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RICHARD H. "PETE" PETERSEN
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is July 14, 2014. This oral history session is being conducted with Richard H. "Pete" Petersen at NASA Ames Research Center at Moffett Field, California, as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project, sponsored by the NASA Headquarters History Office. Interviewer is Rebecca Wright, assisted by Sandra Johnson and Glenn Bugos. Thank you so much for coming in today and traveling up to see us. We appreciate it.

PETERSEN: You're welcome.

WRIGHT: We would like for you to start today by sharing with us why you chose to come to NACA to work.

PETERSEN: I got my bachelor's from Purdue [University, West Lafayette, Indiana] in 1956, in aeronautical engineering. I went to Caltech [California Institute of Technology, Pasadena, California] for a year to get a master's, and at the end of that period, I had been in Air Force ROTC [Reserve Officer's Training Corp] at Purdue, so I was commissioned in 1956. I was in a non-flying status, so I was going to be an engineer for the Air Force. The Air Force didn't know exactly when they would call me up, so I was interviewing at Caltech, telling [employers] it might be three months or it might be six months [before I was called to active duty]. I didn't

know. At that time, engineers were very much in demand, and they were taking people [for short periods] and hoping they'd come back a few years later.

I had a bunch of good interviews [which included one with Ames]. I knew just a little bit about [NACA]. I knew about the wing section research from my education time, but also it was in the San Francisco area, and there was a girl I was very interested in, in San Francisco. I thought, "Well, this is only [for a short time]. I wonder if I could do that? I'd be in the area." [I did chase the girl, but I didn't end up with her].

Anyway, I went to the interview, and the interviewer was Dave [David E.] Reese, who was a great guy, and we had a good chat. He said, "Well, we have a program where the Air Force assigns some of their engineering officers to come and work at Ames, and if you're interested, we'll see if we can work that out."

I said, "Boy," because I really sort of had guessed I'd be out at Dryden, [which is now] Armstrong [Research Center, Edwards California]. I still call it Dryden. I thought I'd at least get three to six months here at Ames. This meant I could stay here, so I said, "Sure, go for it." They did, and they got me assigned here.

I spent one day out at an Air Force base out towards the east, which has now gone away [Parks Air Force Base], and I processed in there. I wore my uniform for one day, processing in, and I came to work here. I worked [here] for two and a half years [while] in the Air Force—I didn't make as much money as a civil servant, but that was all right—and then the [Air Force] said, "Well, we've got too many officers." I was in for three years, but after two and a half years, they said, "We've had enough of you," so I went up to a base that's north of Marin County [Hamilton Air Force Base]. Wore my uniform for one day and checked out. That was my Air Force career, and I came back to the same desk here. That's how I got with NASA. I ended up

35 years, except for leaving for a year and a half at one point, and coming back. It worked out pretty well.

WRIGHT: Share with us what it was like, those first days that you were here and how you became part of the organization.

PETERSEN: I've tried to remember, and I don't remember much about it. I'm sure I was a little nervous coming in, but I told Glenn, my first desk was right down here, under [where we are now].

WRIGHT: In this building?

PETERSEN: Yes. I was introduced around. Sy [Clarence A.] Syvertson was my Branch Chief, who was later Center Director, and Al [Alfred J.] Eggers, who is very well-known, was the Division Chief. I came there and I was put in an office with Dave [David H.] Dennis, and he was sort of my mentor for the first couple of years. The best I can remember, the first thing I went into was estimating aerodynamics at supersonic speeds on wings and on aircraft configurations. Then, developed from there into operating in the wind tunnel, which Glenn didn't know about, but there was a small wind tunnel back that way, toward the 12-Foot [Pressure Wind Tunnel], on the second floor, which had a 10 x 14-inch test section, Mach 3 to Mach 6. We would do estimating aerodynamics, build models, and test them in the 10 by 14-inch, and that was what I did for about three or four years, I think. I came in '57, so I was only NACA for a year before the transition.

WRIGHT: You mentioned about designing and building and testing those models—did you have a part in each one of those, or were you assigned to just do pieces of that?

PETERSEN: There were various ones going in there. Dave Dennis would still work with me, but I could pretty well design a wind tunnel model. Go over to the shops, which were right over there, and they had fantastic machinists. They were people who were very, very good machinists who didn't want to be in unions. They came here because here, they had a challenge, it was always something different. I worked with the machinists to build the model and then take it and test it. Or I'd be involved just in testing models that other people had put in there. We tested a Mercury capsule and Gemini and Apollo. I wasn't there when the [Space] Shuttle came along. It was interesting work. I was doing what I wanted to do, which was supersonic and hypersonic aerodynamics.

WRIGHT: Why did those areas interest you so much?

PETERSEN: I loved airplanes as a little boy. I started building balsa models, carved out models, when I was six or seven. [Japanese attack on] Pearl Harbor was when I was seven, so I was into airplanes in the World War [II], and just loved all kinds of airplanes. Knew what they all were. Eventually, built models with engines. I was into airplanes big time.

I remember in my sophomore year in high school, counselors asked, "What do you want to do?" I said, "I want to design airplanes and I want to go to California," because California seemed like the golden state at that time. I knew that was where a good chunk of the industry

was. It didn't turn out too different from that. I loved mathematics, and so, I signed up for aeronautical engineering and I enjoyed all of that. It was great fun. I made very good grades—I guess the Lord gave me the right qualities—and Caltech was a great finish to that. That was a no-thesis master's. It was just a year of courses from some of the greats there. I'll mention the guy, the guy I worked with was Professor Hans [W.] Liepmann, who is very well-known to aerodynamicists, and he was a great guy. Then, I came up here and met some great people.

WRIGHT: What were some of the challenges that you encountered when you were testing in the wind tunnels with these models?

