NASA HEADQUARTERS NACA ORAL HISTORY PROJECT
ORAL HISTORY TRANSCRIPT

ALAN B. KEHLET
INTERVIEWED BY JENNIFER ROSS-NAZZAL
SAN JOSE, CALIFORNIA – 30 SEPTEMBER 2005

ROSS-NAZZAL: Today is September 30th, 2005. This oral history session is being conducted with Alan Kehlet of NACA [National Advisory Committee for Aeronautics], Langley [Research Center, Hampton, Virginia], and STG [Space Task Group] as part of the NACA Oral History Project sponsored by the NASA [National Aeronautics and Space Administration] Headquarters History Office. This interview is being held in San Jose, California, during the eleventh NACA Reunion. The interviewer is Jennifer Ross-Nazzal.

Thank you again for making time to meet me this morning.

KEHLET: You're welcome.

ROSS-NAZZAL: We sure appreciate it. I'd like to begin today by asking you how you became involved with NACA.

KEHLET: I graduated from the University of Illinois [Urbana-Champaign, Illinois] in 1951, and in 1950 right before the Korean War started, jobs for aeronautical engineers were nonexistent. Some of the people I knew that were ahead of me took jobs driving bakery trucks and things like that. Just before graduation in 1951 the Air Force came to schools recruiting for Wright Patterson Field [Ohio], civil service jobs, and the entire class of twenty-two of us signed up to go to Wright Patterson. Now, that was the first time, because we thought it would be a great job.

Shortly afterwards, the companies, the aircraft companies, came to the school recruiting, Lockheed [Aircraft Corporation] in particular, North American [Aviation, Inc.], and suddenly the class realized, hey, there were opportunities out there besides the Air Force.

A man came from Langley [Aeronautical Laboratory, Hampton, Virginia], the Pilotless Aircraft Research Division [PARD], on a recruiting trip, and I was absolutely fascinated with the type of work doing in PARD, the pilotless aircraft, and decided that that's really what I wanted to do. There were two things I knew I didn't want to do. One was work on structures, and that's what the aircraft companies were offering is designing pulley brackets and details like that. The other thing I didn't want to do was work in a wind tunnel. So the pilotless aircraft individual, and I don't remember his name—so I sent a letter of apology to the Air Force and signed up to go to NASA.

So in July of 1951 I arrived at Langley Field. My first impression of Hampton was one of "I wonder where the city is," not realizing I was in the middle of it at the time. I went to the Employment Office there at Langley and was assigned to the full-scale wind tunnel, which I immediately said, "Now, wait a minute. What I understood I was coming here for was PARD, not the wind tunnel." Well, everything at PARD was filled up, and I said, "Well, gee, I kind of feel very strongly, that either I work in PARD or maybe I better look for something else." This is after spending all my money on airfare to get to Virginia.

Anyway, they said, "Okay," so they put me in PARD. I worked for NACA, NASA [for] about twelve years, and most of my career, it was like having a hobby and getting paid for it. It was absolutely a fascinating experience, something that, in later years as I went into industry, I recognized that few people had the opportunity to do the things that I was fortunate enough to do

and get the experience that I got. In industry there weren't all that many people that actually had hardware experience, which I had the opportunity at NACA and NASA to have.

So my early career at NACA, I was assigned to a mentor, Jim [James H.] Parks by name, and Jim was a fantastic teacher. He would let me do whatever I wanted to do within reason and would only kind of overview the thing to make sure that I wasn't going to do something stupid or something that might be of injury. But he was a really good teacher. We worked for Clarence [L.] Gillis, who was the section head, and than Paul [E.] Purser was the branch head, and Joe [A.] Shortal was the Division Chief. So I learned about NACA then from the recruiter that came to the university back in 1951.

ROSS-NAZZAL: What did he tell you that PARD was working on that fascinated you at that point?

KEHLET: Well, it was airplane configurations, performance, and control. That's what I liked in school. I liked being an aerodynamicist, as opposed to a structural type. I also was influenced by my wife's cousin, who was a recruiter for, at that time, General Dynamics [Corporation] in Fort Worth [Texas]. He said one thing, and that's while I was going to school, "Stay out of structures. We can always get all the civil engineers we need to take care of structures. Be an aerodynamicist." And that's something that I enjoyed, so that fit very well.

ROSS-NAZZAL: That's great that you managed to work into that type of position. You mentioned that Jim Parks let you do pretty much what you wanted, as long as you weren't

putting yourself or others at risk. Could you walk me through how you came up with an idea for aircraft research and how you saw that idea to fruition?

KEHLET: Well, we had a general research program on aircraft configurations, and we had a standard body configuration with different wings on it. We had the swept wings, the straight wings, delta wings in that particular program. An example of what could be done is that it occurred to me that if you put—well, first of all, it was rocket propelled, so you had the solid fuel rocket behind the airplane model. It had to be boosted, and then the rocket separated, and it was free flight from then on. So there was hydraulic systems and telemetry systems in the aircraft that were boosted, as I said, to Mach up to about Mach one-five, one-point-five.

As we started getting configurations that had larger and larger wings on them, it occurred to me that maybe we should put that airplane in the back of the rockets and sling two rockets underneath the airplane configuration. I proposed this to Jim, and he said, "Well, how would it work?" So I sketched out something, and Jim actually helped me with the stress analysis as we designed this particular booster configuration. Then we put a rocket behind it and got a two-stage vehicle on it.

It occurred to me several years later when I was in industry—I was Chief Engineer of the Space Shuttle for North American Rockwell [Corporation]—that the configuration of the Shuttle and that original configuration back in the early fifties are very similar, with the strap-on rockets that were below the vehicle. So that's an indication of innovation that Jim encouraged.

ROSS-NAZZAL: How long would you spend working on one of these type of projects?

KEHLET: Well, I always said that there were three years. There was the year of design, and a year to build and fly it, and a year to write the reports. It was about a three-year cycle throughout the program.

ROSS-NAZZAL: How many other individuals would typically work with you on one of these assignments?

KEHLET: Pretty much an individual. Jim and I published a lot of reports, and I published some of them under my own name, technical reports. But basically you worked on the thing as the project engineer type. Now, we had help. We had a computing pool that would do the various calculations that were needed and help read the telemetry records and things like that, but basically it was you were responsible from start to finish, including writing the report, going through the Editorial Committees, and arguing with the various Centers. We either agreed or disagreed, with some comments.

ROSS-NAZZAL: Can you talk about that whole process of writing those reports? You're an engineer, and coming up with the report, and then, as you mentioned, working with Editorial Committees. Who served on those committees?

KEHLET: Well, in the report writing, in particular, first of all, you had to get the telemetry records and read those, and we used Gerber Scales and all sorts of things. It was a paper with traces on it. You would read those in numbers, and then they would be converted from the numbers to the actual what the sensor was reading, like angle of attack, so many degrees; angle

of attack, angle of side slip, acceleration, rate gyros, and things like that, too. You'd get that basic information and then plot it up into plots of lift versus drag and lift versus angle of attack, and that would be the report on the performance.

So you had to write, and I remember, I think if you looked in the reports that both Jim and I wrote, and I think I had about thirteen or fourteen reports I wrote, it always starts out as part of a general research program. That was the starting line, and that was kind of neat, because engineers don't like reports, and I learned more about English and spelling and grammar and things like that from writing a report than I ever learned in school, which made it kind of interesting. I'm a poor speller, but I can spot a misspelled word right away, because I know it doesn't look right.

But going through the report, you write it. Jim would review it. Then Gillis would review it. Then they'd come back with comments on the things. It always seemed to me you wrote the report three times. The first was the draft, the second was the correction, and the third time was back to the draft, almost. Then it would go to Editorial Committees, and they would be composed of our peers. There would be an Ames [Aeronautical Laboratory, Moffett Field, California] rep and a Cleveland [Ohio] rep [from Lewis Flight Propulsion Laboratory]. Generally, since I was from Langley, those would be the two Centers that would go through and make comments. When they got through and you incorporated their comments to Jim or Gillis' satisfaction, then it would go to another Editorial Board consisting of English majors, who would then check the dictionary to find out whether you used the word right or not.

I recall one incident. An apogee, which is the highest point on there. I had one rocket experience where you fired the rocket up to a high altitude, and then it pointed down from its apogee and fired more rockets to get high speed. I think it was the first time that the woman had

ever heard of the word apogee, and she challenged it as to whether it was being used right. Fortunately, it was, in my case, because I had looked it up in the dictionary ahead of time.

So you went through that type of thing, and then finally it was final typed in paper-type text and figures. Now, the figures would be handmade and lettered with Leroy lettering. I mean, it was a terrible, labor-intensive job to write those reports. Nowadays you can put them on the computer and a word processor, and they're something that wouldn't take very long at all.

ROSS-NAZZAL: Did you make the figures yourself?

KEHLET: Most of them, yes, and that's what our computing pool, or computers, as we called them, were women there that were mathematicians, for the most part, and they would help make figures and things like that.

ROSS-NAZZAL: Did you type the reports yourself—

KEHLET: No.

ROSS-NAZZAL: —or did you have a secretary pool?

KEHLET: No, we had a typing pool that would type the report. Good Lord. I used one of the reports as a master's thesis, and had to get somebody to type it, because you couldn't have any errors or anything else. I really admire those women that could type without making mistakes.

On my word processor now I use the two-finger system, and fortunately, it can correct everything.

ROSS-NAZZAL: When you were working on this research, were you working at all in conjunction with the military or with the aircraft industry?

KEHLET: Yes to both comments. Working with the military, one of the projects I had was the F-104, which was the Starfighter, a Lockheed airplane. It was secret at the time. The Air Force questioned the Lockheed's estimation of drag on the vehicle, so they built two models, both of them secret, brought them to Langley, and I was assigned as the engineer to put them together with a rocket system and launch them at Wallops Island [Virginia]. There was the truck going up to Wallops with a model on it and all wrapped up with guards and everything else, and we flew the model, and the results were fine. The Air Force got the results, and I published a report on it. So the second model was destroyed, because they felt the first one was good. So that was working with the Air Force.

With industry, to some extent with General Electric [GE]on the reentry vehicles. That was later in my career, and how I got into blunt bodies is another story, which maybe we can talk about later on. But worked with GE on the shape of reentry, the ICBM [Intercontinental Ballistic Missile] reentry.

So I've had both, working with that. Then later on, after Apollo was awarded to North American, before I left NASA, I was the engineering representative out at Downey [California] for NASA to answer questions that North American might have.

ROSS-NAZZAL: You mentioned that your work with Lockheed was secret at the time. Did you have to get security clearance to work on that project?

KEHLET: Oh yes, I had a top secret clearance. You're right. Yes, they went around interviewing, and I know I got a call from my uncle and [he] said, "What are you doing? The FBI [Federal Bureau of Investigation] asking me all sorts of questions about you, including how long had I known you." He thought that was kind of humorous, but anyway.

ROSS-NAZZAL: How long did it take for you to get a secret clearance, do you recall?

