NASA HEADQUARTERS NACA ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

STANLEY F. "STAN" SCHMIDT
INTERVIEWED BY SANDRA JOHNSON
LOS ALTOS, CALIFORNIA – JULY 15, 2014

JOHNSON: Today is July 15, 2014. This oral history session is being conducted with Stan Schmidt at his home in Los Altos, California, as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project, sponsored by the NASA Headquarters History Office. Interviewer is Sandra Johnson, assisted by Rebecca Wright, and we're joined today by his wife, Meredith Schmidt. I want to thank you again for agreeing to talk to us. We really appreciate it, and allowing us to come into your home today. I want to start by asking you first a little bit about your background and your education at the beginning, and how you first learned about the NACA and decided to come to work for NACA here at Ames [Research Center, Moffett Field, California].

S. SCHMIDT: [In early June of 1946] I was in the Navy, at Marquette University [Milwaukee, Wisconsin]. I joined the Navy Air Corps in 1943 because my mother and father would sign [my enlistment] for there. They wouldn't sign otherwise to allow me to enlist because I didn't want to be drafted into the Army. I joined the Navy Air Corps, and one of the reasons [my parents] would sign is because I got two semesters of college before I was supposed to be sent to pre-flight. [In the summer of 1944] the Navy [decided they] had enough pilots so I [would not be sent to pre-flight training. Instead, the Navy] gave us all a test, and [if we did well in this test] we had a choice of [being transferred to the regular V-12 program or go to deck officer's school. I passed the test and had to decide] what [branch of engineering I wanted to specialize in]. I didn't know anything—we didn't have electricity at the ranch, even. My dad had talked about diesel engineering as being an important thing. I was sent from here to of all places Dickinson, North Dakota, for my first two semesters. The Navy had schools all over the country where they were using this V-12A [Navy College Training] Program, they called it, that [the participants] were supposed to go after two semesters to pre-flight. Dickinson State Teachers' College was one of them, in Dickinson, North Dakota. I didn't mind that too much—[Dickinson] was about the size of Hollister, which is where I was born.

I was shipped off to Dickinson in 1944, February of '44. A bunch of us went up there together, and one of those that went with us was a ham radio operator. He happened to be right next door to me, in the room next door to me, so I put down communication engineering as my first choice, and second choice, I put down mechanical engineering. There's some little irony in the fact that if I had known that if I'd have put mechanical engineering first, I'd have been sent to University of California. I tell you that would have had an effect.

As it was, I was shipped to Marquette University after two semesters. I spent five semesters there, and because I had one semester in junior college before I entered, they gave me some credit from that. By taking 22 units in my final semester, I was able to qualify for a bachelor's degree, which I got in 1946. In June of '46, I was released from active duty. I had a bachelor's degree—was mailed to me, actually—and released from active duty. That's June of '46, up at Shoemaker [U.S. Naval Training and Distribution Center], I think, was the name of the Navy base that released me. It's up a little bit north of here.

Then, I hitchhiked home. [At that time an honorably discharged GI could apply for unemployment.] I wasn't sure I would qualify for it—it was something like $20 a week for so many months when you were discharged, and I qualified for it, so I went back to the ranch. Then, they had some sort of place run by the state where you could apply for work as an electrical engineer, and so I applied there. Also, I had a recommendation from one of my teachers in my senior year, that he'd gotten out of the Navy and he recommended Hewlett-Packard. I wonder what would happen if they'd taken me. He recommended me there, so I went up and got the interview with them as a result of having this letter, but they weren't hiring anybody unless you had a master's degree.

I also applied to Stanford [University, California] and got accepted to go ahead for a master's and later on a Ph.D., I hoped. Then, it came up that there was an opening at Ames, and so, I came up and was interviewed in early July, I guess it was, and there was a manufacturers' conference, I think was going on. That's what I was told. I always thought I went to work there in August, till I started reading my mother's diary, and then I saw no, it wasn't. I must have got interviewed, but they weren't going to hire me, then. They said, come back in two weeks after this conference was over. I figured that's the end of it, that there just weren't any jobs. I was content that I'd go to Stanford, and so, as it turned out, they did [have jobs at Ames]. I was hired, so I had no great plans about going to NACA. It just was by chance.

JOHNSON: Did you know much about what they were doing with the NACA at Ames at that time?

S. SCHMIDT: No, absolutely nothing.

JOHNSON: Nothing?

S. SCHMIDT: Nothing at all. I had one job offer in Milwaukee, but my family was out here, and that brought me out here. If I didn't get that job, I was going to go to Stanford, which might have been a better plan, really, I don't know. So much of my life hasn't been decided by me. I took the job. I had two choices, even, and wouldn't I know, I'd make the wrong choice, that I could go either to the electrical branch or I could go to the instrument development branch. I liked working with vacuum tubes and this sort of stuff, really enjoyed that, so I should have taken that. My schooling was at a power school, Marquette University, so I decided, well, I better go to the electrical engineering branch.

So, I chose to go there, and then after five years, I left there. I'll tell that later. That was kind of what led to my choices of the power branch, which, at that time, the Branch Chief was Jeff [Andre G.] Buck, and Merrill [S.] Nourse was the assistant. Jeff Buck, when he was there—and he was there at least two or three years—at first, he would go out and find jobs. He found a job for me to work on a stroboscopic Schlieren system, self-synchronous stroboscopic. There was a fellow there that was working on it, but he went on vacation and Buck assigned me to take his place. I didn't like that too well but it turned out that I was the one that did almost all the engineering on it. We had a self-synchronizing stroboscopic Schlieren system.

Harvey [Julien] Allen was one, I guess, and Jack [Jackson R.] Stalder was the branch chief that was in charge, but was in a little 2 x 2 [Foot Transonic Wind Tunnel] Wind Tunnel, which was under the 16-foot Wind Tunnel. A fellow named [L.] Frank Lawrence and I really developed it. He was the optical guy. A Schlieren system is one where you can visualize [air flow about a model]. I think there are some pictures in that [document], I sent. You can visualize what's going on. We had two Schlierens—one of them was split from the other one, and it gave us the ability to monitor with a phototube any things going on.

[The aeronautical engineers] knew, I guess, from theory or maybe from many years of practice, that if you took a blunt trailing edge [model], you would get this Karman Vortex Street peeling off the back of it. [With the model size we had and the air flow over it, the vortices would peel off at a thousand times per second or more. Our] objective was to be able to see [the vortex street with the Schlieren system] and then slow it down, so you could see it in slow motion. Actually, you aren't seeing it in slow motion. You're seeing the effects of many, many vortices, one different from the other. This is going on at pretty high rate, I think it was in the order of 1,000 or so cycles per second, [these vortices are] peeling off.

We first got so we could see [a repetitive signal coming from] a phototube [which was getting its light from a steady Schlieren system picture located at] the [blunt trailing edge of the model]. Then, I converted [this signal] into a [train of pulses] that would [ignite] a mercury arc lamp for the main Schlieren. [The picture from the main Schlieren was the Karman Vortex Street since the flashing of the Schlieren lamp was in synchronism with the peeling off of the vortices]. Then, I designed a scheme by which I could let it go in slow motion for a little while. It would just put a variable delay between the sensing of the pulse from the [phototube], and when the light was flashed, and that delay would literally build up and then jump back, so it looked like it was in slow motion [briefly before] jumping back [and starting motion again].