PETERSEN: I think the first challenge was in order to operate this wind tunnel, you had to learn how to adjust it. Particularly if you had a blunt model like the Apollo, you could do Mach 3 and 4, but as you went up, you were moving the second throat, which is downstream, and if you didn't get it right, the tunnel would choke. It goes back to subsonic and your model shakes and you've got to start over. That was a challenge, to learn how to do that. You were there and you were looking at a manometer board, which is for pressure measuring with mercury tubes, to see what the [pressure] distribution [in the tunnel] was. That was challenging. Doing the aerodynamic calculations, but that wasn't too hard. I liked that. I should mention that in the center of the floor [below us was] where the "computers" were, who you may have heard of. [They were] ladies with Friden calculators.

When we had to reduce wind tunnel data, we would essentially write what was a computer program. It was a big, [wide] sheet [of graph paper] and you would fill in the data [in the left columns], and then you'd say, "Multiply column one by column three," and they would

do that, "Divide that result by column five," and they would do that, and they'd work their way [to the right]. That's really what a computer program is, when you look at it in the simplest sense. We would do those layouts and enter the data over here, and they would reduce the wind tunnel data. A lot of the data came off the manometer board, and that was pictures, and somebody had to go in with scales and measure how high the mercury was in each of those tubes. Across the hall, I don't know what the name of that branch was, but Jack [N.] Nielsen was there, who's quite well-known, also. I later worked for Jack, the year I was not with NASA.

WRIGHT: Whenever we read about wind tunnels, especially during this time period, it seems like everybody wanted time within the wind tunnel. Did you find that to be a challenge in itself, just being able to get scheduled for the testing that you needed?

PETERSEN: Not with the 10 by 14 because we owned that, so to speak. That was our tunnel. That was not a tunnel that industry wanted. It was more a research-type tunnel. We basically did all the design work and all the testing in the 10 x 14. After that, in about 1960, I moved to the 3.5-Foot [Hypersonic Wind Tunnel], which was down just beyond the Unitary [Plan Tunnel]. I understand it's been torn down, now. That was also a hypersonic tunnel with a big air heater on the front end. The higher the Mach number, the flow cools, and the higher the Mach number, the more it cools, and you've got have a hot enough temperature [going in] or it'll liquefy—it'll actually turn into liquid oxygen and liquid nitrogen. We were heating the air up to 2,000 degrees as it came into this tunnel. I was around when we were calibrating, but eventually, they did some tests for industry in there, as well as research tests. Later, I was the division chief that had

the Unitary [Plan Wind Tunnel] and the 12-Foot and the 14-Foot [Transonic Wind Tunnel]. A lot of that was industry work.

WRIGHT: Some of the projects that you were working on at the time, were they any way at all secret or classified?

PETERSEN: Yes, a lot of it was classified. The things I remember, as I said, the Mercury Capsule, we tested, and Harvey [Harry Julien] Allen and Al Eggers were involved in that. I never was involved in anything but the testing of that. Then, we had what was designed to be a manned vehicle into orbit, which had a delta wing and a half-circular fuselage underneath it. We did a lot of testing of that. Eventually, that sort of worked around with the Shuttle, but the Shuttle ended up a lot different.

There was a contest going on between Ames and Langley [Research Center, Hampton, Virginia] as to what was the best type of manned vehicle to reenter, and we had flat-top and they had flat-bottom. [There were] big arguments about which was better. I think they were both good. Then, I got involved with the B-70 [Valkyrie], which was the Mach 3 bomber, North American. They were planning to droop the wingtips [down at] about 40 percent of the span in order to get directional stability at Mach 3. It was designed to cruise at Mach 3, a tremendous airplane. I did a test here of the effect of drooping the wingtips.

We started, and I had a model built. It was not exact as far as the fuselage, but it was representative, and then we drooped the wingtips 15 degrees, 30 degrees, 45 degrees, all the way down to 90 degrees, and took data on the effect on lift and directional stability and so forth. That was all highly classified at that time. They ended up using the drooped wingtips. I never knew

whether they really looked at my data—I think they probably looked at my data, just to check against what they were doing. I still have the model in my house. A lot of it was classified at that time.

That was a the WS-110A project that the Air Force had, and they developed a beautiful airplane by the time that the ICBMs [Intercontinental Ballistic Missiles] came into use and Congress said no more money for the B-70. They built two of them, and one of them unfortunately crashed down at Edwards. The other one's at Wright-Patterson [Air Force Base, Ohio].

WRIGHT: When you were first here, share with us about what type of environment it was for learning. You mentioned mentors, you mentioned people that you worked with. Was it a learning environment? Was it an environment where they expected you to already know? Share with us how you were able to gather knowledge from some of those great minds that you worked with.

PETERSEN: I'd say it was a learning environment. Most of it was informal with the people I was working with, and with Dave Dennis, who kind of oversaw things. It was more particular than my education, but there were no surprises in that you just took what you had learned and pushed it further. I think I was pretty quick to pick things up. I was trying to remember, when I looked at this, if we had orientation classes. It seems like maybe we did, and maybe a little of that was toward scientific knowledge, so that you had a little feel for what else was going on around the Center. I don't know if you've ever run into that, Glenn?

BUGOS: Helen Davies ran some sort of orientation, but not everybody took it.

PETERSEN: I think I went to some sessions of that that were just helpful in general areas. Yes, I remember Helen, when you mentioned her name.

WRIGHT: Did you have a lot of opportunities to cross over and meet other people that were working on different areas?

PETERSEN: I would say no. We were pretty insulated here. Even from the branch that was across the hall, we were not heavily interacting with them. My thought would be not much interaction—we were just doing our thing, here.

WRIGHT: I believe there was what they referred to as inspections back in 1958.

PETERSEN: The Centers would get together and give papers.

WRIGHT: Yes. Were you involved with those as well?

PETERSEN: Maybe you have the history—I certainly was at a couple of them, and I don't know if they were when we were NACA or NASA. I was at one here, and then at one back at Langley, where I traveled on the airplane with Harvey Allen. In fact, we had a little trouble with the room reservations, and I ended up sleeping in the same room with Harvey Allen. He was a great friend later on and a great, great scientist and a great man. I think I gave a paper back there. For some

reason, I remember [once] here, and somebody from Langley gave a paper on boundary layers. There came questions from the audience, and they were some pointed questions and some disagreements. [The speaker] got done and he went over to somebody and said, "Who is that?" That was Harvey Allen questioning him and [Harvey] didn't quite agree with his theory. That was kind of fun. I don't remember a lot about those—I just remember that they were happening. I don't know when they stopped, but probably not long after NASA.