KEHLET: I think it took about six months. I got the clearance before that particular program, and I'm not sure why, but anyway.

ROSS-NAZZAL: I'm wondering if you can talk a little bit about Wallops Island. That's where you did some of your work. How did you get out to Wallops Island?

KEHLET: Basically you flew in an airplane from Flight Research right there at Langley Field. We had a Grumman Goose, and it landed in the water next to Wallops, taxied up to the ramp, and then you got out and carry along—we had kind of screwball rules at that time. All of a sudden somebody decided that engineers going up there shouldn't fish, and it was a great fishing place. So we no longer took fishing poles with us, we took measuring rods. They just happened to have a reel on one end of it. But getting to Wallops Island was by airplane. They didn't have the causeway built at that time. It was pretty isolated. They had a bunk building and a cafeteria

building. It was manned by ex-fisherman type of people, and I always admired these people that did the cooking and the cleaning and things like that for it.

People were always trying to see how they could save money. I know in our own PARD there was one individual that decided he didn't want to pay the I think it was a dollar a night you had to pay for your room and board, plus you had to pay for meals. But your room was a dollar a night. So he took his own sleeping bag and tent with him and slept on the beach. This caused a great deal of consternation by various people that was this going to set a precedent, so the ruling was, "I don't care whether you sleep on the beach or whatever you do, you're going to pay a dollar a night, anyway." So that kind of put a stop to that.

But there were all sorts of things. Wallops Island was a great place. They have lots of wildlife on it. I know I'd go and there's not much to do, so the guards would say, "Well, I have to make a nightly run," and it would be about ten o'clock at night, and I'd go ride with them, because you'd ride down the place and you'd see fox and all sorts of animals there at night.

So once you were on the island, you kind of had to stay there. Now, the people came to the island by boat, and one time the boat did blow up, and some people got injured. I remember that.

The other thing that happened on Wallops Island was Derwood [A.] Dereng lost a hand, anyway. He was up there. Let's see, they had pulled the external power from the model so it was on internal power, and then for some reason the rocket didn't fire, and so in order to save the batteries on the model, they wanted to put the external power back in, so Derwood climbed up on the zero length launch platform we had and tried to put the external power plug back into the model plug. Somewhere along the line in the process of doing that, the rocket went off, and the stabilizing fin from the rocket came along and hit his hand and knocked it off.

Well, that meant we had a real problem, because some way you had to get the external power to internal power, and at that time what we had was just a plug. You pulled the plug out, and it went on internal power. So we decided to put switches on it, and that was a screwdriver blade type switch on the side of the model, and there were two of them, one to turn the hydraulic system on and one to turn the external power on. Well, you couldn't climb up on the platform anymore to do this, so we invented a ten-foot screwdriver. Have you heard the term, "I wouldn't touch it with a ten-foot screwdriver"?

The job I had at the time, with my models, anyway, was to stand on an air stand ten feet away from the rocket and turn first the internal power on and secondly the hydraulic system. Moving that screwdriver, ten feet long, from one slot on the side of the model to the next slot to turn the power on, you'd kind of jiggle around there, and the countdown was going on all the time. In sixty seconds we would turn it on, and if you didn't get the system turned on by T-minus-ten seconds, they would stop the count and go on. But we were always kind of taking care of—and I'd jump off this stand and run behind the concrete barrier, and pow off would go the rocket. So we had numerous cases like that.

One of the famous pictures I liked was the one of Jim Parks lining up the wings of his model with the booster fins. There he was standing at the very end with the model pointed right at his stomach, looking down there and lining up the wings, and I thought, "Boy." That was before Derwood's accident. Anyway, that was kind of interesting, and I always kidded Jim about that.

ROSS-NAZZAL: Can you walk me through one of those aircraft tests that you would work on: the preparation in advance, the test itself, and then what occurred after the test.

KEHLET: Well, as I mentioned before, it was pretty much an individual's job. We built most of the model at Langley, which was kind of nice to see it being built, the nose on the model being spun by craftsmen in the shop and the cylinders being cut and grooves put in them and things like that, including the wings. Many times the wings were built of aluminum and wood, or later on, all aluminum. So there was a lot of going down to the shop. "How's my model coming along?" type of thing, and seeing, "Well, that's pieces of my airplane."

Then it finally got put together in the Assembly Room, and then we shipped it over to the IR&D. Let's see, I don't remember—something—oh, Instrument Research Division, I guess it was. They would install a telemetry system in it. Then we'd bring it back to PARD shop and we'd put in the hydraulic system in the tail section and put it all together.

Then we would do influence coefficients with it, which is holding the model rigid and flexing the wing to see how flexible the wing was, because ideally we'd like something absolutely rigid, so there was no aerodynamic relief on the wings, so you'd get a true idea of the lift and drag coefficients. So after that then the model was put on a sort of a cradle and you swung it to get moments of inertia of the model and the weight of the model. Then it was taken down, put on a crate, and pictures were taken. They'd always have somebody who would stand next to the model with that twelve-inch ruler and a picture would be taken to give some idea of the size of the vehicle.

Then it was shipped up to Wallops, and once you got there, then any type of pyro[technics] , if there were. Some of our models used pulse rockets to make the disturbance. Others had hydraulic systems to flex the tail up and down in a square wave type fashion. I remember when it finally got all put together, and then there was the job of taking the file, and

you'd go around—all the screw heads, you'd file them down. They're nice and neat, and you'd put putty in the screw slots and things like that, and get the model polished up right.

Then it was mounted on the rocket and the telemetry system checked out while it was on the rocket. That was kind of interesting, because the rocket was tied to the wall of the room that you were in. It was an explosion-proof type room, and of course, you couldn't come in there with anything that would cause a spark or anything else. The telemetry system would be checked out, and then it was put on a truck and brought down to the launch pad. We had what was called a zero length launcher. It was pointed at an angle and the whole vehicle only moved an inch, and it was free, so there was no tube or anything else like that.

We shot a lot of rockets like that. After a while, you could get pretty good at watching the thing. I know once in a while the model would break up, for a lot of reasons—the fin would come off of the rocket or something—and there would be confetti all over the sky. You could see that. I remember being up there several times with people that hadn't had that experience, and the model would break, and I'd say, "Oh, it broke up."

They said, "It did?" Then pretty soon you see all the pieces come down.

The other thing you had to watch for was where that rocket would go after it separated from the model. Every once in a while the rocket would decide to peel up and come back to the launch site, and so if you're standing out there taking pictures of the thing, you kind of had to watch it to make sure that the rocket landed in the ocean, not near you.

Sometimes, every once in a while, somebody would get the idea, "We ought to recover the models," and would try to stick a parachute somewhere in the system. I remember Purser, in particular, Paul, said, "We don't recover the models. It isn't worth the effort. Once they hit sea water, the aluminum is going to corrode, anyway, and it would be just too much of a job to

recover it and tow it back in and refurbish it." So the answer was, "No, we don't recover the models." So somewhere out in the ocean is a whole seabed full of now disintegrating aluminum type thing. Every once in a while the rocket with the fins would wash up onshore, and we'd recover the rocket, but it was just scrap was what it was at that time.

ROSS-NAZZAL: I have some more questions for you about Wallops. When you went out to Wallops, how long would you stay?

KEHLET: Generally three to five days at a time.

ROSS-NAZZAL: You mentioned you had to spend a dollar a day for your meals and your place to stay.

KEHLET: Just to sleep.

ROSS-NAZZAL: Was it like a hotel or was it barracks?

KEHLET: It was more barracks. It had bunk beds, three high, and the showers. The cafeteria was nice, basic food type of thing.

ROSS-NAZZAL: And what would you do at night when you weren't running tests or working?

KEHLET: Well, you'd go fishing or go with the guard to make his rounds or work on a report type of thing, and play cards, I think. I think we played cards.

ROSS-NAZZAL: How many men can typically be out there at once?

KEHLET: Oh, not very many. Probably four or five, typically. They had their own programs, projects, going on.

ROSS-NAZZAL: Tell me a little bit about Langley Field. Were you involved in any sort of organizations out there or any softball teams or intramural sports?

KEHLET: We had a softball team. Max [Maxime A.] Faget and I were pitchers, usually on opposing teams. And we had a model airplane club. That's one thing, in spare time I built model airplanes and flew them. I had a roommate. That's at a time when two men could live together, and it was okay. We went to school together at Illinois, and both of us took jobs. He went into the Structures Division, and I went into the Pilotless Aircraft. But the two of us would build model airplanes, and then on Sundays go fly them there at the field, usually in the ramp at Flight Research, because there was a nice big concrete ramp there, and you could take the U-control type airplane models and fly them.

ROSS-NAZZAL: Were there any social activities sponsored by the folks at Langley?

KEHLET: Yes. After I was married and Lois came down to Virginia, we had activities of dances and things like that, which was very nice. That's where I met Max Faget's wife, Nancy, who was just an absolutely charming person, who saw the two of us there as we were brand-new young kids, and came over and sort of chaperoned us and made us feel really at home. She's a very, very gracious lady.

One of the things that I maybe hadn't mentioned is Langley hadn't hired anyone in five years. I was one of the first people to be hired after almost a five-year moratorium. The net result was practically everybody there was at least five years older than I was there. Most of them were World War II vets, which I was not. So they were much more established than these newlyweds that came down to Langley. But they were very nice.

ROSS-NAZZAL: I'm wondering if you can talk about the community at Langley and then the community of Hampton, As you pointed out, you were newlyweds coming to this area, and there wasn't much in the area. Can you talk about that some?

KEHLET: Well, they did have a number of women's club. Lois, being a university graduate, joined the AAUW, the American—what is that?

ROSS-NAZZAL: American Association of University Women.

KEHLET: University Women, right. That was quite a bit of social life just from that organization, and also the women's club. So the people, I thought, at Langley, the NACA people, were quite friendly, so we had a very active social life. The city itself, Hampton, was

sort of like a fishing town type of thing, and I don't recall too much activity that was going on by the city itself. Most of our social life was NACA.

ROSS-NAZZAL: I'm wondering if you can talk about any sort of professional organizations you belonged to while you worked for NACA.

KEHLET: Well, one thing I did was go to night school and get a master's degree from the University of Virginia [Charlottesville, Virginia]. I remember when I graduated, my feeling was, "I don't want to go back to school ever," you know, "I've had enough of this."

Lois and I were married about a year after I started, and then about a year after that, Lois said, "You really ought to get a master's degree." So the university was offering classes which were taught by NACA people. We had a number of doctors there that taught the courses in fluid mechanics and things like that. So I went to night school for two years, and then we went to Charlottesville for two summers and spent—I think it was four weeks you had to spend the residence time at the university. And then waited for six years and eleven months before the seven-year deadline to take my orals and turn in my dissertation or, actually, turn in the paperwork, and got my degree just a couple of weeks before the deadline of running out of time. Procrastination.

Well, it was the middle of the Mercury Program, and I thought, "Yes, I've learned everything I needed to know from getting the degree."

