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ORAL HISTORY TRANSCRIPT

VICTOR L. PETERSON
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is October 3rd, 2005. This oral history session is being conducted with Victor Peterson of Los Altos, California, as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project sponsored by the NASA [National Aeronautics and Space Administration] Headquarters History Office. The interview is being held at the Ames Research Center [Moffett Field, California] as part of a collection of interviews being conducted during the NACA Reunion XI. The interviewer is Rebecca Wright, assisted by Sandra Johnson.

Thanks again for taking time to meet with us, and we'd like for you to start today by sharing with us how you learned about the opportunity to come work with the NACA in 1956.

PETERSON: Okay. It goes back to, actually, when I was only a sophomore in college, which was in 1953. I knew that I wanted to be in some sort of engineering, and I had sort of started off heading my career toward mechanical engineering. You didn't have to declare a major, really, until you were a junior, so about halfway through my sophomore year I had an opportunity to sign up for an optional course in aeronautical engineering, which at that time was part of the mechanical engineering program. After I got into it, I could see that I would much prefer to go ultimately into aeronautical engineering as a degree rather than mechanical.

One spring, that spring when I was a sophomore, one of the fairly senior managers from NACA Ames [Aeronautical Laboratory, Moffett Field, California] stopped at the university—I was at Oregon State College [Corvallis, Oregon; renamed Oregon State University], they called it in those days—stopped by the college to do his recruiting, and as was customary in those days, the recruiter for different organizations would give a little talk to, in this case, the aeronautical engineering group. So I sat in on that and what he told us about what was going on at Ames and in the industry in general really sounded exciting. So I sort of tentatively assured myself that, yes, I was going to be on the right track to go into aeronautical engineering.

Then the next year came along, and the same fellow came back on his recruiting trip, and I was a junior at that time. I made a point to sign up to interview with him, even though I was only a junior, because I wanted to keep my hat in the ring, so to speak.

Also, I at that time was a student member of the IAS, the Institute of Aeronautical Sciences, which was the forerunner of the AIAA [American Institute of Aeronautics and Astronautics], the professional society. The student sections around the country met each year—in this case, it was Los Angeles [California]—to have a paper contest, student paper contest. At that time I was only, I guess, a junior, and I asked if I could go along on that field trip down to the conference, even though I wasn't giving a paper, and I got permission from the professors to do that.

Ames Research Center is a halfway point between Corvallis, Oregon, where the college was, and Los Angeles, so the group would come down as far as Palo Alto [California] here and stay overnight in Stanford [University, Palo Alto, California] dorms and stuff, and then take a tour of Ames the following day, and then on down to L.A. [Los Angeles] for the contest, paper contest. When we made the tour at Ames, as soon as I got through the front gate, I told the recruiter, who was our guide for this tour, I said, "This is where I want to work. What do I have to do?"

He says, "Well, I don't know." He says, "I don't want to be too encouraging, because we really don't hire very many people each year, and you have to really be first-class, and we only take the cream of the crop and all." I think what he was trying to do is say if you really want to do it, you've got to work hard. So that just motivated me to even work harder at school. Then when my senior year came around, the same fellow came up to recruit, and I again met with him and told him I wanted to work here. He said, "Well, we'll see."

So about January or February of my senior year, here comes a letter in the mail offering a job. Of course, I had the form filled out and back in the mail within an hour. By that time I had had an opportunity to visit a number of the aircraft companies up and down the West Coast, and could see where the neophyte, the junior engineers that were hired, would be put in these huge rooms containing maybe two or three hundred drafting tables, and all the new engineers went to work on the drafting boards, working on drawings for whatever project was going on.

I compared that environment to what the environment would be here at Ames, which were sort of individual offices—not individual, but maybe four- or five-people offices, and the research environment versus the rush, rush, rush, just doing routine drawings and things like that. So it turned out that the starting salary that was offered at Ames was one-half, at that time, of the starting salary that I could have gotten with industry, like North American [Aviation, Inc.] or Boeing [Company], those kinds of places.

I got married halfway through my senior year, so we were married, and actually expecting our first child by the time we were coming down here in June. But we came on down, and never was unhappy about the decision, ever.

My first day here I was assigned to an office with about five people. Jack [John W.] Boyd was my immediate mentor, the sort of first-line supervisor. My Branch Chief was a fellow by the name of Charlie [Charles F.] Hall, who was a very well known aerodynamicist, but when the transition to space came along, Charlie later worked into becoming one of the agency's best Project Managers as Manager of the Pioneer Project, the first vehicles to be sent out of the solar system, ever. They were designed to run, I don't know, five, ten years, and they just turned off the last machine fairly recently. They exceeded their lifetime by factors of three, kept sending data back, so he was a good person to work under.

His boss, the Division Chief at that time, was Harvey [H. Julian] Allen. Now, Harvey Allen was also well known in the aeronautics business, but he's probably much more famous for his invention for the space program, which was the blunt body concept, which later was adopted for the Mercury, Gemini, and Apollo capsules. The concept in itself was also used, and it still is used, extensively on all the planetary probes. So my Division Chief was a very creative and inspiring person, also.

Most of these people believed that the best way to develop your younger staff was to give them their head, so to speak, give them a lot of freedom. Turn them loose and see how creative they are. Don't cram a bunch of directions down their throat. We're looking for creative people; we'll see who is and who isn't, and then we can sort out people later. So outside of my first project, which was an assigned project that Jack Boyd kind of steered me into, everything I did since then at the Center, all the way through my career, was pretty much self-directed.

Now, the first six months were not all peaches and cream, because at that time you started off at the GS [General Schedule]-5 level. It was called Aeronautical Research Intern, and the program that they had going here at Ames at that time for Research Interns was if you were assigned to a wind tunnel, and I was assigned to the six-by-six-foot transonic wind tunnel branch, which actually was the country's most capable transonic wind tunnel. It could operate at

Mach numbers variable from seven-tenths [.7] to two-point-two [2.2], all the way through the transonic, and back in those days, that was unique.

But, okay, so part of the training was for the first six months to work out in the wind tunnel test section, learning how to run the wind tunnel. Most of the young people like myself said, "Gee, normally this is done by technicians. How come you've got engineers out here? We came here to do engineering work, not to be technicians." So there was a little disappointment for a while, which I later found was the best kind of training we could have ever had, because if you're going to be associated with wind tunnels and testing in wind tunnels and developing new wind tunnels, the best way to learn the nuts and bolts of wind tunnel work is to go out and learn how they work, and to be instructed on how to operate them and what their limit—that way you found out what they could do, what they couldn't do, what the limitations were, that sort of thing. So we went through this program.