WRIGHT: In your role, did you have a lot of research that you needed to do on what other people were doing? If so, how were you able to obtain that?

PETERSEN: I wouldn't say a lot, but we were looking at NASA memorandums and that kind of thing. The stuff we were doing was mostly coming out of NACA and NASA, and not the industry particularly. The library would have copies of Confidential, and we could get Secret when we wanted it. We were looking at some of those that had testing in the same area, some from Langley, particularly, since there was a lot of overlap.

WRIGHT: Was there a feeling of competition?

PETERSEN: You bet, you bet there was. It was interesting when I went back there [to work]. In fact, that's just a funny sideline, but I went back there as Deputy Director, and at almost the same time, Gus [Angelo] Guastaferro came from Langley to be Deputy Director here. All the people were saying, "Why'd they do that?" I think it was good. They needed to cross-fertilize and it worked well for me, but there was competition. There was competition in those inspections

WRIGHT: Sometimes the officials would come through and see what all was going on in the Centers.

Did you feel there was a change when NACA officially was kind of absorbed by NASA?

PETERSEN: I thought about that from your questions, and I would say it was slow. It wasn't anything overnight. We were doing the same thing. Slowly, what had been a small office in Headquarters that ran the whole thing turned into this gigantic bureaucracy, of which the NACA Centers were a relatively small part. Getting budgets turned into a different game, and one of the big impressions over the years is that it was a constant battle. In the NACA, the Centers ran their research programs, they decided, the Center Director decided, and Headquarters did basically nothing but get the money.

We always called it the brown bag approach; just send the money to the Center and let them do their thing. The Centers knew what was going on and they knew how to do it. My career was a constant fight against more and more control from Headquarters. Now, I think the research program is completely controlled from Headquarters, which is not good, in my opinion. That's what bureaucracies do. It was a slow process. We wondered [when we became NASA] if we were suddenly going to lose all our money, but we didn't. In fact, we might have gotten a little more—I really can't remember.

WRIGHT: All those years that you were here, at the beginning and for a good while, you didn't have a lot of turnover for your management. You had the same Center Director. Tell us about that management style that you learned from him.

PETERSEN: Smitty [Smith J.] DeFrance was the Center Director when I came here. He had come out of Langley, so he knew the traditional style, and that's the kind of place he ran. His people proposed, and he and three or four people reporting to him would decide which projects they thought were good and get them funded. I didn't have a lot of interaction with the Center Director at that point. Most of mine was with my Branch Chief, who was Sy Syvertson, who later became Director, and a great guy.

WRIGHT: Talk about his management style, what you learned from working under him for so many years.

PETERSEN: Smitty DeFrance, I really didn't have any relationship with. As I said, primarily, it was just my Branch Chief. Slowly, over the years, as I moved up, Harvey Allen was Director and I began to have a little bit more interaction. I was getting up high enough in the structure to begin to give presentations. Harvey was good, although he really wasn't cut out to be a Center Director. He was a wonderful manager and scientist, and I think he did a good job, but he didn't last too long because he didn't like all the travel and all the politics and bureaucracy, which didn't surprise me. Then, Sy Syvertson—was there somebody in between?

WRIGHT: Hans Mark.

PETERSEN: Hans, how could I forget Hans? Hans came in, and that was a jolt to the Center. People were saying, "Where did he come from and what have we got here?"

He said, "Things are going to be different around here," and boy, everybody was stirred up. I think Hans did a tremendous job and was very good for the Center, finally. He's a great guy and a good friend. He was very proud of the fact that eventually, he was Deputy Administrator of NASA, and three Center Directors that he had brought up at Ames were Center Directors. I was at Langley, [John M.] Klineberg was at Lewis, and [William F.] Ballhaus was here. Hans thought he'd done a pretty good job, and he had, and he's been a good friend and still is. I guess he had more interaction with what was going on, and was more willing to say, "No, I don't think you should do that—you should do something more like this." I think he did a good job with Headquarters, which is the Center Director's main job, trying to keep them off of his research people. Jack [John W. Boyd] and I were talking about Hans [today] because Jack, of course, is very close to Hans, so I got caught up. Apparently, Hans is doing all right. There was Hans and then there was Sy, and I left about when Sy was coming in, I think, around the late 70's.

JOHNSON: He came in '77 as the Acting.

PETERSEN: I left in 1980 for Langley.

WRIGHT: Did your work change quite a bit after Ames became a research center under NASA?

PETERSEN: It did. I wouldn't necessarily say that it was because of the change, but some of it might have been. I went down to the 3.5-Foot and was involved in calibrating that wind tunnel, and really getting it up to be operational, for about three years. There were a lot of other people

[involved]. Calibrating it is not one of the big jobs; it's just a journeyman job, but that involved building test equipment to go in the tunnel, and taking data.

Then, somebody started the Mission Analysis Division [MAD], and Dave Dennis was, I think, running it in the beginning. It started as actually a part of Headquarters and to do computer studies of future aircraft and satellite missions in order to help direct where the research was going. In other words, if we were going to do [a project] like the Shuttle, we'd do a design with the computer and look at all the various aspects of it. That sounded pretty [interesting to me]. In the 3.5-Foot, I had gotten to the place where I was beginning to think I needed to get out of [Ames]. That was because I wasn't really a research type, and I'd learned and I'd done that.

I was beginning to think about talking to industry and getting out, and then this Mission Analysis came. That was a whole new ballgame of studying launch vehicles for the Shuttle mission. There were about five of us in the original Mission Analysis Division, and in '64, I think, Dave Dennis actually went to Headquarters and was the interaction contact. Organizationally, they made the MAD division, part of Headquarters. He went there for a year to be a contact point, and the next year, I went to Headquarters for a year. That was a different experience, to say the least.

I continued with MAD and it grew and it eventually was located across the street, here, on the first and second floor of the right end of the building. We had about 25 or 30 people in the division. I was in the airplane part of it, and there were satellite parts of it and reentry parts. Some interesting people. I worked there for quite a while, about eight years. I became a branch chief somewhere around 1970. I don't know if you want this history.