But again, Lois said, "Hey, if you start something, you ought to finish it," and she was right. Later on I recognized that having the papers showing you had completed it certainly helped a lot when I went to North American.

ROSS-NAZZAL: Was it typical for NACA to offer classes?

KEHLET: Yes. It was encouraged to go to it. I think when we went to the summer school classes at the University of Virginia taught by outside professors, that I would say 95 percent of the class was NACA. We really made the summer school.

ROSS-NAZZAL: How long did that summer school last?

KEHLET: It was either four or six weeks long, I don't remember which. At first, NACA administration said that they would provide half-salary if you would go there, which I thought was a pretty good deal. Well, it turns out, they paid full salary, anyway, both summers, so that was really neat.

ROSS-NAZZAL: Would they pay for your tuition, as well?

KEHLET: No, but they did pay this salary.

ROSS-NAZZAL: That's really nice. Then you didn't have to take vacation.

KEHLET: Yes, that's right.

ROSS-NAZZAL: That's a big plus. We talked about your papers before. What about presentations at NACA? Did you have to present papers?

KEHLET: Well, I presented different things. Particularly when we started the Mercury Program and the astronauts came there, then we had a lot of press come through, and they would have open house at the Center, and I always got a chance to make presentations about something on Mercury or on the pilotless aircraft before that. See, that was where I got the speaking experiences and things like that.

NACA was a tremendous learning thing. In retrospect, I think back of what I got from the University of Illinois, which I'm very proud of and appreciative of, but the final finishing school, the professional school, was NACA.

ROSS-NAZZAL: What are your memories of Sputnik while you were working out at NACA?

KEHLET: Well, I remember living in Hampton, and we had a couple of kids by this time, and having the television program interrupted with news that the Russians had launched Sputnik, and my thought process was, "My gosh, what in the world is happening?" So when the Space Task Group was thought of, and I don't remember whether it was formed at Sputnik or not, but Sputnik was what, '57?

ROSS-NAZZAL: Yes.

KEHLET: We might have started the Space Task Group. But anyway, I just felt like, "Man, we've really got to get on the ball and do something about letting the Russians get up there first." But it was fascinating to learn about it, that just a couple pound, little ball they put into space; why can't we do that?

ROSS-NAZZAL: Before we talk about NASA, I just had a couple more questions regarding NACA, and anything else you'd like to talk about. How did you get around Langley Field? Did you walk or take a bike?

KEHLET: I mostly walked, if there were meetings at Flight Research. Generally it was between PARD and Flight Research and Instrument Research, you just walked between the places, and it was a good walk. If you had to go to the other side of the field, for whatever reason, over on the Air Force side, you might say, then you would drive over there, drive your car over to that place. I don't recall buses being around at that time.

ROSS-NAZZAL: Where was PARD located on the Center?

KEHLET: Well, it was on the west side of the field right near the big water tank that was up there. In fact, we used that water tank, dropped several models of Mercury to check the low-speed aerodynamics, you might say, of it.

One of the things I think I might mention is how strange, at times, coincidences seem to shape your whole career. Shortly—well, within a couple of years after I started with NACA, our models were going higher and higher speeds. We were getting up to Mach one-point-five and

Mach two. About Mach two the angle of attack indicators would disappear. The channel would just go dead during flight. After two launches, like the first one we thought, "Well, it was a mechanical failure." The second one, we hit Mach two and the angle of attack indicator disappeared, that signal.

I was assigned the job of what went wrong with it, and being fairly new out of college and knowing some thermodynamics, I calculated and said that the angle of attack indicator was being burned off, that the aluminum was just low-cast aluminum, and the temperatures were getting high enough on the thing that it was weakening it, and it was just disintegrating, structural failure on it. So it took a little convincing of some of the people that this was really the case, but out of that came the job, "All right. Then figure out how you're going to replace it."

So at that time the Polaris shape was in the news, so I was trying the Polaris shape, and then they got [H.] Julian Allen's paper out of Ames about blunt bodies and their stability, and so I was trying some blunt bodies on the front of this. I was using the supersonic wind tunnels at Langley to do these tests. What it was, was you put the model of whatever you had on the angle of attack sting, so all the mechanism was there. The only thing different was the shape of the sensor itself. Put it in the wind tunnel and run it at Mach six, ran it there, ran it in transonic wind tunnel.

Got a whole bunch of data on it, and it occurred that what I had was a shape, and I had different Reynolds numbers, which was the energy of the flow of air on this thing, and that might be interesting. So I compared it to Allen's stuff, and sure enough, I had some experimental data that was theoretical, and so I wrote a report on it, which immediately became secret, because it was the entire aerodynamic characteristics of reentry vehicles, different shapes on it. Out of that came the tap on the shoulder by Purser saying, "Would you like to go join a group going to put a

man in space?" So I became head of the Aerodynamics Group, a young kid. I think I was the youngest one in the group, and I was the boss. But it was out of the angle of attack type thing that led to the blunt body that led to the Mercury type and the Space Task Group.

ROSS-NAZZAL: Let me ask you just a couple more questions before we talk about the Space Task Group. Did you have a chance to work with any members of the other Centers, excluding [periods] when you were working on reports?

KEHLET: No, I don't recall too many until the Space Task Group was formed. Then yes, but before that most of it was the report-type interfaces.

ROSS-NAZZAL: Do you think that there was any sort of competition between the Laboratories?

KEHLET: Oh, I think there were. As a matter of fact, it got pretty bad later on, and I think that's one of the big problems that the Administrator has, the overlap between what was happening out on the West Coast with Ames and the [NACA High-Speed] Flight [Station, Edwards, California] there, versus the Flight Research at Langley, and defining the roles of the various Centers. Initially it wasn't that bad, but as things became more complicated, yes, there was competition.

ROSS-NAZZAL: If you had to look back over your NACA career and pick just one thing, what would you say that would be your most significant accomplishment?

KEHLET: Oh, I think that story I just related about the blunt body leading into the Mercury shape, so I have a patent, along with six other people, on the Mercury configuration, and my contribution was the shape of the nose and the afterbody on the Mercury.

ROSS-NAZZAL: What do you think was your most challenging milestone while working for NACA?

KEHLET: Well, I think the transition from building the hardware and testing it to using more and more outside vendors and working with them, and that particularly was true with the Apollo Program, the start of the Apollo Program. Otherwise, it literally was a learning experience and, for the most part, a hobby that I got paid for.

ROSS-NAZZAL: Let's talk about the transition from NACA to NASA. Can you talk about that and its impact on your career?

KEHLET: I don't know. We sort of had a joke of NACA versus NASA. If you take the C on NACA and put one line on it, it's cents, and you go to NASA and you put two lines on the S, and that was the difference between the two. I think that a lot more money came in with NASA, much bigger responsibilities, it caused a lot of travel, because we went from in-house build and fly to going to industry. I think some of the things that happened which were not all that good was the divorce rate went up, and people were then told, "We're probably going to move from Langley [Research Center, Hampton, Virginia]." A lot of people felt uncomfortable about that.

Of course, being from the Midwest, it didn't make much difference to me. I liked Virginia, but any other place could also be all right.

ROSS-NAZZAL: You became part of the Space Task Group, one of the original thirty-five members. What was your reaction when you were asked to become part of this elite group at that point?

KEHLET: Oh, I was delighted. I thought this was fantastic, particularly since Sputnik was around there, and this was going to be our answer to the Russians, as far as the technology is concerned. So it was a chance of a lifetime.

ROSS-NAZZAL: Some of the books I have read about the Space Task Group have indicated that some engineers at Langley discouraged young engineers from joining the Space Task Group and suggested that their careers might be put at risk. What do you think of that?

KEHLET: There's no doubt about it. It happened. My boss—I won't mention his name—told me that this was—well, the Administrator called it a circus stunt to put a man in space. My boss said, "You know, this place is not going to make it. I think you ought to think more than once about whether or not you want to go, because when it does fail, there isn't any job for you back here." Well, at my age in the twenties, that was so what? I could always find a job somewhere else. So I took it. Yes, it was a threat, but I don't think it discouraged anybody. I don't think it caused anybody to say, "Well, I better stick with Langley." Not that I know of, anyway.

ROSS-NAZZAL: As I understand it, while the NASA bill was circulating through the House and the Senate, you had started working on what was known then as the Manned Satellite Program. I wondering if, at that point, was that a secret program?

KEHLET: No. No, I don't think it was secret. No, I wouldn't say it was secret. I think there was a lot of enthusiasm about it, and then moving—we moved out of PARD over to the east side of Langley Field—I think that's where the hours started to get long. That was the other difference between NACA. NACA was an eight-to-five type job, and what work you took home with you was in your brain more than anything else, and thinking about it. When we started the Space Task Group, man, then five o'clock was not a deadline to get out of the buildings.

But I have to tell you that, because later on when I came back to Langley after being out at North American as the NASA rep, I was Joe Shortal's assistant, and we were working on various programs. I stayed in my office to about five-fifteen or something, because time didn't mean all that much. I came down, and the door was locked. I couldn't get out of the building. I had to call Security, who came out with a key to let me out of the building, and they said, "The building gets locked at five o'clock. You ought to be out of the building at that time."

I said, "What is this, anyway?" Anyway, I learned from then that you don't stay past five o'clock, and that was NACA type of timing, even though it was NASA at the time.

ROSS-NAZZAL: So people were encouraged just to work their normal eight-to-five hours, not necessarily work any overtime?

KEHLET: Right. Yes, that's right.

ROSS-NAZZAL: Actually, that brings up another question for me. What was your typical workday like at NACA? Obviously, working from eight to five, but can you generalize [about] what would happen during those eight hours?

KEHLET: With NACA?

ROSS-NAZZAL: With NACA, yes.

KEHLET: It was a combination of a desk job and walking the shop and working in the shop. It depended on whether you were writing a report. Now, generally it wasn't like you spent three years and then you started a new one. You always had models in the pipeline. So you could be writing a report on one flight while the other one was being built or over in IR&D getting a telemetry system or something. So there was a whole sequence of events. So typically it was you get to work by eight o'clock, and you'd sign in and get to your desk, open your desk, and then start the day's work, which, as I said was report writing or walking the shop or something like that.

Now, lunchtime, we would have contests, paper airplane contests of who can get the paper airplane to fly the furthest. We ended up getting ten-foot-long copper tubing from the shop and putting rubber bands on it and using that as a catapult, and then taking the paper airplane, delta wing type thing, and putting a paper clip in the nose and fasten that on with staples, and then using that as a hook. And we'd stretch that thing out to see if you could make that airplane fly the whole length of the hall, all the way from one end of the building to the

other, you might say. We kept hitting the venetian blinds in the end, and Joe Shortal got a little upset at times because we were wrecking the blinds, and somebody was going to have to pay for it.

ROSS-NAZZAL: Did you still have these kind of contests when you were working for the STG?

KEHLET: No, I don't think we had time for that. No.

ROSS-NAZZAL: Why don't you talk about your typical workday for the STG. Obviously, you mentioned you were working very long hours, and you were committed to this new space program.