Now, while that was going on, we also had a formal program here at Ames where the very senior people, and I mean people like R. T. [Robert Thomas] Jones, who was the fellow that basically conceived the swept wing for airplanes, and other of the very senior leaders in the business, would give us new people, I think once a month or twice a month, we had formal courses, where we would go and listen to an hour or two-hour lecture. They would give us exams to take. We had to do the exams, turn them in, and get graded. It was after you went through this sort of relatively rigid training—now, this training was going on while you also had a project. This wasn't all full-time. You were assigned a project to work on, also. But at the end of the six months, if you went through this whole routine and passed the tests and everything, then you were promoted to a GS-7 Aeronautical Research Scientist, and basically you had it made then.

There was a fairly rigorous and formal training program for the new people, new engineers, and most people weren't too happy with it at first, but looking back on it, again, it was the best thing that could ever happen, because here you could get to know the best in the business, and they got to know you, and you also knew the hardware you were working with, like in the wind tunnel. But, like I say, once I had this first project under way and going, then from there on out it was kind of on my own, and one thing led to another.

We worked also quite closely with the aircraft companies. Now, recall that back in those days, Ames, Langley [Aeronautical Laboratory, Hampton, Virginia], and Lewis [Flight Propulsion Research Laboratory, Cleveland, Ohio], and the High-Speed Flight Station [Edwards, California], it was called at that time, were considered the Research Centers, and most of the things that we worked on weren't expected to come to fruition, I mean, really go into a real airplane or into flight for maybe ten, fifteen years. We were trying to work ten to fifteen years ahead of the real today's craft.

It turns out that the military was always the first organization to pick up on new ideas, rather than, say, the commercial air—like Boeing for the commercial airplane. The reason why was because the military, we were in a horse race with the Soviet Union. It was the Cold War period. The Korean War was winding down. Today we call it the space race. Back in those days it was called keeping ahead in aviation, in military aircraft, fighter airplanes, bombers, that sort of thing, because even though they were outside of the Korean conflict, which was winding down—there was no active war going on at the time—you never knew when it might come, so you had to be ready.

The military had lots of money. They had the time, the patience, and the money to struggle with new concepts, put them into their airplanes, get a lot of flight hours on the ideas, and whatever ideas that worked out in those programs would ultimately filter down into the commercial transport business. So once it's proven in flight for the military, then—and the development costs had been borne by the DoD [Department of Defense]. Then the companies would pick it up. So anyway, the industry kept a sharp ear to the ground to all of the Research Centers, and they would send representatives up to visit even once, twice a month, looking in on different projects that might be of interest to their next military programs.

WRIGHT: Did they provide ideas for you to work on as well?

PETERSON: Then that also gave us clues on where the shortcomings were for the pioneering work, and that would give us ideas what to do next, so to speak. Like they would come in and say that, "We're having a heck of a time meeting our drag requirement to get through the transonic speed range with this fighter. What can we do?"

Well, it was ideas that came down from the military like that that sort of steered people like [Richard T.] Whitcomb at Langley to develop the area rule concept, except we called it the coke bottle shaping of a fuselage, where the idea was to keep the area distribution of an airplane pretty constant so that when you went through the transonic Mach number range, the shock waves were less strong and there was less drag, so you could get through that transonic range and then go on into supersonic flight.

They would come in to us and say, "This airplane is working pretty good except it doesn't have enough lift at landing speeds to keep the landing speed down to within reason. We would have to fly this in at 200 miles an hour to keep it in the air. How can we increase the lift at low speeds so that we can reduce the landing speed and have the pilots come in at 160 miles an hour instead of 200, maybe?" So that led people to start thinking about better control surfaces, how to do delicate contouring of the leading edges of the wing so that the wing was more efficient at low speeds and high angle of attack, but yet did not increase the drag at normal speeds and height, low angle of attack. So using their feedback, back and forth, this banter with the industry, helped us a lot to decide what to do next.

I worked in this kind of aerodynamics for two or three years and gradually worked into working on advanced control surfaces and what we call deflected wingtips that ultimately ended up on the B-70 bomber and other follow-on airplanes. If you fly commercially now, you'll see the airplanes, most of them, have little wingtips sticking up on the end of the wing. Well, that's the outgrowth of work that was started 1957, '58,'59, right in that time period and gradually worked through, first on like the B-70, where they had the deflectable tips.

Then we were also working on ideas for how to increase the directional stability of a highly maneuvering airplane, and so we got experimenting with twin vertical tail surfaces with these triangular wing configurations. Instead of a single vertical tail in the middle of the airplane coming up, you'd put one out on each wing panel, sticking up. Twin vertical tails, we called it. Later they came into being in the F-15 airplane and the F-18 and other airplanes, today's airplanes. So that's an example of how the work that was going on in that time period, the [19]'55 to 1960, is out there flying on the latest stuff right now, and as some of it has filtered down into the commercial business.

So anyway, then it came time for transition to NASA. We're kind of out in the sticks here at Ames, or we were at that time, because 3,000 or 2,500 miles from [NASA] Headquarters [Washington D.C.]. In fact, there's old stories about the first Center Director out here, "Smitty" [Smith J.] DeFrance, he was a dirigible pilot back in the thirties.

He had an accident, crash landing, one time, and he—I don't know—he injured himself in some way or other, which wasn't apparent; you didn't know it later on. But he didn't fly after that, and, of course, flying across country back in the forties and so forth, was not all that common, anyway. DC-3 flights would take several days. So Smitty refused to fly, so when they'd call meetings at Headquarters, he would say, "Well, I'll come, but I've got to take the train." So it would be a two- or three-day trip going back, he'd go to the meeting, and two or three days coming back. So there wasn't a lot of face-to-face communicating with Headquarters.

He used to kid about the old days when Ames first started and it came time to prepare the annual budget. So they would call up the Centers and say, "Prepare your budgets." So Smitty would sit down with his people and they would put together a one-page letter and submit it, and that's the budget for the Ames Research Center for 1943, say, or whatever. That was how simple things worked, and yet the output of the organization was horrendously great. So it just goes to show you that you don't have to have a bureaucracy of thousands to make something go, if you've got the right people.

