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INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is July 18, 2014. This oral history session is being conducted with Doug Pearson at NASA's Ames Research Center in Moffett Field, California, as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project, sponsored by the NASA Headquarters History Office. The interviewer is Sandra Johnson, assisted by Rebecca Wright and Glenn Bugos. I want to thank you again for joining us today and coming by to talk to us. We appreciate you taking the time. I want to start today by asking you about your educational background, and how you first got interested in working for the NACA.

PEARSON: I went to the University of Florida [Gainesville], and my first year there, I was thinking of a major in architecture. They offered an introductory course, of course. By the time that year was over, it was pretty clear to me that I was not going to be an architect, but I was also interested in aeronautical engineering. Who knows why? I will say that in his younger days, my father had been a pilot. That may have something to do with it. Also, my uncle was a pilot during World War II, and in fact, he was killed on a mission to somewhere in Europe. The plane ran into icing difficulties, and they turned around and tried to make it back to the airfield and crashed on the way back. He's buried in England. I had this kind of family history of flying, and so, I just decided that that's what I would do, and I did.

My aeronautical engineering instructors, Professor [Robert] Thompson and Professor [John] Hoover, and there was a third one later on, [William Miller], they all knew about NACA and the research that NACA had been doing in aeronautics. In the summer of 1955, when I decided that I needed a summer job to make some money, I applied at Langley [Research Center, Hampton, Virginia]. At that time, it was the [NACA] Langley Memorial Aeronautical Laboratory. I had a very interesting time there.

I was a computer. That was back in the days when computers were people. I sat at a desk with my Friden calculator and I was given sheets of paper with data on them, and my job was to finish the spreadsheet, basically. They didn't call it spreadsheets at the time, but that's exactly what it was. In addition to that, the work that I was doing was harmonic analysis of sound fields from the supersonic propeller that they were researching. The way they did that was they took an old [McDonnell] F-88 [Voodoo] fighter and mounted, I think, another engine, even, but certainly, a propeller on the nose. The propeller was designed such that the tip speeds were supersonic, even when it was standing still. It was designed to turn really, really fast.

One of the most exciting things that I did, in fact, was take some sound measurements, and basically all I did was stand there while it happened because all the stuff was set up and everything. That was all being done. When they started up that engine, and they revved it up, I want you to know I felt it, straight through my chest. Even though I was wearing a big, humongous headset, the noise was deafening. It was spectacular—and scary.

JOHNSON: I bet.

PEARSON: To this day, when people talk about sonic booms as being a real problem, I know firsthand, I mean firsthand, exactly what they're talking about.

JOHNSON: You felt it.

PEARSON: Boy, did I ever. That was very interesting. It was only a couple of months, of course, the summer, but I enjoyed it. The following summer, I took a job with Convair Fort Worth [Texas], doing some drafting and structural design. The specific work that I did was they had the first flight of the [Convair] B-58 [Hustler] bomber happened while I was there, that summer. What they were doing is they were planning to ramp up production, and they wanted to make some improvements to the various features.

There was a beam between the pilot and the navigator, kind of a cross beam that was a support of some kind or other. It had been made by riveting together various pieces. It was basically an I-beam, so they had the two cross pieces, then they had the center, that was all riveted together. What they wanted to do was to build it with a new technology that they were experimenting with that was called chemical milling, where they would use acids of some kind, along with the proper shielding, to etch out holes where you wanted holes to be and make it thinner where you wanted it to be thinner, and make it thicker where you wanted it to be thicker. This was all being done to a single, forged piece.

My job was to say how big the holes were, where they were going to be, and so forth. Basically, what I was trying to do was copy what had already been done, but make it something that could be handled by the chemical milling process. That was 1956, and that should have been my first year after graduation. I should have graduated in June of ’56, but in fact, the program was in transition from a four-year program to a five-year program, and I got started late, and I did some other things that stretched things out, so I didn’t really finish until January of 1957.

Then, I signed on as an aviation cadet. I wanted to be a pilot. That was part of my reason for being into aeronautical engineering. I did that. I went into the pilot training. I made my way through to the point where I had done some actual solo and had some certain amount of hours, in both the [Beechcraft] T-34 [Mentor] and the [North American] T-28 [Trojan], and could have continued. At that time, they announced a cutback in the military budget. This was under [President Dwight D.] Eisenhower.

What they did was they offered all of us some choices: stay in the program, but you have to agree to stay for five years instead of four, or you can switch to a regular enlisted basis. As aviation cadets, we were actually enlisted rather than officers. There was another category of pilot trainees called Officer Candidate School, but we were considered enlisted. The third option, which I took, was that you could switch to what they called the six-month reserve program—six months of active duty followed by a period of reserve status. That's what I did. That ended in September, and of course, I immediately began looking for a job.

Guess what? Nobody was hiring because of the cutback. I tried Florida first, and then I tried the East Coast, and then I tried the entire nation. One of the very first places I asked was Langley because I had really enjoyed working there. I was looking for a job doing research. I didn't really want to get into production or anything like that. They turned me down. Sometime during December, when I had got to the point of blanketing the entire nation, I got a response from Ames, saying that they would hire me if I could start work as late as January 2nd, that they couldn't hire me right then, but January 2nd, they could hire me. I agreed. About a week later, I got a letter from Langley, offering me the same deal.

Clearly, what was going on was that even at that time, even that early in the process, it had become apparent that NACA needed to ramp up its research. We were missing out on things, and we were trying to build rockets, and NACA was not directly involved in that, but sort of indirectly involved in that. The work was being done by the Navy and the Army, mainly. We wanted to get into it, and the president apparently agreed that we needed to ramp up our work. That's all I can suggest as to why they finally, suddenly decided that they were going to give me a possibility of a job.

January 2, 1958 was my first day at Ames. I flew cross-country, stayed in the President Hotel in Palo Alto, at an ungodly amount of dollars per day—which today would seem paltry, I'm sure. It was probably like $35 or $40 a day. Took a taxi to work because I didn't know how I was to get there, and didn't even know where it was. That was the first day, and after that, one of the coworkers who lived in Menlo Park drove me to and from work for a couple of days, until I could finally find a place to live in Mountain View. Since that time, I have lived in Mountain View. [Until I bought my first vehicle, a Cushman scooter, I walked to work. Occasionally I got picked up by another Ames person on the way. I sold the Cushman in the fall, when it started raining and bought my first car.]