KEHLET: It was a lot of fun, in certain respects. What was bad about it was Max Faget on Sunday afternoon would always call up and say, "Al, ha, ha, ha,"—he'd kind of laugh—"there's an airplane leaving at eleven o'clock tonight in Washington [D.C.]," and here we were in Virginia. He was going and, "I'd like you to come along." So that meant the bag had to be kept packed most of the time for a two- or three-day trip. Lois would obviously not like our Sunday afternoon being interrupted, but certainly very supportive of the whole program. That would happen during the week.

Then during working days we were working on the Mercury configuration, and the configuration that we first came up with was a cone—looked like an inverted ice-cream cone—and found out it was a very unstable type of thing. So we were trying various nose radius, and we're using the vertical spin tunnel at Langley for these testings. This was the tunnel designed to

check spin characteristics of airplane models, and we were using it to check the low-speed static and dynamic stability of these blunt shapes. We finally ended up with two configurations. One was the inverted ice-cream cone with a very long cylinder on the back end, which made it stable, and the other was put a parachute. Well, we ended up with the parachute on the back.

So there was a lot of building models and testing them in the wind tunnel and things like that to get whatever aerodynamic characteristics we could. Then we built in-house a number of models, the pad-abort full-scale rocket model, which the first time we shot it, one of the inserts came out. We had machined the nozzles ourself. The insert came out of it, and the thing tumbled, and immediately we had all the critics all over our back about the design of the thing, whether it was feasible and things like that, so it took a lot of explaining. Fortunately, we recovered the rocket, and we said no. McDonnell [Aircraft Corporation] had proposed that, "Well, let's put the rockets on the side of the vehicle for abort rockets." Max and I persisted and went back, and the next time we shot it, it worked like a charm.

The only thing about that, as I remember, is the pad abort. It was in the wintertime. We were at Wallops Island, and the Navy had a helicopter. I was going to ride the helicopter out to sea as they took pictures from the ocean side of this launch, thinking that here I was in a business suit, and my Navy friends were dressed in wet suits with life jackets on there, and I didn't have a life jacket or anything else, and the water was pretty cold, supposedly. But the shot went off all right, and the helicopter came back. But I thought it was kind of amusing that the Navy wasn't worried about me in particular. [Laughs] But that was one of them.

The other thing that happened was what we called Big Joe, which was another pad abort. Oh, that was the Apollo Program, not the Mercury. That was later on.

ROSS-NAZZAL: Let's talk a little bit about the Mercury capsule design. Did everyone work on all aspects of the capsule design, or were you assigned certain aspects, and how was that determined?

KEHLET: Well, basically it was broken up into various things like aerodynamic structures and things like that. The Aerodynamics Group I had, basically, after the basic configuration was decided, and the basic configuration was—Max and I had an interview with I think it was *Life* magazine, and they published quite an article about the Mercury design. Well, we started with a sphere and put a man inside of it, and then wrapped the rest of the vehicle around that, the blunt shape and the cone coming back type of thing.

So my group was primarily responsible for the launch aerodynamics of the vehicle, when did you jettison the escape rocket, what kind of acceleration would the astronaut feel if they had to make an abort at various altitudes, how low can you be before parachutes didn't have a chance to open. And then the whole reentry type of thing with the dynamic and static stability of the vehicle. How could we make sure the blunt end would get around first and you didn't enter backwards? Because the vehicle was stable in both directions. It could enter backwards and fly just as well as it could with the blunt shape.

So we had that type of thing. The structures people took care of structures, and then we had the guidance and control people, and all of the systems inside, the environmental control system and so forth.

ROSS-NAZZAL: Who worked with you on the aerodynamics?

KEHLET: Well, Denny [Dennis F.] Hasson, that I think you talked about, was one of them. Bill [William W.] Petynia was my deputy, and Bruce [G.] Jackson and other people were all in that group, and they were all excellent aerodynamicists. So we did it ourself. Then later on when the contract was awarded to McDonnell, the McDonnell people did the aerodynamics, and we would cross-check stuff. The wind tunnel testing, McDonnell sent representatives out which would go with the wind tunnel people, and my group had the wind tunnel responsibility, also.

ROSS-NAZZAL: Did you start out primarily using theoretical calculations, or did you start using things like boilerplates to determine when you can jettison [the parachutes]?

KEHLET: No, we did the theoretical work, primarily, on all of that, with the exit or launch configurations and all the way through to reentry. Then we checked it against wind tunnel results and checked it against the boilerplate type vehicles, too.

ROSS-NAZZAL: Do you recall any sort of significant changes that had to be made as a result of either calculations or wind tunnels or the boilerplate tests?

KEHLET: If we talk about Mercury, the most significant thing was—let's see, I think it was towards the last flight on the Mercury. We had a fire on board and shorted out a lot of systems on Mercury, and—gosh, I'm trying to remember whether it was [L. Gordon] Cooper [Jr.]; one of the astronauts that flew that mission. It had a tremendous effect on Apollo is what really happened, but that was the primary change was the fact that the wiring inside the capsule could no longer be exposed. It had to be protected from the perspiration and the salt that would

normally come from perspiration of the astronaut. It would actually get in there and in under 100 percent oxygen environment, would cause a fire. Almost anything would burn.

ROSS-NAZZAL: Were you involved at all with the review of applications for the design and building of the Mercury capsule?

KEHLET: Yes, I was on the source selection subpanels to go through the various companies' proposals that came in for Mercury. In fact, I made one of the presentations to the companies about what we wanted in the Mercury configuration.

ROSS-NAZZAL: What was it you were specifically looking for, do you recall?

KEHLET: As I recall, it was just outlining what was expected from the aerodynamics. How much wind tunnel testing and things like that would have to be done, and the schedule, just saying that we had to do certain characteristics and configuration freezes by certain times in order to make the schedule.

ROSS-NAZZAL: At any point when you started working on this Manned Satellite Program first and then what became the Mercury Program, did you ever have a feeling that it might not succeed, that no one had ever done this before, and perhaps it might fail?

KEHLET: You know, we were young and egotistical and felt that we could do anything. When we showed the schedule, we said we wanted to fly this manned vehicle by 1960, I think it was,

somewhere around that time period, General Electric I remember saying that, "You're about three or four years off. You're too short."

We said, "Nah, we can do it." And I know that the first Mercury that got shipped down to the Cape [Canaveral, Florida] from McDonnell Douglas was equated to say that hey, it was nothing but a bushel basket full of parts, which wasn't quite true. I mean, the vehicle certainly was there, and many of the systems were. But it was a case of where we designed, developed, and built the thing all at the same time. Normally you go through each of those phases separately, but this was a let's do it all in parallel, rather than in series.

ROSS-NAZZAL: How much time did you spend working with McDonnell?

KEHLET: I spent a lot of time with them. I went to St. Louis [Missouri] several times. John [F.] Yardley was the fellow in charge of the program, or at least had a lot to do with it, and John and I became very good friends. Later in life, I went to work for John running the Tomahawk Cruise Missile Program out of St. Louis.

ROSS-NAZZAL: What sort of work would you do with McDonnell? Were you primarily supervising their work and double-checking everything?

KEHLET: Right. We'd get a tour of the factory to see where the vehicles were being built and the meetings would be general meetings. Where all of the disciplines—how are you doing structure-wise? How are you doing system-wise? How are you doing from a wind tunnel standpoint? So it would be a general meeting just putting all of the systems together.

ROSS-NAZZAL: Were there any sort of problems that you encountered out at the factory or with the contractor at any point that you recall?

KEHLET: No, I think McDonnell did a good job on it. They had very good people and well-talented people, and so I can't recall any real problems.

ROSS-NAZZAL: Did you do any work with the ABMA?

KEHLET: The ABMA—

ROSS-NAZZAL: The [Army] Ballistic Missile Agency. At the time, were you doing any work with them?

KEHLET: No, I don't recall it.

ROSS-NAZZAL: Did you do any work down at Cape Canaveral?

KEHLET: Well, yes. Going down there, getting ready for the launch, we would be there after the launch to take care of the flight performance, read the telemetry records, and things like that. One interesting experience I had was being way up in the top of the gantry and a workman was putting the adapter between the Atlas rocket and the Mercury capsule, and the adapter—bolting it to the Atlas. And he kept having to climb out of the pit he was in to get a tool. So I was

standing there, and I could see he needed a wrench, so I gave him a wrench, and got a union grievance written up against me. I wasn't supposed to do that. That's just one of those things.

ROSS-NAZZAL: Did you have any involvement with the Little Joe?

KEHLET: Yes. Who built that, Convair [Astronautics Division]? They built Big Joe. I have a little model of Little Joe. That was a test on our parachute system, and as I recall, it worked pretty well.

ROSS-NAZZAL: Do you recall anything else from that day?

KEHLET: No. The only other time that we had a problem was when we had the Mercury on—what was it—the Redstone, down on the Cape, and the booster lifted an inch off of the launch pad, and the engine shut down, and the booster dropped back down on the launch pad. Fortunately, it stayed up, but because the engine cut off, it armed all the systems in the Mercury capsule itself, and so the capsule said, well, we're up at altitude, because the booster cut off, so we jettisoned the launch escape system, and when that fired off, it armed all the barostats that said, well, we're at low altitude. We ought to shoot off the parachutes. So here we had a whole sequence of events of the rocket lifting off, shutting off, dropping down, launch escape tower blasting off, and then the parachute, drogue chute, popping out, and the parachute's lying on the side of the vehicle standing up.

I was at Langley, and Max was down at the Cape and called up and said, "What would happen if we fired the retro rocket on the thing?"

I had some thoughts about it, and then I said, "Max, we do that, the thing is going to tumble and going to look stupid and the parachutes won't open. They're just going to flail around. It might even blow up the rocket. So the best thing to do is probably just leave it alone."

So they sent a crew out there and detanked the thing and stabilized the rocket. What happened was the rocket umbilical got fouled up some way where the ground to the umbilical got released before it should have and sent a circuit up to the rocket to shut off the engines. Now, that was a case where it was "I told you so" to Marshall [Space Flight Center, Huntsville, Alabama]. Now, there was a little more than just a slight competition between Marshall and Langley.

ROSS-NAZZAL: Can you talk about that?

KEHLET: Well, my personal opinion is that [Wernher] von Braun really wanted to run the whole program, and [Robert R.] Gilruth was assigned, Bob Gilruth, was assigned to be the space type of leader, and I think von Braun resented it. So there was a lot of feeling down there. Like the launch escape tower, I had a cartoon of that. Marshall was saying, "We need to know the characteristics of that tower, the whole vibrational characteristics and everything else," because it would affect their engine controls on the booster. So I had a cartoon, which one case was looking at the whole rocket system from on the ground, looking upward, and the rocket disappeared into the sky with a little tiny point up there called the launch escape tower. Then next to it was a picture of looking down at the rocket from the launch escape tower, and there was this big launch escape tower, coming all the way down to a little tiny rocket at the bottom. And I said, "There's two views on this thing. Our view is the launch escape tower is nothing,

and your view, of course, is that it's going to control the whole vehicle." But that was just sort of the feeling. It was all resolved, but there was a little bit of a feeling about the Marshall guys.