The right people, we considered in those earlier days, including when I was starting, was to have managers, all the way from the bottom of the ladder to the top of the ladder, who had kind of grown up in the research environment, familiar with the culture of the organization—not just Ames, but the whole NACA in those days. When you kind of grew up together, you were a big—everybody kind of knew how the organization was supposed to work and how it should work, and pretty much would toe the mark on not trying to finagle things. Nowadays you'd say, "Well, what budget do you need?"

"Well, we really need 100 million, but we know how things go when they start the meat ax going, so we're going to say we need 200 million, because by the time they get through hacking it up, we'll get our 100 million."

So there's all these games going on these days, but in those days you didn't play those games. You sat down and said, "What do we realistically need?" and you sent it back, and all the Centers played the same game, and it all worked out, and you avoided all this overhead. Unfortunately, it isn't that way now, but that's just an example, also, of the difference between NACA and NASA.

So when it came time for the transition, of course, Russia launched their Sputnik [satellite] and the country got excited that—well, I guess it was the "grapefruit" that went up first. We called it the grapefruit. We knew that there was going to be a new thrust into space, toward space, and out here at Ames I don't think any of us really were all that concerned about the whole thing. We were actually excited about the prospects of getting into a whole new frontier. We pretty much had figured out that there are going to be more Centers put together and that there will still be Research Centers and Ames will be still more of a Research Center than a Project Center. In fact, that's kind of the way the management at Ames steered; we don't want to get too much into the projects, and we weren't going to at first, anyway.

But a few of us—again, I always was self-directed, by and large, and I started thinking, "Well, what can I do with my background and knowledge to contribute to a space program?" Well, the thing that was obvious at first was that ultimately we're going to want to fly in planets' atmospheres other than Earth's atmosphere.

So a few of us quickly got a lot of books and literature together on what the atmosphere of Mars was known to be at that time, which, it turns out, was not exactly right, but it was all we knew at that time. We actually communicated with astronomers that had been studying the planets and dug in the literature. We found out as much as we could about the atmosphere of Mars and the atmosphere of Venus, and how did those atmospheres compare with Earth. What was different between an all-air and a carbon dioxide-nitrogen mixture, or in the case of Jupiter, something with a lot of helium in it, in the atmosphere. And we thought, "Well, gee, if we're aerodynamicists, we ought to be able to figure out how an airplane or a whatever would fly in these other atmospheres, and that would be a good way for us to put our expertise to use."

So we worked with theory at first, paper and pencil, and then we said, "Well, we've got to prove out some of this stuff experimentally," so the first thing we did is there was a small transonic wind tunnel at that time called a two-by-two-foot transonic wind tunnel. We said, "Look, why don't we pump the air out of this tunnel and fill the tunnel up with a simulated gas mix for Mars. Let's mix up this much CO_2 and this much nitrogen and put it in the wind tunnel, and then put our models, models that we could envision for planetary probe shapes, into the tunnel, and test them in CO_2-nitrogen mixtures and compare the results with our theory, and also compare them with air." So that's one of the things we did, and we published reports on that.

Then another group at Ames—we had ballistics ranges at that time at Ames, where the only way you could simulate the speed of a vehicle reentering the Earth's atmosphere from orbit, or going into a planetary atmosphere, was to, at that time, launch the models, out of a gun into a long tube filled with air or whatever. At that time it was air, because people were studying what's involved with bringing something back from orbit. That's where Harvey Allen's blunt body concept first got proven experimentally. It looked good on paper, but there's always the "show me" kind that says you've got to prove it experimentally. So the ranges were built largely to prove out Harvey's theory for Earth reentry.

They also were sealed chambers, and instead of filling them up with air, we could fill them up with these different gas mixtures, and then fire the models out of guns at 15,000, 20,000 feet per second, miles an hour. Then what you do is you record—along the way there were windows and cameras set up and other instrumentation. The model would come like that, down the range, and everything was synchronized to grab pictures as it went down, and you could, from that, work back and determine what the aerodynamic characteristics were. You could see what the shock wave patterns looked like. You could test out heat shield materials and how they would withstand the heating from that, because there was intense heating behind the shock waves. So that was one way we took our aerodynamic experience and our facilities and started to think about the future.

Well, it turns out that—when was it we sent the first thing into Mars? It was Viking, and I've forgot what year it was, but maybe twenty years later. So there's an example of how the research was leading, by anywhere from ten to fifteen, twenty years, the real application. That was just our mode. We were always way ahead, except with the military. They were first to grab things, and some of the things that they grabbed, I don't think I can even talk about yet, but they did end up in things that were well, well ahead of what appears out in the public.

WRIGHT: Since you mention that, was there any kind of blanket of security or secretiveness when you were working here during those time periods of the projects?

PETERSON: Yes. Not on everything, of course, but we kind of had our fundamental research programs, sort of, of our own that we were working. But once in a while, the military would see kind of what we were working on, and they'd say, "Hey, we've got an idea for that, how to use that." Then maybe they would have a project going, and while we could continue to publish our more general work that was going on in the open literature, we would oftentimes get into joint efforts with the DoD, military, that were classified programs.

There were mechanisms in place even before I started at the Center to handle classified information. At that time, the first level of classification was confidential. Then it went into secret nonrestricted, and then secret restricted, and then top secret and top secret restricted, depending on how sensitive the material was.

We had the capability to put classified tests in the wind tunnels and to secure the place so that they weren't open to just anybody to see. Most of the military airplanes that were coming along, things like, at that time, the F-104, the B-70, and some of the early hypersonic stuff like X-15, X-Series type stuff, we were routinely running through our facilities, and all that was classified. The results were published, but in classified documents. Later many of those classified documents have subsequently been declassified, so they're in the open literature now, but we did work on that, and I got involved with a few of those programs fairly early on and continuing all the way through, actually, and probably even more so in later years.

Well, anyway, then another thought I had was, after I had done some of this aerodynamics work in gases other than air, we got to thinking, "Well, you know, when we get to the point of putting something in orbit or sending something to a planet, what if there's a malfunction in orbit or in approaching the planetary atmosphere?" We had to learn a lot about trajectory mechanics to find out that it would take seven months to get something from here to Mars. Well, in a seven-month period of transit, who knows what's going to happen?

The thing may get into the vicinity of Mars, but when you get ready to release the capsule, the probe that's going to go into the atmosphere, what if it doesn't get released just perfectly, and maybe instead of being thrown into the atmosphere in a nice nose-forward attitude, like you want it to be, what if it happens to be tumbling or spinning, just randomly tumbling when you throw it off the carrier vehicle toward the atmosphere? That would be catastrophic, unless you could assure yourself that the probe vehicle would, as it started to sense the atmosphere, going into the atmosphere, would tend to right itself and point itself nose forward, so that the heat shield was nose forward. Or, in the case of a manned vehicle coming back to Earth, you want to get the vehicle nose forward so that the heat shield is there.