WRIGHT: That's great because it's all mixing together.

PETERSEN: In 1973, I went to the Sloan Program at the Stanford Business School [California], which you may have run into with other people, but Ames was sending one person every year. That was a great program. There were 40 businessmen from private, government, foreign, and when they sent you to that, you knew they figured you were going someplace, which was nice. I did that, which was a great program.

WRIGHT: Did you learn lots of lessons that you could apply, or was it just the exposure?

PETERSEN: I applied a good deal of it, but I've often said half of what I got was just interacting with the other people in the program who had different viewpoints and they were tremendous people. A lot of it was not the classroom, but I did use some things later on, and it was useful.

When I was almost through with that program, there was a—I don't know whether they called them "Associate Administrators" at that time, but the guy who headed the three research centers at Headquarters—guy named Roy [P.] Jackson from Northrup came to that job, and somehow, he got the idea he wanted me back at Headquarters, for some reason, which was probably [reasonable].

When I had been in Headquarters before, I'd said, "The next time I do this, I'm not going unless I get a raise when I go." I told [Mr. Jackson] that—he didn't want to do that, so we went back and forth. I was still going to the Stanford Sloan, but I went and talked to him, and then Hans got involved. Hans, he was funny, he'd call me in and he'd say, "You Scandinavians are

more hard-headed than us Jews." He was really funny and he said, "Can't I talk you into doing this? You ought to do it," and I was firm and said no.

Finally, Roy Jackson said, "Well, you come or I'm going to fire you," essentially. First thing, he said, "I'll fire you," and I said, "Fire me?"

I had to go to the personnel people here because I'd never heard of that at NASA, and they told me, "Well, yeah, he can do that." Okay, so I knew he could, [and I said okay]. Right before it happened, he offered me another job and said I could stay here and do this job, but by that time, I was ready to get out. I went with Jack Nielsen [ex-Ames], who had a company in Mountain View, and did work for Ames and Langley. That was fun because the Stanford program had gotten me interested in small business and running a business. I was basically number three in Nielsen Engineering [& Research, Inc.], which had about 30, 40 people. That went well, although eventually, I began to get itchy again, after a year or so.

Roy Jackson, who fired me, left about three months later. That's typical. About a year after that, Hans called me up and said, "How'd you like to come back as a Division Chief?" I said, "I'd like that," and so I came back with the Aeronautics Division, which had all the Unitary, 12-Foot, 14-Foot Wind Tunnels. That was a great job, and that's what I was cut out for, was being a manager rather than being a researcher. That went very well.

WRIGHT: Did you have some interesting projects while you were a division chief, while you were there?

PETERSEN: Yes. A lot of it was testing for the industry, and we would see a lot of what was going on there. We had researchers, but there, they only had maybe 25 percent of the wind

tunnel time. We did a lot of Shuttle testing. It was very heavy because that was 1974, and they were beginning to firm up the Shuttle. We tested that through all of the tunnels. We tested the Stealth Fighter [Lockheed F-117 Nighthawk], which only a few people could get in to see when it came. That was interesting because they would put guards at the doors to the test section area, and nobody got in there except those who had a need to know. That was interesting, and there'd be the Boeing and Lockheed transport airplanes. We saw a whole bunch of what was going on. That was about 250 people, so it was a good job for me, which I enjoyed.

Len [Leonard] Roberts was our, I don't know what they call the next step up, but there was a step between Center Director and me, I guess he was something for aeronautics. He decided to retire, and I figured I ought to have a decent chance at that. Sy was the Center Director at that time. That ended up going to Ballhaus, I believe, and he and I were sort of rivals. That didn't set real well with me, but I was going to go along with it. I knew people still felt I had a lot of capability.

Then, Don [Donald P.] Hearth, who was the Center Director at Langley, said he was interviewing for a deputy, and I was on the list. At that time, Headquarters had done a fair job of developing a list of 10 or so people, and the Sloan Program was in there, so they had a list identified which I was on. I went back, never met Don Hearth before, and interviewed. We had a good chat, and I came back here, and lo and behold, he selected me to be the Deputy Director. The summer of 1980, I went to Langley as Deputy Director with a great deal of trepidation. Turned out very well.

WRIGHT: Moved from one coast to the other, didn't you?

PETERSEN: Yes, right.

WRIGHT: From one rival to the other?

PETERSEN: Yes, that was interesting. I think it was good, as I said, to have somebody from Ames there and somebody from Langley here, and maybe quieted things down. It was a friendly rivalry; we were just trying to outdo each other.

WRIGHT: What do you think you brought from Ames that benefited the operation at Langley?

PETERSEN: I'd say the overall knowledge of aeronautics and the management capability, but a lot of it was I understood what was going on, and I could be a good leader in terms of developing a program and putting it together. Don Hearth and I agreed to split things up, and I ran the aeronautics part at Langley and he ran the space part. I ran the facilities part and he ran the personnel part. We worked very well together. We took some psychology tests and we were opposites, which was good. He was a guy who liked snap decisions and I was the guy who liked to toss it around and think about all the possibilities. He was honest, he said he thought that was helpful, so we worked well together. That was a good time at Langley.

WRIGHT: Then at some point, you moved in and were in charge of everything, is that correct? As a Director there?

PETERSEN: Yes, in 1986, Don decided to retire and he very nicely did everything he could, and I think was influential, in being sure I became the Director. He really wanted that to happen, which was nice. I became Director in 1986, and by that time, I was very comfortable there and had a good Deputy, and had five years as the Center Director, which was also a good time, and I think I was well cut out for that. Arguing with Headquarters about money and so forth.

WRIGHT: That never goes away, does it? Did you have a challenge of direction there, with maintaining a balance with aeronautics as well as the other operations that Langley was moving into?

PETERSEN: Yes. I would say in a very general way, you did. You were maybe not quite thinking of it exactly that way, but you were shifting or strengthening one area and toning down in another area. Some of that would come by where the money was coming from. Some of it would come from we'd get together with the staff, with the six or eight guys who were under me, and we did strategic plans and decided where we wanted to go. We did have changes in emphasis as we went along, and Headquarters would force us in some ways.

