NASA Headquarters NACA Oral History Project
Oral History Transcript

Clarence A. Syvertson
Interviewed by Rebecca Wright
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WRIGHT: Today is September 29th, 2005. This oral history session is being conducted with Clarence A. Syvertson, formerly of the Ames Research Center [Moffett Field, California]. It's being conducted at his home in Saratoga, 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. This interview is one of many being collected during the NACA Reunion Number Eleven. The interviewer is Rebecca Wright, assisted by Jennifer Ross-Nazzal.

We thank you again for letting us come to your home and talking with you today, and we'd like to start by asking you to share with us how you first became part of NACA.

SYVERTSON: Okay. I have to go way back a little bit to when I was even a child. My dad was an immigrant from Norway, born in Oslo, Norway, and he died when I was only seven. So when I got old enough to go to the university, I thought I'd go for only a year and become a draftsman. Well, it turns out I lucked out; I was the world's worst draftsman, and I got to go four years—well, three years—and get my degree. Then I went in the Army. Came back with the GI Bill. The day I came back to go to graduate school, the University of Minnesota [Minneapolis, Minnesota] where I went to school, was hiring people to start a little lab in an old arms plant, a little aeronautical lab. So I went to work for the university and went to school part-time.

The following summer a former graduate of the university, about three or four years ahead of me, came home for the summer to look out for his dad. His mother had just died. And he worked at the Ames Aeronautical Laboratory [Moffett Field, California]. His name was John R. Spreiter; very good man. Anyway, he and I became friends for the summer, and then he came back out here. Then the following January he had to give a paper in New York, an NACA paper, and he stopped in Minneapolis, where I lived, on the way back to out here. He said to me, "What you ought to do is quit your job and go to school full-time and come to work where I come to work."

So John, I realized he was a very sharp individual, so that's what I did. I finished up in the following August, and in September of '48, September 7th, I came to work at Ames Aeronautical Laboratory. John steered me right a couple of times. I was assigned to a little group, and I wanted to be with this big new wind tunnel they just built, and he said, "No, stay where you are," and he was right. That worked out very well for me.

So that's how I came to work, and I was housed in the office building associated with the six-by-six-foot supersonic wind tunnel, which had just been built. It turns out that they were very disappointed with the quality of the flow in the wind tunnel. I was assigned a drafting job, which I really didn't want because I was so poor at it.

One day I was working on my drafting table, and my boss, Al [Alfred J.] Eggers [Jr.], was talking to his boss, Harvey Allen, H. Julian Allen, about the problems of this new wind tunnel. Well, in graduate school I had taken one course that had a little bit to do with analyzing supersonic flow, so I thought, "You know, if you're going to get anyplace in this organization, you'd better get off the drafting table." So I was listening to them talk next to my drafting table, and I said, "I know a little bit about that." It was a very little, actually. Anyway, they asked me to do an analysis of the flow.

Well, it turns out that I predicted their problems almost exactly, theoretically. I was more surprised than they were. But for some reason that impressed Harvey Allen, who surprised me a few times later. But anyway, when they figured that out, they were just getting ready to build a whole new complex of supersonic wind tunnels, a big complex known as the Unitary Plan Wind Tunnels in those days. There were big facilities being built at Langley [Aeronautical Laboratory, Hampton, Virginia] and [Flight Propulsion Research Laboratory, Cleveland, Ohio] and at Ames, and they were just getting into design phase.

So after my success in predicting the problems of the six-by-six, they gave me three people to work for me and gave me the job of designing the nozzles, supersonic nozzles, the expansion part of the wind tunnel for the new facilities, which I did. Turns out they had pretty good flow, so anyway, that sort of was the lucky point for me. It went very well. So I spent several years doing that, and we used to kid about it a lot, and ended up developing the techniques that were used to design all the supersonic wind tunnels at Ames. Later they were even computerized, as I recall, when computers came in much later.

It was a totally different time. Jets had just come in. When I was in the Army, I saw one jet go overhead, one military jet go overhead when I was in the Army back in '47—'46, '47. Then supersonic flight hadn't happened yet, and so a lot of interesting things to do. After I got done with the design job for the new wind tunnels, I started analyzing supersonic flows so we could understand them a little bit, using some of the same techniques that we'd used with the wind tunnels. So I worked directly for Al Eggers, and he and I developed some new theoretical methods, some new ideas for aircraft, and as I said, it was a time of great changes.

Another thing I want to put in, there was one hell of a lot less paperwork in those days than there is now. I think Harvey, who was our Division Chief, had one or two or three things called RAs, research authorizations, that controlled everything. They were the only paperwork he had to deal with.

In those days, the organizational structure was a lot simpler, too. There was the Director. There were, I think, three research Divisions at Ames, and there were Sections. There were no Directorates. There were no Branches; Branches hadn't been developed yet, or invented yet, I guess, but Sections. So there were two layers of management we have today that didn't exist in those days, so it was easy to get things done, easy to try new things, and you were given a hell of a lot of freedom. If something worked, you could do just about anything you wanted.

WRIGHT: Could you share that process, how that worked if you came up with an idea? How did it develop from an idea into full-fledged research?

SYVERTSON: Well, the main controlling document was a [job order]. It was mainly an accounting tool. You've got authorization for this [job order]. But all you'd have to do is get your boss and his boss to sign off on it, and it was more of an accounting tool, because everything you did or any resources you needed or supplies or anything like that were charged to this one number.

So this required no approval by [NACA] Headquarters [Washington D.C.]. There were just sort of areas under the research authorization that Division Chiefs were allowed to do research in those areas, and you had to get one of these things—[a job order]—as an authorization, and that's all it took.

WRIGHT: Did you have to come up with a budget or a timeline?

SYVERTSON: No, no.

WRIGHT: Nothing, huh?

SYVERTSON: No, no.

WRIGHT: That's exciting.

SYVERTSON: You just were assigned a number for accounting purposes that this job was going to be—everything was going to be charged to this one number. I'm sorry I can't think of that. I'll see Al Eggers at the banquet—

WRIGHT: We'll fill it in, yes.

SYVERTSON: —and he'll remember, because he [became] part of the problem. When he went to Headquarters, he started expanding some of these additional paperwork things.

WRIGHT: Can you give me an example of an idea or a research that you had that went from this no paperwork into fruition?