So we started, first with theory and calculations and things, and then later with some experiments, we started to think about how could you shape these vehicles so that even if they were just tumbling and spinning at random conditions and you hit the atmosphere, what shape do you have to have so the thing will straighten itself out nose forward before peak heating, or before you have to deploy a parachute later on after peak heating.

That's what led us to do all kinds of work on shaping vehicles, afterbody shapes, so that if it came in base forward, it would flip itself around and go nose forward. So I guess I was one of the first in the agency to get really into what we called six-degree-of-freedom motions of bodies undergoing reentry under random conditions. So we had some calculations and wind tunnel tests going on to get data, and I had my computational program coming along that I could take the data out of the wind tunnel, put in the computer, and then fly the thing on the computer to see if it would really behave correctly.

In fact, it's really serendipitous how some things happen, but that computer program turned out to be the biggest computer program operating at Ames, and in terms of requirements for computer time and that sort of thing. They would only run my stuff overnight, because a single trajectory case would take, on an IBM [International Business Machines] 650 machine, which was the supercomputer of the day, which is about like your handheld HP [Hewlett-Packard] calculator now, but that would take eight or ten hours, sometimes, to run one solution. Then you'd run it, and you'd say, "Oh, my god, I made a mistake on the inputs. I've got to do it again."

So, anyway, that kind of introduced me to the idea of high-speed—at that time high-speed—computers and what you could do with computers, and you could do things with computers you couldn't do any other way. There was no other way to get the information, how would this vehicle really fly under these random conditions, unless you did it on the computer. Well, you could do it on the computer, or you could do it a little bit on these ballistics ranges, but the ballistics ranges really weren't long enough to really simulate a tumbling body all the way down the range.

So anyway, we worked in the planetary atmosphere work, figuring out shapes, and it turns out then that when the country got serious about putting together the first Mars mission and the first Venus probes, Pioneer Venus, Pioneer was an Ames project. A person by the name of Alvin Seiff was working here at Ames, and Al Seiff was the guy that originally got the idea that when we send a probe into the atmosphere of a planet, we can actually work backward. If we measure the accelerations that the vehicle undergoes during the reentry, during entry, we can then work back and determine what the atmospheric properties were, density versus altitude and a whole bunch of other things. So he worked up the original theory for that.

Of course, I had been working on this idea of tumbling bodies and shapes for probes and motions of the things, so I got kind of working with Al a little bit and worked up a bunch of the error analysis theories for reconstructing planetary atmospheres from the motions of probes entering. Well, my work was picked up and used in the Mars probes, the Venus probe work, and even the Jupiter, the Galileo. Again, that was done twenty—well, fifteen—ten, fifteen years ahead of when it actually came to fruition. But you could see it coming, so you say, "Well, what are the problems that are going to have to be solved?"

So I started off as an aerodynamicist and then got into aerodynamics in other-than-air gases, and then when the space program, we knew there were going to be problems with tumbling bodies and that sort of thing, so what can we do about that. Then the tumbling bodies led to the computers, because you needed the big computer. It just—one thing led to another.

But the only way that could have happened is to have an environment like we had at Ames, where you were self-directed—the good people, not everybody. We had a lot of engineers that were very happy being supported, and I always had a couple or three people working off and on, you know, on the side with me that weren't the lead people, but they were real good, solid people. We didn't count on everybody to go their own direction, but you did have the freedom to do it if you were so inclined.

WRIGHT: While you're here, if you could, give us some details about how self-directed projects worked, because you had to have funding. I have to assume you had to have some approval. So when you came up with a concept, what was your next step? What did you do, and how did you get that support, and how did you get to fruition?

PETERSON: Well, you hit upon a good theme to work up a little bit here, and that is when I started at Ames and in the early years, we were a completely self-contained Laboratory. We did not have a single contractor. Everything was done by civil servants. The lawns were mowed, the mail delivered around the Center by civil servants. All the mechanics, the shops were all civil servants. It was self-contained.

So the only cost to do a project was the manpower and the electricity. We used more electricity at Ames than all of the [San Francisco, California] Bay Area at that time. In fact, we oftentimes had to cut back on our wind tunnels in certain periods when they were short of electricity around the area.

But anyway, so everything was self-contained, all done by civil service. So all you had to do was have a project that was worthy enough that you could, if you had to build some wind tunnel models, that you could write a work order, send it over to the shops, and get enough priority to get in for the machinists, who were civil servants, to spend their time on it. So it didn't really cost a lot of money, and there was enough oversight up the line of people that knew what you were doing and knew whether it was important or not that they could, if it came to a crunch on priorities, they could say, "Well, do this project first over in the shop, and then that one." So it didn't cost anything, and if the good ideas came along, you'd get them.

Nowadays it's completely different. I don't know what the numbers for Ames are right now, but when I retired in 1994, we had 5,000 people coming through the gate every morning, and 3,000 of them were contractors, 2,000 of them were civil servants. Not only that, we were spending a lot of money for outside contracts, grants to universities, contracts for industry.

These earlier days, we didn't have contracts. Everything was done in-house. Even with the universities, we didn't send money to a university. What they did is they basically sent money to us. Not really money, but they sent people. We had relationships with the local universities, and actually, NACA, had relationships across the country where selected graduate students were allowed to actually come here and spend at least one year of their Ph.D. training on-site at either Ames or Langley or Lewis.

The reason universities did that is because they recognized that the people at these Research Centers were writing the textbooks that were going to be used twenty years downstream, and if you want to train that student, one way to train him is to put him in touch with the people that are writing the books and that have the facilities. We had facilities that just weren't available in the university, huge wind tunnels and all that stuff, computers. So we had the benefit of graduate students helping, but it didn't cost us anything except we had to provide them a place to sit. And if their work was related—it was always related closely to what we were doing, so they rode our coattails on shop projects and things like that.

So you had much more flexibility. That gradually changed. The crunch started at the peak of Apollo funding, which was 1965. I can just about see the curve right now, and the peak is right there, at '65 or '66. The agency funding went way up to a peak and then, roughly, I think it was right after the first Apollo flight, it started to tail down. By that time even the Research Centers had been gravitating toward—especially during the Apollo Program and the early Shuttle days, we were being asked to do much more work than we could do with just our in-house, so that's when the money started, extra money started flowing in to bring on our support service contractors, to award grants to universities, to give contracts to industry.