The first several places that I lived were all residences, and I was a roomer. The first place, the landlady was doing this for the first time. She wasn't too sure it was such a great idea, and it didn't take long after I moved in that the mildew stink from my trunk full of books got to her, and she kicked me out. In the meantime, a friend of hers a block away was a boarder. You could go to her for food, for meals, and so, that's what I did for several months. Just kept going. What was I doing at work in the meantime? I think that's what's really interesting.

JOHNSON: It's all interesting.

PEARSON: My very first job was to help start up and shake down a brand-new wind tunnel. It was designed to operate at low density and high temperature. The way they did it was they had a big plenum chamber filled with zirconium pebbles that could withstand high temperature. They would heat these pebbles with a gas burner of some kind, till they were really hot. Then, they would pressurize this big plenum. I think there was no valve between the plenum and the test chamber. I think that once they started, it just ran as far as it could go. What pulled the air through all these pebbles and into the wind tunnel was vacuum produced by a small steam ejector vacuum plant right there in the same building.

The building we're talking about is that building right next door. The tunnel that I was working on was very much like one right next to it, but it did not have a heater. It just had low density and high speed. I don't know how high the speed was. In my case, the speed was supposed to be Mach 6. Frankly, I don't remember well enough to know whether we actually reached that Mach 6 or not. At that temperature, if you start with room temperature air and expand it to Mach 6, it liquefies the air, so it doesn't work. You have to start with hot air, really hot air, and that was the whole reason behind this pebble bed heater.

Probably the first week or two, the pebbles at the top, at the very hottest part, fused together into a big, fat lump. [We] had to tear it all down, find out how extensive it was, and so forth. What are we going to do? We tried again, and we were able to get a few runs through it, verify that we were able to get reasonable pilot tube readings, that's basically all we used. We just had that and the airflow. Basically, it was a failure, and yet, this was the prototype for the 3.5-foot wind tunnel a few blocks down the way, which was already under construction. They left off the top part of the plenum—they only had the bottom plenum—and they did not heat it as much as we tried to heat ours. Even so, when they started running that, one of the biggest problems they had was sand. The pebbles began to disintegrate, and so this fine, powdery sand would hit the model and just do terrible things to it.

They were able to get some good data. The way they did it, they had three different nozzles to expand the air. One of them was Mach 7, a little bit higher than what we were doing. One of them was Mach 10. I'm not sure they were ever able to make that work—I can't believe that they did because of the temperature problem. I think the other one was something smaller, like maybe Mach 5, or maybe Mach 3, even. I don't know. I was never really terribly involved with it, but it was that kind of really experimental work. It was not just the models that were experimental, it was the tunnels themselves. Very interesting. That lasted for, I don't know, a year, a year and a half. It's in my records, actually how long it lasted.

Of course, all it had to last was nine months, and bang, we're NASA. Right then and there. It happened so fast. Clearly, in my opinion, the Agency, the president, even the Congress, was already thinking that we needed to make something bigger than just NACA. That's what they did: NASA combined not only NACA, but a goodly part of the Redstone Arsenal, the Army's Redstone Arsenal, in Alabama. Basically, what we got was Wernher von Braun's team of workers that were working on rockets. That was a big deal. Without their rockets, we would never have done any of that stuff because their rockets were ultimately the ones that were successful, whereas the Navy rocket, I'm not sure it ever managed to get anything up.

It was a really exciting time of change and growth and redirection. Already, even then, we were moving away from aeronautics into space. The fact that we changed our name to "Aeronautics and Space," the old-timers, Aeronautics is the first A in NASA—we have to make sure that aeronautics is represented in every way. It didn't really help too much. We still have aeronautical work going on, but it's a smaller and smaller part of what we do year after year after year, I'm sorry to say, really. On the other hand, it's what we are doing in space that is so exciting, so much, and so many different things, the different satellite experiments and so forth. After my first work with the low-density heat transfer wind tunnel, there was a new building right next door to the 3.5-foot [Wind Tunnel] building, where we were opening up a new wind tunnel in which, instead of trying to heat air so that it wouldn't liquefy when it got to Mach numbers we were interested in, we built a 20 inch, probably Mach 12, wind tunnel, powered by helium. Helium has to get a whole lot colder than nitrogen before it liquefies. By pressurizing helium to 6,000 psi [pounds per square inch] and letting it flow into the vacuum. [We got the high Mach number.]

We got the vacuum the same way we did over here. We had a giant steam ejector plant that pulled the air out of two huge spheres. The 3.5-foot had four spheres because they had bigger stuff. I know all the 3.5-foot spheres are gone. I'm not sure whether ours are gone too, by now. They took them all down. By doing it that way, we were able to run for a few minutes at a time before the pressure in the spheres grew to the point where the pressure couldn't keep it going. I did some experiments there. I actually wrote a paper on the first experiment in that tunnel. Basically, Thor [Thorval] Tendeland, who was managing the whole deal, said, "Here's a model that has already been tested—why don't you test it in this and see how it compares?" That's what my test was all about.

Of course, in order to do all that, I had to use the brand-new data acquisition system that has been built as part of the construction of this new wind tunnel. I got interested in dealing with data acquisition, data analysis, by using these computerized data, and it was on paper tape, and then ultimately on magnetic tape. Not the kind of magnetic tape we have today; it was really primitive stuff, but we had it.

I began writing programs. Back in the days when I was starting up the low-density heat transfer wind tunnel, that was when NACA got the IBM-704 computer. What happened there was they offered all the engineers on the field, you can have a class in how to write programs for this new computer. I took the class, FORTRAN II. To help me with my data acquisition and analysis of this puny little one-pressure measurement that I was getting out of the low-density heat transfer wind tunnel, I wrote a program to generate a look-up table. I had a manometer, a McLeod manometer, McLeod meaning that it was designed for very low pressures, but it was not a linear relationship because of the way it was designed. It was a nonlinear relationship. You could calculate the relationship, and so I was able to do that and show, for each height of the tube, what the pressure was.

That's what I did—I put that into a table, so I could look it up and make the conversion from measurement of height to what the pressure was. The pressures were not what people considered a vacuum today. We were not talking about really, really low vacuum; we were talking about microns, tens and hundreds of microns. It was still what I did, so I just kept getting interested in writing programs to do these things, and ultimately, did more and more of that. Then, in addition to all this work with wind tunnels and wind tunnel data acquisition, I also got involved with Carr [B.] Neel, who had an experiment on the Orbiting Solar Observatory [OSO].

