INTERVIEW TRANSCRIPT

GEORGE E. MUELLER
INTERVIEWED BY SUMMER CHICK BERGEN
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BERGEN: This is an interview with Dr. George Mueller on August 27, 1998, for the Johnson Space Center Oral History Project, interviewed by Summer Chick Bergen, and assisted by Carol Butler.

Good morning. We thank you so much for agreeing to participate in our project.

MUELLER: Oh, and I'm so sorry for missing you last evening, but I was involved in a few things. Running a little project like this keeps you fairly busy.

BERGEN: I'll bet it does. That's perfectly okay. We're just honored that you're agreeing to speak with us. It's a privilege.

Well, let's start back at the beginning of your career involvement in the space industry. How did you first get involved working in space?

MUELLER: Well, let's see. I was teaching at Ohio State, and I took a year's leave of absence to go to work for a little start-up company called Ramo-Wooldridge [Inc.]. Ramo-Wooldridge, it turned out, was doing the systems engineering and technical direction for the ballistic missile program. This was before the Russian space activities started, but we knew about them. So when the Vanguard Program failed, the Air Force moved in, as did the Army, to launch some spacecraft. As it turns out, before Sputnik started, Ramo Wooldridge started their satellite program. There was both the secret one, in which I was not involved, but the overt one where we built the first of the Pioneer vehicles. Actually, we built a lunar probe. That was a small, wonderfully compact little vehicle that was supposed to go out to the moon

and survey it. It actually had a little camera on board to take pictures of the moon. This was about 1958.

Unfortunately, the launch vehicles were not that reliable at that time, and although we had three attempts, we actually never got one to go out to the moon. The very first one went part way and got up to—I don't know, got out into space a fair distance, halfway around the world, as a matter of fact, but it didn't qualify as an orbiting vehicle.

That was my first introduction to space, as such, and that led eventually to my being asked to go back to run the manned space flight program. I don't think that many people realize how much was accomplished in those few years, though, beginning in about 1953 through '60, in developing the basic technology we're using today throughout the space program, because I remember, for one thing, we developed what was then the—I'm drawing a mental blank on the name of the program—it was the Thor Program, which is now the Delta. I was thinking the Delta and trying to remember what Thor was. But the Thor started out as a short-range ballistic missile, and was developed in one year starting from the idea to flight of the first vehicle. Of course, the first vehicle blew up.

BERGEN: It wasn't terribly unusual back then, was it?

MUELLER: No. But it was amazing how quickly we were able to—we actually deployed them in two years, so they were over in England launching out of pads over there within two years of when we started the program. That kind of ability to move out is to some extent lost today. We've become so involved in doing everything by rote that we can't break the rules and get out of that habit to do something new and exciting.

BERGEN: You did lots of exciting things. From my research it looks like you worked on the Atlas and the Titan and the Minuteman.

MUELLER: That's right. I was involved in the guidance control systems, really the electronics laboratory at Ramo-Wooldridge I ran for a while. That did all of the guidance and control for the ballistic missile program. We actually did all of the software there, because that gave us the insight into the programs that you need for being able to do the technical direction and the systems engineering on it. We were fortunate in getting a tremendously capable group of people working on the program and recruited from everywhere. But it was an exciting thing. It was one of these programs that was super secret, so no one was supposed to know anything about it, and very few people did.

We really developed a whole new technology in that time frame, which took place in about six or seven years. Then it grew and grew and grew like things do. Si and Dean [phonetic] started out in a schoolhouse, and, well, actually started in that little storefront office on the street, but then moved into a schoolhouse, and then eventually began to build buildings so that you have that complex which is now down in Manhattan Beach and Torrance that they have built and developed, which is now shrinking, which is characteristic of life. If you aren't growing, you're shrinking.

BERGEN: You were involved in Pioneer I, which was considered the first successful probe.

MUELLER: Right.

BERGEN: Can you tell us a little bit about that?

MUELLER: Well, that was the lunar probe. It didn't quite make it, but it did get far enough to get into the record book. It was an interesting development. We built the first of the digital

telemetry systems for it, because we couldn't figure out how to make an analog system work all the way out to the moon and back.