ROSS-NAZZAL: Did you work at all with von Braun?

KEHLET: Not personally with him, but some of his people, yes.

ROSS-NAZZAL: What were some of the biggest challenges that you had? You mentioned, obviously, working with Marshall could be difficult. What were some of the biggest challenges you had while working on the Mercury Program?

KEHLET: I think the unknown that we had. You know, we got a pig one time. We were going to launch a pig, and put him in the cradle and started monitoring him, and the pig died. One of our secretaries was a farm girl, and she said, "If you'd asked me before you had the pig in there, I would have told you that you never put a pig on his back, that the belly fat on there will suffocate the pig." And that's exactly what happened. So we went from pigs to monkeys, and the monkey was kind of interesting, too, because he would get an electric shock if he didn't perform his duties rightly.

I'm drifting away from your question, but I think the concern about controlling the vehicle and getting the right aerodynamics on the vehicle for reentry. To such an extent, when John [H.] Glenn [Jr.] flew in the Mercury, I had asked that John turn off all jets and let the vehicle oscillate, because I wanted to find out just how good our aero characteristics were, and we could tell from the oscillations the static stability and the rate of decay or rate of expansion on

the dynamics. Of course, it was voted down. "We're not going to take the first flight and do it because some dumb aerodynamicist wants some data on it."

Well, as I recall it, John, on entry, tried to keep the spacecraft at a certain angle of attack, and was using up and did use up all of the reaction control system fuel. So the vehicle did rock; it did oscillate. And it diverged exactly what we had predicted it to do, and then the parachutes came open and stopped the oscillations. But I thought, "Well, we did get our data after all."

ROSS-NAZZAL: Did you have any role to play during the missions?

KEHLET: No, except for listening to what was going on. But, no, it was mostly doing the analysis afterwards.

ROSS-NAZZAL: And what did that involve?

KEHLET: Well, all of the records, the telemetry records, like did the launch escape tower jettison when it should, and things like that. I remember the launch escape tower, I put three legs on that, and the theory was that they all had explosive bolts. You had to get that launch escape tower off of the vehicle, because that covered up the parachute system and everything else. So it was a single-point failure that had to make sure it worked. So we had two explosive bolts for each leg, and the theory was that if two out of the three explosive bolts worked—the legs, two out of three—the third leg would just break, break off, when the rocket fired, which seemed to be the case.

Well, it turned out that running wind tunnel tests on the thing, you had to run the launch escape tower with one leg into the wind, and then you had to turn around and get the flat. I thought, "Man, if you had four legs on that thing, you wouldn't have to do something like that. You could have sort of a symmetrical aerodynamics." So on Apollo I put four legs on it, and the explosive bolts turned out to be very reliable and didn't have any problems. That's all I can think of.

ROSS-NAZZAL: What did you learn from the first mission, from Alan [B.] Shepard [Jr.]'s mission?

KEHLET: Well, that was kind of interesting. Alan wanted a parachute, and I kept telling him, "Why do you want a parachute?"

"Well, just in case the thing enters backwards and doesn't do right. I want to be able to climb out."

I said, "Look it, here's the calculations. If it enters backwards, there's going to be so much pressure in the hatch, you can't open it."

And he said, "I don't care. I want to parachute down there, anyway."

So we said, "All right."

And then I thought, "Maybe what we ought to do is not wire up the abort system on the vehicle." But anyway, that was—it would never happen that way, but we had all sorts of schemes, you know, a bit of humor on the thing. But I thought putting a parachute on the thing was kind of dumb, but it satisfied the astronaut, so the parachute went on it.

One of the things that came out of Mercury that affected Apollo, another one, was that Mercury was a ballistic entry, so because the manufacturing couldn't guarantee that you'd get the center of gravity exactly on the center line, that we rolled the vehicle deliberately. So the vehicle, on entry, did have a slow roll and sort of wobbled around its point down to the predicted impact point, where the ships could be for recovery.

We came along in Apollo, and we were trying to figure out how to put controls on the thing to control down-range, cross-range, up-range, type of thing, and first we were going to put fins on the back end of the vehicle or something, and then it occurred to me that, "Hey, we put the spin on Mercury because of the offset. Why don't we just offset the center of gravity deliberately?" Then the vehicle would pull lift, and all you had to do was roll the vehicle to the different attitudes, and you'd get cross-range or being away or down-range or up-range, depending on how you rolled the vehicle. Rolling the vehicle was very easy, because you had no damping and roll. That's the system we used on Apollo, and I guess that was one of my claims of fame to the Apollo Program was the guidance system using roll control.

ROSS-NAZZAL: While you were working on the Mercury Project, what sort of contact did you have with the astronauts?

KEHLET: I had a lot of contact. The big fight with the astronauts was the instrument panel display. Every astronaut wanted his own little peculiarity, it seemed like, to such an extent that Bob Gilruth assigned an engineer and an astronaut to go figure out what the panel was going to look like. I was the engineer and John Glenn was the astronaut, and so we put the panel together.

Right or wrong, that's the panel everybody had to use. We probably did all right, because everybody seemed to be upset about it.

ROSS-NAZZAL: What was included on that instrument panel? What did you determine would be necessary?

KEHLET: Well, where the switches would go and where the rate gyros would go and where the attitude gyros and things like that on the panel.

ROSS-NAZZAL: Did you have any other sort of contact with them, in terms of providing them some guidance on how the systems worked or giving them lectures on Mercury?

KEHLET: No, I don't recall too much interface with that. We had a whole training section that was going through that stuff. The input I would have would be to the training session, what the aerodynamics would be, so running any type of simulators that would get the data. But I didn't have that much contact with the astronauts.

ROSS-NAZZAL: You mentioned you were interviewed by *Life* magazine. What was the press interest like in the Mercury Program itself?

KEHLET: Oh, I think it was very high. Basically it was just the story of Mercury, how the configuration evolved. That's about what I remember. I think I have a copy of it somewhere hidden in boxes someplace.

ROSS-NAZZAL: That would be a nice memento to have. Were you interviewed by any other magazines or newspapers?

KEHLET: Well, yes, the local newspaper in Joliet, Illinois, did quite an article several times, on the Mercury Program and the Apollo Program. Local boy does good or something like that.

ROSS-NAZZAL: That must have been exciting. Max Faget passed away just last year. What are your memories of working with Max Faget on Mercury and also in PARD?

KEHLET: Max was a fantastic individual. We were friends, both socially as well as professionally, and Max would have a hundred ideas a second, of which probably he could discard ninety of them after first conversation, but ten of them would be good. He would walk around—he would pass you right in the hall. He'd look at you and pass right on by, and man, Max didn't see you at all. His brain was going around a thousand miles an hour type of thing. He was a fun guy to work with. He was my boss during the Space Task Group.

ROSS-NAZZAL: When did you officially stop working on the Mercury Program?

KEHLET: Well, I'm trying to remember. Long before the first shot, even, we had an advanced design group going as to what the follow-on to Mercury ought to be, and we had already picked the Moon. So there was a small group. Kurt Strauss was the leader, and Bob [Robert O.] Piland,

who was Max's deputy, sort of put it together, and I was in charge of new configurations and had a group of people looking at how I could do this.

There was a whole progress of the Apollo. It started off, it was going to be two people, and then it ended up being three. The configuration changed from a small reentry vehicle that was directly landed on the Moon type of thing, so it just flew to the Moon and it came back. It was a small reentry vehicle with a living module behind it. We even looked at Earth rendezvous, and then looked at lunar rendezvous, and finally the decision was made, after the contract had been given to North American, by the way.

North American had the contract for almost a year before the decision was made of what was the mission going to look like. It caused North American a lot of trouble, obviously, not knowing what the requirements really were. There was this not complete struggle going on, but a difference of opinion whether there ought to be direct lunar descent or whether there ought to be lunar rendezvous or Earth rendezvous.

But finally when the decision was made to go lunar rendezvous, then the configuration changed to one of make the vehicle big enough to put everybody in it and forget the living module, because there was a problem with what do you do with the LM [lunar module] in that case. How do you hook that on if you've got a living module between the spacecraft and entry vehicle. So Caldwell [C.] Johnson did a good job, and he was a fantastic drawer. He could sketch things out that really looked neat. I wish I had that artistic talent that this guy has. So a lot of it configuration-wise came from Caldwell.

But we had three study contracts on the Apollo. General Electric had one, and Martin [Company] had one, and Convair, General Dynamics, had one. We would do what was called the red-eye express. I remember we would get on the airplane and fly up to Valley Forge

[Pennsylvania] for General Electric, leave that night and go over to Martin Company and have a meeting there, and then fly to San Diego [California] and then have a meeting the next day there, and then fly back at the red-eye type of thing. They'd get back, and then you were expected to go to work the next day. Here you probably get four hours' sleep during the first three days of this marathon. We did that about once every couple of months type of thing.

ROSS-NAZZAL: Let's take a break for a second. I need to change my tape.

[Tape change]

ROSS-NAZZAL: We are back with Mr. Kehlet, and I'd like to ask you about your work on the New Projects Panel. Who was involved in that panel, do you recall?

KEHLET: Well, Kurt Strauss was the leader of it, with Bob Piland, also, and I had the configuration part, and most of the people in my Aerodynamics Group were assigned part-time to do this in addition to the Mercury Program. We came up with several configurations, one of which you had mentioned, the lenticular vehicle. I liked the lenticular—that flying saucer type thing. It appealed to me because it could reenter just like Apollo—or like Mercury does, except it had the capability of landing horizontally. What it had was retractable elevators and rudders on the back of the vehicle, and the saucer entered like the Mercury, and then the fins unfolded in back, and it took a horizontal position at about Mach five, and then landed on its bottom. It didn't even need landing gear, although, in retrospect, it probably should have had it.

Max didn't like it, because Max wanted to use parachutes. He thought that making something, an airplane type thing, would be too complicated, and he probably was right. But anyway, we pursued the lenticular and did a lot of wind tunnel testing on it, and it looked pretty good. Then Max came up with the criteria: it had to land in sea state four, which are huge waves. So we had a basin, a water basin, there at Langley, and they created a sea state four wave panel, and we shot a model of the lenticular into the waves, and man, it hit like a pancake and rolled on the waves and then flipped upside down and landed upside down. So that was sort of the death knell.

Well, when Shuttle came along, I remember teasing Max about, "Well, Max, are you going to make this sea state four, also?" The answer was no, because you couldn't put the Shuttle down on sea state four. It just wouldn't launch in sea state four. So it wasn't until ten years later that that requirement got relaxed. Otherwise, we would have had a lenticular Apollo rather than the Apollo shape we have.

ROSS-NAZZAL: Was that the only problem or challenge that you encountered with the lenticular?