WRIGHT: Did you find many threads of NACA at Langley when you got there?

PETERSEN: Yes. I was first going to say no, but yes, I did, actually, because a lot of the top people had been in NACA, and there were people who had been in NACA for 10 or 20 years, and so, there was a lot of talk about the good old days and the history and I could get to where I could brag about the history, too. It's quite a place. R.T. [Robert Thomas] Jones was there, later

he was here, Harvey Allen had been there, Smitty DeFrance had been there, and Dick [Richard T.] Whitcomb, who was a famous aerodynamicist, was there, and became a friend. I can't think of his name, but the guy [John C. Houbolt] who invented the lunar orbit rendezvous mission was there. He just died a couple of weeks ago. There was a whole lot of Langley history in the people I got to know, and I got to know a lot about it through them. It's a very historic place.

WRIGHT: I'm sure it made them feel a little more comfortable, knowing that you had been part of that as well. If you had to look back and think about some of the lessons that you learned through the years, whether it might have been from NACA or even from other programs that you would like to pass on as good knowledge lessons that people can use in the future, of working in aerodynamics and aerospace?

PETERSEN: It's a good question. I don't think I ever contributed that much to aerodynamics. As I got into management and I understood it and I enjoyed seeing what the folks did, but they were smarter than I was in aerodynamics, so I was just following along. I'd say in management, one of the things I always said was that I worked for the employees; they didn't work for me. It was my job to get them the right resources and direct them in the right directions so they could make use of all the talent that was there. I think I was fairly successful at that, and I think I was a good manager and I was popular with the people, mostly. Some people, you're never popular with.

WRIGHT: You'd be really special if you were with all of them.

PETERSEN: Right, yes. There were some challenges in management, but nothing too serious.

WRIGHT: I was going to ask Sandra, do you have any questions you want to ask?

JOHNSON: Yes, in speaking about management style, is there any one person or any people that you patterned your management style after? Since you said you worked for the people under you.

PETERSEN: I think back—Sy Syvertson, I think, was somebody who had the type of management style that I did. Hans definitely didn't.

JOHNSON: Or maybe an example of what you didn't want to be.

PETERSEN: No, I wouldn't say that. He just had capabilities that I couldn't touch. He was a special kind of guy. Harvey Allen, I don't think was a good example because he was a little bit out of his element. Don Hearth, who I went to work for, I think was a good, good example. I think working for the people was just natural with me. That's what I was interested in. I saw, unfortunately, more at Headquarters, managers who were interested in working for the people above them, and they wanted to make a good impression upward, which I think is bad, but that's the way it goes. Somehow, some of them get up there.

JOHNSON: You were at Langley at an interesting time when you became Director, '85-'86, around that time, and of course, the Shuttle *Challenger* [STS-51L] accident. Do you have any memories of that, or how it affected Langley?

PETERSEN: You bet I do! It didn't have much direct effect on Langley, but we were having a staff meeting with six or eight people who ran the Center. We had the TV on to watch the launch, and boy, it was a terrible shock and we were all, I think, in kind of a shock. The Center was in shock and nobody knew what to say or do. Just really sad.

The big memory I have, of course, there was the teacher in space who was on board [Christa McAuliffe], and there had been 10 candidates for the final selection of the teacher in space, and the 9 who didn't make it were distributed to the Centers. We had one [Niki Wenger] at Langley for a year, to sort of work with people. I'm not sure what she did, but she was involved in things that were going on. She and I flew down to Johnson [Space Center, Houston, Texas] for the big [memorial] ceremony. One of the things I remember, as we went along, she said, "I still wish I had won, even though I know this happened. I wanted it so badly." We went and [President Ronald] Reagan spoke, and it was very moving. I remember *Challenger* well, and I must have gone to Langley—what year was *Challenger*?

JOHNSON: In 1986. You were already there as Director.

PETERSEN: Yes, that's right, I was at Langley. That's the staff meeting, pardon me. The new Administrator came in, which was—you'll have to check that.

JOHNSON: [James C.] Fletcher.

PETERSEN: Fletcher, yes, Fletcher, right. That's it. He had a big meeting with all the Center Directors and the people from Headquarters and his staff, and I remember very well he said to the group, "Should we start flying right away, or should we wait through the investigation," and all that rigmarole. It was interesting—the group split about half and half, and I was on the side of we should keep flying. The idea of keep flying was you could fly three missions a year and you would be so diligent in checking everything that you'd be far safer than you were when *Challenger* happened, and that it would be very bad for the program to stop for three years.

He decided to stop, which I think didn't do NASA a lot of good, but it was one of those things that was hard. I wouldn't have fought for my position. It was just what I thought. That was interesting. Bill [William R.] Graham was the one who was in charge when the *Challenger* happened, unfortunately. Hans worked [as deputy to] Jim [James M.] Beggs, and they wouldn't have let [*Challenger*] launch, but life goes on.

JOHNSON: I was also thinking about the time you came in, in '57, and then, as you mentioned, you had human computers, the women that worked and did the computer work. Computers, the actual analog computers were starting to come into effect, and then as they did work with the wind tunnels, all that technology was changing. I was just wondering if you could comment on that, and how that technology changed.

PETERSEN: It changed quite a bit. I remember the first view of the IBM 360, maybe, which was over there, maybe where the cafeteria is. I'm not sure. It was almost as big as the floor of this building, here, with all the boxes of stuff. We began to program, and of course, programming was you wrote the program out in certain formats and then you handed it to women like the

computers, and they punched cards to put the commands in to do it. You had a deck of cards and you'd write down what inputs you wanted to put in, take the deck of cards, and take it over to the computer people, and get [your results] back the next day. That was a big change. It was quicker.

The one thing you didn't want to do was drop your deck, which was one of those things that happened occasionally, because you were in real trouble, then. That was most of my computing, and actually, I went into management about the time they started to have workstations and they eliminated the card decks, and you could type the program into the workstation—which was not a personal computer, but could, I think, put it on tape or something and it would go to the computer. Of course, they got better and better during the time I was there.