SYVERTSON: Well, I remember one I got involved in personally. We were just testing simple bodies of revolution—you know, like a missile—at supersonic speed, and one of the fellows I worked with, a fellow named Stan [Stanford] Neice, was measuring the pressures along these things, and he made the comment one day that, "Oh, the pressures on the back end of it fell off exponentially."

I remember him saying that, and I thought, "Hey, I wonder why the hell that is." So I started fooling around with mathematics and developed a logic why it should do that. From that, developed a theory for the pressures over a body of revolution. When you put them at a little bit of an angle of attack, I could also calculate the derivative of how fast the forces would change, how fast the moments about it would change.

So I did all this and figured it out, and then the question was to test it. So we built a whole family of bodies of revolution, every variation we could think of, and we tested them in a little wind tunnel we had; it was only ten inches by fourteen inches in cross-section, but it developed test speeds from Mach number two-point-seven to six-point-three. So we tested these things, all right, and then we compared our test results to this theory we'd come up with, and found out it worked pretty good. Then we published a paper.

That's about all it took was hearing this fellow say, "It's exponential." It was an observation of his. Tried to figure out why. I got a [job order] to do the work, and that was about it. I could charge the work of a machine shop to build the models to that, and the test time to run the wind tunnel was charged to that. It didn't require any more than I think my Division Chief's approval. So it was very simple.

WRIGHT: When you talk about the things that were built, did you oversee what happened in the shops?

SYVERTSON: Oh yes. That's another kind of interesting thing. Yes, we'd just walk over to the machine shop. It was across the street. We'd draw it up, and we'd say, "Hey, would you build this and charge it to this number." That's about all it took. We really had a good interface with the machinists.

In fact, going back a little bit to the design of the wind tunnels, we were building small wind tunnels to test out the flow for the big ones. We had developed a set of coordinates for the shape of the nozzle, and I was over there talking to the machinist who was building it, and he says, "I'm having a terrible time with your coordinates." He said, "If I follow the coordinates exactly, then it kind of wiggles." He had a three-pointed gauge called a curvature gauge, I think. Had two fixed points on it and had one in the middle that attached to a dial. He said, "If I ran that over the surface, the dial went all over the place." He said, "It made it very hard to machine."

Again, one lesson I learned was you've got to listen to what other people said. So I thought, "He's right, you know. It's very difficult." So we developed a technique for designing the wind tunnels that assured that when he put this curvature gauge on, it ran very smoothly on it. It turns out that made a better wind tunnel. … But anyway, I always credit that machinist, [Mitch Radovich], with giving us the ideas to build good wind tunnels.

But that kind of interface existed all the times. You'd walk over to the shop, and maybe you'd hang around for swing shift, so you could walk in and see the guys on swing shift at the shop, and you knew the mechanics in the wind tunnel personally, and if you designed something that didn't work very well, they'd tell you, and then you would straighten it out. But it was very easy. There was no barrier there at all. I figured I'd learned an awful lot from those machinists. I didn't have that kind of practical experience, but talking to those fellows, you got it real quick.

WRIGHT: Was there an average length of time that took this process when you came up with an idea?

SYVERTSON: Probably you'd spend a couple of years on one by the time you got the idea and developed it—designed the models, had them built, tested them, analyzed the data, compared it with the theory, and wrote the report.

Then the reports, in those days, we had an Editorial Committee, and after you wrote a report, they would appoint about a four-man committee, four or five-man committee, with a Chairman. They'd read your report, and then you'd meet with them, and they would review your report, and if they didn't like something, they'd give you a hard time about it. It led to a good, easily read report.

I remember George [M.] Low—I expect you know who George was—talking to him once, and he said—he was always on an Editorial Committee at the Flight Propulsion Research Laboratory as "the average engineer." He said he was always the "average" one that they put on a lot of reports. So he went through the same process, too.

That was common. I'm sure it came out of Langley and spread to the other Centers when they were developed, the other Laboratories when they were developed. I'm not sure it's done anymore.

In fact, I had a very bad experience when I was Director. Some people wrote a report that was controversial, and more sensational than scientific, let me put it that way. It's the kindest words I can use. They kind of half snuck it out, without going through a review process internally, for a national meeting, and we had to pull it at the end, and they really got unhappy about it, and so did I. I lost my cool, one of the three times I lost my cool at Ames, but I really did that day. I threatened to fire the whole Division [laughs]—

WRIGHT: Yikes.

SYVERTSON: —because I was so mad at them. But it was a good process, a very good process. The whole thing, the ease with getting approval to pursue an idea was wonderful. The interface with the technicians that really did a lot of stuff for you, and if you listened to them, you could learn from them. And then the writing and editorial process. Oh, we had a Chief Editor, too, Gerry [Gerald E.] Nitzberg. He's long gone. He'd go over it for the Director's Office, and you'd get into some pretty good arguments with him. Oh, then we had a gal, Carol Tinling, her name was, who was head of the Manuscripts Branch, and she went over the English, and I learned English from her. So there was all these interfaces that were really helpful in developing engineers that are researchers, I found.

Anyway, that's how the process—and take the simplest things. I remember a conversation Al Eggers and I had. He was our Section Head or Branch Chief, whatever we called them in those days, and I was his assistant. We were trying to figure out a way to get higher lift-drag ratios for an airplane at, say, five times the speed of sound and six times the speed of sound. We were contemplating it and looking at the equations and this, that, and the other thing, and I said to Al, I said, "We're not going to get anywhere this way. We've got to find a way to change the equations."

Well, he thought about that for a little bit, and he came up with the idea of making the airplanes asymmetrical, basically, and developed a technique or an idea called interference lift. We designed up some models and tested them, and they worked pretty good. Well, we wrote the report. It was written in '55, I remember that, 1955, and it led to the B-70, XB-70 bomber. Somebody read the paper in North American [Aviation, Inc.], and that's what came out of it. It all stemmed from a conversation we had one day at closing time, I think; it was right at the end of the day. And then Eggers picked up on it, and I did a lot of the test work and theoretical work, and we wrote a paper, and it attracted a little attention and led to a whole airplane design.

WRIGHT: That was one of the questions I was going to ask you was how did you get your papers distributed, or your reports distributed, for others to read?