His experiment was very interesting. What he wanted to do was to find out if various paint samples would degrade in space because if they did, it would change the temperature of whatever satellite they were trying to protect. The way he did it was he designed a holder for the various samples. I think on the first one, they had seven samples, one in the center and six around it. Each was a little circular sample. The samples were isolated, thermally, from the spacecraft by a series of reflecting, tissue-paper-thin aluminum shields, and of course, by the vacuum of space, ultimately, because they were on the outside of the satellite. The way it worked was as the satellite went around the Earth, when it moved into the shadow of the Earth, the temperature dropped abruptly because there was nothing heating it, but depending on the color of the sample, it was emitting, more or less rapidly, heat out into the cold darkness of space. The rate of change of temperature at that point gave you an idea of what the emissivity was. When it came back around and saw the sun, the temperature immediately began rising.

Again, the rate of change of the temperature as it rose gave you an idea of the albedo [reflection coefficient]. The albedo is the relationship between the absorption and emittance. By knowing the emittance and the albedo, you can calculate the absorption. You have two important properties of the paint. If they stay the same, over time, fine. You've got a nice stable paint. If they don't, they get degraded. You can even get an idea of what the degradation really meant. It was a very ingenious and elegant experiment, in my opinion. The first or second Orbiting Solar Observatory, and he was going to do it on at least two others, I think.

One of them failed pretty badly. People were killed. They were doing the final assembly, getting ready for the launch, and somehow or another, a spark happened in an environment that was flammable. It exploded, and the person who was testing at the time was killed. Really, really sad and really bad, but we did keep going, and we did at least one other test, maybe two others. I can't remember all that much. There were several papers written. I wrote six in all, and somewhere, I don't think it's in that history notes that I gave you, but somewhere, I have a list of all of those papers.

The first paper was about the wind tunnel experiment I told you about. One of the latter papers was about a data acquisition and analysis program that my boss, Roy [M.] Wakefield, had written, but I had also helped him with it. It was going to be given as a paper at some proceedings or other. He asked me to present it, so he tacked me on as a co-author, so I could present it. The last one was about four years after the Orbiting Solar Observatory experiment, and we went back, we were still getting data. We went back and said, "Okay, what's happening now, four years later?" What we were able to see was that the degradation happened quickly at first, but then become much slower over time. The change from the first few months was pretty dramatic, but from there to the four years mark, not that big a difference. An interesting data point that we were able to put together.

I got involved in writing. I only wrote one paper by myself, I think it was the first one on the Orbiting Solar Observatory, and all the rest, I was a co-author. It was an introduction to what engineers do—they write papers. That was the last paper I ever wrote. This was still in the 1960s. The solar observatories went up in the early and middle '60s, four years later, maybe '68, something like that, at the latest. The only thing even approaching a, quote, "paper" after that was when I had to make presentations about ADP [automatic data processing] acquisition plans or budgets. Of course, those were not really papers, but just presentations.

You asked a question about how things changed from NACA to NASA. The first paper that I wrote was a NASA paper, patterned after the types of papers that the NACA had done. I don't think it ever got any real publicity because it wasn't really valuable there, but also because it had to be classified because it referenced classified material. That was confidential, it was very low level of classification, but as a result, it was not even publicly available until fairly recently. That was my introduction to classified. I had a classified level—at one time, it was secret or top secret, even. I can't remember how high I went. It never seemed to have anything to do with what I was doing. It always seemed to have to do with, well, because of your work, you have to have access to this information that’s classified, so you have to have a classification so you can see it.

As you probably know, it takes two things before you can see a classified work: you have to have a need to know, and you have to have the right classification level. Through these various experiments—and not just Carr Neel, but John [C.] Arvesen and John [P.] Millard had their own experiments on some spacecraft, and I helped them do some of the data analysis for that kind of stuff, too. It was all very interesting. I became the guru of computing within the organization and the branch.

By this time, we’re talking about 1970s. I was setting up and operating a new data acquisition system for a new set of arc jet tunnels in a new building, 238, using the same steam ejector system for the vacuum. It has had a long history, and it’s been rebuilt recently, I understand. I got interested through that, in the idea of the acquisition planning, which you have to do in order to buy the computer and other stuff for this new data acquisition system. I had to write an acquisition plan. Other people had to write acquisition plans, and I helped them write theirs.

During all of this time in the 70s, I was a GS-12 [General Schedule pay grade], step 10. Nothing, nothing, nothing in the way of promotion capabilities—nothing. In ’75, I got married. In ’77, we had a kid. In ’75, we bought a town home, which was a spectacularly high price, $27,000. In 1979, one of our neighbor kids shot out our own windows with a BB gun because he didn’t like the way my wife, who was part of the homeowner association management, had told him to quit hiding his papers in the water meter box, and quit riding his bicycle in the pool. Crazy kid. We decided that we’d better move, so we did. We bought our present home, then, mainly because a friend of ours, a good friend of ours, who we told that we were now looking for a home, said, “Wow, what a coincidence, the house across my back fence just came on the market.” We jumped on it, and it turned out that we were able to buy it.

This was 1979. I don’t know if it means anything to you, but what it means to me is 11.5 percent interest—oh, that hurt. It was awful. Of course, I was still a GS-12, and granted, pay was going up because of inflation at least, thank goodness, so financing the house from the very beginning was difficult. We were able to get a pretty good down payment because we sold the town home, even though it was only four years later, for about three times what we paid for it, but of course, the new house was even more expensive than that, $129,000. It seems so cheap today. As a matter of fact, today, the house is worth just a touch over 10 times that much. The value of housing in this area is out of sight.

The house is not that big a deal—it’s 1,600 square feet, 400 square feet of which is a conversion of the garage to a family room, and so, well, it’s not that big a house, but we’ve got it and we’re still paying on the mortgage. It’s been refinanced so many times, I can’t count it all. I hope we’re finally down to the end, oh, I hope so. We just refinanced it for 10 years because we were able to get 2.5 percent interest. It was a lot less than we were already paying, which was already a lot less than we’d ever paid before.

That’s another aspect: it’s not inflation anymore, but as a result, interest rates are low, this is good for people like me who want to borrow money. People who are lending it, not so good, and people trying to live on interest, it’s hard, really hard for them, because they’re not making the interest.

I did go into the ADP, automatic data processing equipment acquisition plan process. There was a person at Ames who was getting ready to retire, and they were looking for a replacement. I jumped on it—I said, “I can do that,” because I had that familiarity with what the process was all about. I even knew the guy because I had to meet with him so many times. That was 1980 or ’81. It’s in my history—I finally have some real dates, there, and some real dollar amounts, by the way. Let me show you this. I started early on, making a chart of how much money I made—and I made a copy for you. From here to here, I just finished. I got tired playing with it after a while. See here, this is inflation. It was awful. Yet, towards the end, when I was finally able to start getting promotions, then the promotions show up as big increases, instead of just inflation.