KEHLET: Well, that's as far as we'd gone. There might have been other problems with a retractable system, but I got a lot of support from the other Centers, Ames [Research Laboratory, Moffett Field, California] in particular; and the high-speed, [NASA Flight Research Center], Edwards [California]; and even Langley had some lifting bodies that they were proposing for the Apollo. There was no question that Max didn't like the lifting bodies. He wanted to have parachutes on there. He didn't want this horizontal landing.

I suspect that one of the reason that Max won in putting that configuration on the Apollo we have is because there was a lot of work done in lifting bodies, and if we had gone to a lifting body, the requirements or the responsibility for the vehicle could have gone to that Center, and I think maybe—my own personal opinion is that by that time Johnson [Space Center, previously called the Manned Spacecraft Center, Houston, Texas] didn't want any other Center to be working on the vehicle, because when the—well, anyway. So there were a lot of things, but the Apollo certainly, configuration of the ice-cream cone inverted, was the simplest of all designs.

ROSS-NAZZAL: What were some of the other topics or questions that the New Projects Panel visited?

KEHLET: The number of people on the vehicle, the type of mission, the sizing the boosters required, the number of stages that might be required to go on that. At that time it was you take off from the Earth, and you go to the Moon, and you land on the Moon, and then you lift off from the Moon and come back to the Earth. We hadn't analyzed it deep enough, I'm convinced, to know whether we should do like Apollo had to do, or eventually did do, and that was it actually went into Earth orbit for a coast period. Then the S-IVB fired a second time to go to the Moon. So I think the whole mission type thing is what we looked at.

But the rocket got pretty big, there was no question. Even the size of the vehicle landing on the Moon and taking off from the Moon with the reentry vehicle, with all this thermal protection and everything else for the coming back to Earth, the question was, well, how do you land that thing and make sure it doesn't tip over in the process of landing on uneven surfaces, but we weren't sure what the surface of the Moon looked like. At that time I recall there were

people that were predicting there was a twenty-foot dust layer on the Moon, and you land on the Moon and it was going to be buried in dust. Surveyor proved that not to be the case, but in the early days, we weren't sure what the lunar surface really looked like.

ROSS-NAZZAL: You mentioned that you were looking at the number of people who might be on this mission. Do you recall how that might have changed over time?

KEHLET: I think we started out with two, initially, just go to the Moon. But then when the LOR [Lunar-Orbit Rendezvous] came into being, then it was recognized, hey, you probably need two people to go to the Moon, but you ought to have somebody tending the vehicle, the mother ship, you might say. So it ended up being three.

ROSS-NAZZAL: When you looked at the type of missions, what sort of issues were you looking at?

KEHLET: Radiation on the way to the Moon; where you should pick samples type of thing. Basically, the whole mission.

ROSS-NAZZAL: And were you working with other scientists who had background in these fields, like radiation or geology, or was it primarily just this group?

KEHLET: Well, it was primarily the group. We weren't that far along on the vehicle, and it was recognized at that time there would be a lot less hardware being built by JSC or Langley than

there had been on the Mercury Program, and that's one of the reasons I left NASA was that the emphasis was going to be on contract management and not on hardware, and I still had the feeling, well, I liked to see hardware being built and design it and build it.

ROSS-NAZZAL: When were you assigned to work out at Downey for NASA?

KEHLET: Well, it must have been about 1961. Might even have been 1960; somewhere in that time period. Right after North American won the contract—by the way, that was kind of interesting. I was on the Source Evaluation Board, and I thought North American did a terrible job on theirs, and the Martin Company, in my opinion, had a far superior product, both design and capability. But we had a couple of people on the panel, Walt [Walter C.] Williams, in particular, who worked with North American on the X-15 Program, and he thought the world of a couple of people there, Charlie [Charles H.] Feltz, by name, and—my memory slips me now—the leader of North American. I'll think of him later. And was very persuasive. In fact, I had to go back and justify my score, my panel score on the vehicle. Well, I didn't change anything, but the feeling was that whatever North American was weak in, NASA could help and straighten them out, which was probably true. So North American got the contract.

So I went out there to—whatever I could do. I remember going down, and they were designing the launch escape system on the vehicle, in the group, always, at one time, and I remember the guy, the supervisor, looking, and he said, "So you're Al Kehlet. You're the guy that put four legs on this thing." [Laughs] It was quite a—well, we can talk about the transition, because that was kind of interesting, going into industry.

ROSS-NAZZAL: What were working conditions like out at Downey, compared to working at Langley Field?

KEHLET: I think industry doesn't recognize a time clock, for the most part, and industry always has the attitude, well, you're on salary, and that means you're 365 days, 24 hours a day, available. That isn't true in all cases, but in the aerospace, I think the aerospace industry is probably the most intense of all industries, as far as expecting a lot. At that time, anyway, the salaries were the highest of any industry, so once you get paid for it, you were expected to work pretty hard for it.

ROSS-NAZZAL: When you were working out there for NASA, what was it like?

KEHLET: There was a lot of respect for NASA by the people, and just being a NASA rep gave you a lot of—I found I could go anyplace I wanted. There were no secrets or anything else. I was invited to any meeting I wanted to go to, anything else. It was very good.

ROSS-NAZZAL: Were there any problems that you noticed at North American that perhaps foreshadowed what occurred in 1967?

KEHLET: No. The fire was a terrible thing. I think North American got blamed for an awful lot. As the roles shifted from building a lot of this stuff at Langley like we did in Mercury to being contract administrators, NASA still had a lot to say about the technical details of things, and weight was always a problem. As you know, or maybe you don't, we actually built two vehicles

for Apollo. We built a Block I, which had a half a dozen vehicles which were far too heavy for the boosters, which were coming along, to ever be used in the mission. Most of the vehicles you see in the museums and things like that are either boilerplates or Block I type vehicles.

Block II was a real vehicle. That's the one where we were to go to the Moon, we had a weight requirement to meet. The fire occurred in a Block I vehicle, and we never launched one. So walking around on canvas covers for wires, and using Kapton wire without recognizing the problems with the insulation on Kapton.

I learned a lesson on that thing about Kapton wires, and used it many years later when I ran the Tomahawk Cruise Missile Program. Guess what kind of wiring that Tomahawk had? Kapton wiring. One of the vehicles, cruise missile, failed on the final checkout, and it was found out that the wire, the insulation was cracked on it, Kapton insulation on the thing. So we had to change the whole procedure on how to put the wire panels together for the missile because of the Kapton wire.

I was talking earlier about coincidences and things. Everything in your life will repeat one time or another, it seems to me, at least in the career.

ROSS-NAZZAL: How many people were working for NASA out at Downey when you were out there.

KEHLET: Oh, probably three or four.

ROSS-NAZZAL: And were those people all engineers, or were they from different aspects, like Public Affairs and Procurement?

KEHLET: Most of them were engineers. In fact, there might have been one or two contract-type people, but most of them were engineers.

ROSS-NAZZAL: What was the expectation for you while you were working out at Downey?

KEHLET: Well, from NASA's viewpoint, straighten out North American. From North American's viewpoint, get NASA to make decisions quicker. So it was a two-way street.

ROSS-NAZZAL: How often were you in contact with people at NASA?

KEHLET: Oh, probably daily.

ROSS-NAZZAL: Were you in contact via telephone or correspondence?

KEHLET: Yes. Mostly telephone.

ROSS-NAZZAL: And who were you mostly speaking with?

KEHLET: You know, I don't remember. Well, let's see. I guess it was Aleck [C.] Bond, and Piland, Bob Piland, would be the people I remember.

ROSS-NAZZAL: What were some of the issues that North American wanted resolved from your end?

KEHLET: Pick the mission was a big one, because as I mentioned before, the LOR hadn't even been finalized at that time. I think North American was way behind the power curve, initially, as far as the talent, getting the talent together. They had a bunch of internal problems. As an example, the Los Angeles Division which built all the airplanes, the F-86 and the F-100 type airplanes, and Downey was a Space and Information Systems Division type thing. It wasn't even a full space type of thing.

Again, I'm trying to remember the name of the North American guy that made a commitment to NASA that North American wouldn't have to hire people because we had all this talent at the Los Angeles Division. Well, Los Angeles Division didn't want to lose their talent, so they made a moratorium, and that was you couldn't transfer. If you wanted to go work on that space program, you had to resign or retire, quit. What that meant to the individual is they lost all of their seniorities, which was more important towards retirement than anything else. So all of a sudden they had no seniority at all.

A lot of them came to Downey to do that, but they timed it to go get people, anyway. So NASA said, "We're not going to give you any money for recruiting, because you have this pool." That hurt North American getting the talent together, so that was not good. So there were a lot of startup problems, really, really startup problems, with confusion on the requirements and confusion on talent and things like that.

When I was being processed by North American to be an employee, there were two people in front of me, and I said, "Oh, who's your supervisor?" They didn't know. I said, "Well, what do you mean, you don't know? What are you going to work on?"

"Well, we're not sure yet."

I thought, "How could you possibly get a job with somebody when you didn't know even what discipline you were going to work in?" I think North American was out shotgunning the employment.

ROSS-NAZZAL: Did you know who you'd be working for and what branch?

KEHLET: Oh yes. Yes, I did. Yes. I ended up being the technical staff to the guy that was in charge of Apollo systems. He died shortly after I—he had a heart attack and he died. I don't remember his name. That's too bad.

ROSS-NAZZAL: I understand that you worked on LOR with Chuck [Charles W.] Matthews. Can you talk about that?

KEHLET: Well, I don't recall that much working with Chuck. I remember knowing Chuck in the Space Task Group, and I have a tremendous respect for his technical abilities, management ability. He was a good guy. As I recall, most of the work that I did was on the direct lunar landing, and although I did work on a couple of committees, the Fleming Committee being one of them, which was the direct lunar landing, and one of the other committees in Washington. I was in Washington the day that the President [John F.] Kennedy made the announcement about

we're going to go to the Moon. We had just finished up our committee work on the size of the boosters and things like that. What we did was worked with the Marshall people, and I was the rep from the Space Task Group, to determine the number of stages we're going to have, and the size of the stages. So that was kind of interesting.

ROSS-NAZZAL: You had been doing all of this work prior to hearing Kennedy's speech. When you heard Kennedy's speech, what was your reaction?

KEHLET: Oh, I thought that was great. We didn't have much space experience, by any means, but so what? The big thing was going to be to line up this contract. Who were we going to give the contract to? Because the studies were already going on. So Kennedy made it sound like we were going to start from scratch. Actually, we had about three years of work on configurations already done through both our in-house studies and the three contractors we had, so by the time he made his speech, the configuration was established. The command module was going to be the entire living quarters, and the lunar landing module was going to be attached to it, and so forth.

ROSS-NAZZAL: That's a different perspective. I know in the history books you often read that people were shocked. We'd only flown Alan Shepard for a fifteen-minute flight, and how were we going to go to the Moon. Here you'd been working on these studies for quite some time.

What was the feeling like at STG when they heard that they were going to be moved out to Houston?

KEHLET: Well, first of all, Hampton was a small community, and moving 200 families out of the thing would devastate the real estate market, and so actually, moving down to Houston was known probably before the public announcement was made, because we sent a team down there. I was part of the team that went to Houston. I don't remember the name of the buildings that we had, but we went down there. Flew down there, and there must have been a crew of maybe ten or twenty people that went down and looked at the various building sites and things like that in Houston.