SYVERTSON: Well, there were a whole series. There were the unclassified technical notes [TNs], they were called. Somebody gave me a bound copy of mine when I retired. Then if they were classified, they were called RMs, research memorandum. They were the same document, except one was classified and one wasn't. And technical notes, and then there was another series. If you really wrote a good technical note and it stood the test of time, then they would convert it to a TR, technical report, and they were published annually in a bound volume in Washington [D.C.], and all the TRs for that year were published, and the TNs, and at least the unclassified stuff got to industry right away, got to universities right away. They knew about it and they had copies of the stuff. So it was a regular publication system, a whole series.

There was a wartime report, too, that was published during World War II, but that was before my time. But the technical notes were the key document. In fact, there was a lot of needling going on between our engineers. Somebody you would work with would say, "Oh, you only published one TN this year, huh? You're not going to get promoted." You know, that sort of thing. So you published about once a year. I think it came out just about even for me. I was with Ames thirty-six years, and I think I got thirty-six reports, so I came out even. [Laughs]

WRIGHT: Hey, that worked out good.

SYVERTSON: Then sometimes, too, you would give papers at technical meetings, especially the old Institute of Aeronautical Sciences, the IAS; became the AIAA, the [American] Institute of Aeronautics and Astronautics. But they had an annual meeting in New York. That's where Spreiter was when he came back to [Minneapolis] and had dinner with my mother and I in 1948. He'd been to New York.

There is an annual meeting in Reno now, I think, that the AIAA puts on, because I've gone to that a couple of times, and I know others have, too. In fact, Bill [William F.] Ballhaus [Jr.], who succeeded me as director of Ames, I know he goes regularly to that, or did. But there were the technical meetings, but NACA had a whole series of published documents. Industry knew about them. They requested them, and they read them, as far as I could tell.

In fact, that theory that I was talking about that started with somebody saying the pressure distributions are exponential, that theory, a fellow I worked with, Dave [David H.] Dennis, who's gone now, he helped me with the tests, and he was very good at that sort of thing. We wrote the report together, and it happened that the guys over at Lockheed [Aircraft Corporation] got hold of it, and they used it to predict the stability of the Polaris missile in certain speed ranges. It was only good for a relatively narrow range of speeds.

One day, Dave and I were working, and we got a call from one of the young engineers over at Lockheed, and he said, "We'd like to come over and talk."

Well, they had taken the theory that we had done, and in those days all the calculations were done by a group of women who ran Friden and Marchant calculators, all hand calculations. Well, computers hadn't really come in yet, but Lockheed had one, and they'd taken our theory and programmed it for a computer, and they brought us over a copy of the program, because by then we had a computer, too. So we repeated all our calculations on the computer and compared them with the hand calculations the gals had done, and it checked out okay.

But I got to know more recently a fellow who was—well, he ended up Chairman of the Board of Lockheed Martin. Dan [Daniel M.] Tellep, his name is, and he was head of Polaris at the time, and I told him about that. He didn't even know about it, and he didn't believe me at first, I don't think. But I can remember. Unfortunately, Dave Dennis is gone. I remember he interfaced with these guys mostly, because he understood computers better than I did. So I couldn't confirm it with Dave, but anyway, the industry people knew these reports existed and sought them out, basically, or tried to get the whole series and then go over which ones were pertinent to them.

WRIGHT: Did the industry ever work with you to do specific projects when you were with NACA?

SYVERTSON: Yes, they did, like people from—can't remember the company now; it was one of the smaller rocket companies, Aerojet-[General Corporation] or something like that—developed a sounding rocket called the Aerobee-Hi, and when they read this same paper I was talking about that Dave Dennis and I did, two or three of them came and talked about it, and we built a model of the Aerobee-Hi based on what they told us. Then we tested it, and we applied our theory to it and showed them the results. I don't think we wrote a special report.

Another case, too. The old ABMA, Army Ballistic Missile Agency, is actually Wernher's outfit, Wernher von Braun's outfit. They had a couple of guys come up, because they were testing bodies that were pointed and then cylindrical and then had a big flare in the back. Again, they came and we built models of what they were interested in and tested them and found some funny problems that we finally figured out. But again, there would be this exchange. We had the flexibility to do that. Basically going over to the machine shop and build one, and then say—that's all we had to do. "Build one of these, please."

The other thing that was nice about that, we could test a model at night in the wind tunnels. I was single in those days, so the single guys do all the night shifts. You could run something in the wind tunnel, and you want to make a change, you just walk across the street to the machine shop, and they make a change for you, and you put it back in the wind tunnel. Very, very informal, so that made research easy to do, and you didn't get all tied up in funding and that sort of—I don't think we ever worried about funding, actually, as long as our salaries were paid. That was that.

WRIGHT: Wow. How many of these projects did you have going on at one time—at the same time?

SYVERTSON: Not a whole bunch. Well, a Branch or a Group or a Section or whatever they were called, like the group that Al Eggers had it and I was the assistant of, we had about twenty people in the Branch, or organization, let me put it that way. We'd probably have twenty, twenty-five projects going at the same time, essentially one for each person.

WRIGHT: That's what I was going to ask you, how many people were in your group then?

SYVERTSON: About twenty. I don't remember exactly. In fact, I was commenting coming home from Santa Cruz [California] to my wife that just about all of them are gone. Eggers is around and I'm around, and I don't know anybody else that's left.

I'm anxious to see at the reunion if anybody shows up I know. [Laughter] I know some of the fellows coming from other Centers that I've worked with over the years.

Oh, another interesting one that I'd forgotten about, but it's kind of important, is that I guess into the NASA days or in same mode of operation, Eggers and I came up with what we called the lifting body. It was an entry vehicle that could maneuver some during entry. A fellow down at what's now Dryden Flight Research Center [Edwards, California] read our paper. So he was a model airplane enthusiast, so he built a model airplane that's a tow plane, and he built a model of our lifting body. Both of them were radio controlled, and he'd fly them as a hobby. The Director—oh, think of his name; I've met him.

WRIGHT: Was it [Paul F.] Bikle?

SYVERTSON: Yes, Paul Bikle came by this—I'm having trouble with names again—asked this fellow what he was working on, and he told them about this model airplanes he was building. So they went ahead and he said, "Let's do that full-scale." So they built a model. Had an outfit, built it out of plywood, I remember, the entry vehicle, and originally they bought a Pontiac convertible and hopped it up, and they'd tear across the desert towing this thing. After that worked for a little bit, they finally towed it up with a tow airplane and flew the thing. The pilot's name was Milt [Milton O.] Thompson. I remember him. John [G.] McTigue is coming to the reunion. He was the Project Manager in building them, but he wasn't the guy that did the model airplane work. … [That was R. Dale Reed, he was a very good man.]