If you can read it, I’ve indicated the GS grade level and the step. That was something kind of unexpected to me. I thought that I deserved to get a promotion, but it didn’t happen right away. I had to spend a year on TDY [temporary duty assignment] at [NASA] Headquarters [Washington, DC], doing the job at the Headquarters level. I had to get a master of science degree in systems management, which was very interesting in and of itself because it was offered by the University of Southern California, but all the classes that I took were either right in this area, they would send a teacher up to teach a class, and they taught a given class for two months, six classes per year, 12 classes to graduate, and then there would be another class. The classes could be taken in any order, but you had 12 classes that you had to take. I think there were a few where there were some options, you could take this class or that class, but you didn’t have to take both.

During 1983, when I was at Headquarters, I still had half the work to do. I took the classes at the Pentagon [U.S. Department of Defense] and in Crystal City [Virginia] because they were offered there, too. In fact, the whole program had been set up originally for the benefit of the Department of Defense, and it was open to anybody who wanted to attend, but of course, a lot of military people were attending. I felt like, hey, it was pretty good. NASA paid my tuition—I think I had to buy the books myself; I can't remember that much. The tuition was $700 per class. If you think of it in terms of semesters, six classes per year, three classes per semester, that's $2,100 per semester. My son went to the University of Southern California. We paid, like, $13,000 per semester, and that was a long time ago. You can't get those prices anymore.

JOHNSON: No, you cannot.

PEARSON: As I was doing my job, as ADP guru, the job changed. Initially, everything that you bought that was IT [Information Technology] had to have an acquisition plan. The guy who did it before me had already taken the first step in trying to ease that job for everybody. He had developed a two-page form that you could fill out that would provide all the necessary information that you needed, if the price was low enough. Most of what people were buying then was low enough—think about it, 1980. The PC [Personal Computer], okay? People jumped on that. They were already buying earlier PCs from other vendors. When that PC came out, "This is great, let's do it." It was already past the time when the HP-35 [Hewlett-Packard pocket calculator] had come out. That was a big deal. People could buy that for I don't know, $350 or $500, whatever it was. It was a big deal.

You didn't have to have a slide rule anymore. Forget the fact that the slide rule was 1/10 the price, but the calculator was much more precision, instead of three or four digits' precision, depending on your slide rule, you could get 8 or 10 out of the HP calculator. It was really a fundamental shift in what the engineer carried around with him to do these minor little calculations that just seemed to crop up everywhere you turn. We were already buying mini computers. The computer I bought for the data acquisition system I told you about in the Arc Jet Wind Tunnel, that was a PDP-11/45. It was not a big mainframe; it was a little computer. It still cost many thousands of dollars. Many, many thousands of dollars.

I decided that I would continue Jack's work. Jack Tunnell was the predecessor. The first thing I did was I simplified the form even more and made it just one page, and then, as I started getting a flood of purchases of VT-100 and VT-200 terminals, they were buying these for $1,000 or less, and it just creeped me out, to have this big, elaborate acquisition planning requirement for such a puny little bit of money. I thought about it and I said, "On the back of the purchase request form, there's a space for you to say why you need to do it and what you're going to do with it." People were not filling it in. I told people, "You fill this in for this purchase, and that's your acquisition plan." There you go.

They did that, and as time went on, the threshold between when you could use these little forms and when you had to have a real major plan began to rise. GSA [Government Services Administration] had to approve plans above a certain threshold. Sometime in the late '80s or early '90s, they started increasing that threshold. My feeling was, okay, that's the way you want to play the game, I can do that, too. I started raising the threshold also, and letting more and more of the work be done by these little, bitty forms, or a minor thing on the acquisition plan of the purchase request form.

It was well received, needless to say. People hated filling out those forms. By this time, I had two people working for me, and one of them had a visit from a guy who had been trying to write an acquisition plan that satisfied all the requirements. The guy came in, threw a little note at him, said some things about what he thought of the whole idea, and started walking out. He picked up the little note, and it was a death threat. He immediately jumped up, went out and caught the guy, started calling for me, “Doug, Doug, come here, watch this!” I did, and as soon as I saw the guy and verified that John knew who it was, I said, “Okay, you can go.” I immediately called his boss.

I said, “This guy made a death threat in writing,” and it turns out that the guy was just a little bit off his rocker, and they had had some similar problems with him in the past. I don’t know what exactly happened, but he never came back to us, thank you. John Spicer and his coworker, Rosalind Miller, were both good friends of mine, partly because we worked so well together. Not too long after that, several people at Ames, including John, went to Mount Shasta for the annual climb to the top of Mount Shasta for July 4th.

John was a runner, top physical condition, had every expectation of making it all the way to the top, no problem. He forgot to think about altitude, and he got altitude sick on the way up, and it kept slowing him down, slowing him down. Whether he actually reached the top, I can’t remember. People who did reach the top could sign a little register up there. If he did sign the register, then he made it to the top, but it was after dark when somebody coming down from the top saw him not too far from the top, still going up, and offered to stay with him, or something like that. He said, “No, it’s okay, I can make it.” On the way back down, he slipped, fell into a crevasse, and was killed. The body was never recovered. The only reason they knew where it happened, in fact, was that his ice axe, which he had been carrying a rented ice axe, was still there on the slope, leading into the crevasse. To this day, it really hurts me to think about him.

We had got to the point now where acquisition planning was no longer as onerous a thing as it once had been. Congress began passing laws. The acquisition plan was the result of a law written in 1965 by Jack Brooks, the congressman from Texas, the infamous Brooks Act [Public Law 89-306]. There were several laws that were passed in the ‘90s that were important to IT.

One of them was the Paperwork Reduction Act, and as a result of that, the government had to stop requiring people to fill out so many forms. This is part of the reason why GSA started increasing the threshold for these acquisition plans.

At Ames, because of my involvement with acquisition planning and forms, one of the added duties that I was given was to make an annual report of what we were doing to cut back on paperwork. That didn't last very long, and was not a really big job, but it was part of what was going on. It was a change in the way the government dealt with IT. There was also the Clinger-Cohen Act [Public Law 104–106]. This was a major change. First of all, it abolished the Brooks Act. Not to say it made a change, because a lot of what it required was the kind of thing the Brooks Act was requiring all along, but it divorced IT from procurement. This was a new thing. Up until then, the requirement to do acquisition planning was built into the federal IRM [Internal Revenue Manual] regulation, which is a procurement regulation.