I, at that time, set up the first of the deep space network. This was an Air Force project. We put sixty-foot dishes in. We actually used the radio telescope in England as part of that, but we also put in the sixty-foot dishes down at the Cape [Canaveral], and we put one up in Australia at that time. We had to have the dishes working within the time frame that we had to launch the vehicle. I think we started it in 1958 and we lost it in 1959. Of course, the launch vehicle was earlier. Although we did develop a new upper stage at that time frame for this thing as well, that was called the Able stage.

Besides the digital telemetry, which was new, we also did a fair amount of work on thermal control. We developed these little disks that adjust the temperature by the radiation [striking] the vehicle, and they are thermostatically controlled with a little spring that keeps it at the same temperature, hopefully.

Quite a lot of interesting developments in that time frame. We developed the idea of a—we were spinning the vehicle for stability, and at the same time we put on a little photo detector that would scan the surface so that we got a scanner built in as we were going around, and we were going to use that for sending back the pictures of the moon.

BERGEN: That must have been very exciting working on that, just getting into space technology.

MUELLER: Well, it was interesting because I was doing that and running that program in addition to running the laboratories. So I spent my days working in the lab organizing and keeping things going there, then I spent the nights working on this thing. We had a few other people. It's amazing what you can do with a very few people. I think we didn't have more than ten people working on the spacecraft itself, plus some technicians.

But I remember very well that we were about halfway through the program, NASA took it over, so we had some NASA people out watching the program. So they kept asking my bosses, "If George is running the program, how can he do that? He's never working on it?" Of course, I was doing my work at night, and they weren't around. [Laughter]

BERGEN: Spent a lot of hours then/

MUELLER: Well, those are days when you slept just enough.

BERGEN: Then at some point you left Space Technologies Laboratory [STL] and went to work for NASA as a deputy associate administrator for manned space flight.

MUELLER: No, as the associate administrator.

BERGEN: As the associate administrator. Okay.

MUELLER: Actually, I started that associate administrator business. I was the first of that breed. Homer [E.] Newell became an associate administrator for space science. Let's see, who was running the technology group at that time? Associate administrator for technology was—he went to work for Avco up in Connecticut when he left us. Well, his name will came back. That long ago, names are a little more difficult to recall.

BERGEN: When you entered your job at NASA as associate administrator, what did you think was ahead for you at that time?

MUELLER: Well, you know, back in STL we had been participating in the Apollo Program. We worked with GE [General Electric] and made a proposal, and eventually [unclear] for the original Apollo Program. So I was relatively familiar with that. By that time I was actually running the [business development as] STL became a profit-seeking corporation. I was running the marketing as well as the engineering at that time. We were short-handed and I did a lot of things.

But in any event, we were involved in that proposal, so I knew a fair amount about the Apollo Program before I went back there. I also had worked with the various—well, for example, STL provided the engines for the lunar LEM, Lunar Excursion Module. So I got to know the contractors as well as the NASA management team during that period. Matter of fact, we were working rather closely with Marshall at that time to convince them that they needed some systems engineering support. Wernher [von Braun] thought that was a great idea, so [he] went back and sold it at headquarters, and they hired GE to do it instead of STL. But those are the kinds of things that happen when you're bidding on proposals.

It turned out that when I went back to NASA, [D.] Brainerd Holmes had just been allowed to leave, and so I came in at a time when it had been somewhat chaotic, about the time that Congress was asking, "Is this something that we really ought to fund?" Will it work, is what it amounted to. Probably, from what I had seen of the operations around the country, it didn't look like it would work, because there wasn't any coherent drive that was well enough organized to make it come together. You had three centers involved. Well, you actually had five or six centers involved, with three principal ones, and they barely talked to each other. We just weren't going to get there, in my opinion, unless we changed the organization.

So, before I came back from NASA, I had discussed sometimes with Jim [James E.] Webb what I felt I had to do in order to be able to run the program. He agreed, and we set up this associate administrator with the centers reporting directly to me at that time, and they

restructured NASA with associate administrators with the other centers reporting to the other associate administrators. That gave me the control I needed, but it still didn't provide the organizational basis on which you can build to go forward.