I found out later, I thought, "Well, if this is so terribly important, what they really want to have is a picture of the single vortex coming off and what it does." I proposed how to do this with the mercury arc lamps, really, and triggering things, and a high-speed camera that would take the pictures, which was at that time, they didn't have like they have them today. There wasn't much of anything. I'll tell that when I talk about the Free Flight Wind Tunnel later. That wasn't understood, so I proposed a way by which we could get at least a short burst of high-speed pictures.

That was never approved, and then I found out later, this whole thing wasn't for the purpose of doing research. At least, I don't think it was—its purpose was to show to congressmen so they'd get approval of budget because it was very impressive. I'm pretty sure Harvey Allen and the other people in charge had that in mind. They weren't interested in taking a single picture of it that would show one [vortex. Instead,] the [atmosphere] of [the noisy wind tunnel and the picture on the Schlieren screen were the selling point they really wanted] as [it was really impressive to a congressman]. At that time I [was too young to understand].

We got it working good and they put this self-synchronizing stroboscopic Schlieren system in quite a few of their wind tunnels after that. That was handled by the electrical branch for a long time, rather than the instrument branch, because of Jeff Buck, that got promoted in, that we were successful. You want me to go on with what work I did?

JOHNSON: Yes, go right ahead.

S. SCHMIDT: One of the first jobs I was working under [John C.] Dusterberry. By the way. I think I had something to do with John being able to get out of the electrical branch. I'll tell that later, too, when I had a branch. I have to think a little bit to remember where I was. The old brain doesn't function as fast as it should.

WRIGHT: Sounds pretty good to me.

JOHNSON: Yes, no kidding. When you first started?

S. SCHMIDT: Yes, the first project I got in the branch was I was to go out and watch a technician pull a mandrill through the ducts of the 6 x 6 [Foot Supersonic] Wind Tunnel. They were just building it at that time. They had this young engineer go out and observe this mandrill being pulled through the ducts to see that they were clean. I don't know why they needed an engineer for it, but they did. That's supposed to give something to keep the kid busy.

Then, I was to do a job that I really didn't like at all. I'd taken drafting in high school and I wasn't half-crazy about drafting. I wasn't very good at it, I think, is one of the reasons. I was to draw up the ducts and their locations when I was through with that. That was my first job, and then, when they found I liked electronics—I'm not sure which was the second job, whether it was self-synchronizing stroboscopic Schlieren or whether it was this other project, where Jeff Buck had come up with a special lamp that they'd used in World War II. They'd flash it with a lot of capacitance going through, charged up to 3,000-4,000 volts over enemy territory to get a picture, while flying [at around 10,000 foot altitude]. It was very bright—Xenon, I think, is what was the gas used [in the lamp]. In any case, they had the [Curtiss] C-46 [Commando] aircraft flying at that time, that was flying for weather [research], and you might have noticed those pictures of the ice on the [propeller in the report I sent you]. Jeff Buck got the job for us, which he gave to me, to build the [power unit] that would flash the lamp [when an external circuit was closed]. John [V.] Foster, whom you've might have interviewed—did you?

JOHNSON: The name's not familiar.

S. SCHMIDT: John Foster, he later on got a pretty highly advanced job. He was one of the, I don't know whether it's assistant directors or something, but he was there at that time, and he was fresh out of the Navy, an officer in the Navy, so he was a little older than me. John was the one that was responsible for the camera, that would give me a closer of a switch and I would use to trigger a little circuit that would flash through a thyratron, and in turn, would go through an ignition coil, and would put a high voltage on this [Xenon] lamp. I think it was Xenon, but whatever it was. That would then flash and take the picture. John built the camera parts of it, and I built flashing parts of it.

JOHNSON: Was he a photographer?

S. SCHMIDT: No, he was an electrical engineer also. He was an electrical engineer, but he worked in the instrument development branch, you see, which I could have worked at. I belonged there from my desires, rather than the [electrical branch where I was]. I think that was the first project I had. That went fairly successful. They flew that C-46 all around the country. They looked for the worst weather, and that's where they'd fly.

JOHNSON: Did you ever fly on it?

S. SCHMIDT: Yes. I can tell you one flight that was quite an experience. There were too many of us to have seats, which they'd never allow today, so DeWalt, I think it was Frank DeWalt, I think, was the name, he and I both sat in back of the bulkhead, on the floor. The pilot on this flight was named Rudy [Rudolph] Van Dyke. He was a hotshot fighter pilot from World War II, and the copilot was a mechanic that was also a hotshot [fighter pilot]. Neither one of them, I think, had much experience flying on a big old—well, they weren't very big—C-46. We took off on a flight and we said, "Where shall we go?"

"Well, let's go up to Yosemite [National Park], so we could see something out the windows while we're doing our test." This was on a test, to see that everything worked. We went to Yosemite and went up and flew right around, Half Dome [granite dome]. I don't think we were supposed to be there, even in those days, but we flew around Half Dome and continued up the canyon. It had a certain ceiling, the plane, and these trees kept getting closer and closer to us. I didn't know anything about danger, so I wasn't worried. I trusted in the young pilots, that knew a lot about flying, but not this [C-46] plane. We were climbing, but gradually, we got to a big enough place where they could turn around and we came back. The trees were down there, not too far off, hundreds of feet rather than thousands.

We turned around to come back, and then, I think the guy that had done the weight and balance things had made a mistake or something because when we touched down to land, we started going down the runway and then headed right toward the control tower, and we're back, and here I am, sitting in back of the bulkhead, with I think it's Frank DeWalt, was his name. He was the guy that did the weights and balances anyhow, so we were sitting back there, and felt this going on. Didn't know all the excitement that was going on, but [the C-46] came to a stop. Everything was all right. I think that was done before I did this other project.

JOHNSON: I know they were doing the icing research at Lewis [Research Center, Cleveland, Ohio] at that time, too. Did you work with Lewis on any of that?

S. SCHMIDT: Not us because we were just the engineers building the stuff. It turned out there was one of the meteorologists, that I don't know whether he flew or not, but he was working there at NACA. His name was Bill [William] Lewis, and as a matter of fact, when I first went to work there, I didn't have a car, and Bill Lewis had a ride group from San Jose, which I could catch a ride with him out to Ames and back.

JOHNSON: Is that were you lived, in San Jose, when you first came?

S. SCHMIDT: I rented a room from an old maid who was a friend of an old maid relative of mine. That's how I happened to be living there. I just lived there long enough to get back to Hollister because I am musically inclined, and so, I like to get back to Hollister. There were two brothers, they owned a bar there in Hollister, and I'd go in and play the piano for them on Friday night, and that'd bring a pretty good crowd. I did it for nothing for them because everybody was buying me drinks, which I didn't need too much. This was a great time that we had. All of us that were there were veterans.

JOHNSON: Everybody was young, and I know there was a lot of social activities going on at that time, too, there at the Center, and different groups would get together and have parties.

S. SCHMIDT: I took part in some of those, too. I think you saw the pictures. I don't know what the name of that woman that was posed on top of the piano and I'm playing the piano.

M. SCHMIDT: Rita Emerson.

S. SCHMIDT: No, no, it wasn't Rita. No, no, no, this is not at Ames. If you looked at my book, you would see a picture of the gal. She's right on top of the piano. She's posing there. I don't know who she is—at least, I've forgotten her name—but I think she married one of the engineers from Ames, but I don't know that for sure. This affair, which they put on in the cafeteria, I think, it was a little affair where they're having for old-timers or whatever they call that, 49ers or something.

JOHNSON: The Gay Nineties, I think, this one says. Gay Nineties Revue.

S. SCHMIDT: Gay Nineties, that's it, yes. I played the piano and acted a little in the part. Several of the other people were there that actually—remember, Meredith? The woman and her husband that both worked there, but they were in the hospital, she was the same time as you? Whether she's the same time or not I don't remember.