Now, that was changed. The regulation still requires planning, to this day, but today, the planning is now budget planning. It's no longer a specific plan for a specific acquisition. Interestingly enough, all along, the procurement regulations had required, if your acquisition is big enough, important enough, expensive enough, you have to have an acquisition plan—duh. That's still there. If you're buying something that's big enough, important enough, expensive enough, whether it's IT or not, you have to have an acquisition plan. It's a procurement requirement, now. It's not an IT requirement.

The IT requirement now is for budget planning. Good budget formulation process today has a component related to IT, and that component is managed by my office. In fact, I now call myself an IT planner, for that reason. I don't have an office anymore—I'm working half-time—and the guy who is overseeing the work that I do, the civil servant, is the enterprise architect for the Center, John [W.] Stebel. My boss, of course, works for ASRC [Federal Research and Technology Solutions], like I do, but my task under that contract is to provide support for the IT planning function that goes on every year at Ames. Basically, the routine there is the organizations around the Center are asked to provide an IT budget for what they're planning to do. There is a lot of detail about what the budget contains and how many years it covers and all these other kinds of stuff. My job, then, is to make sure that they do a good job of writing that plan. I help them with it, give them instructions about what to do and how to do it.

Guess what? Changes every year! It's not necessarily a big change, but it seems like there's always some new tweak that's different, never been done before. It's built-in job security. If I were to quit today, somebody else would have to do it. They say, "Who could do it? Nobody knows how, except you." I'm the only one. Ever since I became a contractor, even before, before I retired, even, I've been training people to do my work. I get them, they say, "Okay, fine, I'll be glad to let you teach me how to do this stuff." They learn how to do it, and they get another job. There must be a dozen of them that no longer do this kind of work.

Rosalind Miller, I mentioned before that she was in at the beginning of this emphasis on IT budget planning. She found another job. Year after year, I would get new people, they would find other jobs. Recently, I haven't even been trying. Interestingly enough, some of the new people I got came from interesting jobs programs. There was a program called New View. This program was aimed at women above college age, well above college age, who were reentering the jobs market, and they were going to school at community colleges—Foothill-De Anza—and they wanted some work experience, and so, I would hire them.

There was also summer students, high school students, or sometimes even college students. I had some high school students helping me from various times. Probably everybody who ever helped me was not really interested in budget. "Thank you very much, this is mundane, idiotic, because it doesn't lead to anything, I'm sorry to say. It's just finance—I'm not an accountant; why do I want to do this?" They wanted a job doing research or at least something in engineering, they didn't want this kind of monkey motion.

The fact that I enjoy it, even though I'm a graduate engineer, is just an indication of how crazy I am. It has been a gradual shift from experimental aerodynamics, in the beginning, to budget planning, then a very specialized link—so specialized that the CFO [Chief Financial Officer] is not even involved in it. This is all a CIO [Chief Information Officer] activity. Frankly, I think it's a bad idea. In truth, the data that I collect, most of the people who provide it, almost without exception, there's only a few exceptions, most of the people who provide the data are resource analysts or resource executives in the organizations. They get the data from their knowledge of the organization's financial work, spending, planning, and they also get some of it, I'm sure, from their bosses. Fine, I get it. It is a non-engineering activity, I mean, really not engineering.

JOHNSON: It is kind of interesting, that shift you took. Back in 1955, when you first went to Langley as a computer, that was also interesting because so many of the computers were women during that time period. The group you worked with, were there very many men or very many students in that group?

PEARSON: There were at least one or two other students like me, and yes, they were boys, men. Yes, I'm pretty sure there were some women. Certainly, the computer groups here at Ames that were in my branch were women, almost exclusively. Some of them were not. Mike [Michael J.]

Green, who eventually went on to become a pretty high-level engineer here at Ames, doing some really cool work, both theoretical and experimental, in aerothermodynamics, meaning really high-speed flight, he was one of those, quote, "computers" in the early days.

JOHNSON: When you went to Langley, where did you live? As a student, for that two months, did they provide housing?

PEARSON: No. They gave me names of places I could look for, and I did. That was an interesting little deal. I got a room in a rooming house. I'm pretty sure the landlady was the owner of the house, and it was a house, but it was a big house, lots of rooms in it. The room I was in was shared with an older man, who was in construction. I don't know what he did—carpentry. One day, I got up, went into the bathroom, and I was brushing my teeth, and something felt kind of strange. I looked around, I didn't see anything, but the [shower] curtain was drawn, so I pulled it back. There was this guy, standing there, fully clothed, in the bathtub/shower, smoking his cigarette. I'll get into that, later.

That was strange, but across the street, to the boarding house, run by another landlady, it's undoubtedly her house, and I talked to her about it, told her what had happened. She said, "That guy, I know him, he's got DTs [delirium tremens]." She told my landlady, and he was gone. I don't know whether she moved him to a different room, or moved me to a different room. I can't remember the details. That was my first experience with DTs. I don't think I had to ask what DTs was, but I had never known anybody who was that bad enough, that they actually got DTs.

JOHNSON: It is quite an experience, for a young person.

PEARSON: In 1955, I was 20, the summer of 1955, I was 20 because I turned 20 in November of ’54. I said something about smoking. My parents both smoked heavily, both of them, all the time we were growing up. I hated it. My sister did, too. My sister got allergies. She had asthma, in fact, and yet, they continued smoking and didn’t seem to realize that she has asthma. Of course, when I came to work here, everybody around me smoked. It was the accepted thing. Adult men, in those days, more than half—maybe as much as two-thirds—were smokers. They smoked at the desk, they smoked in the hallways, they smoked everywhere they went. I just did not like it, but there was nothing I could do about it. It never occurred to me to say, “Would you put that out, please? I just can’t stand the smoke.” No, no, no—not me. I just put up with it because I thought that’s what you had to do. Much later, our building was one of the first at Ames to have a no-smoking policy, cannot smoke inside the building, you have to smoke outside. I was so grateful—so, so grateful.

JOHNSON: That’s an interesting little cultural aspect that people do forget about. It really wasn’t that long ago that they eliminated the smoking, so that’s interesting.

PEARSON: My father actually quit, but my mother never did. She died at a fairly young age—64 or 65, something like that. Not of lung cancer, as you might think, because of all that smoking, but she had long had a kidney problem and she ultimately died of kidney failure. They talked about dialysis, and she said no, she was not having it—she was not going to do that. I can’t help thinking her real objection to it was the cost because it’s expensive today and it was expensive then. Daddy did finally quit. I don't remember whether it was before or after, but he did get a cancer in his throat, and luckily it was caught early enough that it was not the kind of radical surgery. There was a lady in the shop where you went to get your badges, she had a little microphone that she had to hold up to her throat to talk because she had had that kind of surgery.