I had observed what it took to run a program in the military, and one of the basic problems is getting free communications throughout the organizations so at least you know what's going on everywhere and everybody knows what's going on. That was one of the things that caused me to set up the management structure that I did employ at that time, which consisted of essentially my five boxes, one of which was systems engineering, which was an important ingredient to any program, and what I did was to have these program—well, I set up a program office for Gemini, and one for Apollo, and one for advanced programs. So we had three program offices to begin with.

The Apollo Program office—at that time Gemini was just starting, but it was far enough along and it was a small enough program, it didn't make sense to have a lot of program offices around, so we had one in Washington and one at the Center, which is now Johnson [Space Center]. But I did set up in there the same equivalent set of organizations internal to the program office that linked from Washington to Johnson at that time.

But for Apollo, because it involved all of these centers in great depth, I set up an organization that started with a program office in Washington and then program offices in each of the three centers, with their own program managers, who reported directly back to the program director in Washington. Initially I was the program director in Washington, among other things. That was just until I could get that organized and running.

Then each of these program offices, in turn, had these five disciplines which also reported, or just communicated directly with their counterparts in Washington. So you had five boxes with the three centers, five boxes in Washington, each one of which was communicating independently of the program office, but part of the program office, but had their own disciplines that they kept track of throughout the operations.

BERGEN: These were called the GEM boxes, or G-E-M boxes?

MUELLER: Yes, among others, in polite times. [Laughter] But it did establish then lines of communication by discipline within the program offices, and those, in turn, then kept the program themselves informed as where things were and what the problems were. It floated them up more quickly than they had in the past. But in order for that to work, I had to get the center directors to become part of the solution. So I set up the Program Management Council, which was the three center directors and myself, and we met monthly to go over the program. These center program managers reported both to the center director and to the program office in Washington. So they had dual reporting responsibility, and that took a while for NASA to understand. Dual reporting was not a normal part of that culture. That took a while to put in place. By the time of the fire, it was working very well indeed, and it carried us through the trauma associated with that and following, and really made it possible to recover and move out with Apollo 8.

BERGEN: Since you brought up the fire, can you go ahead and talk a little bit about some of the circumstances surrounding that?

MUELLER: That's one thing I'd just as soon forget. In all honesty, that was a traumatic experience. We were having a dinner plotting the future course of the space program with our key contractors, and Jim Webb was there, Bob [Robert C.] Seamans [Jr.], and everybody at this dinner in Boston, when the fire occurred at the Cape. Jim did exactly the right thing in setting up immediately a review committee, an independent investigation committee under Tommy [Floyd L.] Thompson of Langley. Then we moved out to find out what had happened.

That was, however, one of the things that caused a great deal of stress within the organization, both internally and externally. It was very difficult to hold everybody at a calm and keep them organized and moving at that time, because Congress was investigating, the newspapers were investigating, the White House was investigating, everybody was investigating. As is characteristics of these things, probably the problem was simple and straightforward, and you would never do it if you thought about it, but the use of pure oxygen in an enclosed space is quite dangerous, and certainly it's more dangerous as the pressure increases. So although we had used pure oxygen, and really in order to condition the astronauts for the extravehicular activities as much as anything, and to save weight on Mercury, on the Gemini, and on Apollo, until the fire, and we really had no appreciation of what happens when you have a spark in pure oxygen in an enclosed space like that. We discovered that that was a problem, and we spent a great deal of time trying to find the source of the problem, the source of the fire, but literally it was a time bomb just sitting there waiting to go off.

But the good part of that—that was an experience I would rather not go through, the astronauts' wives and all of the funerals, and the investigation, testifying, and trying to explain that it was inevitable that something would happen sometime. Unfortunately, it had to happen then. But we did persevere, and I would say that the good thing that came out of it was that we really understood what causes fires on spacecraft. We really redid most of the wiring, not that we knew the wiring was at fault, but rather we redid the wiring on Apollo, and did it much more professionally than the first time around. I think that's probably why the Apollo Program was relatively accident-free.

BERGEN: Talking about making the corrections, one of the things that was brought up in the investigation was the Phillips Report, and there's, of course, been a lot said about that, but, in

hindsight, do you think you should have handled the Phillips Report any differently than what you did at that time?