So over time, then, we gradually became less self-contained and more dependent on money to keep things going. Then, of course, when we went past the peak funding years, the agency started to contract until Shuttle came along, and then it picked up again a little bit, but never quite up to the same peaks.

So right now the agency is in a serious crunch, because we're being asked to do more, look ahead to a return to the Moon, put people on Mars, finish the Space Station, build a new Shuttle, on and on, and there's no really new money for a lot of this stuff. So it's created the situation now where the Research Centers are no longer what I consider real—except in pockets. The research atmosphere is altogether different. Contractors come and go. You don't have the same continuous culture. You don't have people managing at all levels in the organization that have grown up with and have the trust and understanding of each other because of the money crunch.

Back in the early days, when you didn't have all this, there was competition between the Centers, but it was very friendly. Langley was working in aerodynamics. We were working in aerodynamics. Lewis was less of a competitor, because they had kind of an isolated mission in propulsion, and neither us nor Langley were doing much in that area. So it was mainly between Ames and Langley, and it was a very friendly competition. We would have periodic, I think every couple of years, we would have an NACA aerodynamics national meeting, and each Center would prepare its best people and papers, and send them. We would get together and present our papers and share the results and talk about it and say, "Well, that's pretty good, but I think we can do better. We'll try."

But we weren't at the same trough for dollars. No matter how competitive we were, we knew that we weren't going to get any more people from Langley, and they weren't going to get any from Ames, and our budgets weren't going to be any different, so we had a very constructive competition. A little bit like athletics, you know, like the Olympics. It's competition. You're trying to do better because that's just your goal in life is to be the top in your business.

Of course, nowadays, there's competition, but it's very tooth and nail type stuff, because one Center is out to steal a program from another Center because they're trying to replace funding, they lost their funding for that program. So it's not a research environment when you're in that kind of competition, as compared with the previous type that I mentioned.

In fact, NACA was largely aeronautics, because the space thing didn't really come on until later, but at that time our aeronautics program for this country was the cream of the crop in the world, and it was the envy of the world. Our airplane companies were far ahead of the foreign competition in airplanes, commercial, military. With the situation we have now, aeronautics is continuing to take a less and less role in where the priorities are, where the spending is.

Now, I think that's proper, to a certain extent. Every technology has its heyday, so to speak, and when a technology is just starting off, it's down in the noise and nobody knows about it. Then all of a sudden, like an Apple computer is invented, and there's a big rise, and it reaches a peak. Then pretty soon computers maybe get obsolete, or that type, and so it falls off. So every technology is sort of the same way; it has its heyday. And aeronautics has had its heyday, because we have almost done as much as can be done to improve the performance of an airplane, except for things like supersonic transports and hypersonic, greater than Mach five steady flight for people, where you can go from here to London in a half hour or an hour. There are a few things left to do, but you have to let go of the past, and I recognize it's proper for aeronautics to go down.

But it can't go down to zero, because this country will be in deep trouble, I think, in the future. We're having trouble right now keeping competitive with Airbus [Company] on the commercial transports. Aerospace Corporation is eating our whatever they eat now, selling airplanes competitively with Boeing, and it's just a horse race. One year they sell more than Boeing; then next year, Boeing. But we're really taking a backseat.

Why is that important? Well, it's important because commercial aviation was at one time the strongest contributor to the positive balance of trade for the country. If you look at all the exports and all of the imports, and you add it all up, what export accounts for most income for the country? It was commercial airplanes for many years. It's no longer that. So even if you're not interested in flying an airplane, people should be interested if they're interested in this country's balance of trade issue, for example. So aeronautics is important, but not as important. The research in NASA in aeronautics is basically gone, so good or bad, that's the way it is.

WRIGHT: Yes. During those years when you were first here and you met so many of these people that were kind of legends in their own time, and still are, did you see a lot of turnover? You mentioned that it was really hard for you to get in the door, because they didn't hire very many people, but did a lot of people leave and move on?

PETERSON: You know, there was a little turnover, and where did the people go? Well, a lot of them went to become university professors. You start off early in your career, and then you build a career. You come up with your achievements. You publish. Then when you get to be—not always, but some people, they get to be middle age or a little above, and they just kind of lose some of their creative juices, but yet they know everything in the world about the business. Then they get to thinking, "Well, gee, you know, maybe I'd rather just be a professor and teach what I know to others."

So we had endless numbers of these giants move on into industry. R. T. Jones. Of course, he kept working here till he was sixty-five or so. I don't know whether that's exactly the right age, but he was along in that time period. And then he went over to Stanford, and he taught some courses over there until he officially retired. Dean [R.] Chapman, who was a giant, actually my boss, immediate boss, for ten years, ended up, I think, leaving the agency at about age sixty and going to Stanford and becoming a professor. [Dr.] Leonard Roberts, the Director of Aeronautics here for a while, went that way. On and on, Milt [Milton D.] Van Dyke, Alex Charters, on and on. Walt [Walter G.] Vincenti, giant at Ames, professor for many years at Stanford, and now, as an emeritus professor, he's interested in the history of technology, and he has started teaching a course in the history of aeronautics and that sort of thing, aviation.

WRIGHT: How wonderful for the students to have someone who contributed to that history.

PETERSON: Yes. So we had some flow that way. The other way was industry would give their eyeteeth to hire some of these people to go into industry and lead their—although they weren't as big and grand as our research efforts, they did have internal research programs and other projects that were longstanding, long-running. The other avenue for transition for people to leave was to go into industry at twice the salary, three times the salary, and we had a number of people that left the agency.

They were not beginners here at Ames, but maybe fifteen years into their career, had established themselves, were sought after, because industry visited all the time. They knew these people. They knew what they could do. They knew whether they were good managers or the type that would work out. And they would come in and offer them double salaries, so a few of them left. I turned down jobs twice the salary numerous times, but I was more interested in—I said, "I can't duplicate this environment out there. I don't want to get caught up in that rat race."

So outside of that, there wasn't a lot of come and go, and again, we didn't have the contractors, who wouldn't leave because they wanted to, but because they had to. They would come in. They would be hired to do certain contracts, and the contracts would either be recompeted and they would not win the second round. Some of them could stay over, because the second contractor would come in and pick up the good ones from the first, but other contracts just went away and weren't replaced, and those people left. They were maybe here five, six years.