JOHNSON: When they first offered you the job here and then you said, a week later or not too long after that, Langley offered you that job and you had initially applied at Langley because you'd been there before, what made you choose Ames over Langley?

PEARSON: I had already done it. I thought, why would I change my mind, just because this late offer came in? Of course, I could have—I could have done that, I could have written a letter to Ames, "I just got an offer from Langley; sorry, Ames, you're out of the running now," but I didn't. I had accepted and I felt like I should accept.

JOHNSON: You mentioned when you were here and those classes were offered on programming, did very many engineers take them up on that?

PEARSON: Yes, it was a fairly popular class, as I recall. There were 15 or 20 people in the class, and that was not the only class they offered. Here's another thing—I don't know if I ever mentioned it in response to the questionnaire that you sent me—when I was hired, I was told, "Your pay, your starting pay, is going to be low compared to what you could get working for industry, but we have a fast promotion policy here, and very soon you'll be making pay that is consistent with what you could get in industry."

The way they did that was they offered seminars, I think on a weekly basis—I can't remember for sure. I remember going to quite a few of them, one of which was led by Jack [John W.] Boyd and other luminaries, people who had real reputations in the aeronautical engineering world, and people who had growing reputations in space. I don't think I ever actually had Harvey [Julian] Allen give one of those seminars, but I do know that somebody talked about his insight, that you can handle this problem of reentry heat by making it as short as possible and spreading it over as large an area as possible by using a blunt body.

There was nothing magic about it from a science point of view, but it was a brilliant insight. He looked at it this way: you're coming into the atmosphere, this is where the problem really arises, at too high a rate of speed. You've got to slow down. How are you going to slow down? Put on the brakes? Something big to slow you down, so why not make the whole thing big? Then, whether you slow down fast or slow, you're going to go through a period where the temperature is outrageous. I mean, outrageous—it is enough to melt steel. Again, make that as short as possible. Again, make it as big as possible. Slow things down quickly. Brilliant insight.

An example, I think, of how engineers can solve seemingly complex problems by using an insight that simplifies the problem down to a point where there's an obvious solution. That was what Harvey Allen did, and it's my understanding that that's not the only thing he did that was brilliant. He was brilliant in many other ways as well, but that was the one thing just at the time I came to work that was a very, very important element of access to space. Even today, were it not for that blunt body concept, which was carried over into blunt bodied aircraft, we designed and built several down at [NASA] Dryden [Flight Research Center, Edwards, California]—"we" not necessarily meaning Ames only, but certainly there were plenty of people at Ames who were involved in it—and the [Space] Shuttle itself is designed not to come in this way, but to come in this way [demonstrates]. It's a whole different concept. The entire bottom of it gets screaming hot. Were it not for those tiles [Thermal Protection System], we'd be in deep trouble. What was I doing in the Arc Jet Wind Tunnels with my data acquisition? I was testing the tiles. There are so many things that tie together.

JOHNSON: In those wind tunnels, early on and then when you started the data acquisition, as you mentioned, the way you got that information, you started out with paper tape, and of course, it evolved to computers that we carry around in our pocket. How did you keep up with those technological changes, especially as part of that IT and acquisition? Were there continuing classes here, or did you just keep doing research on that to stay ahead?

PEARSON: Since you are only here for a short time, you may not be aware of this, but every year, for many years, Ames has had a summer student program. For years, the summer students were asked to solve a problem. I remember one year, they came up with a potential habitat in space—basically, a giant wheel. The wheel turns, gets big enough in diameter and turns rapidly enough that the gravity on the outside rim is whatever gravity you want, thank you. Of course, this is a big thing—it's bigger than the [International] Space Station.

People never actually figured out how to do that, but when you saw the movie, *2001*, the ship goes up into space, goes to the space station, and guess what? It's this big, giant wheel. [Way bigger than the London Eye Ferris wheel.] Docks in the center of the wheel. As the spaceship approaches the wheel, it begins to turn. The music, at that time, the waltz, in keeping with what was going on because it was a slow, dreamy sequence. At the end, it was turning at exactly the same rate as the wheel and it just eased right into the center, right where it belonged. So cool. Of course, that's not what actually happened, and I'm sure that one of the reasons is the engineering feat of building a bicycle wheel that big in space was just more than people could figure out how to do. It would have taken far, far more pounds, tons of material than the Space Station. [The London Eye of today would be tiny by comparison.]

That was an example of the kind of work that they were doing. Another kind of work that they were doing, they decided that they would design a way of moving material mined on the Moon back to Earth. They designed what I think they called a rail gun, railway track, and they put the ore on a sled, and they put a rocket behind it. Zoom, speeds off, escape velocity, leaves the Moon, trajectory brings it to Earth. They didn't talk anything about how it enters the atmosphere without burning up—I mean, this is only ore, folks, just ore—but it was a concept that even today, you could turn that around.

You could talk about using that to launch a rocket from the Earth. You could put it on one of these rail gun deals, get it up to maybe two or three times the speed of sound. If you can imagine something that huge arriving at that speed. Bang, off it goes into space. It's never been done, and it looks like it probably never will be, and yet people are still talking about it as an alternative, a possible way of doing things. Today, I don't hear so much about that kind of stuff.

Today, what I hear about—and in fact, actually attend—they have a series of seminars offered to all the students and to everybody else. Anybody can go. Often, there's two or three in a week. I missed some this week. In fact, I miss most of them. They come in the afternoon and I only work half-time. Yes, if I want to, I can stick around, but I often don't. They have these, and they're interesting. Some of them are really interesting. In addition to all of that, there's a Director's colloquium series. This is all-year round, and they only have, I don't know, five or ten of them a year. Again, you're talking about really far-out research, really interesting stuff for people to keep learning about.

As far as me learning about IT and the changes that took place, I never really did. I know conceptual things. I understand how PCs took over the world here at Ames, just as they did everywhere else, because of factors that the mainframe proponents didn't really grasp. Number one, yours, personally. Not really, of course—it's owned by the government—but that doesn't matter, it's yours, yours, personally. Number two, it's a real computer. It can do real processing. You don't have to use it as a terminal to get to a big mainframe, the way you did with TSS [time-sharing system for computers], for example. I used TSS, I thought it was great, but it was slow as molasses because it had too many people on it. The third thing was cost. TSS, when we'd got it, cost us over $1,000,000 and we got it as a bargain because it was excess property from the Army, or from the Air Force.