MUELLER: Well, you know, I didn't think the Phillips Report existed, because if I had known it would have gotten a wide distribution, I would have handled it differently, because the one thing you know about Washington is, if it's in writing it's going to be—we had rather naively, I thought, caught it before it got into print. It turns out I was wrong. It got printed before I caught it, but I didn't realize that at the time. I don't think Sam realizes either. So the copies began to show up all over the landscape.

Now, the basic reason for not wanting that distributed was that it was a fairly negative report that was not, however, constructive. It didn't say what had to be done; it simply said what was wrong. That kind of thing, if you feed it into the newspapers—and remember, we were in a fishbowl in Apollo. I didn't want to get a whole set of negative things out about one of our major contractors, and then have to explain to Congress and everybody else how that was good that we were learning things and we were moving forward. It's much more difficult when you start from a negative point view than from a positive.

Then the fire came along, and this was after I thought I had buried that report deeply, and was replacing it with—it never was published, it was a draft. I replacing it, getting them to replace it with what needs to be done rather than what was wrong. But then the fire came along, and it surfaced in the course of the investigation. No, if I had known it would have existed, I would have immediately disclosed of it. The one thing about Washington, if you've got any problem, better to tell everybody early rather than later.

BERGEN: It's a hard lesson learned. In speaking about the Phillips Report, which was written by Sam [Samuel C.] Phillips, you initially wanted him to be a representative on that

investigation board, but Joe [Joseph F.] Shea felt that he should be the representative. Can you tell us a little bit about that situation?

MUELLER: Joe, of course, is the person responsible for the capsule. It was a question as to who was the best person for the investigation. Joe knew so much about it, it made sense for him to be on the investigating committee. He knew all the interior problems more than anyone else did. In retrospect, I wish that I had had Sam serve on it instead of Joe, because Joe got so deeply emotionally involved that he lost—well, he really had a nervous breakdown, and that created a whole host of personal problems for him and for us as well. Because of that situation we had to replace him. Very difficult. He was a friend, a very good friend, of mine, and one of the most brilliant people I've ever known. Yet he became too involved, too emotionally upset about it, that he just became unable to do the kind of constructive things that we needed at the time. Spent too much time worrying about what had happened, not enough about how to get from where we were to where we needed to be.

BERGEN: But the program did continue. I want to go back just a little bit and talk about some more positive things, hopefully. You were the one responsible for deciding on the all-up testing procedure.

MUELLER: Correct.

BERGEN: Can you tell us a little bit about how you came to that decision?

MUELLER: Oh, it was easy. You just looked at the schedule, and you could see that if you went through the test program that Marshall [Space Flight Center] had laid out for the launch vehicle, that you weren't going to be landing on the moon in the decade. I don't think that

anyone really who had been involved in the program disagreed with that. I don't know that that time they were all that convinced that we they were going to be able to land on the moon in the decade.

BERGEN: Did you convince Dr. von Braun and his associates that that was a good idea?

MUELLER: That was an interesting move, I must say, and it was also necessary to convince Bob Gilruth and his associates that it was a reasonable idea, because they weren't used to doing that either. But Werner probably had the more vocal opposition.

It was easy. You laid out the program and then—well, we started with what we had, and then we spent some time working with the centers, because by now I had the program offices in place, or at least beginning to be in place, and so we laid out a schedule that made sense. Looking, from my experience in flying these things, it was clear that you're much more likely to have a failure on the second stage than you are on the first stage, because you spend more time on the first stage than you do on the second stage, and so on and on and on.

We were in production on these things, so we were bringing everything together as rapidly as we could and in a sequence that would get them all together at the same time. So it didn't make much sense to fly the first stage and then fly it with the second stage, or fly the second stage separately, which was also proposed, and so on. By the time you had to do all of the work necessary to fly a single stage by itself, you hadn't really done the work you needed to fly the whole stack. If you lost a vehicle, you were likely to lose it at any stage so you might as well go as far as you can and find out where the problems are. At least that was my philosophy at the time. Still is.