So there wasn't a lot of throughput of personnel in the earlier years, and even today with the civil servants, most of the people that come here, they'd be here for life, except for the few that went into universities or whatever.

It's good to have some flushing of the system, and unfortunately, a lot of times the people you would want to leave didn't leave, but with civil service regulations, it was kind of hard to do anything. But you would find ways to make good use of these people. I had people that were under me at different times who just weren't—they got to a certain point in their life and they just ran out of gas and weren't good anymore, but we would always find something they could do. We'd move them into Public Affairs or real weird things. They might have been engineers, but they turned out to be perfectly happy being editor of an *Astrogram* [*Newsletter*] or newspaper or whatever. So we tried to make good use of these people.

So you have to worry about that, but the way we circumvented that worry, to a large extent, was we said, "Well, how can we bring in new blood and new thoughts with our civil service complement restrictions and that sort of thing?" So when I was a Division Chief a number of years ago, and I was interested in scientific computing, I thought to myself, "Well, we really need to get some real new blood and ideas into supercomputing and that sort of thing."

So I created an institute, Research Institute for Advanced Computer Science, we called it, RIACS. I went out and I lured a very well known professor in the computer science business to come and lead the institute. First I had to go to Headquarters. I had to get a bag of money and sell them the project. But the University Affairs people back at Headquarters have a certain amount of money to put into developing relationships with universities and that sort of thing and I showed them how this would be beneficial not only to them but to the agency as a whole, because we could bring people in on up to three-year cycles, and we picked the best of the bunch. If we'd get a senior person to run this institute, that is well known in the business, it will attract the right people.

And it worked. We built that up to about thirty people, and we had a continuous flow of the best people, not only just students, but people that would take sabbaticals from their respective universities, because they said, "Well, this is an opportunity of a lifetime. We can go out to Ames. They have the most powerful computer in the business out there. They have an in-house group of people that know what they're doing in computing and software work." We got a leader for the institute that's an idol of the industry. "So I'll take my sabbatical and go." So we would get these guys coming in, and they would spend one, two, or three years.

Then they had to leave. We didn't want a body shop where people would come in and stay for their life. The whole idea was to bring new ideas in and good people that could do something productive for us. So that's one way that we circumvented the idea of limits on the civil service complement and that sort of thing.

We could do this at a much lower cost. Well, first of all, it didn't cost as much to bring these people in, either, as it would to just set up a contractor and tell them to go hire engineers, because the overheads were different, and the universities oftentimes would cover part of the expenses of these people because they were on sabbatical, or they felt that they would get it back when the person came back, etc.

So that was one creative way that we got around the deadwood problem and of keeping new ideas coming in. And it's still operating here today.

WRIGHT: That's rewarding.

PETERSON: Yes. So then, I mentioned early on that outside of my first assignment, everything else was pretty much self-directed, except for when I was asked to take on my first management responsibility. I forgot who it was; somebody got hold of me one day, one of my bosses, and said, "We're going to do a little reorganization. We're going to have a new branch. We want you to be the Assistant Chief of the Hypersonic Aerodynamics Branch."

I said, "Well, okay." So I was asked to become a manager. I wasn't too concerned about it. I wanted to really do research, but I was kind of interested in developing people, too. In fact, I'm quite proud of—that's one of my best accomplishments here at Ames, I think, is developing people. Over the years I was able to mentor and train and develop and bring along people that later—two people later became Center Directors at other Centers. Both became Associate Administrators for a while. Both ended up leaving the agency and becoming corporate presidents. In fact, one is leading Aerospace Corporation right now. Those are just two examples. Many more have come through and become full professors or this, that, and the other thing. So these things I was proud of. One was mentoring and bringing people along and making them better than me.

In fact, this one fellow, one of the guys that went on and became a Center Director elsewhere, he actually was promoted over me one time. I had selected him to be a Branch Chief. After he had been a Branch Chief for a while, they needed a new Organizational Director, and he sort of jumped over me. I was a Division Chief at the time, and he went from Branch to Organizational Director. He was so worried. He came into my office. He said, “Now, look,” he says, “you really were the one that should have been that.”

I said, “No, don’t worry about it, because you won’t be there very long, and then it will all work out.” He was so worried about it, I wrote him a letter that night and said, “Now I know what a university professor feels like when his prize student goes out and passes over him and becomes a company guy or president or whatever.” I said, “The professors I knew, that’s what made them the happiest, was to have their students become leaders in their business.” And I said, “I feel that way, too.”

He said, “That’s the best letter I ever got.”

WRIGHT: That’s neat.

PETERSON: That was good. So anyway, I forgot what we were talking about now.

WRIGHT: I think we’ve covered quite a bit, not to finalize everything out, but you were talking about your tasks. Most of everything was self-directed, except for this one time when you got picked for management.

PETERSON: Yes, that led to that appointment. Then the next thing they did, about three years later they said, "Well, we're going to do a little reorganization again, and we want you to be Chief of the Aerodynamics Branch."

So I said, "Well, okay." Each of these management positions I undertook, I always continued my personal research on the side, maybe to a lesser extent, but—so then I became a Branch Chief.

Then a Division Chief job—Dean Chapman was appointed Director of Astronautics, in those days, and he called me up one day, and he said, "Look, I want you to come over and take over my old division, and I'm going to be your boss."

I said, "Great. Let's do it." In those days you could move people around a little easier. So anyway, I was asked to be Division Chief and worked for Dean for ten years, and then he moved on to the university. He's one of them who went there. Then I did compete for a job. Then his position as Director of the—we were calling it Director of Aerophysics in those days—that opened up, and I threw my name in the hat, and I was selected for that. Who was the Director? [Dr. William F. Ballhaus, Jr.], I think, was the Director at that time. Anyway, I got to be Director of Astronautics.

Then our Center Director later was sent off to Headquarters for a year, and the Center Deputy Director was moved up to be Acting Director, so they called me up and said, "You be Acting Deputy for the year while this fellow is gone."

I said, "Okay," and so then the fellow that went to Headquarters never came back, and so I became permanent Center Deputy. So I had that track, management track, and I continued to do my thing. In fact, the last twenty years of my career, when I was like a Division Chief and a

Director and a Deputy, that's when I did my best work in another area of the three that I'm the most proud of, and that was the supercomputing.