WRIGHT: So that was your paper that first talked about the lifting bodies?

SYVERTSON: I don't know where they got ahold of it, but there was a lot of exchange between the Centers, informal exchange. Competition, too, which was very healthy.

WRIGHT: Yes, would you talk about that, how you were able to communicate but yet compete at the same time?

SYVERTSON: Well, all I can remember is that one Center would find out what the other Center was working on through some device, spying, I don't know, and so they'd start a competing program that would have a better idea, a better refinement of that idea. I remember that so strongly in the lifting body business, because Langley came up with what they called the HL-10, and it was a better flying airplane, I think, than the M-2, which is the one Eggers and I had come up with. But the key guy in that was a fellow named Gene [Eugene S.] Love.

Oh, another thing that was important. We used to have to send the reports to the other Centers for comment. I forgot that, and that's an important loop, because I got to know a fellow that worked at Langley. I don't know if I should put this in print or not.

WRIGHT: You could put it in and take it out if you'd like.

SYVERTSON: Okay. I got to know a fellow—I was on a committee with him or something—and he worked at Langley. He said his boss, who will go nameless because he's a friend of mine now, but this report that Eggers and I wrote about interference lift and it led to the B-70, that came back to Langley for comment, and it came to this Division Chief, and the Division Chief brought it to this fellow I knew, and he threw it on his desk, and he said, "You've got to find something wrong with this."

It was that kind of competition. It was sort of friendly, but it was very intense at all stages, because I remember when I was even in the wind tunnel designs early on, there was a fellow at Langley who had been a scientist in Italy and come over after the war, by the name of Antonio Ferri. He was involved in the wind tunnel design work, too. One of the scientists at Ames had gone back and talked to him and then came back. I don't ever remember talking to the fellow, who was also going, a fellow named Dean [R.] Chapman.

But when we came up with our techniques for designing the wind tunnels and I wrote a paper with a fellow named Ray [Raymond C.] Savin about it, Ferri accused us of stealing his ideas. I couldn't see any similarity at all in the ideas, but he thought there was, so I think both papers—his paper—oh, the worst one like this was—got stuck on this one for sure.

We were having a conference, and I had a paper about the interference lift stuff Eggers and I had done, and there was a fellow from Langley had a paper. His name was Charles [H.] McLellan; I remember him well. He had a paper at the same conference that came to the exact opposite conclusions that we did.

You had to go through rehearsals before you gave a paper at a NACA technical conference, and during the rehearsals I gave my paper and Chuck gave his paper, and it was obvious they were opposite. I remember they handed them both to me, and he said, "Okay, you combine them and give one paper." That was a neat job. So I did the best job I could of combining them into the paper. Anyway, I gave the paper at the conference, and some guy in the audience from industry got up, and he said, "Okay, which way is right?"

Harvey Allen, who was the Chairman of the session, he got up and he said to me, "Don't answer that." [Laughs] So the competition was very intense. Always came to the opposite conclusions. The trouble with that, they were probably both right, but under different conditions. Anyway, it was a lot of fun; exciting.

WRIGHT: Very interesting that information from one Center to the other reached the other Centers when you consider that we didn't have Internet, you didn't have tele- or videoconferences. Long distance phone calls were not common. So it was amazing how that information traveled from one to the other.

SYVERTSON: Yes, those days if you wanted to call off Center, off the Lab, you had to get the Director’s approval. If I wanted to call home, I had to—

WRIGHT: Goodness.

SYVERTSON: —which you didn’t do. There was a pay phone someplace, if you could find it, but you had no authority to call outside the Center, only the Director. It is interesting. But there were the technical meetings, which were really good.

I can remember when Harvey came up with the blunt body idea, Harvey Allen, and then Max [Maxime A.] Faget from down at Houston really figured out how to apply the idea to a manned spacecraft. He came up with all the details, like the—we called it the Faget couch. It was a foamed rubber, something or other, couch that the astronaut would lie in when he experienced the high G [gravity] forces coming back into the atmosphere. I can remember at the conference when Faget mentioned all this stuff, when he presented a paper on it.

Sometimes the conferences were not formal conferences with industry there and universities there, but just meetings. Just people from one Center and another would come to the Center, and you’d just have an exchange. I had a thought that slipped my mind there for a little bit. But anyway, there was a lot, a lot of exchange of ideas.

Oh, there was a time, too, when we had a sort of an internal competition again. There was something called Round Three, and if I remember Round One and Round Two, [I believe] Round One was the X-1 supersonic plane, and Round Two was the X-15, the Mach six research airplane. They were looking at something to go like close to orbital speed, maybe twenty,

twenty-five times the speed of sound, a research airplane. We got involved in design studies, and Langley had one approach, and Ames had another approach.

The main competition was between Ames and Langley, because their missions were too similar, at least in those days. But we did extensive studies on those wind tunnels tests and theoretical calculations. I mean, we had big teams put together that would work on these projects. Then we'd meet and sort of exchange ideas with the other Center, more on a competitive basis than openly, but the exchange was there. There were just a lot of exchanges. I got to go to Langley a number of times, even when I was a fairly young engineer.

WRIGHT: How did you travel there? Was it commercial flight?

SYVERTSON: In those days a lot of it was by train. In fact, I remember one trip to Langley I took the train to Chicago [Illinois], flew to Baltimore [Maryland], and took a boat to Langley. [Laughter] There used to be a boat from Baltimore that came into Old Point Comfort at Langley, and it was a standard way to get down there. I took it a couple of times.

WRIGHT: Not a fast trip, but sure trip, huh?

SYVERTSON: Oh, it was overnight, I think, yes. It went outside the three-mile limit or something, and so they had gambling on board, that's what they did. [Laughter] So you didn't get much sleep on that trip.

WRIGHT: The work that you were doing at Ames those first ten years, a lot of that was during the Cold War years. Were you working on things that were classified and secretive, things that you didn't discuss with other people outside the Center?