So we went around talking to the centers, and the first time through they looked askance, and said, "You couldn't possibly think of anything so silly." They didn't quite say it that way, but made it real clear. It turns out that Kurt [H.] Debus was a strong supporter of

the idea as soon as he thought about it, because he'd seen the same things I had, things blowing up all over the place, and indiscriminately stage-wise. So we had a major meeting at Marshall when he had all of his troops together and we talked about it. I finally said, "Well, the only way I can see to get to the moon in this decade is through this program." Werner finally said, "Well, it's risky, but I agree. I support that idea."

I must say we had a precursor to that. We had an offsite where we had all of the center directors together, and we talked through the program and what the alternatives were so that everybody was on the same baseline at the time. But it was a decision that I did make, and it wasn't unanimous by any measure or means.

BERGEN: Did you watch the Apollo 4, the first all-up test launch?

MUELLER: Apollo 4?

BERGEN: AS-501?

MUELLER: Oh, yes. Yes, of course.

BERGEN: What were you thinking? Were you a little anxious when you did that first test?

MUELLER: Actually, I was deeply enough involved in the program so that at that time I was more interested in making sure that everything that I could think of, or anybody else could think of, was done so it was going to work right. Of course, it worked very well. The 505 didn't work quite so well.

BERGEN: Can you tell us a little bit about that?

MUELLER: It was a great learning experience. One of the things that our organization, the organization we had set up with the program offices and the technical groups supporting and so on, worked exceedingly well in terms of identifying the problems and curing them, and identifying them clearly enough so that we certainly knew what the problem was, because that's always a problem when you're dealing with things at a distance.

We produced the problem on the ground and made the corrections necessary, and then we went through the entire history of those two flights, to be sure there weren't other anomalies in them, in as much depth as I've ever seen programs reviewed, and on the basis of that we felt confident enough that this thing was going to fly, that we flew the next one to the moon.

BERGEN: And that decision to send Apollo 8 around the moon, you weren't initially convinced that was a good idea, were you?

MUELLER: No, as a matter of fact, I thought it was great.

BERGEN: Oh, did you?

MUELLER: But I wanted to make sure that we did this thorough review, and so I used it as a—I said, "You've got to convince me it's going to be safe." No, I used that as a lever to get that kind of a review, which really found a few things and straightened them out before we were satisfied that it was safe to go.

BERGEN: You sent Apollo 8 around the moon, and it was successful, but several flights later on Apollo 13 there was a lot of problems on that flight, but they got back with the lunar

module. Did that make you reflect any differently on Apollo, what you had done on Apollo 8?

MUELLER: No, it proved that I was right. Of course, I was out by then.

BERGEN: Right.

MUELLER: It was Dale [D.] Myers—we're working together on this thing now—that was running it. I told him it was his fault. [Laughter] This was later, of course. But, no, we had anticipated that kind of a problem. If we would have known what the problem was, it wouldn't have happened. But we anticipated that there could be problems that had that kind of a consequence, and we had looked at how to get back, and that's the case. It was worse than we had anticipated, but still it worked.

BERGEN: So as you were working on the Apollo Program, you realized that there as going to have to be something after Apollo.

MUELLER: Yes. Or I hoped there was going to be something.

BERGEN: What eventually came after Apollo was Skylab. So how did that all get started?

MUELLER: Let's see. It was clear that if you wanted—we had set up the Apollo Applications Program Office at the same time we started the other program offices. So we kept that going, trying to find applications for the Apollo hardware on other programs. One of the things that we did was to look at what we needed to do in order to get into space really, and that's what led to the Space Shuttle. It became clear that if you're going to really exploit the

space environment, you had to have a cheap means, an inexpensive means, of getting into space and out of space. As you know, we did a long-range plan, and part of the plan involved the creation of space stations in order to serve as a node in the transportation system going out to the planets or to the moon, for that matter.

The real question, though, at that time was whether or not people could survive for long periods of time in weightlessness, without having to have artificial gravity. A big debate that still goes on between artificial gravity and zero gravity for human exposure over long periods of time. And the only way to prove that is to do it. We set up the Skylab really to test long-duration exposure of man to space, and that was about the same time we started the studies on the Space Shuttle, because in order to get to Mars we needed to have some ways of getting into space more cheaply than we were doing, and with a lot more energy than we had at the time. So we started the two programs almost together in their studies phases. It's been studied to death since.