The people in Houston really, really welcomed us. We went into a restaurant one night, about ten of us, and the waitress said—she could tell by our accent we were not Texans—"Where are you from?"

We said, "We're from Virginia."

"Virginia? What are you doing here?"

"Well, we're looking to see about setting up this space group."

She said, "The Moon men are here! The Moon men are here!" And they were very, very good, very gracious.

ROSS-NAZZAL: What did you like most about Houston when you visited?

KEHLET: Well, there wasn't much that I liked, in all honesty. I thought, "My god," you know. Lois' father is from Texas, and we'd had a couple of trips to Texas after we were married, and I thought, "Well, it's all right." But my exposure to Houston was in the summertime, and I thought, "Man, it's probably as cold as Texas in the winter and hot like this in the summertime." I had second thoughts about it.

But the real thing that happened was when we lived in Virginia, our oldest son was playing across the street where they were building houses, and fell on some ice and slit his wrist and cut five tendons and the nerve. Fortunately, our next-door neighbor was our obstetrician, and he hadn't gone to work yet. Lois took Rob over there—and I was at North American at the time—took him down there, and the doctor that looked at his hand said, "Well, if it were my son, I'd take him to Duke University [Durham, North Carolina] and get it fixed."

Well, there was no reason to stay at North American, so I transferred back to Langley, and I was Joe Shortal's assistant, so I was Assistant Division Chief to Shortal while we were undergoing this operation at Duke, which took several trips down there, because one time Rob gets sick on the way, and we had to cancel the operation. He fully recovered from it, but while I was at Langley, I was not very happy at all the second time. The whole work ethic type of thing was almost back to NACA, and it just didn't have the spark or the interest. It seemed to me that, well, if you don't have to do something till tomorrow, don't worry about it today.

Now, that's probably unfair to a lot of people at Langley, but I got a call from Charlie Feltz at North American, and he said he had talked to one of the NASA guys that was out at North American at that time, because by this time they'd set up an official office. Anyway—Gray, I think his name was—Charlie had talked to him about what procedure he had to go through to get a NASA employee hired, and said that, well, you had to talk to Gilruth and so forth. So anyway, Charlie called me up and said, "We'd like to offer you a job and go to California."

I thought, "Well, I think I'd rather go to California than to Houston, and I certainly would rather go to either one of them rather than stay here." So I said, "Well, I don't think it's quite all that simple."

He said, "Well, why don't you talk to Gilruth about it."

So I did, and Bob Gilruth said, "Well, I'd rather have you work in Houston, but I'd rather have you work on the program than not at all." So I took the job with North American.

ROSS-NAZZAL: And so you moved in '62 out to California?

KEHLET: That we did, in August, and I had been at North American during the springtime. It's such a beautiful place in southern California in the spring. We drove down Route 91 in August, and there was dust all over the trees and everything looked like it could use a good rain, which they hadn't had for a long time. I thought, "Is this the same place that I visited before?" But the kids were just starting school, so that was a good time to move. Our oldest was just starting first grade.

ROSS-NAZZAL: What were your first few days like at North American?

KEHLET: Well, it was you didn't leave work at four-thirty or anything like that, you worked. It was going around to the various places. I only stayed in that technical job for a short time. Then I requested and got moved to the Project Office, and was the Deputy Chief Project Engineer. At that time the way the organizations were, project engineering was the interface with manufacturing, and so they were the ones that—project engineers signed all of the drawings to be released to the files and to manufacturing. It was quite a responsible job, and one of the reasons I got it was because I did have the hardware experience out of NACA and NASA.

So shortly after being Deputy Chief Project Engineer, I got assigned to do the design work on Block II. So here now I'd carried Apollo from the conception of the various configurations at NASA to being able to actually do the design work on there. So I led a design group of several hundred engineers to go through the Block I vehicle and fix it.

Now, what happened was on Block I, North American accepted the contract with a spec [specification] that read something like "We will build a vehicle and go to the Moon." Well, North American wrongly assumed that NASA was like the Air Force, who pretty much left North American alone, and North American would tell them when they needed money—not quite this way, but almost—tell them when they needed the money, and would tell them when the product was ready for acceptance by the Air Force. And they expected NASA, the same thing. Well, it was a big surprise to find out NASA was going to send troops in, and you were going to have technical reviews every two weeks and design reviews and contract reviews and everything else. Contract changes kept flowing in, and North American said, "Hey, this is out of contract."

They said, "No, it isn't. Read the contract. You're going to go to the Moon. This is to go to the Moon."

So North American ran out of money and started to use their own money to keep the program going while they fought this contract thing. Well, on Block II, we started going through what are the problems with Block I. Well, the contract was a big problem, so we wrote a specification that specified—

[tape interruption]

ROSS-NAZZAL: You were talking about writing a specification.

KEHLET: So we wrote a specification that was pretty detailed, including the control documents for the subcontractors. Not only that, but we made an actual drawing list, which Block I didn't have. One of the other things that was done was we invented a vehicle called EM^3, to Engineering Manufacturing Mock-up Module, EMMM, and that was—one of the systems—engineering had a mock-up, which they would build to the engineering drawings released at the time. Manufacturing had a mock-up, which they would take the engineering drawings and make them into manufacturing drawings. The two mock-ups, the engineering mock-up was probably six weeks behind the engineering. The manufacturing mock-up was probably six months. And the net result was there were changes all over the place.

So it occurred to me, "Why don't we put the two mock-ups together and, since we sign the engineering drawings as the engineering,"—by that time I was Chief Project Engineer—"we sign all the drawings, we'll just have the one mock-up and make sure it's up-to-date." Well, the Chief Engineer and the Operations Vice President weren't all that good friends, and so the big problem was a political one of convincing the two of them that we could actually make this thing work. Finally they agreed to it, and we built this thing. The first Block II vehicle that went into the systems area for checkout had less squawks on it than the last Block I vehicle had, which was the sixth or seventh vehicle.

So it was a fantastic invention, you might say, and when they came around and said, "We're looking for cost-saving items to enter a contest," or some darn thing like that, I submitted this thing, and they came back and said, well, they couldn't figure out any tangible cost, because it was such an advantage over the Block I that they didn't know how to price it. So that worked

out all right. So after that Dale [D.] Myers, who was the Vice President of the program, formed a Spacecraft Group, and there would be Assistant Program Managers, and each one would have a vehicle. So we started with the thermovacuum vehicle went to Houston, and the vibration vehicle went both to North American and to Houston.

So we started with Apollo 7 was the first Earth orbit vehicle, and so the Program Manager, or Assistant Program Manager, was assigned to that, and then Apollo 8 was the ring around the Moon type of thing, and Joe [Joseph W.] Cuzzupoli was named that. Then Apollo 9, I don't recall who was head of that; and 10 had Bud Benner [phonetic], and then I was assigned Apollo 11. I knew that could be the lunar landing vehicle, the first one that would actually land on the Moon.

But the reason Apollo 10 didn't land on the Moon was the LM was not the lightweight LM, so LM was running behind the command module, as far as the weight reduction. So Block II command modules and service modules were ready before the LM. Otherwise, if the lightweight LM had been there, Apollo 10 would have been the lunar landing. Nine was the checkout, the rendezvous docking with the LM, and of course, we didn't have a LM available at all for 8, so 8 just went around the Moon, proved the systems really work, and the guidance system. And the roll control system worked, which was great.

So it was very good fortune there to feel in my career that I started with the concept of the Apollo Program, all the way through designing the actual ones that went to the Moon, building it, and then being in charge of the one that landed on the Moon.

ROSS-NAZZAL: Let me go back and just ask you a couple of questions. What impact did Frank Borman and his Tiger Team have on the design of the Block II?

KEHLET: Well, tremendous. But not all good, necessarily, in my opinion. The Program Office at Houston did a good job of keeping Max Faget and his crew from changing things. As soon as it started to become apparent that the spec we wrote for Block II was a pretty tight spec, whereas on Block I, practically everything was within contract. Now, Block II, it turned out that not much was in contract, and so North American, we got schedule relief and money from schedule changes.

So the Program Office in Houston then started putting the squeeze—Joe [Joseph F.] Shea, in particular—putting the squeeze on Max's group to cut down these goodies. One guy I remember saying, "I want the latest technology to be on that vehicle when it goes to the Moon." Well, there was a three-year time lag for most of it between the time it was technology and what you could get on the vehicle, so making that statement was stupid. But that was not necessarily typical, but it was close to it. So Shea's office put a crimp to it.

Well, after the fire, the floodgates opened, and Frank Borman's group came in there, and every one of these goodies that some of the engineers wanted got socked to us as being flight-critical, and that was the key word. You had to put "flight-critical" behind all these changes. The very fact, though, that we converted, and the first Block II vehicle was almost ready to be shipped to Houston for the thermal vacuum test when the fire occurred. We were that far along with Block II. We had about six in the pipeline, and the first vehicle was ready to go to Houston.

The fire occurred, and that was in January, and we shipped that vehicle with all of the modifications, everything else into it, less than a year. I think that's pretty remarkable. Look how long the Shuttle's been down for something. So it was a recognition that time was critical, but also I think there was a lot less nitpickers on there. There was probably more willingness to take a risk to get the thing within the time schedule than there would be today. I think we have

too many QC [Quality Control] and safety people that outnumber the people doing the actual work.

ROSS-NAZZAL: What impact did it have on the program when Joe Shea left and George [M.] Low came on board as the Program Manager?

KEHLET: Didn't have too much at all. George Low was a good guy. Shea was a good guy. I knew George Low from Lewis [Research Center, Cleveland, Ohio], and John [H.] Disher, a couple of really pretty fine individuals.

ROSS-NAZZAL: What are your memories of Apollo 11, since that was your vehicle?

KEHLET: Well, went down to the Cape when it launched, and got on the airplane after launch and went to Houston, and was at the lunar landing type thing. I wasn't in the control room. We had a lobby of a motel down there, and most of the North American people were there. That was where we saw the lunar landing type of thing. Well, of course, concern about coming back. It was only after the pickup of the crew and everything else I felt, man, we really did it. We brought the samples back and all three guys came back without any mishap.

We did have one scary thing during the mission on Apollo 11, on the way to the Moon. The next vehicle, which was Spacecraft 109—it was the second vehicle, and we had 107 and 108 came along, and 109 was in the pipeline. In final inspection, it was determined that the parachutes were not tied to the vehicle, the parachute risers, and so there was concern. My god, you know, command module 8 was okay, but what about 7 on the way to the Moon? Well, I had

in my private file that I brought with me to the Cape closeout photographs of the whole thing, and so I had a closeout photograph of the area where the parachute was, and it was definitely tied. So that sort of took care of that thing, but that was the kind of scares that were going on.