M. SCHMIDT: Lorraine Vernon?

S. SCHMIDT: Yes, Lorraine Vernon and her husband are dancing in that picture. That was one of the affairs. They called on me to do some things like that, and I liked to do it because I was doing this. I don't know, given another year or in another time, might have been playing piano in the joint, I guess.

JOHNSON: You enjoyed it, that's good.

WRIGHT: Those would have been different memories to share, if you have been playing the piano in an establishment.

S. SCHMIDT: Yes. Yes, I was accused of that by Meredith and our friend Jack Stalder, who used to be a branch chief there at Ames and later on went off with Jack [N.] Nielsen in the ‘50s and they formed a little company which I’ve forgotten the name of. They wanted me to join them, but I didn’t. They figured I knew more than I knew I knew, to do that. I wasn’t quite prepared to go into consulting work like that at that time. I didn’t have my Ph.D. yet. I didn’t join them. There were four of them that left Ames and formed this. Do you remember the name of it?

M. SCHMIDT: I don’t know.

S. SCHMIDT: Four of them—Jack Stalder and a fellow in heat transfer that was a heating guy, and Jack Nielsen, who was a supersonic researcher, and he later formed his own company, and one other that was the branch chief of the 3-foot Wind Tunnels, I think. They all left in the ‘50s. I’m not sure. It was before Sputnik [Russian satellite], so ’56 or ’57.

JOHNSON: After that icing, is that when you went to the Supersonic Free Flight Wind Tunnel?

S. SCHMIDT: That came someplace along, I think the stroboscopic Schlieren system was next, but I’m not sure on the order of doing them. This was quite an interesting project that Jeff got for me to do. He couldn’t do anything of this kind of work, but somehow, he was able to promote things pretty good. He promoted that our branch, rather than the instrument development branch, which would have been the natural choice, I think, because they were electronics guys. This free flight wind tunnel was set up so that it was a blow-down wind tunnel and it was conceived, I think, by

Harvey Allen or some of the high-speed aerodynamicists. It would have a blow-down wind tunnel giving up to Mach 3, I think, there in the test section. Then, they'd fire a bullet up there with a model on it, and that would fire it, giving it some more, so they could look at Mach numbers up at pretty high values. I'm not sure how high, as a matter of fact.

That was the plan, and so, there were four test stations where they wanted pictures of the bullet. There were two things that had to be: they had to take a picture of the bullet, which really had a model on it, and then they also had to know the time, timing between the pictures. They wanted to get out the drag of the vehicle and they could also observe any attitude changes, but in four stations. It isn't very much of a view, but that's all they could get at this very high speed. This was the plan, and I was to build the device that would flash a Libessart arc lamp, which is a little spark gap, that would flash that through a point source of light, and that would create a shadowgraph, which is like a Schlieren, but it's really more detailed than the Schlieren.

I was chosen to build the system that would pick up the bullet coming by, and then create a signal after a delay, a variable delay, because that would depend on the speed of the bullet, which they would know approximate, so after it would flash the lamp, and at that time, the lamp would expose this point source of light directly on the film, and that would create this shadowgraph picture. They'd be able to, by carefully knowing the exact position of that film—they used glass plates, I'm sure—they were able to locate its position quite accurately relative to the other ones.

There was a mechanical engineer named Jack Blackburn, I think was his name, but he left Ames not too much after we finished this. I don't know which department he worked from, but anyhow, we were doing it. We had a set-up in the 3-Foot Wind Tunnel, or one room they had assigned to us, where we could go and we set up a gun and we'd fire it through a set-up kind of like a fan-beam light source, and then when [the bullet] intercepted this light source, [a pulse] would come from the multiplier phototube signal. Then, I built a delay circuit that you could adjust the delay, and then it would flash the [Libessart spark gap], and the way the lamp was flashed was—again we used something similar to what we used on the C-46 flash thing—would discharge or use a thyratron to make it discharge through an ignition coil. The ignition coil would build a real high voltage and that would trigger the gap to spark.

That was basically this Libessart lamp was named after a General [Paul] Libessart, who was in France, French, I think, that I don't know what they developed it for, but all I know is we got the drawings on how to build them from [U.S. Army] Aberdeen Proving Ground [Maryland], would that be possible? I'm not sure. Someplace back east. That's where we got the ability, then, to get a machine. We machined them right there at Ames. We did this. I had it all developed, and would go through and trigger and show the picture up. I think I showed some pictures in there [document]. One of them was going right through a big piece of paper, amazingly enough.

JOHNSON: That one right there, yes. Yes, that's amazing.

S. SCHMIDT: That wasn't intended; it just happened to come out that way.

JOHNSON: That's pretty amazing.

S. SCHMIDT: Yes, it was a lot of fun job, of course. Then, I don't imagine I was much over 20, 21, probably, at that time. We were doing that, so then I wanted to have the electronics built real short wiring—point to point, they called it—but I was given that it had to be done in the electrical branch technicians. They weren't going to do any point to point wiring out in the electrical branch.

They knew how to wire circuit boards, and that's the wiring I got. I was afraid of it, but we put it all together with the four stations, and then okay, now we're ready for the first test. We fired the bullet down and all of them went off, and everything just looks great. Then, what we found is we had a picture of one bullet, namely, the first one. That big, high-energy pulse that was created in this circuitry, it fired all the others automatically. All of them were black, and so, I was really in a quandary—what in the world to do about it? They didn't want to completely rewire this thing to try to reduce the [coupling], and fortunately, Ames had, at that point, hired a young man who was going to become very famous later for developing the mouse.

M. SCHMIDT: Doug [Douglas C.] Engelbart.

S. SCHMIDT: Yes, Doug Engelbart. When Doug suggested building the blanking circuit, I didn't know what that meant. Doug had been a technician in the Navy during his service, and the only work I had was work I'd done myself, like building a little radio. When I was in the Navy, I bought a mechanical, wind-up [turn table]. I wanted to play some music, so I bought a mechanical wind-up thing, and then built an electronic amplifier, sort of amplifying the sound, so that I could play records, as a portable record player. It wasn't too much. I had some records, though, that were pretty good, I'd bought.

One group was of Bunk Johnson. I've kind of always been a fan of Dixieland music, but Bunk Johnson was one of the great old trumpet players in the '20s. By this time, in the '40s, he was old, lost his teeth and lost his trumpet, and he's an old black fellow, so he was having a hard time, I guess. Some people at that time that were looking to record history, they found out about him, so they bought him a trumpet. He said he could play the music still, so they got a bunch of recordings of it. You could buy them, Bunk Johnson. I had bought a set of what I think they had four records and 78s that they had, and then what happened to mine? They were at the ranch and my mother sat on them.

JOHNSON: Oh, no! Goodness.

S. SCHMIDT: That was the end of my Bunk Johnson. Of course, now you can still get Bunk Johnson on the Internet. It didn't sound the same to me as it did when I was 20 and 19. Where were we?

JOHNSON: We were talking about the wind tunnel and Doug Engelbart.

S. SCHMIDT: Doug Engelbart had suggested this, and I didn't know what to do, but he said, "Well, you just put a blanking circuit in." He kind of implied that. I went in, and the tube has a screen grid and then the grid that the signals amplify on, and what I did is put a big negative signal on the screen grid of all of the things that if ever they went through this, that it'd blank them after this pulse. They were blank, but it didn't matter for the one you were using because it was going to flash at the right time. Its signal already had gone through. It would blank out all of the units, and by God, that worked. So, Old Doug came to the rescue for my system and the fact of having it built in the electrical match that I didn't want to do, but I didn't have a choice. It worked, and they used it for quite a few years, I guess. That was, I think, in the electrical branch, that was one of the key things that Jeff Buck had done.