When I was at Langley, about '85 or so, they started to get personal computers. They would come around to me in the Director's office and say, "You ought to have a personal computer on your desk."

I said, "I don't know what I'd do with one. I have a secretary and if I had one, I would just waste time on it," so I never had one while I was Director. Later, then, I learned how to work computers, and I was right, I would have wasted a lot of time on that dumb thing. We were there when the first big, huge computer came. Like the Mission Analysis Division, the studies we were doing there, would not have been possible without the computers as they developed because they were just computations that would have had the computer people working for a year, or something like that, to do one that the computer could do in an hour.

JOHNSON: It's interesting how the research drives the technology, but the technology also drives that research.

PETERSEN: Yes, it does, it certainly does. One of the big examples was Hans was always the guy who—we aren't going to need wind tunnels because eventually, we'll be able to compute the aerodynamics. I used to be on the other side, with the ones who were arguing against him, at least, not as soon as he thought, so we were both right, so to speak. Now, they've pretty well taken over, but it's 10 or 15 years after he thought they would. Sorry to see wind tunnels go, but time marches on, yes.

JOHNSON: The only other question I was thinking about is a lot of the NACA Centers, during that time period, each one had a lot of social activities and the people we've talked to at other Centers, they felt like they were one big family, and they usually had newsletters and social events and that sort of thing. Did you find that also when you came here?

PETERSEN: We had a newsletter. I don't remember a lot of social events. There may have been, but we had social events within our branch and things there, and then I played on a basketball team that played for six or eight years, until we got too old. One night, I was playing and I sort of ran out of breath and had a little panic attack and I said, "That's enough of that." The problem is, I could slow down, but if I'm on the court, I can't slow down, I had to give it everything, and I didn't have it anymore. That was fun. One of the interesting things is one of the guys that was on that team was J. [Jerry] Lohr, if you've drunk his wine?

WRIGHT: No.

PETERSEN: He was working here at NACA, and he married a gal whose father was in the real estate development business. He went into that business and did very well, and somewhere along the line, started a winery. I tried to make it through the screen with an email to him, but it didn't get to him. You know how you do that, you try to say where you knew him and so forth. Tom [Thomas J.] Gregory, who's still around, was on the basketball team, and we had some good poker games among the guys who were in the branch. But, I don't remember big social events. There may have been. I don't know what you've got.

JOHNSON: It varies.

WRIGHT: Glenn, did you have some questions?

BUGOS: I did. Can we go back to Purdue a little bit?

PETERSEN: Sure.

BUGOS: Why is it you decided to go there? What were the sort of things that your classmates were interested in? Did the studying that you were doing there really prepare you to run a supersonic wind tunnel, or did that happen at Caltech?

PETERSEN: Okay, that's a good set of questions. Why did I decide to go to Purdue? As I said, in high school, I knew I wanted to be an aeronautical engineer, so there was never any question about that. It was engineering schools, and I had really good grades. I looked at Caltech and I looked at MIT [Massachusetts Institute of Technology, Cambridge] and I looked at, for some reason, University of Connecticut [Hartford]. I did not look at [University of] Michigan [Ann Arbor], which was also a very good school. I somehow got on to Purdue, and I liked college football, and Caltech was definitely out on that basis. It was too high-powered and not social enough, and MIT, a little bit the same, and it was further away.

Purdue was a good fit for me. I think it's a tremendous school. I went there and I got a hell of a good education, and I was somewhat prepared for supersonic wind tunnels. We did have a wind tunnel course with a subsonic wind tunnel at Purdue, where I tested a Piper Cub, and developed all the data. Never did find out why we couldn't get it to stall, which was very odd, and the instructor couldn't figure that out, either. The instructor for that [George Palmer] is still with us. He's an honorary emeritus professor, and I still communicate with him. He's a fun guy.

When I got really deep into it, I took the aerodynamic courses, which were tremendous and really did build a good foundation. Although we did not study supersonic a lot, as I recall—it was mostly subsonic aerodynamics—but a very good base on which to build aerodynamics. Then, at Caltech, we got deeper in the supersonic and hypersonic. I'd say it developed quite well along the way. Purdue did what I wanted. It had football and girls—didn't have enough girls, but it had girls and a social life and so forth—so I was very happy with my choice.

BUGOS: Where did your other classmates go?

PETERSEN: You mean after?

BUGOS: Yes, from Purdue.

PETERSEN: They scattered around with engineering jobs. Goodyear, some of them, a lot of them Indiana. One of them became a politician. I was in a fraternity, and they were fraternity brothers, mostly. One of them worked for the steel mills in Chicago as an engineer, and they just scattered all over the place. Purdue engineers, I'd say a third to a half of them stay in Indiana, but the rest go all over the place. You could go to any company and there were lots of Purdue engineers—Boeing, Lockheed. In fact, Purdue, for many, many years, had more graduate engineers than any other school. Texas A&M [University, College Station] became a competitor for the most engineering alumni. Good school, good reputation.

WRIGHT: A lot of astronauts come out of [Purdue], too.

PETERSEN: Yes, there was a contest between the [United States] Naval Academy [Annapolis, Maryland] and Purdue as to who had the most astronauts, and Purdue was ahead for quite a while. I don't know what it is now. I went to an astronaut reunion at Purdue, somewhere like '98 or something like that, and they had 21 of them there, which was quite something. I knew Neil [A. Armstrong] and I knew Gene [Eugene A.] Cernan from Purdue, and Jerry [L.] Ross, who was later, I got to know because he did a lot of work at Langley. I got to know a bunch of the astronauts along the way.

Met Neil, had a class at Purdue, aircraft drafting, where Neil was a student instructor. He was a year ahead of me. That's how I met him, and that class took place in what they called the Quonset huts they had from World War II in one position on the campus. Where those were located is where the Neil Armstrong Engineering Building is right now, so that's kind of a kick. Neil was a great guy. I was sorry when we lost him.

WRIGHT: And someone who thought of himself as an engineer, first.