Apollo 11 had less anomalies in their flight than any of the Apollo things, which I think was because I stole every good part I could find that went in that vehicle. When one of the vehicles had a problem and they wanted to steal my engine, I told them no, and we took it off of Spacecraft 109, which turned out to be Apollo 13. But that had nothing to do with the mission.

Right before the launch at the Cape, there was a meeting on what was going to be the follow-on to the Apollo Program. George [E.] Mueller made the statement. North American was betting it was going to be a Space Station, because it was obvious. We had all the Apollo hardware. You could use the Apollo modules, the crew module, to go back and forth to Earth, and the Apollo system. So you could put a Space Station up there.

A fellow named Bill [William D.] Bergen, who was the Division President of North American, said, "Al, you're going to be the Chief Engineer of the Space Station when you come back to California." And George Mueller made the statement I would be in the Shuttle. Bergen said, "Well, I correct myself. You'll be Chief Engineer of the Shuttle." In all honesty, Apollo was like working on a race car; the Shuttle was like working on a truck. It was all right, but it didn't have the excitement, by any means, that the Apollo Program had, so it was a letdown.

ROSS-NAZZAL: So what was your role in the Space Shuttle Program? Other than being the Chief Engineer, what were those requirements and duties?

KEHLET: Well, I had a whole Engineering Department, and so all of the configuration studies and the launch studies, systems, and everything else, was done by that. It was a dynamic program initially. In order to win the contract, you had to win—after winning Phase A, which was won, I guess, in 1968, the company had to beat out Grumman [Aircraft Engineering Corporation, and McDonnell Douglas [Corporation] and Boeing [Airplane Company] were entries into the contracts, so if you won A, then you had to win B, and then we had to win B Prime, and then B Double Prime, and then finally C, which was the hardware contract. So each one of those episodes, you were writing proposals and answering NASA's questions at the same time.

So, in the meantime, the Apollo Program was winding down, and trying to get talent from Apollo, as they were winding down, to come over to the Shuttle was a problem.

ROSS-NAZZAL: Why was it a problem?

KEHLET: Well, NASA kept asking questions, so there was always a hook, you might say, or a line attached to the guy that would come from Apollo. When I need him, I want instant response type of thing. Well, we had our own problems, too, so you couldn't always guarantee that the person would be available on an instant basis.

ROSS-NAZZAL: What were some of the challenges that you encountered while working on the Space Shuttle during these various phases?

KEHLET: My personal opinion was the Shuttle was too big, but the real problem was that all of the professional organizations wanted the Shuttle. AIAA [American Institute of Aeronautics and Astronautics] went out strongly for the Shuttle. The Air Force wanted the Shuttle. Industry wanted the Shuttle because all the hardware for Apollo was being built, and there wasn't any new money going to come in for big programs or anything else, and the industry needed a program. So it was like who's going to win this. You want to be a billionaire? Who wants to be a billionaire? The winner of the Shuttle contract. So there was a lot of pressure from the corporations to build the Shuttle, too, as opposed to the Space Station.

The problem was what the Air Force had in mind for a Shuttle and what NASA had in mind for the Shuttle were two different animals. One was an elephant, and the other one was a gigantic elephant, I guess. The requirements were never nailed down, and we went the whole program, from Phase A through practically the hardware contract, where the requirements changed every contract.

The Air Force wanted a big payload bay, and they actually wanted the vehicle to replace the Titan IV, which was a aging hypergolic vehicle. NASA wanted a small vehicle to refuel the Space Station, and the two of them can't meet. NASA wanted a fifteen-foot by twenty-foot cargo bay, and the Air Force wanted sixty feet long or some number like that. And the cross range, the Air Force wanted to be able to launch out of Vandenberg [Air Force Base, California] and come back to Vandenberg on one pass, which meant you had to have almost 2,000 miles cross range. NASA needed no cross range at all. So they said, "Well, we'll just stay in orbit till we line up with the—down at the Cape, and we'll land at that time."

So you had these conflicting requirements which determined the size of the vehicle and the booster. General Dynamics was our subcontractor on the booster initially, and every time we

changed the vehicle, you change the size of the booster, and those poor people were down there really on the ragged end of things. So I think the program was a disaster from Phase One, because the requirements weren't nailed down till the last time, when they were finally nailed down. But you know what the maximum cross range we've ever had on the Shuttle? It was designed for 1,200 miles cross range. Four hundred and fifty miles.

Now, since the thermal protection goes up as the cube of the cross range type of thing, we got more thermal protection on the Shuttle than you could shake a stick at. It's unfortunate that that leading edge got banged up the way it did, because that was just a hole, but as far as the rest of the tiles and everything else, they're way overdesigned.

ROSS-NAZZAL: I see you left in 1973.

KEHLET: That's right.

ROSS-NAZZAL: Why did you make that decision to leave?

KEHLET: Well, I didn't really make that decision. I got sort of helped. It was another one that "I'm going to make you an offer you can't refuse." By that time the Shuttle had come along, we'd pretty well staffed up, and the opportunity was to—well, a fellow named Rockwell, the guy that's the Rockwell of the Rockwells, said he wanted to take three space cadets to go on commercial programs. I was one of the three and assigned to go to the Airplane Division. I didn't even know Rockwell [International Corporation] built airplanes at that time, besides the military airplanes. So I was Division Chief Engineer of the Sabreliner Division.

Then in 1979 they were having a lot of trouble in Bethany, Oklahoma, on General Aviation, Rockwell General Aviation, and I got assigned to go there. It was quite a promotion, but I sure as heck didn't want to have to move to Oklahoma. But Lois agreed, and we had two children still in school at that time, and Jennifer had got moved from school to school in the next several years, but has done okay.

So I went to General Aviation as the Executive Vice President, but the problem was that the airplanes we were building, which was a beautiful little single-engine airplane—was one of the series of airplanes—was costing us $72,000 to build the airplane, and we were selling them for $68,000. Well, it doesn't take a mathematician to figure out you're probably not making any money off of that one, so I stopped the line, and the Flight Department almost came unglued.

So all of a sudden I got inducted into "Why don't you get your pilot's license?" so we had a company airplane, so I flew the dawn patrol, six o'clock in the morning. We'd fly for a couple of hours with the instructor. The corporate rules were you had to have twenty-five hours of dual before you could solo. Well, I had already soloed with a Piper Cub twenty years before that. That didn't make any difference, so anyway, I finally got my license there, and still stopped the production of that airplane, because we had a turboprop that was in huge backlog.

We started making money, and all of a sudden Rockwell said, "We ought to sell that division." So they said, "You're going to St. Louis as President of the Sabreliner Division," my old division that moved now from El Segundo [California] to St. Louis. So I went down there as President of the Sabreliner.

ROSS-NAZZAL: How long did you stay there?

KEHLET: I stayed there two years, and then I went to work for Fairchild as President of the Metroliner in San Antonio [Texas]. Huge increase in salary. It soon became apparent that the Metroliner was a General Aviation type airplane, going backwards again to me; it wasn't all that challenging. John Yardley called up and said he needed somebody to run the Cruise Missile Program, and going back to St. Louis was a benefit to San Antonio, although I like San Antonio, but it's a nice place to visit type of thing.

So we moved back to St. Louis, and I ran the Tomahawk Program from scratch. Competition with General Dynamics on the vehicles; we did a good job on it. Then they were running into problems with that Delta launch vehicle in Huntington Beach [California]. And we've always wanted to go back to California. In fact, I was promised when we went to Oklahoma City [Oklahoma], that it would be a three-year type of thing and we'd come back to California. Well, didn't happen, so Yardley said, "Send you back to California."

At that time Lois was working and Jennifer just graduated from Texas, bachelor's degree, and had a job lined up in the airplane company there, at MacAir. So it was a good time to move. And Steven, our youngest, was just going to start high school. So if you're going to move, that was the ideal time. So I said, "Okay, I'll go if you can also package Lois, who's working here, and Jennifer, who has a job. So you have to do that."

John said, "Fine." So we went to California. I stayed on that program, and it got going fairly well, and they had an opening in technology, and I finished up my career as running the technology for carbon-fiber wings and spars, and all the supersonic transport was in that division. Also, as a sort of a side job, we were converting DC-10s into aerial tankers for the Air Force in foreign countries. We did two of them for the Dutch, and that was interesting. It was back to

hardware again. This time I turned sixty-five and got kicked out because I was a VP [Vice President] and officer of the company. Mandatory retirement.

ROSS-NAZZAL: And you've been retired ever since?

KEHLET: Yes, I did some consulting work for Aliant [phonetic] on their solid-fuel rocket, and I did some consulting work for George Mueller in his Kistler [Aerospace Corporation]. But I find that between all the traveling we've done, and we own a number of fourplexes and townhouse and the house in Tucson [Arizona], that the rental property, just managing it, is a full-time job.

ROSS-NAZZAL: That's what I've heard. Let me just ask you a couple of general questions. If you looked back over your entire career, what do you think would be your most significant accomplishment?

KEHLET: Oh, the Apollo Program. There were a lot of interesting things. The story about the angle of attack indicator and blunt body, the roll control, and even Apollo, of being asked by North American to come to work for them type of thing. I think there were a lot of being at the right place at the right time. Being picked for the Space Task Group. I was the youngest of the youngest in there type of thing, and I thought that was very, very considerate of Purser to pick me, but on the other hand, I guess I was willing to work hard, and so that sort of compensated for the youth, I think.

ROSS-NAZZAL: How old were you when you were selected for the STG?

KEHLET: Well, let's see. That was '58, so I must have been twenty-nine. I think of my kids as they went through their twenties and their thirties. Would I trust them to do that? I guess the answer is yes. Youth has a big advantage, not being afraid to tackle ideas.

ROSS-NAZZAL: What do you think has been your most challenging milestone over your entire career?

KEHLET: Well, I think the Space Shuttle Program was probably the one that was more work than it was fun, and that's because, again, I go back, the requirements. I've done a couple of lectures at the Cal State [California State University] Long Beach [Long Beach, California], and just push, emphasize, you've got to get the requirements done. If you don't have the requirements done, you're lost. That's, I think, been a trademark of mine that you nail down the requirements first, and then you go work on it.

ROSS-NAZZAL: Do you think there's anything that we didn't cover—I know you brought some notes today—for NACA or your NASA career, or even your post-NASA career, that you'd like to talk about?

KEHLET: No, I don't think so. I think we've pretty well covered it. I think the people I worked with at NACA and NASA are just tremendous individuals. Lois had a saying that the people always seemed to do well that worked for me. I had a number of jobs that, including the Cruise

Missile Program, that I had at least three or four so-called deadbeats in the thing that turned around to be darned good people. They just weren't challenged enough.

Trying to not micromanage a job has been the other aspect. I like Jim Parks' attitude. Let you go as far as you can go as long as you don't sink the ship type of attitude, which I think is very good. Having that type of thing, I was extremely fortunate to have somebody like Parks. I don't know if he's still alive.

ROSS-NAZZAL: I'm not sure. Well, I thank you very much for your time today.

KEHLET: Well, you're welcome. We took a little longer than we talked about, but that's okay.

[END OF INTERVIEW]