SYVERTSON: Well, there were a number of projects that were classified. Virtually all of our tests, at one stage, was classified confidential. There was restricted, confidential, secret, and top secret. In those days, I never saw anything that was top secret. I did see an occasional document that was secret. But virtually everything, and some of our reports, were confidential.

We had a good interface with industry. Like in the very early days of the Ballistic Missile Program, Convair [Division], which is part of General Dynamics [Corporation], had the contract to study the entry problem of the missile. They were calculating trajectories. They had a computer, a big computer, which I don't think we had in those days. But they were calculating trajectories, and they'd send the trajectory information up to us. I was busy plotting it up, and so was Harvey Allen.

Well, Harvey recognized certain characteristics in these trajectories. The first thing he told them was that the decelerations were so large that gravity didn't count, so you could neglect gravity. Well, that made the equations of the motion during the trajectory simpler, and from that he could simplify the heating and this, that, and the other. That's how he came up with the blunt body idea. I always thought if I'd been as smart as Harvey, I could have come up with that idea. But he was a very, very creative guy.

There was lots of simulation. Like when I came to work—I worked, as I said, in the six-by-six building, well, I don't know if you've ever heard of R. T. Jones, Robert T. Jones. He's the man who thought of putting swept-back wings on airplanes. He started at Langley and came to Ames, and unquestionably the smartest man I ever knew. He was incredibly bright. He was in the office next door to where I was placed when I started. Harvey Allen was down the hall. I figured out once, in that building—it was one building at that time—there were two future Center Directors, two future Deputy Center Directors, one future Associate Administrator, four members of the National Academy of Engineering—future members—and one future member of the National Academy of Sciences. Now, there's a lot of overlap in that. A lot of people had several of those. But that was an incredible pool of talent.

Oh, I've also got a quote. Abe Silverstein. As far as I understand, he said this once. You know who he was? He kind of epitomized the NACA in many ways for me. But he is quoted—I've been told he said once, "The problem with the NACA is that Langley gets all the people, Lewis [Flight Propulsion Laboratory, Cleveland, Ohio] gets all the money, and Ames gets all the brains." Now, of course, we liked that one. He was Director of Lewis, but I've been told he said that. You may watch for that if you run into any Lewis people, if they had ever heard that. But it was conveyed to me. Ames had incredible talent at the time.

WRIGHT: Was there a lot of competition for your spot? Were there a lot of people from the outside wanting to come to work at Ames, and did you see a lot of turnover, people leaving to go to other places?

SYVERTSON: Oh, no turnover to speak of. A little. We didn't see any turnover to speak of at all until Lockheed moved into this area, and then they were paying more than Ames was, so some people left to go to Lockheed. But some of them regretted it, because they made the mistake—I always tell my kids, "If you quit a job, leave them laughing." Because some people left Ames, made nasty comments, went to work for Lockheed. Lockheed got in a situation where they were letting people go like crazy, after the Cold War, I guess, and they wanted to come back, and the man who was Associate Director, number two man at Ames then, Jack [John F.] Parsons, wouldn't take them back. So my motto is leave them laughing.

WRIGHT: Leave them laughing. That's a good one.

SYVERTSON: But we did lose some then. Some tried to get back on. One fellow started at Ames, went to Lockheed, came back to Ames, and then went to industry again. He was a very, very bright guy, from Minnesota like me, but he shifted around too much. But there was very, very little turnover. It was sort of you were there, a lot of people, for their whole career.

WRIGHT: You mentioned earlier that you came to work there at a time of great changes. Could you share a little bit more about that?

SYVERTSON: It was a time when there were lots of new things to do. … [Supersonic flight was still experimental at best.] We didn't have jet transports or anything like that. I remember going to Washington [D.C.] in DC-6s and the like, old propeller airplanes, take forever.

But there were a lot of opportunities. There were so many new things to do that you could really—in every aspect of aeronautics there was, from propulsion to aerodynamics, the higher speeds, understanding transonic flows. Spreiter was good at that, my friend Spreiter. And there were others. Even the handling qualities, how these things flew, were changing, and we really were just beginning to build sophisticated simulators, which Ames, and Langley, too, became very good at building, very, very fancy simulators. So then there were very few supersonic wind tunnels, so that was a whole new field. Space hadn't started yet or ballistic missiles or any of that. But the technology was exploding so fast, you were almost running to keep up.

WRIGHT: What was your typical day like? What time would you get to work, and what time did you get to come home?

SYVERTSON: Oh, it varied a great deal.

WRIGHT: Did it vary based on what part of that process you were in?

SYVERTSON: Well, what you were doing, and sometimes if you had tests going in the wind tunnel, you were around lots of hours. Then sometimes you were in a car pool and you had to go at a certain time whether you wanted to or not. Everybody having two cars is a new invention, you know, a new trend now that everybody's got two. In those days you were lucky to have one, and you often were in car pools. I remember—well, of course during the gas shortages, but that was in the seventies; that was '73, I think, the gas shortage. I went back in a car pool. Then later, well, this is past the NACA days, but when I ended up in the Ad[ministration] Building, I used to come in very early, seven o'clock sometimes, like that, because you could call [NASA Headquarters] Washington [D.C.] before they went to lunch. [Laughs] That was one requirement.

WRIGHT: That's your incentive, huh?

SYVERTSON: Yes. In fact, to keep track of the time in my office, I had a clock on the wall, and I had them put a second hour hand that was ninety degrees ahead of the regular hour hand, because that was Washington [D.C.] time. I had that one painted black. [Laughs] But anyway, the later days, at least, there was a lot more exchange of ideas.

Oh, another thing I forgot. We used to have what they called inspections. Once every—I guess every Center, every Lab did it once a year—Ames would do it one year, Langley the next, Lewis the next, maybe [the NACA High-Speed] Flight [Station, Edwards, California] the next. So about every three years you'd have an inspection, and they would invite congressmen, industry people, academic types, even the local politicians, into the Lab, and they'd arrange a series of about eight talks. They'd hire buses and they'd haul the groups around to these various places, and you'd give them a talk. They'd run groups, so you'd maybe give three talks a day. But it was always the same talk. It was canned. I mean, in fact, the worst problem with that is I got right in the middle of one once, and I had given it so many times I forgot where the hell I was. [Laughter] Fortunately, it came back to me. It became rote [routine]. [Laughs] I've got to tell you a couple of other ones.