PETERSEN: You bet, yes. He was a first-class engineer and he was very humble and didn't like a lot of hoopla. Later on, the dean of engineering [Henry T. Yang] had an advisory board at Purdue; we'd meet twice a year, once on a football weekend and once on alumni weekend, with about 10 or 15 people on it. He talked Neil into joining that about '85. I was on it from about '80 to '90. One of the things that Neil said was, "I will do it if you will make sure that nobody knows I'm on campus," because he just hated the press, the hoopla, and who wouldn't? They're just a pain in the neck. They did that—they did not know he was there. He'd come and we'd have good sessions with Neil.

BUGOS: Purdue engineers go all over the place, but I sort of got the impression you found your job with the NACA because the Air Force commitment required you to get a job with the government. Is that more or less fair?

PETERSEN: Yes, because I thought I was going to work for the Air Force, I thought probably I'd go out to Dryden as an engineer. I discovered that I could get assigned to NASA, or NACA, at

that time. That sounded pretty good, and I had a girlfriend up here, so that's how I got here. Then, when I got done, I considered, should I go to industry? I liked what I was doing and I liked the set-up. I could have made more money in industry, but the money wasn't that bad, so that's how I got to NACA.

BUGOS: If it's not too personal, your wife, how did you eventually meet her, then?

PETERSEN: Her name's Jody. She was raised in San Francisco, went to Cal-Berkeley [University of California, Berkeley], but she had a boyfriend, who came back to Purdue and joined our fraternity. He was a year behind me. When I got to San Francisco, I lived in Palo Alto, actually, and started work here, he and his wife came because he had just finished [Purdue and was going to go to Stanford Business School. While] I was at Caltech, he was finishing Purdue. [In Palo Alto] they moved into an apartment two doors away from mine—partially because I was there and they saw this place and it looked good. He kept saying, "I know this great girl, I think you'd really like her." I was a little involved with a couple others, but after about three or four months, I finally said, "Okay, Mark, I'll try it," and she said she'd given up on blind dates, but she trusted Mark. We went on a blind date and that's where it all began.

BUGOS: You had a social life outside of work?

PETERSEN: I always had a social life. I liked to be involved, and of course, San Francisco was great. I don't know how I did it sometimes. We were dating and it was not only the weekend, I was up there a couple nights during the week, and we had the Kingston Trio and the Gateway

Singers, and all kinds of good entertainment. I did a lot of driving back and forth, and sometimes didn't get as much sleep as I should have had, but it was fun.

BUGOS: If I do my math right, you were here at Ames when Sputnik [Russian satellite] went up. Do you remember that being a major event around here? Did everybody sort of start talking about how things were going to change?

PETERSEN: Was I here?

BUGOS: October '57? You would have just started?

PETERSEN: Yes. I remember hearing that it was up there, and knowing that if we hadn't screwed around so much with the Navy and Vanguard [TV3 rocket failure], that [Wernher] von Braun could have done it before they did it. They finally let him do it. Knowing a little more about it, it wasn't so disastrous as the laymen thought, that they were way ahead of us. They weren't that far ahead of us. It was kind of distressing that they did it first. That's about all I remember about that. Of course, we were already looking at Mercury [Program], and that actually came out of Langley, but we were looking at it here, too. That was already started, and that's what I remember [Sputnik].

BUGOS: Specifically, to learn a little bit more about Sy Syvertson and Al Eggers. The blunt body was, of course, Harvey Allen's idea, that Langley and [Maxime A.] Faget had picked up on. Al Eggers, a year or two before Sputnik, had started talking about what became the

[Northrop] M2-F2 lifting body, the semi-[conical] one, which I think was one of the first things you tested.

PETERSEN: Which was a flat-top, by the way.

BUGOS: I think Eggers' thought was that this was a better returnable capsule and the way to go with Mercury, and then, of course, Sputnik accelerated everything and they went with the ballistic [blunt body]. Did you remember your first research project there being part of the larger Mercury Program, or had they already decided on the blunt body, the ballistic capsule sort of shape?

PETERSEN: I think when we tested it, it was pretty well decided. I'm not really positive about that. The M2-F2, in my mind, came later, later than 1960. I was certainly aware of the development of that, but I think that was when I was down at the 3.5-Foot, as best I can remember.

BUGOS: Do you remember much interaction between Sy Syvertson and Al Eggers?

PETERSEN: Yes.

BUGOS: Were they competitors, collaborators?

PETERSEN: No, no, collaborators. They worked together quite well. Al was the senior, but they were good together. We used to laugh because Al Eggers is one of those guys who has a million ideas, and you'd get together and he'd say, "Well, we ought to look at this idea."

Somebody would say, "That's not going to work."

He would say, "Well, I think it might," and he might get talked out of that, but then he had another idea. He was one of those, 1 out of 10 was pretty good, and we could sort out, maybe, 5 of the 10 without having to do them. There was one other thing that Al was famous for. If you do supersonic stuff, you have very pointy noses and you have very sharp edges on the wings, but particularly the nose. He was famous for coming in and saying, "Boy, that's really sharp," and he'd try it out with his finger, which would bend it a little bit. People used to say, "Don't touch it, Al!" I didn't have a lot of interaction with him, but he and Sy, I think, worked very well together. Then, eventually, he left for the National Science Foundation, so he got out of the loop. I thought for a while, he might [have been] a Center Director. He was a good guy.

BUGOS: You have any thoughts on why he didn't?

PETERSEN: No. I can't remember whether it was because they didn't select him or because he just got interested in the National Science Foundation. He was competing with [Dean R.] Chapman, who was a pretty popular guy at that time. I'd have trouble putting together, time-wise, who was involved because we didn't see a lot of Eggers, but he was still around quite a bit.

BUGOS: What about Vic [Victor L.] Peterson and Loren [G.] Bright? Were they people that you worked very closely with?

PETERSEN: I didn't work closely with Vic, but we were in a lot of parallel things, and we were in meetings together and there was interaction with Vic. It was interesting, when we processed in, Vic and Pat Peterson were also Air Force. When we processed in at [Parks Air Force Base], we all processed in the same day, and our service numbers, theirs are right together because they both came from the same school, and mine is [before them]. People [at Parks] would say, "Another Peterson?" We always joked about that. They were S-O-Ns, and by the way, I'm S-E-N, be sure you get that right. Yes, we had sort of parallel careers, with some interaction, but not a lot.