The Skylab itself then was envisaged to not only test the long-term exposure of weightlessness in human beings, but also to test what you could do while you were in space. So I thought, I guess, of what are the things that one can do in space that are important. One of these is telescopes to look at the universe from outside the atmosphere. So we did some studies on telescopes. In fact, at one time I proposed to the astronomers in California, one of our advisory board members headed the telescope of the Astronomical Observatories along the West Coast. So they had a meeting and I came out and proposed that we fly one of their telescopes on our Saturn V, and we would provide them with all of the viewing they could hope for outside the atmosphere. They didn't think that was possible, but, as a matter of fact, my friend did.

In any event, so the Skylab was developed, first of all, to be an S-IVB stage that was refurbished in orbit. That S-IVB stage was to be flown on one of the Saturn I rockets and placed in orbit, and then we were going to build the crew quarters and stuff once it's in space.

Werner was given the task of doing it because he had enough infrastructure there, we thought he could do a lot of the work in house, because I was trying to get them back into doing some in-house work as well as managing programs. I have the view that one needs to have a combination of program management, but you have to have a solid internal structure to support that program management, or else you have to hire somebody else to do it. NASA had, certainly in Marshall, a tremendous internal competence in building these kinds of things. So they got the task of doing the conversion of the S-IVB stage, because it was their stage anyhow.

After we had it along to the point of starting the design, Werner got the idea of building a neutral buoyancy facility to see just what was involved in doing that in space. So he built this thing down there, and without any permission from anybody. One day when I was visiting, Werner always had Eberhard [F. M. Rees] take me around the controversial things so if I blew up, I'd find some refuge. Anyway, Wernher [had] Eberhard [take] me over to show me this new facility they had in one of their old hangars, which was this huge neutral buoyancy facility which is big enough to take the S-IVB stage, or at least most of it down there, so they could actually carry out the activities that they were planning to do in orbit. It was a great idea, I thought. I was happy he didn't ask for permission, because it might have taken some time to get permission to do it.

I decided I wanted to see for myself what was going on. So that was my first experience with scuba diving, was to go down there and see how hard it was to move the valves and so on. So I did that one Saturday, and decided then that it wasn't likely we were going to be able to refurbish this thing in space. It's just too hard to work in a neutral buoyancy environment. If you're in a spacesuit, it's even worse. You had to do the initial refurbishment, or furbishment, I guess, without air. So you had to be in a spacesuit and you had to do all of the preliminary work in a spacesuit. I figured by the time we did that, why, we would have exhausted several astronauts and our program in the process.

So after that, and after we got a little further along in the design, I made the decision to fly it on a Saturn V instead of on the S-1C. Surprisingly, there was a fair amount of objection to changing it, because I guess everybody was conditioned to the earlier thing. But it was clear that if we wanted this thing to really work, we had to simplify the process of putting it into operation. So we built it on the ground.

I got Raymond Lowey to come in to work on it to provide—because looking at what the original design was, it didn't look to me like anybody would want to stay there or be able to stay there for ninety days, which is what our objective was.

MUELLER: It just wasn't a place you could live that long. Now, people can live under very trying conditions, but it didn't seem like we wanted to make it too difficult.

So Raymond came in and did a great deal of human factors work on it that I think was outstanding. At first the engineers objected to the idea that here's this character who designs automobiles coming in and telling them how to build a space station, but I think eventually they recognized the—and certainly the astronauts did when they got up there. That was a major step forward. I've always regretted that they decided not to fly the second one, and regretted even more that they made the—well, they had depended upon the Space Shuttle to maintain its orbit, and the Space Shuttle slipped several years, and the thing eventually came in before they could get up to give it a boost to a higher orbit.

BERGEN: There was a little controversy over the Apollo telescope mount when you were working on designing development of Skylab. Can you tell us a little bit about that, what that controversy was about, and a little bit about the LM-ATM [Lunar Module-Apollo Telescope Mount] Evaluation Board?