We couldn't have afforded it, otherwise, and in fact, we got stuck with it because we couldn't go on. It needed to be replaced almost immediately. We couldn't go on because we couldn't afford anything beyond it. We got stuck with it, far longer than we should have, just because we couldn't find a way to get around it. This has been a recurring problem at Ames with regard to big computers. We have always had a requirement for big computers, but we have never been able, really, to afford them. Today, by a stroke of luck—and I think Bill [William F.] Ballhaus and Hans Mark were ringleaders in this whole idea—we have convinced upper management and the [U.S.] Congress that if you have any hope of having a big computer that is capable of doing the big jobs you needed to do, you have to keep spending money on it. You can't just buy it one time and forget it. Doesn't work that way.

The computing requirement is growing, even today, so rapidly that we cannot keep up with it, despite spending over $40,000,000 on high-end computing here at Ames, and another $20,000,000 or so at [NASA] Goddard [Space Flight Center, Greenbelt, Maryland] for their super computer. We have more computing than these super computers can really handle. People are doing things, they're buying clusters of smaller computers that takes a long time to do it on these small computers, but on the other hand, I can't get on the big computer. Got to have it, so they do.

Similarly, the vertical motion simulator, the big, big simulators, not just that, but there were others as well, in our simulation laboratory, require massive computing. There, the requirement is real-time computing. The complete computation required for whatever purpose must be finished in less than 50 milliseconds because if it takes longer than that, the person using the simulator perceives the hesitation, and it doesn't work. This is not just something that they did on the spur of the moment, that they just said, "Oh, let's say 50 milliseconds." No, no, no. They did some work to figure out what that requirement really is. IBM did, too, back in the early days, in the '60s, '70s, IBM did some research on transaction processing.

What they said was a typical transaction processing is you have a person entering data into the computer, the computer does something with the data and gives a response back. What is the delay in that response? What impact does that have on the productivity of the worker? They were able to show, with time and motion studies, that if the response time back was more than 200 milliseconds, the user would begin to lose their grasp of what was going on, to the point where it would slow down the productivity. It had to be faster than 200 milliseconds, just to keep up with the user.

Until that time, people had always said, "The computer is so much faster than a person—what difference does it make?" The difference it makes is to the person. The person's pace of work must be constant for maximum productivity. If the person perceives a hesitation, a slow-down of some kind, because of the slow response, it doesn't work. Productivity is reduced. There's been research in all of this. As far as the motion simulators are concerned, the reason for the 50 milliseconds is because it's not just a transaction back and forth. The simulator has to respond not back and forth—it has to respond with you, and that's where the faster speed comes in. It has to respond not just as an answer—it has to respond with you.

A computer capable of doing all the necessary processing at that rate of speed was really hard to come by, and the depth of the simulation was not as great as they wanted it, for that reason. Ultimately, they found a way to buy a [used] CDC 7600 that was able to do all of their processing at the speed they need. While they were trying to come up with the money, they had to write the acquisition plan for it because it was back in those days, and they were explaining all of this to me. I said to them, "Why don't you have a lot of little computers doing all these little sub-tasks, working together?" They had thought about the idea, but they had not been able to conceive of a way for all these little computers to work together without slowing things down. Maybe they've done that by now, I don't know, or maybe they've just said, "The level of simulation we have is acceptable." We've got computers nowadays that can do the work, so we're okay. I lost track of it, partly because there's no more acquisition planning requirement. They didn't have to explain things like that to me anymore.

JOHNSON: If you don't mind, I'm going to ask Rebecca if she has any questions.

WRIGHT: I don't think I have any, no.

JOHNSON: Glenn, do you have any?

BUGOS: Yes, four. Do you remember Sputnik [Russian satellite launch]?

PEARSON: Yes. I don't know what I was doing when it happened, but I definitely remember Sputnik.

BUGOS: Do you remember a change in the national conversation about space that excited you, involved you?

PEARSON: Not so much that it excited and involved me because I was already really interested. The fact that Sputnik came up before any of us did was a disappointment to me. I was already on board with this space race idea. The idea, when [President John F.] Kennedy proposed going to the Moon and safely return within the decade, that was a magical moment for me, as well as for a lot of other people.

In my personal life, during the late '60s, I began to get interested in automobile rallying. The rally type that I was interested in is not the races that you see on television, but rather, what they called gimmick rallies. You're given a set of instructions that tell you where to go, how to get there, what path you're supposed to follow. All you do is follow the instructions—follow the instructions properly, you get a really good score. The instructions are tricky. You follow them incorrectly, you miss some points and you don't do so well. It's a game of the mind more than of the speed of your car. In fact, the slower you drive, the more likely you are to do it right because it gives you more time to think about what you're doing. A typical gimmick rally might cover a distance of 10 miles in 3 hours. We were just on the verge of forming a new club, I and several other people, and we met at one of the member's houses and watched the Moon landing as part of the meeting. It was cool.

BUGOS: When you first started in the low-density heat transfer tunnel, who were some of the people that you were working with? You didn't mention your branch chiefs or division chiefs or anything like that.

PEARSON: The branch chief was Jack [Jackson R.] Stalder, and he retired fairly soon after that. The deputy chief was Glen Goodwin and of course, he went on to bigger and better things. He became a division chief, and then ultimately, a director. Carr Neel, I did mention. The first engineer that was my boss was Ernest [L.] Winkler, Ernie. He was a good engineer. He knew what he was doing, knew about the capabilities of the materials, and things like that. Whether he was actually aeronautical or not, I don't know that much, but certainly he knew what he was doing about the machine shop.

That was another interesting thing—the building I told you about, that's a two-story building now. At the time, it was one story, and the basement is where the small steam ejector set was contained. Our two wind tunnels were right at that end, so we could be directly connected. Down the length of the building, running right down the middle of it, was a long wind tunnel, 10-inch test chamber that was operated by another branch in this same building. We were on one side downstairs, they were on the other side downstairs. Trying to think who was that branch chief, can't remember, but he ultimately became the division chief, I think, of the division that was above the 3.5-foot wind tunnel.

When I moved to this other building, I don't know whether that was still the same division or not. It's in the history—you'll be able to see what the name of the branch was because it's in the history. My boss then was Thor Tendeland.