Loren Bright is a really good friend. I don't remember a lot of interaction—I met Loren through playing golf, and we played a lot of golf together over the years. I'm still occasionally in touch with him—he's up in the Sierras, now. Great guy. He had the facilities and the machine shop, I think. Again, when I got up to be a division chief, we were in meetings together and development projects and so forth. He did a hell of a good job. Again, I don't think we had a lot of interaction, working together.

BUGOS: You described yourself as primarily a manager, but you did some publication in there, over the 1960s when you were with MAD. What sort of publications did you write? What sort of research were you doing? Did the work that you did prior to that, did that reflect the sort of work that you were doing with the MAD?

PETERSEN: With the wind tunnel work, it was TMs, technical memorandums that we wrote. You probably heard about the process of editing that went on with those, which was a real

process. You'd have three or four of the top guys reviewing it and critiquing it, and it was a real interesting process to finally get a TM approved and out of here. That was a real education because those people would really let you know about how you were doing it.

That, by the way, became a sore point with NASA. It was very quickly that they eliminated that process. Most of the old-timers felt that was really a loss, not to have that, because that was an educational process. It wasn't just editing for the sake of editing. Those were the kind of publications, and in MAD, we had publications but they were different in that they were not quite as technical. We'd use some technical equations, but they were more about what came out of the study, how did it fly, and what was the range, and so forth.

By then, there wasn't that big editing process. We'd just write, I think they were still TMs, but I wrote five or six of those in MAD. Wrote a couple more reports for Nielsen. I looked before I came because somewhere in my files, I thought I had a pack of the publications that I'd written, and couldn't find them. I did send a bunch of my stuff to Purdue a couple of years ago because it was just taking up space. They may have gone with it. I don't think my CV had publication information, did it? No.

WRIGHT: I don't think so, no. We're glad you put them somewhere, and not toss them, as some people do.

PETERSEN: Right. If you go to the library, you can use my name and you can find them.

BUGOS: Can I ask one final question? Since you spent some time at Headquarters, late '60s, most of the '70s, the four NACA Centers were grouped together in OAST [Office of Aeronautics

and Space Technology]; how do you think that worked? Given that all the Centers have their heritage in the NACA, did you find more of an NACA sort of mentality reflected in that organization?

PETERSEN: Yes, I'd say so, mostly. I think it was a good organization—the research centers needed to be separate from the development centers. Sometimes, that caused problems because the development centers wouldn't listen to the research center ideas, but that was a usual kind of problem. [The four Centers did] the same type of work and I think the organization worked well. It was always a question of whether aeronautics ought to be separated and into a separate organization there, or separate from NASA, which never got very close to happening. It was always, well, in aeronautics, all [NASA does] is research, and space is very different—but in fact, in OAST, aeronautical research did far better than space technology. Space technology was way under-funded, and that's part of the reason we didn't have anything after the Shuttle. They just didn't [fund] the research through the '80s that should have been done and could have been done.

Most of the people through the '60s and '70s who were in OAST had come from the Centers, so they were pretty knowledgeable and they knew a lot of the people who were working, and they were pretty good to work with, in general. That slowly changed over time, and the Associate Administrators for OAST or whatever they were called at the time, I think they were AAs, were pretty good up through the '60s and '70s.

Neil Armstrong, he was a deputy in charge of aeronautics in OAST for a year or two after he got out of the astronaut business, and then he got tired of the bureaucracy. He and I had some good interactions at that time. I think it worked pretty well, the organization, basically, and then

that was the organization I became AA of, from ’91 to ’93. I had, as I put it, I may have said but I don’t think so, I had been bitching about Headquarters for 10 years while I was at Langley, and all the things they were doing wrong and so forth, and I was waiting till I was eligible to retire. I wasn’t going to go up there until I had control of my own destiny.

Finally, Ray [Raymond S.] Colladay, who’d been in charge there and we’d had a lot of arguments—and Ray’s a friend, but I had a lot of disagreements with him—he left, and I said, “This is about the time, now.” I went to [Richard H.] Truly [who was Administrator] and said, “If you’re interested, I’d like that job.” He gave it to me and I said, “I’m going to go up there and do what I can. I’m not dumb enough to think I can make a huge change, but maybe I can bend them over this way a little bit.” I got up there and started doing some of those, and I wanted to cut down the number of people because I think bureaucracies just tend to grow. This guy doesn’t have enough to do, so he develops things to do until he’s too busy, and then he needs a helper. That’s the way it goes, so I was going to try to do that.

Six months after I went, Dan [Daniel S.] Goldin came in, and that kind of screwed up everything. After about six months of Dan, he basically fired me. He took me out of that job and said he wanted me to do a special study which was very important, and I said, “I’m out of here.”

WRIGHT: You’ve talked a lot about really wanting to work for the people that were working for you. If you would close by sharing with us some of the attributes of some of these people. Like you mentioned, you could have gone to industry many times, but you chose to continue working in this environment. Tell us why; what made those people so special, you wanted to work with them?

PETERSEN: They were good researchers. There were people like Dick Whitcomb, who was one of the top researchers. They were all highly intelligent, highly knowledgeable in aerodynamics and what they were doing, and they had the facilities, which were very important, to test their ideas. They were just great people to be around because they had the ideas and they knew how to go after ideas and they knew the process and they did a tremendous amount of work that helped the US aircraft industry be the best in the world. Since I was interested in airplanes and aerodynamics, I loved to watch them and be involved and see what they were doing. It was a good time.

WRIGHT: We thank you for your time today. We learned a lot and appreciate you sharing.

PETERSEN: I've enjoyed it. We took an hour, that's good.

WRIGHT: Anything else you want to add before we close?

PETERSEN: No, I think I'm about worn dry at the moment. I'm sure I could go on, on other things, but thank you.

WRIGHT: Thank you.

[End of interview]