MUELLER: Well, I don't recall it as being a controversy so much as it was a question as to was this something useful to do and should we spend our resources on something like this. The engineering group at both Marshall and certainly at Johnson were never very enthusiastic about scientific experiments or science as such. They were more interested in the things they wanted to do. But it was clear that astronomy was one of the real uses of space. So I convinced them, I guess, that we needed to do some things that were scientifically useful.

They had the same problem on the Lunar Program. Initially, everybody was just going to go up there and come home. But the scientists were complaining, and I thought that was justified that, after all, you're going to do all this, you ought to do something scientifically useful as well as engineering prowess or demonstration.

BERGEN: You claim to be a real proponent of space science. Did you work with many people that felt like you did, or did you encounter more opposition to that?

MUELLER: Well, you know, it's interesting. Of course, basically, my background is in science, so it was something I thought of as being important. My background's also in engineering, so I have both sides of the coin.

There was not a great deal of enthusiasm initially anywhere in the organization for science, but because it was impacting Johnson more than it was Marshall, Marshall was much more enthusiastic about science than Johnson was. So it took a fair amount of direction to get a scientific program installed in the program. It wasn't so much opposition to it as it was, well, they didn't really see why it was useful. And none of the astronauts, of course, had any scientific background, so they weren't particularly—In fact, Jack [Harrison H.] Schmitt was the only one that had any scientific training, and he almost didn't get to fly.

BERGEN: So do you think that ensuring that science was done on those helped NASA?

MUELLER: Oh, it did indeed. In fact, the amazing thing was shortly after the program ended, one of the chief opponents of the Apollo programs from the scientific community came around and said, "You know, we were wrong. We should have insisted on more science and more flights." Because it turned out to be very, very useful, and certainly if we had continued the program, it would have been very valuable from a scientific point of view, because we had a far more capable exploration tool in the lunar excursion module than we're likely to have in any of the robotic programs we now are contemplating.

BERGEN: One thing we didn't talk about when we were talking about Apollo was how you felt when you watched from Mission Control the landing of the lunar module on Apollo 11.

MUELLER: Oh, yes.

BERGEN: Tell us about that.

MUELLER: Tell us about that?

BERGEN: Yes.

MUELLER: That was a fascinating time. I was in the little side room they have there, and listening and listening and listening, and thinking, "Are they going to be able to land?" It happened very rapidly. So they had landed by the time all of the worries got organized in my mind. But that was a traumatic moment. And as it turns out, it was one of those things, that somebody decided that they would just do a little experiment on the landing altimeter and left it running instead of turning it off as it was supposed to be, and that just overloaded the shift

registers in the computer. Fortunately, it was self-correcting, so it would come back on again, but then this landing radar would cause it to overload and shut down, I guess. That was driving them until they got down to the ground. It turned out that the folks on the ground, the support troops, recognized the problem, or [they] analyzed it and recognized it in the few moments they had to do it, and decided it was safe to proceed. But that was a hectic five or ten minutes.

BERGEN: That was in 1969, and that's the same year you left NASA. Why did you decide to leave NASA and go back to industry at that time?

MUELLER: Well, several reasons. One is that the decision had been made to terminate the Apollo Program, and that was a good time then to leave before, and let someone else take over for the next phase. From a practical point of view, I needed to go make some money so I could keep my family going. It was costly for us to join the Apollo Program. My salary was half what I was making in industry when I went there, and it was just a strain to keep the family going and work going at the same time. So I went back to industry.

BERGEN: It was nice to leave with the triumph of landing on the moon.

MUELLER: Well, it was a good time to leave in that sense. You know, it looked like it would be another five or ten years before the next program was going to come to fruition. There's also the general thing that if you stay in Washington long enough, if you do anything, you create enough enemies to make it difficult to get anything done. I'd left before I think I created that set of enemies, but it's clear that you have a limited time of effectiveness in Washington if you really are doing anything. If you're not doing anything, you can stay there indefinitely.

BERGEN: We're coming to the end of our time here for our appointment that we had scheduled. Before I wanted to close the interview, I wanted to ask Carol if she had any questions for you.

Butler: Just one brief one, hopefully. Looking back, you were talking about the science and how there wasn't a lot of support for the science programs in the early time frame when you were working on the Skylab. Do you think now there has been a fundamental change in NASA, because now if they don't have a good scientific justification they're not going to get the money to do the program? Has there been a change in thought like that?