Then, Jeff Buck, he promoted too good and he got moved up to Assistant Division Chief under Jim [James A.] White. Merrill Nourse was put in, and I like Merrill real well, but he was a guy that wasn't going to change the power thing. If you didn't do the telephones and you didn't like power, there wasn't much of a place for you in that branch. One of the things that inspired me to leave relatively quick from the branch—I was married about this time, and even at that, I thought we can make out all right. I don't think we had moved into [this house] yet, and so, I had it all planned out to leave, and then go back to school and I was going to work on a Ph.D. I didn't know whether I'd make it or not, but that was my plan.

I went to leave, and then Jeff Buck entered and had me transferred. I got interested in servomechanisms, and this is through G. Allan Smith, who I think I might have mentioned earlier, that Dr. G. Allan Smith, he was quite a guy. He was there at Ames and he had a great education. One thing about it that I didn't have near the education, I wish had, because he had a Ph.D., I think, from Yale [University, New Haven, Connecticut], I believe it was. He'd worked during the World War II on servomechanisms and pointing guns at aircraft. He was teaching a course at Stanford and I took the course from him at Stanford, I think it was. I'd learned about servos, so this was while I was still in the electrical branch, and that looked attractive to me, to work on servos. I guess it was the theory or something that was attractive. I'm not sure—I never considered myself much of a theoretician. It may have been that. Jeff Buck talked me into taking a job in instrument, so I finally got to [the Instrument Development Branch].

JOHNSON: Got to where you wanted, yes.

S. SCHMIDT: I think it was five years. I think it was in 1951, I transferred to instrument and worked under Al [Smith]. At that time, they were working on the F6F aircraft, which was a variable stability aircraft. Al and a fellow that worked in the flight research branch, they'd come up with this idea of making the ailerons and rudder, later, which I helped put in, feed back a little bit of the quantities like a sideslip or depending on what you fed back into it, make the plane fly differently. This was all done by servos that would take the vane and other signals and make the aileron or the rudder that later we put in [follow them].

I went over there just at the time they were installing the rudder system, and a fellow named Smoky Patton, who later went to Lockheed, he was doing the work on it under Al. I was assigned to help put that in, and then later, to monitor whenever anything went wrong. The fellow named Walter [C.] McNeill, now, I don't know whether you interviewed him or not, but he still goes to the OWL [Owl Feather Society, Ames alumni] meetings. Walt McNeill was one of the engineers in flight research that would set up the plan for testing and then make sure the plane was working okay. I was to do this, and if Walt had any problems, he'd call me. I learned pretty quick that if they had a problem, that what Al did is he'd go over there, study over all the diagrams and everything. It wasn't long before I said, "Jeez, I don't need to do that—I know what's wrong. If they have any problems, it's the vane, just send the vane over and have it cleaned."

I told Walt to do that, and so I didn't have to go over and muddle around on that plane. I was free to do other things. It was nice working on the servos, but I didn't really do much work. I helped install the rudder system so they could control the rudder by these signals like sideslip and roll rate and a bunch of other quantities, which made the plane fly differently. They knew why because it affected through the standard equations of motion when later, I guess, would show you that their CL [life coefficient] beta was rolling due to sideslip. CL alpha and their rolling due to angle of attack, and then there'd be some of the others, I've forgotten. It's been well over 50 years, so my mind is a little dull on that, remembering the aircraft equation things that they effectively were changing. The cockpit was set up and it had knobs you could set it at several different positions, and so they'd have the setting worked out, and then the pilot would set whatever they put in, and then he'd fly it. George [E.] Cooper was the test pilot, head test pilot. I still see George [occasionally]—George is pretty near 100, isn't he? He's in the business of selling wine.

JOHNSON: Really?

S. SCHMIDT: I think it's his son, or some of his relatives that are running the business. I got off-track again and I got to be put back on track.

JOHNSON: One of the things you did, I think, when you were still in the electrical side of it, was the power? I think you were scheduling power. I was reading that part; I thought that was interesting, how they scheduled that power to the wind tunnels, and how people had to sign up if they knew they were going to run. That's an interesting process because I know the electrical draw that Ames was using was quite a bit. I know a lot of times, they ran at night because of that. Maybe you can just talk about that for a minute.

S. SCHMIDT: This was one the jobs I kind of had as a junior engineer in the electrical branch. We had a book there that was the power scheduling book, and so we knew how much that had been scheduled and by who, and the maximum that it could be. The tunnels that needed to call were like the 40 x 80 [Wind Tunnel] because when they were going to go full power, they were going to use quite a bit. I think they'd call and schedule it ahead, knowing they were going to run at night, or such and such. They would call and they'd get marked down on the book, then they had that power. If they didn't use it, they were supposed to call so somebody else could use it. It was a relatively simple thing to do, was to just allocate the power up to a certain amount. I'm sure we were never using the [power] at the full amount very much. They finally got a system put in so everybody could see what was being drawn in the wind tunnels. If they got on, as long as we stay below this red line, it's all right. They could use the power better.

JOHNSON: What would happen if you went over the amount that was allocated?

S. SCHMIDT: You'd get charged more. Your bill would be a lot higher. Not yours, but the government's bill. I don't know what they have today out there. Of course, they aren't running many wind tunnels anymore, so when they were, I guess I probably saw much of the life of most of the wind tunnels. By '61, it was concerned with other things. That was done, and one of the first things, any of the engineers there, when you got a phone call, one of the first things you're taught is we want to schedule this, you go right to the book. It was in a certain place, so we'd go right there and we'd schedule, say, "Yes, that's okay," or, "No, you can't have it then—how about this?" A little negotiation, that's all that was involved. It was even a young engineer with no experience could keep from fouling it up, which I'm not sure this young engineer can.

JOHNSON: I know once you moved over into the instrumentation side, and I was looking through what you'd written down here, you worked on a motion simulator.

S. SCHMIDT: Yes, this was with Smoky Patton. I think Smoky and I built the first one that was built there at Ames. What kind of trainer is it?

JOHNSON: Pitch axis?

S. SCHMIDT: Yes, but there's a well known company that was making trainers. We took the cab from this trainer and we put a hydraulic [ram on it to drive the pitch axis].

JOHNSON: A Link Trainer?

S. SCHMIDT: Link, that's the name I couldn't remember. We took the cab from a Link Trainer, and then we mounted this [hydraulic ram which] came from MIT [Massachusetts Institute of Technology, Cambridge], again, due to G. Allan Smith. Al knew their hydraulic testing back there, and so we got this 3,000-pound hydraulic unit that would [drive] the [cab] up and down. Then, we made a [simulated a] signal to drive the [cab] up and down, according, I guess, probably to pitch attitude. I'm not sure what it was exactly. I had a simulation set up on an old Boeing analog computer. This was before I got in charge of all the [analog] computers, but we had a setup in the lab, and so, we had this [servo driven cab] working, and there's a picture, I think, in [the report I sent you], with me sitting in the cab. It probably looks like a kid, but I looked like a kid, mostly.

JOHNSON: You were pretty young then, yes.