Myself, together with several others, key people at the Center, conceived the idea of—well, I had been leading—in my division was the computational fluid dynamics activity, and I had been responsible for the branches doing that kind of work for ten years and been nurturing that work and getting it going. We needed more and more bigger and bigger computers to do our job. Ames became known as sort of the computer-oriented Center, largely because of that.

The Advanced Research Projects Agency, ARPA—or they call it DARPA [Defense Advanced Research Projects Agency] now, I guess—had funded the development of the ILIAC-IV supercomputer, and it was put together by the University of Chicago [Illinois]. When it became operational, it was the most powerful computer in the country, and probably in the world, at that time.

Our Center Director stole it. He went to DARPA, and he said, "Look, this is a national treasure, this machine, and it's just coming into operation. It's on a university campus." There was a lot of student unrest on the campuses in those days. He said, "Look, I'll make you a deal. I will build a small building and provide the utilities and set up the whole thing. If you give us the computer, we will operate it here for you. You can be users, and we'll be users. We'll put it on the ARPANET [Advanced Research Project Agency Network], which was the forerunner of the Internet. You'll have a safe environment. We'll protect it, and you'll have the advantage, also, of our users, who are pushing the state of the art in supercomputing, the need for it." So we got the ILIAC-IV stationed here, and that made us the king of computing.

Then the question was, well, where do you go next? Well, we stole a CDC [Control Data Corporation] 7600 from SDIO, Space Defense Initiative Organization. Or no, it was the MOL

Program, the Manned Orbiting Lab[oratory] Program, which, remember the DoD had a companion program to Apollo, sort of, which later fizzled out. Well, they had a Control Data Corporation 7600 machine, and Hans knew all those people, our Center Director. So he and a few others, he called up whoever was in charge, and he said, "Hey, can I have your computer? You're going out of business, and otherwise it will just go on the chopping block."

They said, "Sure, but I can't sign any paperwork for it, because I don't want to be responsible." He says, "But I'll tell you where it is."

WRIGHT: So you literally stole it.

PETERSON: Literally stole it. They got a truck, and they went over and picked it up and hauled it over here, and nobody complained. So we added to our computer inventory.

Well, as I say, my organization was the biggest user of computers, and so where do we go from here? How do we get the next one? So a few of us got this idea of the Numerical Aerodynamic Simulation Program, which led into what the Center has now, the Columbia system, which was at one time the most powerful. Now it's the third most powerful in the world. So we put together this program. We needed $100 million to get the thing going. So a few of us, including the Center Director, would traipse off to Washington [D.C.] every week, and we'd go and we'd say, "This is kind of coming along. We're going to come back and ask you for a lot of money."

In the meantime, we said, "Well, how can we get even more support?" So we said, "We'll go around to industry, aerospace industry, and we'll tell them about our plans and how they would also have access to this capability for their program, programs that are of interest to us."

So three of us, we actually went and visited eighteen different companies around the country, everywhere, all the key companies, and visited at the Vice President of Engineering level, or corporate CEO [Chief Executive Officer], if they were available. We had a dog and pony show, and we took it around. They knew we were working in these computationally intensive disciplines, because they had their people following us all the time. They said, "We need more computing power, and we want to go to Congress—well, our agency in Congress—and ask for a big program to build a system for the future that will not just be one computer, but will be a evolution of computers. We'll start with the best, and we'll evolve to the next up the line, and just keep this thing right at the state of the art, all the time."

So we said, "You know, you guys are on all of our advisory committees, and your congressmen and senators call up your company presidents and say, 'What do you think of this? It relates to what you guys are doing.' So we want to first convince you that we, NASA Ames, should be doing this, and secondly, if anybody asks, please support it." So we spent a year, and not only just one visit, but several visits to a number of places, big places like Boeing.

We kind of got them on board, although they were very skeptical, because we were trying to convince them that they're going to go from computer to flight with their next commercial airplane. Well, that was unheard of, because these people always had done their evolutionary work in the wind tunnels. But there were a few forward-looking people, and you know what? With the 777 airplane that was designed several years ago by Boeing, it went from computer to flight with only a few verification tests. So our vision came true, pretty much.

Anyway, we finally got the program sold and got a brand-new building built. We spent $100 million the first year or two getting it all going, and it has continued to evolve since. Now it's so noteworthy that the current system, the Columbia system that's in there now, was partly funded by local industry. Intel [Corporation] and Silicon Graphics [Inc.] and some NASA money, but collectively, they put together the system that's over there running now, that's right now the third most powerful in the world. So that's one of my proudest accomplishments, because it's an ongoing thing. It goes on and on.

And it doesn't just do aeronautics. It does astrophysics, simulating the motion of galaxies and the evolution of the universe. It's doing ozone hole eco-type calculations for how do you simulate and model the whole Earth environment. If you put these contaminants into the atmosphere, will that hurt or help the ozone depletion problem? The tsunami that they had, there's undersea modeling going on, using that machine. They were able to model the tidal wave. They put in a simulation of that level of magnitude of earthquake in the location it was, and calculated the waves, the—what do they call them?—the wave that comes out, and it agreed almost exactly with experiments.

In chemistry, for the Shuttle and future planetary missions, it can calculate the properties of gases under these high-temperature conditions, especially for the planetary atmospheres, where we don't have a lot of flight experience into those, and determining what the chemical reactions that are going on in the shock layer are.

Modeling the human vision process, we were thinking about the lunar missions at that time, and how to put eyes in robots so that a robot can literally see and run it through its own simulated memory, to try to model the way a human would be up there, see something, and be able to make a decision without having been preprogrammed ahead of time or without having to send—right now, in the Mars Rover, the Mars Rover creeps around, takes pictures, sends them back, but the transit time is so long that you can only go so far in a particular move, and then you've got to stop the machine, send the data back, let the operator on the ground say, "Well, really, what should my next move be? Is this a productive way for me to continue exploring or not?" Then you send a new direction up.

Well, you'd like to have those robots have a little more mentality and creativity, and there are people trying to model some of that on the computers that we have nowadays. Until we got these big machines, you couldn't do that sort of—it's an immense job. Anyway, that's how my theory of tumbling bodies entering atmospheres, going back in the 1970 period, ended up in the supercomputers.

WRIGHT: Gosh. Well, as our time starts to close, I wanted to thank you for going through some of the details of all that you've done, about all your tasks and self-directed tasks, and I guess your most recent self-directed task is you took on the planning and coordinating of the reunion for the NACA group, which has just concluded. I know that you had the opportunity to talk to so many of them, some of them maybe that you never had met before and some that you did know. If you had to sum up and explain to someone who did not know these people or did not know what the NACA community had provided for the country, and actually, for the world, how would you do that? How do you characterize this group of people and their work ethic and their creativity?