MUELLER: Well, there was always the science group that was enthusiastic about science. They just thought manned space flight was siphoning off all their money so that they couldn't get their science programs done. In actuality, the opposite was true. I was siphoning money into them off of the Apollo Program to keep them going, in a sense. Through our scientific work we asked them to manage it, keep it going. That was why we brought the deputy director, Homer Newell's deputy over to be my deputy in order to provide that link into the scientific community. Not Homer Newell. He went back to Langley eventually. I'll remember his name after we finish this, probably. [Edgar M. Cortright]

I think that today it isn't the scientific justification, it's the lack of a vision of where man is going in space that causes the problem, and to try to justify manned space flight on purely a scientific basis, I don't think it will work. The scientists have an experiment. They are able to develop that experiment in such a way that you don't need men involved unless you could take them there. Now, they would be sure, if they could go up there and do it, that it would be much better, and it would be. But since our ability to take people into space is so limited, it creates a negative feeling about manned space flight that shouldn't be there. You

think of the pioneers that opened up the West. Well, you ought to think of pioneers opening up space in the same fashion. If we'd only found diamonds on the moon, we would have been able to really get that program going. [Laughter]

BERGEN: One more question about science before we close the interview. You established the Science and Technology Advisory Committee. Who made up this committee and how did that contribute to the space program?

MUELLER: You have the list of names, and I'm having a little trouble remembering them. Actually, it was started because we'd had so much flak from the scientific community, from the White House Science Group and so on, that I needed to do something to offset that. So I asked Charlie [Charles H.] Townes, whom I knew from Bell Labs days, whether he would be willing to organize this Scientific Technology and Advisory Committee. I got Jim Webb to agree that this was something that would be useful. Charlie went and selected a remarkable group of engineers and scientists that represented every aspect of the spectrum of science at that time, and were also characteristically interested in something new and exciting. They were not the scientists that have a single-track mind; they were the multi-tracks-mind scientists. Well, three of them were Nobel or became Nobel [Prize] scientists. The others could have been, or should have been. That was done both to bolster our internal science, because I needed somebody or some group to foster science within the—most of the experiments on the lunar modules were developed or suggested by this group. They became quite involved in the program.

Charlie and I went down to Arecibo to set up a first radar measurements of the moon to determine whether or not there was that hundred feet of dust that Tommy [Thomas] Gold insisted was there. Well, we were trying hard to answer some of the criticisms in some

logical or scientific way. Because at that time we'd lost two Surveyors, and it wasn't clear we were going to get one to land. But we did, and answered that question.

But that group was very instrumental in deflecting the criticism. You couldn't criticize three Nobel scientists, is what it amounts to, although I guess two of them got their Nobel laureates after. So the scientific community criticisms were somewhat blunted. We got some really very useful advice, and actually they promoted some of the experiments. I told you one of them was the head of this astronomy group on the West Coast, and he brought me out and I talked to them about the wonders of space and what was all going to happen in the future, and how important it was to have telescopes in space. [Francis Clauser] That was the way we interacted with the scientific community, which was important in building that, and at least keeping them from undermining our approach, and in some instances getting positive support.

BERGEN: Science of space has added greatly to our lives here on Earth. I think that was an important contribution made by the space program.

MUELLER: I know. What I'm trying to do is get us out into space. [Laughter]

BERGEN: We thank you so much for your time that you've spent with us.

MUELLER: I'm jealous of John Glenn. I very much wanted to go on Apollo 8, but Jim Webb wouldn't let me.

BERGEN: Yes, John Glenn is very fortunate.

MUELLER: Yes, he is.

BERGEN: We'll be hearing a lot more about him in another month.

Butler: You'll just have to put in your little program plan for getting yourself up there. Sell yourself to Dan [Daniel S.] Goldin.

MUELLER: [Laughter] Ah, yes. Well, I don't have quite the political clout that John has.

BERGEN: Again, we thank you so much for participating.

MUELLER: You're most welcome. I enjoyed it.

BERGEN: It's been very interesting. We've enjoyed it.

[End of interview]