S. SCHMIDT: Smoky was there beside it, and so, we were running this testing. I think I mentioned one of the things that happened during this is I got the wire plugged in wrong on the analog computer one time, and we started it up, and it was unstable. [The cab] started banging back and forth, and walking across the room. It had a whole lot of weights in the bottom of it. It scared the devil out of the people downstairs before we could get it shut down. It was me that [caused] the problem. I hooked up one of the plugs wrong [on the analog computer], and I probably knew this, but I did it unconsciously because you're plugging wires in and using it to run a powerful rig. Somehow or other, I'd done the wrong thing. Got it fixed right away, but we learned not to do that again. It just scared the technician downstairs. The instrument section was down right below us, so this boom-boom-boom, they thought it was an earthquake.

JOHNSON: They thought it was coming in? Gosh.

S. SCHMIDT: That happened, yes. I worked on that. That was one of the first, I think it was the first simulator that we had there at Ames that you could hook up to an analog computer.

JOHNSON: Who would use that simulator?

S. SCHMIDT: The flight research branch would conduct experiments there with putting the pilot in it. I'm not sure that [this motion simulator] was used very much because it was pretty limited. Later on, they built a pitch-roll simulator. I had a little to do with that because the electrical branch built it, but once they got it built, the damn thing was unstable. They said they wanted to put [a mathematical model of] it on an analog computer. I looked at it and I saw that [the problem was one] that I had done some research on while in the [Instrument Development] Branch. It was caused by the [error signal] getting too big [compared to] the pitch rate [damping signal. The pitch rate damping couldn't work] because there [were saturation] limits [on the sum of the two signals]. I had devised a way to avoid [this problem] some years before. Bill [William C.] Triplett and I had written a report on it. I put in a limiter [on the error signal], which is all that was required, and that solved the problem so that it didn't do this anymore. I think one of the best motion simulators that they made, certainly not by far, not the worst or the most expensive, by all means, because they have some really expensive ones out there. But they made a simulator that would have 6 degrees of freedom, and limited, of course, because it only goes so far back and forth and up and down.

This could simulate almost perfectly the problem of when you were flying behind an aircraft with a tanker to fuel up. I tried to talk them into somehow getting that involved in their evaluation because that didn't require anything, "Well, we'll fool the pilot," but you weren't fooling him with this [simulated] motion. That's foolishness, to believe that, but here, you really weren't because you were giving him the motion and the feel from motion would be the same as was in the actual problem. It's where they have to track real close to get that [fuel hose] hooked up. That was one of their best [motion] simulators, in my opinion.

I had a lot of [motion simulators] under my control at one time that I didn't agree with, like they decide to build a simulator, they said, "Well give that to Stan"—well, Stan didn't want it. One of them that was that way was the one that Jeff Buck had to do with, and that was they built a big centrifuge that ran around in a track inside the hangar. It was noisy as all get-out, and they claimed that it did something for some person that needed to have the Gs [force of gravity] on it for some kind of an operation, but I have an idea that this might have been a little bit of propaganda.

They tried using it, but it would go up and down and go around with I don't know how many Gs it would work, but it would do a fair amount. It was just too noisy on that track.

I got out of the branch; I had the Dynamics Analysis Branch. I was too successful on a high-speed computer, which we haven't talked any about, and helping out flight research. Because I'd come to this high-speed computer, which, again, G. Allan Smith had promoted and he bought it from some little small company at MIT called GPS, General Precision Systems, not GPS as we know it today, but General Precision Systems. It had a flock of integrators and summers in it that you could plug in, and plug in linear dynamics, and then feed a step or pulse to it and see what the subsequent response was on the screen.

When I would run this, we'd get through the study [in a short amount of time] and Harry [J.] Goett, who was our Division Chief then, was quite pleased because he thought there's some miracle that I was doing. Maybe there was, but I was just running the system and setting it up for the flight engineers. I guess I did too good a job because the next thing happened to me, I got changed from being an electronics engineer to being a machine-programming mathematician. I was assigned to a section in charge of the Reeves [Instrument Corporation] analog computer in Bill [William A.] Mersman's branch, which was a computing branch, I think, at the time. I don't know how much I've said about that, but I was assigned that [position] in, I think, it might have been '53.

The next thing in that regard that happened was I think they threw a turbine out of one of the high-speed wind tunnels, and so they couldn't use the power, so they had a lot of money extra. Harry Goett came to me and told me to put in a purchase request for about $50,000 worth of analog computing, or $100 [thousand]. I went up to $150,000 as this was a new computer over the old REAC, which had been there since '48. Lo and behold, I got the most expensive [analog computer] that I did propose. I didn't really quite believe in spending lots of money, but in this case, I did. We got Electronic Associates [Incorporated] analog computers for it, and then I had to find out where I got to put them.

I decided I wanted to put them in the small hangar. I had a few fights with the division chief in charge of the airplanes at that time. Not fisticuff, but they didn't want to give up the space. I think it was Harry, again, had the strength to convince them that was the right thing to do, so we put them in there. The intent I had was able to hook up the analog to an airplane, which really never was done, but we hooked them up to [motion] simulators. I had visualized I could do this, but we never did it. It wouldn't have made much sense unless you had a special airplane that was made to hook up to an analog computer, and we didn't have [such a plane]. That's where I got the new analog facilities [located in the small hangar]. Then, in 1955, I went from being a section leader to a branch chief, all in one fell swoop. That was a tough thing. I didn't ever want to be a manager.

JOHNSON: You were pretty young at that time, too, to be a manager, weren't you?

S. SCHMIDT: Yes, I was under 30. I was 29. Not the youngest branch chief by far because I think Al [Alfred J.] Eggers was a branch chief pretty young, but he was in the business of aerodynamics, and I wasn't that at all. It was something different. I was given a branch and I insisted that I wanted to have some ability to do some research and control work, besides, as a result of going to Stanford. I got a master's by that time. My interest in dynamics of the aircraft, not so much that as the servo controls of the plane. They always say that all Schmidt believes in is automatic control, and that isn't quite true. They'd sit a pilot in front of a screen and he's supposed to work the screen and keep the dots sitting in the center. I said, "God, I can do that automatically a lot easier than the poor pilot there doing that."

They were trying to get at things like the pilot's delay in reaction and this sort of stuff, I guess. I don't know whether that was primary or secondary, but they called it gunnery. The pilot would hold this knob, and then if something would be fired, like his guns at an aircraft like that. They really worked on it in World War II, I guess. A lot of it was like that, that once they had radar. In any case, I envisioned that we would be able to do something with airplanes in the hangar.

You'll see some pictures in there of our new lab—one of them with that gal, Vickie [Harper], in it. Meredith worked for Bill [Charles W.] Harper, and Bill and Vickie and Meredith and I, we went out. There was a French restaurant, a very, very good restaurant. We liked to go to that. This is quite a few years before we had any children. Now that we're old, we'd still like to go there, if it was around, but it's not around anymore.

JOHNSON: That's an interesting time because computers were changing so much. I know you were working with the analog computers, but how did you keep up with that technology and what was going on with the computers, and to keep moving on with the simulators and hooking those computers up and that sort of thing?

S. SCHMIDT: I would go to places like White Sands [Test Facility, New Mexico]. They believed that you could do everything with a bunch of computers. They're never successful with—analogs was not the way to go. If digital had been the way it is today, they might have been able to do what they—they were Germans down there. They were far ahead of what they could do with the existing equipment because they were trying to use analogs. I went to White Sands, they had meetings on that, and I traveled to other places that were using analogs, even to the place which had them tied into this centrifuge, and that was in [the Johnsville Naval Air Development Center, Pennsylvania]. I think that's where it is. There was some place where they had a centrifuge that they put the astronauts in. I've been there and seen that facility. I knew generally the people in the analog business in the other, like Langley Field [Langley Research Center, Hampton, Virginia] as well as not Cleveland [Lewis Research Center, now Glenn Research Center], so much, they weren't really involved in that, but Langley Field and flight research. I was around quite a bit in terms of not only to the analog, now. The digital, I was dumb enough not to believe the people that were advocating all this stuff because I could see what they could do and I said, "Hell, that'll never do anything." Can't do it fast enough. Of course, I was quite wrong. Look at those little things you've got.