One time we were just building the ten-by-fourteen-inch wind tunnel, and we'd built a water tunnel that demonstrated how the wind tunnel worked, but the trouble with that goddamn thing was it leaked. So here I am, rehearsing this talk for the people from Headquarters. I remember for sure John [F.] Victory was there. Harvey Allen was there. "Smitty" [Smith J.] DeFrance, I guess, was still Director. Here I was giving this talk, and I was a very young engineer. I was very nervous, because I wasn't used to giving talks, and the tunnel behind me started to leak, and the water started to trickle out in front of me. It was obvious I was nervous, and Harvey broke down. He started to laugh, and he broke up my whole talk, because it looked like I was too nervous. Damn water tunnel. I've never had them build another one since. [Laughter]

Then another thing I remember about the NACA was John Victory, he always came out to Ames in the spring, and the reason he came out in the spring is he loved fresh cherries. He'd come out at cherry time, and he'd sit there doing his reviews and eating cherries, and the pits are all over the place. [Laughter]

One other thing is—I think it was a very informal operation, and everybody knew him, but the first time I met DeFrance at lunch, I happened to be sitting at the table. He came and sat down across from us. We had a long bench, long tables. And by God, he knew what I was doing. You know, here I was, I probably hadn't been there a year yet, and he knew what I was doing. I was really impressed with that, that there was no barrier at all in the communication within the Center.

That is how informal it was. I remember once at lunchtime a couple of guys had got into an argument during the morning about whose car was faster. Somebody had an old '37 Chevy and somebody had a sports car, and they were arguing over [whose] was faster. So Harvey Allen, who was Ames' second Director, but a Division Chief at this time, refereed a drag race down the main street of Ames, which I don't think would happen anymore. That's the way it was. It was much easier.

WRIGHT: How big was the workforce?

SYVERTSON: When I came to work, Ames had about 850 people; no contractors. Well—no, they didn't. Not even in the cafeteria did we have contractors. There were civil servants running the cafeteria. Then they gradually started contracting more and more things. When manpower got tight and the work, you couldn't handle it with the civil servants, then they started bringing in contractors in all fields. I remember I was even part of that, so I remember well that transition.

WRIGHT: Did you have other duties as assigned? I know you were working on the projects that led to your reports, but did you work on committees or subcommittees for NACA?

SYVERTSON: Oh yes. Most of the committees were made up of industry types. They were advisory committees in aerodynamics and this, that, and the other thing. Usually there was one senior man who represented the Center. But if that committee came to visit, you would give them a talk on what you were working on.

There was good interface with the Air Force, or the Army Air Corps in those days; I can't remember when the transition was. But they would have a committee that would meet there, and I remember giving them talks about some of the things we were doing pertinent to what they were interested in. You served on Editorial Committees. Maybe you didn't write that report, but you were one of the four or five that reviewed it. I remember the first one I was on was a paper written by Dean Chapman, a very senior scientist at Ames. So I kind of learned about it, learned things from him. I remember the experience, because I took the time to check the mathematics, and he thought that was great. But you did that, and then you also took a turn at running the wind tunnels, running the tests for somebody else. We'd have maybe three guys on the wind tunnel, plus a couple of mechanics, and they ran a lot of the facilities three shifts a day, so you'd end up on swing shift or graveyard shift periodically. Especially as a young engineer, you caught a lot of that. It was good experience.

Oh, Chris [Christopher C.] Kraft [Jr.] told me once that he thought the way the NACA did business, of getting the theoretical idea, designing the models, laying out the test program, running the test, analyzing the data, and writing the report, made [some of] the best systems engineers. That training made the best systems engineers. Now, this is Chris' comment, and I'm sure he was right. But it just stuck in my mind.

You had to go through the whole gamut from one end to the other of the project, and you had to make sure all the pieces fit together. And you had to understand it well enough to lay out the computing sheets that the gals with the Fridens used to calculate your test results, or to analyze your test, calculate the data from your test results; reduce it to a form that you could use. So you had to lay that out. I can remember they had these computing sheets, and they're about that long. [Gestures] I remember they had fifty-five columns, fifty-five steps that you could lay out and tell the girls what you wanted to do in each step. Sometimes, I remember, I had a few where I had two or three of those tacked together. There were maybe a hundred steps in the calculations.

So, you know, Chris' comment was very astute. You had to do a lot of different things to carry out a project. You didn't have somebody design the model for you. You designed the model yourself. You laid out the test programs yourself, and then you ran the tests, or part of the tests. Probably the first series of tests you would run yourself, and then you would turn it over to another test crew. So it was great training, great training.

WRIGHT: I was going to ask you at what point did you start supervising others? [Syvertson laughs.] And how did that change what you wanted to do?

SYVERTSON: I can remember debating within myself, do I want to stay a researcher only or do I want to go into management, and I kind of vacillated for a while. Then the thing that finally flipped it a little bit was that my boss, Al Eggers, got married, and in fact, I had introduced him to his wife. He's still married fifty-some years now. I had introduced him to her—or her to him, I can't remember—and he went off to get married back in Massachusetts and he went off on his honeymoon, and so I had to run the place while he was gone. So that's what really—I kind of got used to it then.

Then they would build organizations around people. Let's talk about Harvey Allen; he was my Division Chief when I came to work. He went up ultimately to be an Organizational Director and then Director. We called them Organizational Directors; they had a whole Directorate. He moved up to that level, and they made Eggers a Division Chief. Then they created some Branches, and they gave me one of the Branches. A new wind tunnel, a new high-speed wind tunnel, had been built, so I got to be in charge of that. I've got to say perfectly frankly, if it hadn't been for Eggers and especially for Harvey, I'd probably still be down in the basement someplace. Harvey had a number of close friends at the Center, but I never felt like I was one of them, but he would surprise me at times. Let me give you a few examples. Now, this gets past NACA into NASA.

Well, I became head of a three-and-a-half-foot wind tunnel out at Ames, and then for some reason, Eggers had this idea to create what he called the Mission Analysis Division to study future missions for NASA. So he arranged for me to be head of that Division. So we had about twenty people at Ames. Then Eggers went to Headquarters, and he decided it would be good to have a function like that at Headquarters, so he arranged to have the Division—I actually reported not to Ames management, but directly to Headquarters, even though I was a Division Chief, which was weird. It was uncomfortable, let's say, too.