PETERSON: Well, if a person had no previous understanding of what NACA, National Advisory Committee for Aeronautics, meant, I always start off by saying, "Well, you've heard of NASA, haven't you, the space program?"

And everybody says, "Yes," you know.

I says, "Well, before NASA, there was an organization called NACA, and it stood for National Advisory Committee for Aeronautics. The NACA did for aviation what NASA is doing for space." And I would go back and say, "The Wright brothers had their first flight in 1903, and some of the other countries, the French, in particular, and others, were coming along with early flights, and there were some visionary people back in those days that could see that eventually aviation might even be practical for something, carrying the mail or whatever, people. Some people even thought about people transportation in those days, and military. Wartime, they're always looking for the best way to fight a war.

"So some wise people got together, formed a group called the National Advisory Committee for Aeronautics, and it was formalized by an act of Congress in 1915. The first operational sort of Research Center was Langley. They came on in 1917 and were sort of the only Laboratory for many years until the approach of World War II, and a number of forward-looking people at that time said, 'Well, if we're going to get ourself into World War II, the most likely place that we would be attacked would be on the East Coast, coming from Europe, Germany, in particular.'

"By that time, aviation was starting to become practical. I think the first commercial flights were [19]'36, [19]'37, [19]'38 time frame, and mail was being carried routinely by airplane. So people thought to themselves—not only to themselves but out loud—that we've got to do something to protect our capability in aviation research. We can't have all of our eggs in the basket at Langley. So they said, 'Well, let's go to the other coast, as far away as we can.' Plus, it turns out that a number of early aircraft companies were stationed up and down the West Coast, so it made sense to consider putting a second Laboratory in on this coast, and that was done.

"Then a short time later, they said, 'Well, we need another installation with even more diversification, and let's put a third one, then, the Lewis [Flight Propulsion Research Laboratory], at Cleveland.' So that's kind of how it all got started."

So this was the oldest time period, actually from about 1917 on up till the beginning of NASA, was the creative period of aviation, and as with any new technology, the leaders, the people that kind of lay out the disciplines and do the early work, from that group of people always emerge a number of sort of giants; we call them technical giants. A number of these people, having worked in this environment and organization for a number of years, clearly became the leaders of the business.

All through this process, a great degree of camaraderie, esprit de corps, and family feeling developed with the three, and subsequently, the four, NACA groups, one down at Dryden [High Speed Flight Research Center, Edwards, California]. The person that led the NACA when the NACA was transitioned from NACA to NASA was a fellow by the name of [Dr.] Hugh [L.] Dryden, and the Secretary to Hugh Dryden is a person named Jo Dibella, and when the transition occurred, Dryden became the first Deputy Administrator of NASA, and Jo Dibella was still his Secretary. Okay, well, eventually Jo retired, and then a few years later, 1976, she got the idea, "Gee, we were all one big family in those old days. We ought to have a reunion."

So she got a group of people together, and they organized a reunion and held it back in Asheville, North Carolina, in 1976. This family that had developed from the NACA, all of a sudden converged on this reunion and they had a great time. So a few years passed after that, and everybody had such a good time that they beat the drums a little bit and said, "Let's have another reunion." So I think the second one was held in 1982 or thereabouts, and then the process had been started, and some of these people have been to every reunion.

Jo has come—the one that started it; she was at ours—she's been to every one. Others have been to every one. So we just recently had the eleventh such reunion. At first they were running on three-year cycles, and then people started to get older, and they said, "We'd better speed these up a little bit," because between each reunion, you lose a hundred people. They pass away because of age or whatever. So it got going on a two-year cycle.

Right now, at our reunion that we just concluded yesterday, in fact, we still have a mailing list of 2,800 people, 2,800 addresses that we sent out letters a year and a half ago. It takes about a year and a half to put one of these together. We sent our first letter out to 2,850 people, I think it was, or addresses. We got a number of them back marked "deceased," either by the Post Office or by family or whoever.

In this letter, we said, "We're going to have this reunion, another one, and are you interested," that sort of thing. We got a number of letters back. I don't remember exact numbers, but they said, "Well, we're kind of interested in these reunions, but we're going to wait till one comes closer to where we live." Others responded that they thought they would be interested and they would like to get more information.

So we ended up with about, I don't know, somewhere between 3[00] and 400 responses that were positive and were sort of interested. Now each response counts for two people, in most cases, and in fact, a lot of cases, the husband and wife were both NACA people, because back in those days, it was truly a family thing. If husband and wife both worked, oftentimes they worked at the same place. So when all was said and done, we ended up with 320 people for this one.

But in going through this process this last round, I'm particularly sensitive to the fact that we can't have too many more of these, because if you stop and do the arithmetic, you can't be much younger than I am to have been part of the NACA. You can only have been maybe sixty-nine or seventy years old and still be part of the NACA, so there won't be a whole lot more of these.

So we decided at this current reunion that we probably should try to have another one, but let's have it at Langley. Let them be the host. A couple of reasons for that. One, they're the mother Center. They started it all back in 1917. And secondly, they're the Center that has the largest population of retired NACA people, and they can also draw from the Washington, D.C., area. It's just a bus ride away. So that's why I recommended that we have the next one at Langley, and I think that was agreed at our meeting here. So presumably, two years or so here—three years would be really ideal, because that would be the fiftieth anniversary of the demise of NACA, and it's always good to mark the fiftieth and the hundredth and the seventy-fifth or whatever.

So, did that give you the—

WRIGHT: Yes. Yes. It's a remarkable group of people, so it's nice for everyone to get back together.

PETERSON: Yes, it's just amazing, these people. We all knew each other, if not personally, at least by reputation, because as I mentioned a long time ago in this thing, we would have these annual or biannual conferences in aerodynamics and structures or propulsion, and all of the clan in those disciplines would gather at that time, and you'd talk to each other for a couple, three days. You'd hear papers from the key people, and you got to know everybody. You always had group functions. After the day of the meeting—the meetings went on, you'd go out someplace and have a barbecue as a big party, or whatever. So it became a family and continued on that way, and these people remember each other, and by and large, most everybody knew everybody, either by reputation or personal account.

WRIGHT: Sounds great. Well, thanks for your time this morning. Appreciate it.

PETERSON: You're welcome. You're welcome.

[End of interview]