JOHNSON: I know. We carry them in our pocket, now.

S. SCHMIDT: That's right—when we were simulating the Apollo mission, which we haven't got to yet, when we were simulating it on the computer, all they had at Ames was a [IBM] 704. We would schedule a run and use up the full digital computer facility of Ames to make one run around the Moon and back. Now, I could do it on this little iPad far faster and far better. That's the way things have grown.

JOHNSON: It's pretty amazing, the changes.

S. SCHMIDT: The digital is the one that they were advocating. The people that knew—and Engelbart was one of them, as he developed the mouse and quite a bit of other stuff—he's a very well known man. He died just last year, I think, Doug was just about a year older than I am. My son met him. I would have liked to have gone, talked to Doug, and his comment to Greg was, "Is your mother still married to that guy?"

JOHNSON: You joined the Dynamics Analysis Branch, according to this, in 1955?

S. SCHMIDT: Yes. The Dynamics Analysis Branch then took over running all the simulations for the flight research branch. We had the responsibility of programming the analog computers. I had found out in the work that I'd done separately on it, that you learn so much by doing the programming, that I set up a course and taught the flight research engineers how to do things themselves. I knew they'd learn from that, and I promoted this quite a bit. Here, we go through the work of setting up the problem and solving the problem, really, but not due to what's up here, and not my guys that are working for me, so much. I would help them solve it if it needed to have some automatic control in it, or what have you. Then, of course, they'd get all the credit for this and Goett knew who was helping them all write it up, but he thought I must have really liked that, I guess. I was not crazy about being a service manager. I wanted to do some research myself, particularly after I got the Ph.D.

It went on. After the branch, I had a small group that was supposedly working in the area of controls and they were working on a modification to Wiener filter theory. I've had three people working on it: Gerald [L.] Smith and Frank Druding and Elwood [C.] Stewart. They'd come to the branch when it was formed. I think they were not too happy with me, the three weren't. I think the guy that caused the problem was Frank Druding, but anyhow, they weren't very happy with me, said I didn't understand research. Frank Druding went to Lockheed, and Gerald Smith, I think, later I know he went into Howard [F.] Matthews' branch, who was my assistant. This was another thing that got me disgusted with things, that led up to me leaving, was they transferred my assistant and gave him the branch that I wanted, which was the Guidance and Control Branch. I couldn't get rid of the Dynamics Analysis Branch. I had to stay with this simulator at all cost. The cost to them may have been high—maybe they thought it was small—anyhow, I quit. I went to work at Lockheed after that.

NACA, of course, quit in 1958. At that time, of course, the Sputnik, and I had the branch. I think by that time, the guys that left, Druding and Elwood Stewart and Gerry Smith, they had left. Gerry had gone on to MIT, I think, to go to school. He later came back, and Elwood, I don't know where he went. Frank left and went to work at Lockheed. He did fairly well there, too, because he later had his company that he sold out to Litton, I think, bought them out. He promoted a bunch of people from Lockheed to leave. That's the past, that's history.

I wanted to really work in this area. Sputnik coming along kind of helped me because they were looking for what to do, what to do? It turned out that Harry Goett got one of the jobs to lead [a committee] under [President Dwight D.] Eisenhower, what they should go do next. He had pretty much selected the circumlunar mission. They wanted something that wasn't landing at first—it just took circumlunar. I do know the guy that promoted the landing because he could see how they could do it with a LM [Lunar Module]. They were discussing things that I thought were too complicated to do, but they weren't. They did kill a few people in doing it, though, but they weren't too complicated. I wonder whether they could do it again or not. We had enough problems with the [Space] Shuttle. It was done, and so, when Sputnik arrived, well then, Goett was assigned the thing of looking for what to look at. I forgot the name of that committee that he was on, but he was the head of it. I wrote a report that spells that out, in about 1980. This was on the Kalman filter, which we developed there at Ames.

Sputnik made a big change because now, what are they going to do next? Goett was working on this, and I was a branch chief. George [A.] Rathert, who I don't know whether you got George's before he passed away, but George and I, Harry wanted us to come back. He was, at that time, the Director of Goddard Spaceflight Center [Greenbelt, Maryland]. They had the Space Task Group under him, so he directed us to come back, or asked for us to come back, and go down to the Space Task Group. Harry wasn't satisfied that they were thinking ahead enough on what to do next. This was after he had, I think, done this study on the circumlunar, it must have been. When I came back from that trip and I met some person which I remember the name that they'd quoted for me, but that I talked to at the Space Task Group. They were involved in the Mercury Program, so they really weren't thinking about what to do next. This was Harry's way of getting something started on his Apollo Program, as it later became known.

George Rathert and I went back there. George was the guy, at that time, that was running the flight testing in the Flight Research Branch. He might have been a section head; I don't know. I was in charge of the analog computer for Dynamics Analysis Branch. We went back to Harry's, and then Harry took us down to the Space Task Group, and we talked to the guy quite a bit. I came back, spent a lot of time trying to promote various things. I promoted Bill [Merrill H.] Mead, I think, where I thought he'd be a good guy in charge of [Russell G.] Robinson's office, at that kind of level. I think that I helped to promote him for that job, but I don't know for sure, that is not a guaranteed fact.

This was done, and I had a meeting in the branch. I don't think we did this till '59, so that was after meeting back there. I said, "Well, it looks like we ought to be concentrating on one or two problems." I didn't want to concentrate on more because one or two, I said, we should either look at the navigation guidance for the circumlunar mission, or we should go in and work on the problem of fuel sloshing problem of big boosters.

It was unanimously agreed that we'd do something that we knew the least about. That was the start of it. I got busy traveling quite a bit down to JPL [Jet Propulsion Laboratory, Pasadena, California], for example, and found out how—and Leonard [A.] McGee played a big role in getting a simulation done for us. You had to decide on a three-body, that namely, the Earth, Moon, and Sun, for getting our trajectory, and finally were able to generate a trajectory. By 1960, we knew quite a bit about the guidance of the thing. We'd been able to generate sensitivities and this sort of stuff, so we were able to do some stuff, understanding that. That came with a lot of help from Minneapolis-Honeywell.

I traveled back there, and [at that time] there was a Japanese Ph.D. there. He and I got along quite well together. From them, I got enough knowledge for us to do some stuff, but we weren't making much progress, particularly on the navigation, which is determining where you are. We knew, since it was supposed to be onboard, that it had to be done with optics, so we knew that much. I had this gal, Vickie Harper, doing some studies under me, but they're more manual kind of things, where we could do a trajectory, then.

We knew that much, but how do you translate this into a knowledge of the aircraft position and velocity? In the fall of 1960, a person who I had met at several conferences, Rudy [Rudolf E.] Kalman, came to visit us. I suspect he was looking to promote some funds, that's what I suspect, but in any case, he came to visit. He knew me from our meetings at the IEEE [Institute of Electrical and Electronics Engineers] conferences. He came down to visit us and he gave a presentation of his work that he had published in the spring of 1960 and it was hard reading it to see what it dealt with, but it was really where the Kalman filter—which we probably named more than he did—eventually evolved.

