actually have a boiler, so it wouldn't operate any higher than 212 degrees, but you'd transfer the energy in different ways on that. So I was really excited about doing that. Then the whole project went south. Government cut out the money. The Naval Reactor Board didn't get the money, and I thought, "Jeez, what did I do? I picked the wrong field." So then I went back and thought, "Well, what do I do with this?"

At that point in time is when, and I had mentioned earlier, I'd worked in Fort Dietrick in [their] biological warfare. That was a great experience, too. What our job was at that point in time—I'm glad they've closed it all up, because, boy, that's scary stuff. From a physical standpoint, if you take an agent, how do you take a biological agent and have it arrested in such a way or dormant, then take it out in the field, and then activate it? What do you have to do?

So we used to use processes called lyophilization process, freeze-dry. You can freeze-dry agents and hold them in a freeze-dried state and they're dormant. But then when you get them in atmosphere and you start putting water into them, then they become viable agents. How are you sure you're controlling—being a physicist now—how do you control that distribution of those agents if you wanted a selective group that you wanted to effect, okay, so that you don't have your own troops affected by it? That was scary stuff. I'm glad they closed that place down.

So that's one of the things I did there was—and we used to work with an organism called E. coli. E. coli is not—the species we worked with was not detrimental to your health, but you knew when you got it, because you got dysentery. [Laughter] That's a hell of a way to know whether you're successful or not. [Laughter]

At any rate, I was in a very unique operation, as a matter of fact. It was mostly civilians up at Fort Dietrick. I was in a platoon of people that were all scientists who all had experience outside before they went in the Army. This was during the Korean War. There's another war story, because I was in the 101st Airborne Division as a paratrooper and got pulled out to go to this operation. I didn't want to be a paratrooper, but I got to be one. I was drafted into the Army. I spent the time in Fort Dietrick in that facility as—well, we were working as lab assistants to the scientists in the lab. It was a rather unique experience, but I would rather have spent it in another part of the Army, like intelligence, where I spent the rest of my life instead of something like that. [Laughter]

But I worked with a guy who had Y12 experience. He came from Oak Ridge. Just at the time I was looking around for what to do with the nuclear stuff, now that we'd seemed to have reached a stone wall, he got in touch with me and said he was now out of working for the Army at Fort Dietrick and he was working for American Machine and Foundry Company as a contractor. That is the same thing we were all doing here, and did I want to come to work with him. His ultimate notion was to go out in business for himself, but did I want to come in and they could hire me in American Machine and Foundry Company, which I did, and went to work for them for a couple years, responsible for developing nuclear instrumentation for the Navy as a contractor.

Then Pat and I decided to put our own company, and I gave you the war story on that one, and we did it. I don't know, things just always seemed to fall into place at the time I needed them. Life is strange that way. They come from places and in ways that you never think they were going to come. I never thought I—as a matter of fact, when I went to work

for NASA, I thought I had to change my whole approach to life, because we were delivering these chambers, and even though I was the executive vice president, I'm—guys later said to me, in recent years, "There's no problem that's too small for Calio to solve." [Laughter]

I mean, I can't sit in an office. I had to be out in the field working these things. I went to Los Angeles, worked with a construction crew in putting the whole thing together, starting up the whole development, worked with the engineers in the design. When we used to run in these chambers, used to do them twenty-four hours a day, there were many days I'd work twenty-four hours. People would come back to work and they'd find me sitting out there either asleep in a chair or taking some measurements on a test chamber, etc.

I had come to the conclusion my life had become a gun-barrel approach. I was looking through a very small diameter, because I lost a lot when I lost the company. I had not paid attention to my children and my family. We divorced, and that was not a very pleasant time at all, sort of collapsed at one time.

So I thought, well, I always wanted to go to work for NASA, anyway, now that it was a changing point for me. So I applied and got the job. That's why at the time I was ready, willing, and able to go to Houston, because I was not married at the time. Things just happened that way. [How did] I got off on that one?

There was another interesting one, too, on a Baldrige situation, talking about having confidence in people that worked for you.

Jeez, wow, what time is it. Four o'clock?

BUTLER: It is.

CALIO: You're kidding me.

BUTLER: No. [Laughter]

CALIO: Holly smokes. I'll let you people go. There was an issue at the time when they were first commercializing the launch vehicles, which agency should have responsibility for it, Department of Transportation for the licensing of companies, Department of Transportation or Department of Commerce. The Secretary said to me, "That's an issue on the Domestic Council, so [I] want us to put together a pitch, and I think we should do it."

I said, "Come on, boss. What do you want to do that for? We don't have that kind of expertise, and we're not responsible for transportation. It's just another vehicle."

"I just want to do it. I don't believe that Department of Transportation will do a good job. I think it's a commercial responsibility."

"Yes, sir." I said, "You're just pouring good money after bad."

I got a lecture from him on being a quitter. He said, "What's the matter with you? You're a quitter."

I said, "I'm not a quitter. You know that."

He said, "Well, I want the President to know that everybody sitting around that table is not in agreement with him that th[is] should go with the Department of Transportation. Now, once he makes a decision, we'll do it, but in the meantime, he's got to know where everybody around that table is. None of this managing by mediocrity."

"Yes, sir."

So I put the pitch together. So the week before we were ready to go over and have the meeting, he got wrapped up in something, and so I didn't get a chance to brief him as much as I normally did. So we were coming down to the day and something else happened and he said, "Well, bring the pitch with me, and as we're going over in the car, and you tell me what I'm supposed to say in this pitch."

So we're going over in the car, and it isn't very far from the Congress, the Department to the White House. It must have been a fifteen-minute ride. So we're getting out of the car,

and he said, as we were walking up to the side of the West Wing, he says, "You know, you know that stuff better than I do. Why don't you pitch it."

I thought, "Oh, my God, I can't believe he did that to me." [Laughter] But that's the kind of man he was and I learned a lot. I learned a lot. So that's a good note.

BUTLER: Well, it certainly sounds like you've had a very interesting career and you've learned a lot all along the way.

CALIO: Yes. Well, not enough.

BUTLER: We thank you so much for sharing all of that with us.

CALIO: Okay. Hope it helps.

[End of Interview]