Also, I was given the right to go to Langley and to Flight and to Lewis, and recruit people to run this Division up to about fifty people, which I did. [Laughs] This is NASA, but some people at the other Centers didn't like that too much, and I had a lot of—well, at Langley they'd send somebody around behind me if I was talking to [a potential recruit], and talk them out of even thinking about going to work [for me]. At Lewis I'd have to go see Silverstein first. That's how I got to know him so well, and I really liked him. But he'd threaten me with mayhem if I recruited any of his, literally. But anyway, it worked out, and I ran that Division, and I was round the whole circuit. It got me a reputation outside of Ames and throughout NASA.

Well, anyway, Smitty DeFrance retired, and Harvey moved up to be Director, and Jack Parsons stayed as Associate Director. A week or so or a month after Harvey became Director, he sent Parsons down to my office where I was working in the Mission Analysis Division, and he asked me, "How would you like to take Harvey's old job?"

Well, I literally almost fell out of my chair, because I had picked two other guys as most likely candidates. But I didn't ever quite understand it. In fact, I've asked Harvey's closest assistant, Jack [John W.] Boyd, about this. I said, "Why the hell did Harvey do that? It really surprised me." But anyway, he did. So then I took over his job, and now I had a Directorate [with] three Divisions.

Then Harvey decided to retire. He didn't like being Director. He was only Director a couple or three years, and he didn't like some of the things you had to do as Director, like let people go, or some of the tough decisions you had to make. So he decided to retire, and Parsons, the Associate Director, was going to be Acting Director. Well, Jack Parsons took a week or two off before becoming Acting Director, and while on vacation, he got sick. In fact, he got terminally ill, so he never came back to work. Harvey had a retirement party, and they said, "Okay, you can retire, but come back Monday as Acting Director."

Well, Harvey wouldn't have anything to do with that. You had to know Harvey Allen. He was a free spirit of the first order. Anyway, after he came in on the first Monday, and he walked into my office and he said, "I'm going to stay home. You run the place." That was the charter I had. I was the youngest of the people and least experienced of the people in that tier. In fact, all of them were senior to me, and he gives me the job. That one really surprised me, between the two. I never understood why he'd do that [for] me. I mean, it was obviously greatly to my advantage, but I never understood it. I never got a chance to ask him before he died. I've asked Boyd about it, and he said he didn't really know, either.

But anyway, then they went out looking outside for a director, and they found [Dr.] Hans Mark. But with Hans coming in from the outside, they needed somebody that knew the Center, so they made me Deputy Director, because I knew the Center. That's kind of almost by accident how I fell in to it.

WRIGHT: It came out that way.

SYVERTSON: Yes, it's really—they really treated you very well. I mean, NACA and NASA, you know, just outstanding how they treated people and how they knew their people. How they did some things, I don't understand. [Laughter] I don't think we had the advertising for jobs like we have at the end, but they just seemed to do things like that.

WRIGHT: Well, tell us about when NACA was going to transition into NASA, how that affected your job. What was it like at Ames, and what were people thinking how it was going to affect them?

SYVERTSON: Well, the one thing that I remember, the NACA, and Ames, in particular, maybe, was kind of below the radar. Few people knew we were there around in this area. Our Public Affairs men didn't do much. Then it became NASA, and it was much more interfaced with the press and things like that. That's the main change I can remember. The paperwork grew at that time, too. You were much more in the spotlight. I think that was the primary change. It just wasn't that way in NACA. You just kind of did your thing and people left you alone. Now you were much more in the limelight.

WRIGHT: Were you free to choose those projects you wanted to do?

SYVERTSON: For a little while, but they gradually increased the level of control from Headquarters, and Headquarters grew by orders of magnitude, in my opinion. I don't remember the details. But they just, you know, [with the NACA] you didn't have to get that kind of approval. Headquarters told you what area, I think told the Center what areas they could work in, but that was about it. [With NASA], there was much more detail control.

I remember in their control system, there was—I've forgotten. It's been too long; I'm sorry. But there were various levels of control, and an RTOP [Research and Technology Operating Plan], which was the smallest one. Pete [Richard H.] Petersen, who was Director of Langley, was asked to go up to Headquarters for a while and become I don't know whether it was Acting—or actually, Associate Administrator OAST [Office of Aeronautics and Space Technology], and his primary goal was to raise the level of control. …

We used to try to find ways around it, but the project management techniques which were great for things like flight projects, not only the manned projects but the unmanned projects and things like that, and the aircraft, building research aircraft, that project-type control began to be applied to the research part of the organization. I think that made it more difficult to be creative. If [something] didn't work, you could hide it, and nobody ever knew you did it. But [under NASA] that's not possible; it wasn't possible. It was a very gradual transition. It didn't change much the first day, but in the first five years, it changed.

WRIGHT: Did you see a lot of people leave, or did they stay?

SYVERTSON: Well, the Center grew, and we went more and more to contracting, so there were more people and more areas. Ames got into life sciences, which they'd never been into before. So there were more people coming in then going. I don't think anybody left, in particular. I don't remember. Some did leave, I guess, to go to industry, because industry was growing, too, and there were opportunities in industry.

One other thing I just thought of. These things come back to my mind. A guy from industry told me that in NACA days they brought their models to test in our facilities not just because of the facilities, but because of the people who would use the facilities, the in-house staff, understood the research problems, and they were as valuable as the facilities themselves. So it was this expertise that kind of went along with the facilities that the industry sought after. If they ran into a problem during their tests, there was somebody who knew about that problem right there.

WRIGHT: I was going to ask you, though, before we leave, looking back on your NACA days, what do you feel was your greatest contribution from your work? Where do you think it had the most impact?

SYVERTSON: Well, there were two, I guess. One was the techniques that we developed for designing supersonic wind tunnels. That refined them a significant step and was used to design—although I wasn't doing it personally, the techniques were used to design every supersonic wind tunnel at Ames from then on. So I think that was probably important.

The other thing was the theoretical work that Eggers and I did, both in that theory I told you about that Dave Dennis and I developed. I think that was important. It was used by industry, I know, because I was told that, and some of the theoretical work we did to develop ideas for new shapes for supersonic flight that came out of the theoretical work. I think those two things, and especially in the kind of intermediate speed range, let's say from three to ten times the speed of sound. I don't think I did anything that was particularly useful going up to orbital speed. Harvey did that and Max Faget and people like that. But in the intermediate, I think some of the ideas were used, and every now and then I run into something that shows up again.