What he had said it was, was the theory to remove the constraints, the time constraints, on the Weiner filter problem because Weiner filter was made for stationary systems, and he wanted to have a [filter for a]time varying system. This was planned as a way to remove that time invariant [constraint of the Wiener filter]. He had no idea where it was going, but in any case, he gave a presentation at my group, which composed then of Gerry Smith and Joe Carlson was our mathematician that was working for me, and Leonard McGee and John [D.] McLean, Vickie Harper, and I, I guess, were the ones that composed the group that was working on this [navigation and guidance problem].

They all sat there, listening to Rudy, and I did, too. When he got through, I don't think any of them understood, and I didn't understand it much, but after his discussion of it, I said, "I think this might have something to do with the [navigation problem]." We started. I assigned Gerry and Joe Carlson to study this report over and figure it out, and then report to me what it says. They studied it over all right, but they didn't figure it out. They made little pieces here and there, so on one of my trips back east, I arranged to go up to talk to Rudy, who was working in Baltimore [Maryland] at that time. I was there to go down to the Space Task Group.

I arranged so I'd have some time with him in the process, and we were working along on it, but some of the theory that he had, like e^{Ft}, where F is a matrix and e is [Euler's number], and I'd never seen that, but that was his definition of the transition matrix. Then, he defined it as a series expression of that, which is $I + F\Delta + F\Delta^2/2$, and so forth. This, I just couldn't understand—

how do you put a matrix into an exponential? That did not make any sense. We weren't making too much progress, but I went back there, and after talking with Rudy and reading [his paper] more, I began to see what our problem was.

Really, the thing that we would had been generating, which was the transition matrix for our guidance study, but I didn't know that. We were integrating the partial differential equations of the nonlinear set with respect to time and integrating those [along the nominal trajectory]. They gave us the sensitivity, depending on where you took them, but you could find a sensitivity [of position] at the Moon to some change [in velocity] earlier. This was really [part of] the transition matrix, so once I found that out, we knew immediately what to—well, not immediately because there was a problem of a breaking his equations apart because they're all collapsed together. The time [and measurement] update of the [navigation equations] were all thrown together in this one equation. I had to separate those out into two equations, what you did at a measurement and what you did between measurements.

We there at Ames developed the so-called extended Kalman filter. All that means it that you linearized about the current best estimate of state in which was our integration for following the trajectory. That was kind of simple. All of the things, thinking back on it, were kind of simple, but taken together, it was quite a step. We had then met with Rudy in December or something like that, 1960. By the following summer, I had Gerry give a presentation on navigation, and John McLean, a talk on guidance, at Stanford. One of the mathematicians that I had taken a course, Statistics, there, came up to me afterwards, "You made a tremendous discovery." [I told him that] I didn't do anything. I just showed how to use [this abstract theory].

That was followed with a paper we gave at the AAS [American Astronomical Society] conference, which was in San Francisco. [Engineers came] from all over the country [to hear our presentation]. This was because by this time, the knowledge had been passed that one of the guys said, one of the fellows told me later, that some said "This damn fool thinks he can solve this problem without inverting a matrix." They sent [a mathematician who] was later on—one of my bosses out to find out what the hell it was. After the talks there, we had a room in a motel that some of the people were renting, and they had paper pinned all the way around the side of the wall, and I was writing things down. It was quite an affair. That was kind of the Kalman filter started, and then, that naturally led to my eventually leaving Ames, too, by the way.

My boss was then, Bill Harper, who, I liked Bill fine, but he was an old wind tunnel man, and he told me, "Well, now that you've got all the work done on this, you can start working on supersonic inlets." That attracted me about as much as I don't know, the least thing you can think of. There was so much work left to be done, then, and so, I didn't go for that too much. Somehow, it was always bred into me, you did what the boss asked you to do. If I just argued with him a little bit, he might have changed his mind, but I don't know—at that time, I was in the state of mind that I really had to go someplace.

Bill Mead, that I'd helped to get this job under Robinson, at least I thought I had, he at one time told me, effective of, "Well, you can't possibly understand both the reentry guidance system and the circumlunar," and I'd got a Ph.D. in these subjects. But, I was told I couldn't possibly understand; it was too complicated. For him, maybe, I guess, but guidance systems are guidance systems, so it didn't seem that terribly difficult to me, the principles [are the same for any application]. It wasn't to him, perhaps if he'd have been an expert in the field. But when I get those sort of things from people, the idea that, well, this is just a small thing that you're supposed to work in, the contained sphere. Well, if you don't expand yourself, you're not going to go. There's so much work that needed to be done.

Later on, some of the contributions I made, is I think I was the first to have the square root version of a Kalman filter available. Why is that important? It turned out that people are taking measurements like a very precise range rate from the Moon and [processing] it into the Kalman filter equations. This, in theory [should not cause a problem] and of course with infinite precision in your [computer]—the theory is correct. It's a stable equation. What was happening was that the positive semi-definite covariance matrix would [become singular processing the] high-precision measurements with the computers they had. [The computers had pretty large] words but the truncation made a difference.

This [anomaly] was discovered after I left Ames, by other people, and I heard about it [when I was] at Philco-Ford. Of course, I knew the [covariance matrix] is supposed to be positive-definite, and so I said, "Well, hell, that's a simple thing, we'll tweak it, we'll add a little noise that makes it stable when we do the time update problem." That was quite successful. We never had the problem. There were different means that I developed some of for helping that problem, but the square root is something where it absolutely can't happen because it's a square root and you square it, so there's no way it can be anything but positive-definite or semi-definite. The square root [method] was originally developed by [James E.] Potter, at MIT, but his [method] wouldn't allow you to use any random forcing functions. I heard quite well known men, such as [Arthur E.] Bryson, who was at that time, at Stanford, and [Richard H.] Battin, who was at MIT, make a statement that that'd be an impossible to formulate a square root method that would include random forcing functions.

It bothered me—I hate to hear the word "impossible." It turned out I was out of work for a while, or wasn't getting paid for much work, so I had free time to think about things. Fortunately, I was making enough so we didn't have to worry about going hungry, but I had joined AMA

[Analytical Mechanics Associates], whom I stayed with the longest of any of the places I worked. I worked at Lockheed for a year, and then Philco-Ford for four years, then I went to AMA, and I stayed there until I retired since I couldn't get any more contracts with Northrop. At that time, I was working as a consultant for Northrop, and they couldn't give a contract to AMA [Analytical Mechanics Associates]. I was old enough to retire—I retired from AMA and I still worked for Northrop, but I worked under a body shop arrangement. I worked until I was just about two weeks shy of being 75.

JOHNSON: Goodness. That's a long career. Looking back over your career at the NACA, I guess what you're most known for is that Kalman filter, but would you consider that the thing that you're most proud of for your time there?

S. SCHMIDT: The thing I did my thesis on was intriguing too because this was on the subject of using nonlinear [functions to improve stability in control systems]. If you take a control system and if it's a certain type, you can get a system that you put a step into it, if it's little, it's fine. You [increase the step size and the response] starts to oscillate a little. You [increase the step size further and the system] can go totally unstable. This happens because of limiting in the system, as everything has to be limited by something. You can have control systems limiting, so that your elevator is limited, period. If you apply a step to that system, it could go unstable, unless you make it so that the thing that's causing that, like a step input feedthrough, which is the error. If the error rate or the output rate is unlimited and the error is limited, then what it becomes in that case it would, instead of going up and oscillating, it would go up at a fixed rate till it got near the [size of the input], then it would [gradually stop]. By keeping the rate-limited signal so it can tell when to shut off the [error signal and] make it reverse sign—because to go somewhere in the minimum time, you want to accelerate half the time, decelerate the other half. If you accelerate too long a time, you're going to overshoot, you can't help it. That's basically the idea.

This work on this [non-linear system design], which I had a few things on, Gene Franklin, who's my professor at Stanford, said we should have promoted that more. That probably still hasn't been received—there's damn little work done in that that is applicable to systems, but this is, although it may be such a simple thing that other people get it right away. This, I taught at Santa Clara, after I left Ames and was working at Philco, actually, when they asked me to teach part-time. They were having a morning course, and so I taught there for, I guess, two years.

The first time I taught this business dealing with the nonlinear systems, and two of my students, they won a prize for work that they did for the IEEE. One of them became, later on, working at Stanford, he got to doing things I wanted to do, to apply some of this theory in medical stuff. He actually got some work and wrote some reports. I read some stuff in the local paper that he got connected with. I was proud of that, that they'd been able to do this. I didn't play any part in them doing it, other than teaching them. They came, I think, by one time, but that's just one of the two that was doing that. I don't know what the other did. That was one of my students at Santa Clara. Then, I taught another group the following year, on Kalman filtering. I don't know if some of the people from Ames took the course, that's a long time ago, whether it had any significance or not, I don't know.

I had an opportunity to go into teaching, but I really liked to do work. One of the jobs that I've really enjoyed the most was when I finally got to work on this [Northrop-Grumman] B-2 [Spirit] navigation system. It was a very sophisticated navigation system. It has a stellar system and a regular system. One of them is on one side [of the aircraft], and one on the other side. These are linked together by a common measurement. The relative position is quite well known and [a constant. One can form the difference between the two inertial systems] indicated [positions and the known constant. This is used] as a measurement [in the Kalman filter]. Then, you can keep these [systems] synched together and hold the best performance. They're each receiving different [signals], so if you get a roll, then one [side's accelerometers] will get different [measurements] than the other one [side. The accelerometer outputs caused by roll rate will be opposite in sign], as a matter of fact. The stellar system could hold the position accurately for a long time. The people at Northrop, this was before I took any part of it, [made error analysis] studies that showed if one could combine a stellar system with a free inertial system, [one would get the performance required by specifications. The] space stabilized system accelerometers [would get] calibrated [during the long straight and level flight to] the [target area, then the space] stabilized system, since it had calibrated accelerometers, [would be the most accurate during the] maneuvering [required to go to the target]. They would have a better system from the standpoint that they could start maneuvering and not lose any accuracy. That's the reason they proposed it. What happened? After I got assigned to it, I wondered, what in the world is going on? They were saying it was magic, kind of, you do this, combine these two, and I think they were using something that never would work. [This was using] relative velocity [as the filter measurement], and that never would work as you don't know that accurately. That's what they were doing in their simulation of this combined system.

After I got there to Northrop, I introduced this relative position measurement, and that makes it so at least you can do it. What happened was that the bidders on the space stabilized system didn't bid, so we had bids, but from a standard platform system. What do we do? I thought about it, I said, "Well, what we'll do then," after I understood what they were looking for is, "Why don't we just slowly rotate back and forth the non-stellar system?" Stellar system, we had to keep it as being kind of pretty much at level that were stabilized by this tracker. The other one, we could [slowly rotate back and forth and the] gravity would give us the change on the accelerometers to help to calibrate the accelerometers. That was studied on their programs they had for error analysis, and that indeed did work. That's what I was planning on, in the system. Later on, the person in charge decided that it was quite complicated to make that work. I'm pretty sure I could have, but it was running out of time and money, too. He somehow came up with a [theory] and said, "We really don't need to calibrate those accelerometers—they're good enough. That isn't the problem." He convinced the people up above, so they accepted it the other way. The other thing that may have been happening is the GPS [Global Positioning System] kept getting better all the time. They wouldn't need it in the first place. As it turned out, we did not rock the platform, and they built the specs [specifications], they said. I don't know that necessarily because we took the test data on a long flight that went around, over the desert and other places, and with the B-2.

We had our best system in it, and we got through the test. It was no good at all. But I talked them into, "You would record the accelerometer data, you will record all of the gyro data off the platform that you need, and we will be able to simulate exactly what happened in flight." We record all the measurements, and we could do the same thing on the ground with a computer system as we're doing in flight." The fact that we could do this, we're able then to find out that something was wrong. When we simulated what we thought we had in flight, that wasn't what we had because the simulator didn't agree with that. The only possibility that was left was the flight software didn't agree with the simulation. I worked on that, started looking through.

I'd never programmed in JOVIAL [computer programming language], which is what it's in, but you could learn to read [JOVIAL]. I programmed assembly language, as well as FORTRAN, so this is another language. I started looking through the flight software, and lo and behold, I found a problem was that the JOVIAL allowed the definition of local variables and remote variables as constants.

If you did assembly language programming, you knew there's a difference between being on the stack and being in memory, permanent location in memory. This was a case that the guy that programmed it had forgotten, in the rush of building stuff, so he defined some local variables. Our compiler was such that he wanted to compile it on as constants or as variables in more common place, where nothing could override it, it was a lot of extra compile time, so he programmed with local variables, as they weren't called a stack, or he didn't know, no, you don't do that. In any case, it called a subroutine, in the meantime, and the subroutine had modified some of those local variables before it got back to the other program, so it didn't work right. They wanted to fire the guy, I says, "Hell, no. You don't fire a person for your mistakes, or not label things right, for God's sakes." They didn't. We got the problem understood, and so, what did the Air Force want us to do? Not to go fly it again. That cost too much time and energy. They let us take the results from the simulator. Well, you see those little fudge factors going on here and there. In reality, they were going to use a GPS anyhow, so it didn't make a hell of a lot of difference by that time. The system today runs on GPS or it'll run the other way, but it's obvious they'll use GPS. The GPS is pretty reliable. I don't know whether the Russians could shoot it down, but I don't think the Russians are our enemies so much as the Iranians and Islam.

JOHNSON: Is there anything that we haven't talked about during your time at NACA that you wanted to mention before we go?

S. SCHMIDT: If I didn't mention in that book, I wrote that to try to be a history for my kids. I intended to write the rest of my history with Lockheed, then Philco-Ford, then finally with AMA and then later on, as what did I call it, the job shop. That's basically what it was, although I specified my salary. I think the last work I did, I did on a trip to Tinker Air Force Base, in Oklahoma. That's where they have the simulation of the B-2 and where they are doing the work today. I made that down there to give this paper that I wrote up on—it isn't classified, but it's really on the analytics of the Kalman filter tied into inertial system and stellar system. It gives all the equations for that. Northrop funded me to put some work that I'd done on my own and done just under DOS, and I made a nice report using fancy equations, which you can do with MathType [graphical editor software]. I don't know if you're familiar with it, but it's a beautiful program for writing equations. I had done all this work, really, pretty much paid for part of it, at least. I just wanted to do a decent job on it, so I had the report and I also had a summary of it that I presented there at Tinker in 2001. Before, the other event in 2001, that changed everything [September 11, 2001, attack on World Trade Center, New York, and the Pentagon, Virginia].

JOHNSON: Before the big event of 2001, right.

S. SCHMIDT: I haven't flown since, by the way.

JOHNSON: We appreciate you talking to us today. It was really interesting, and I think we could probably sit here and talk for another two hours. You have a great memory. We thank you for doing this for us.

S. SCHMIDT: I'm glad that you could come now because it might be going. It's hard to always say.

[End of interview]