WRIGHT: During those days was there a time or an aspect that you found to be the most challenging that you had to deal with, working?

SYVERTSON: Oh, boy, that's a tough question. One that I've thought about is how to deal with and manage people who are smarter than you are, because we had a lot of people at Ames who are a lot smarter than I was. But the people problems were very challenging, and you had to understand what the other guy was thinking if you were trying to manage them.

I know I had a couple of friends who had worked for me and I got to know real well who were African Americans, and periodically something would happen, especially this one, Oscar Whitfield [phonetic], and he'd get upset. And I could see why. Like he would have a minor complaint, and instead of talking to him face to face, they'd bring him in with four other guys, all nonminorities, and he felt like he was going before an—

WRIGHT: An inquisition?

SYVERTSON: —an inquisition, yes. So I'd go down and talk to Oscar. Even when I was Director, I would do that.

But learning how to deal with high-caliber people was the most challenging, and, in many ways, the most rewarding, other than the technical stuff, which was fun. People problems are never fun, but they can be rewarding, especially when you see people come up through the system and do well.

WRIGHT: Well, before we close today, do you have anything else you'd like to add? Jennifer, do you have any questions?

SYVERTSON: No. Well, another thing I remember saying as a young—well, middle-aged—engineer was that one of the things that always impressed me or bothered me a little bit about both the NACA and NASA is if you do something right, you get rewarded three times. If you do something wrong, you get dinged three times. And so much depends upon your reputation. I saw people who were pretty good, made a big mistake, and then kind of fell by the wayside. But I always had a feeling I was—you were sitting on the edge, a knife edge or something like that, and you either fell one side or the other.

But I still think the most challenging were the people. But also it was really great to be associated with some of those people, and you learned a lot. If you kept your eyes open and your ears open, you could learn from the machinists, you could learn from the mechanics, you could learn from other engineers, and that's another thing I liked.

Oh, and the lunches. I'll never forget the cafeteria. [Many of us] went to lunch at [eleven]-thirty, because that's when Harvey went to lunch, and having lunch at the same table with Harvey was a real experience, because he was far and away the most creative guy I ever knew. R. T. Jones was the smartest, between the two of them. But just listening to Harvey was a real privilege almost, to sit there and listen to him at lunch. I thought that was—the caliber of the leadership that they had at Ames when I was young, Eggers, Allen, DeFrance; Parsons, in his way. Parsons was a little different, but he understood people.

Oh, and there was another fellow. I'm sorry I forgot him. Vic [Victor I.] Stevens [Jr.]. He was Harvey's Assistant Division Chief at one point, and he taught me a lot about management, how to get things cleaned up and get them simple, and how to deal with people. So, again, the caliber of the people. You could learn so much from them. Anyway, that's about it, I think.

WRIGHT: That sounds great. Did you have a couple of questions?

ROSS-NAZZAL: Sure. I'm wondering if you can paint a picture for us of what it was like coming to Ames from Minnesota. What did the facilities look like, and what was going on at the time?

WRIGHT: And the climate.

SYVERTSON: The climate is a big difference. [Laughter] I remember coming in here. I had a beat-up '41 Ford that I'd bought, first car I ever bought, and I had trouble with it all the way out. Anyway, this is not changed so much, but I was looking for somebody to help pay expenses and to do part of the driving. This has got nothing to do with coming to Ames, really. But I picked a fellow who was a Chinese exchange student. He didn't know how to drive. But I learned a little bit about China. He went back to China and became Vice President of a university over there. I lost track of him recently.

Well, there was a huge change. One of the things that got me to leave the lab. One thing that got me to do what John Spreiter said—is quit work and go back to school full-time—was the leadership that the lab in Minnesota was not good. I don't know where they got those people, but I had a feeling—not that I had a big ego, but I thought I was smarter than the people I was working for because of some of the decisions they'd make. I never had that feeling at Ames at all. [Laughter] But it was really coming into the environment of the caliber of the people and the huge facilities and variety of facilities we had at Ames. It was like going from the smallest to the largest, you know. It was just mind-boggling, basically.

John Spreiter is gone, and I never did figure out—it's funny; not funny. He worked—ultimately ended up working in Space Sciences Division, and when I took over Harvey's job as Director of Astronautics when he moved up to be Director, John ended up working for me, which was kind of reverse because, again, he was smarter than I was. Anyway, he ended up a professor at Stanford [University, Palo Alto, California].

Then he got something, and he knew he was going to die, was convinced of that, because—he had some property in Hawaii, and the last [few] months of his life he went out to Hawaii and just—it was clear he knew he was going to go. He had some form of cancer. I never did talk to his daughters—he had three daughters—to find out exactly what happened.

But I think the differences were—the primary one was the number and the caliber of people at Ames, compared to Minnesota. Then, of course, the facilities that went along with—they had created the facilities. So I always thought getting the right people is the most important, because they can conceive of the facilities and they can sell the programs.

But the caliber of the people is number one, and somehow Ames did that. That was Parsons and DeFrance, or DeFrance and Parsons, I should say, and Harvey, to an extent, too. Somehow they really could pick [people], you know. There were a lot of good people. Harry [J.] Goett, the first Director of Goddard [Space Flight Center, Greenbelt, Maryland], he came out of Ames as a Division Chief. There was quite a crew. Well, in fact, one of the Directors of Lewis started his career at [Ames], well, a Director of Langley started his career there. A Director of Lewis started his career there. A Director of Goddard started his career there.

WRIGHT: Wow.

SYVERTSON: So there was a lot of people that went out. Probably not as much as Langley, but it was sort of like Langley, because Langley, all the people like [Robert R.] Gilruth and Chris Kraft and all those, Faget and all those people, came out of Langley. Anyhow.

WRIGHT: Another one?

ROSS-NAZZAL: No, we're out of tape here.

WRIGHT: Okay. So are you about through for—

SYVERTSON: It came out even? [Laughs]

WRIGHT: I think it did. Well, we thank you for the day.

SYVERTSON: It was fun.

WRIGHT: It was fun for us, too.

SYVERTSON: I wish I could remember some of the things. If I think of some of the names and some of the terms I forgot, if I see you during the course of the reunion, I'll try to pass along a note.

WRIGHT: Okay.

[End of interview]