Let me talk about Carr B. Neel, before I forget it. In one of the history books, his name is listed in the index with the wrong middle initial. I don't know that it's necessarily yours, so the fact that I pointed to you when I said it doesn't necessarily mean you made a mistake. There is one that actually, his name is listed in the index and has the wrong initial. I mentioned John Arvesen and John Millard, they were coworkers. Back in the early days, the very first days, the other members of the branch, besides Carr and Ernie, were all in aerodynamics. They were doing some experimental work, but they were really theoretical. They were doing things like trying to delay the transition from laminar to turbulent boundary layers, and they were thinking about things like blowing air out the skin of the plane in some elaborate way, maybe perforating the skin, or maybe other ways of dealing with it. I never was really terribly involved with any of that, so I don't know very much about it. Don't even remember their names, I'm sorry to say. The one name that I remember is Connie [C.] Pappas, and he's the one who drove me to work the first few days.

BUGOS: Let me ask about Carr Neel, then. I assume that you and he worked together, which is why he asked you to join his team, but it seems like a fairly abrupt shift from the heat transfer tunnel to OSO.

PEARSON: It's not that it was such an abrupt shift. First of all, the purpose of the tunnels was high-speed flight, not just flight. High-speed flight and thermodynamics, temperature, heat, were key components. His concept of testing the paints was to test their ability to protect the satellite from heat, or from cold, as the case may be. In that sense, it's part of the reentry thermodynamics problem that the division faced over a long period of time leading to the Arc Jet Wind Tunnels. In that way, it's not that diverse, but going back to the other researchers that I didn't talk about, that I don't remember their names and what they were doing, that was not involving any of the wind tunnels that I was doing. The fact that I began working with Carr was simply due to the fact that I knew him because I knew all these other guys, even though I don't remember their names, and he had a job that he thought I could help him with. Maybe it was because I had taken that class in FORTRAN, I don't know why.

He had a technician working for him, whose name unfortunately I cannot remember, who did a lot of that analysis work by hand, the hard way, and also did a lot of it with analog capabilities. He and Carr came up with a concept where they used resistors, capacitors, to simulate the way high heat causes temperature to vary in low density. They were able to build graphs of the results by running this analog simulator. The computations involved in building the graphs was things that the technician did, and ultimately, I began helping with that kind of stuff. When Carr came up with this idea for testing the materials in space, you had to be able to analyze the temperature history in order to come up with these coefficients. He knew what the analysis entailed, he gave me the formulas to follow, and things like that, and I wrote the programs. It's basically what it boils down to. It's not as though I was showing some brilliant insight, inventiveness, or anything like that. I was just following his instructions, just doing what he said.

[Either by tradition or law, the NACA was "inspected" each year, and the site of the inspection was rotated among the laboratories (now Centers: Langley, Lewis, Ames). The 1958 inspection was the last and happened to be at Ames. I think the "inspectors" were the Committee (the NACA was an actual committee), members of Congress, and local dignitaries. The inspection took three days, as I recall, and was done by a tour (much like the recent Open House) with groups of inspectors going to various sites to see presentations by Ames Research Scientists. One of the presenters was Carr Neel, and I helped him by running the projector as needed during his talk. He was explaining the problem of aerodynamic heating in hypersonic flight, illustrated by some of the analog simulator graphs I mentioned earlier, and by a mock-up of the leading edge of the X-15 wing built of an Inconel (I think) sheet that would glow like an electric stove burner when Carr turned on the electric power. Also, John F. Victory, "the first NACA employee," was at the 1958 inspection, and I was asked to escort him to the Headquarters Building 200. So between the two of us, we have spanned the entire NACA/NASA history, almost 100 years. Subsequently, in the '60s (I think), Headquarters people would come to Ames for an abbreviated inspection on occasion.]

I had this ability to write the programs, and he didn't really get into that. He wasn't really that interested in becoming a programmer himself. That was true of a lot of people. People wanted to become programmers and people didn't want to become programmers. We built, over the period of the '60s and '70s and '80s, gradually, a large contingent of computational fluid dynamics, starting from mathematical work being done by Harvard Lomax and his group—I don't remember their names, either—Marcie [Marcelline Chartz Smith] worked for them. That's where she got her start. Do you have a history for her?

BUGOS: We tried to contact her and weren't able to.

PEARSON: That's too bad. John might be able to help you with that, John Humbert, because yeah, she was back there in those days. She really knew what she was doing. She started as a computer, like me, and she was a manager. She really was. I always thought that they gave her short shrift because she was a woman. She could have done more, I think, as a manager. Be that as it may, she did what she did, and she was the division chief of the computation division for quite some time. I lost track of where we were headed.

BUGOS: My final question would have been the relationship between ADP, computational fluid dynamics [CFD], and the simulation computing, the sort of stuff that John [C.] Dusterberry would have worked on. All these three types of computing, CFD, of course, gets the most press these days, but in the early 1960s, did you see any stark divisions between the sort of FORTRAN ADP work that you were doing, what Lomax and Smith and [William A.] Mersman would have been doing in CFD, and what the simulation computing people would have been doing? Were you all using the same equipment? Did you see the fields developing in different ways?

PEARSON: The fields did develop in different ways. The computational capability I needed to do my work, whatever I programmed it to be done, was easily done by PDP-11s, the 11/45 I mentioned in the Arc Jet Facility, and in the original building, there was a series of arc jets there, too, but there was a PDP-11/40 doing that work. There were many computers were capable of doing that. What they needed to do, computational fluid dynamics is very computer-intensive. If you want to get anywhere at all beyond just the most rudimentary elements of it, you really have to move it out. I left it in my office, but one of the early histories has a picture of it, has a picture on the front of a broken simulation of airflow over a simple, two-dimensional wing. That illustrated both the promise and the problem.

Getting that picture to be straight, so that it was not messed up like a jigsaw puzzle, was a difficult problem in and of itself, separate from the computations itself. We solved it. That picture, which appeared on the Ames history book, was a predecessor of the finished picture which appeared on the cover of *Scientific American*. It was not that far later. Just an example of the massive amount of complication. This is back in the days when they were starting to do work on the ILLIAC IV [parallel computer].

The ILLIAC IV was here for a relatively short time—I don't know how many years, whether it was two or six or what. I was never really very involved with it. What I really know about the ILLIAC IV was that from the time it was installed and we were able to turn it on and start running it, it was seemingly more trouble than it was worth to get it to do anything productive. As I understand it, it had a very short mean time to failure, which is bad when you're doing massive computations that require hours, days, and weeks. Also, it was a very unusual architecture. No other computer in the world, as far as I am aware, has ever been built with the same architecture. It had a master computer that told 64 processors what to do. They all did the same thing. If you didn't need all 64 of them to do the same thing, some of them would sit idle. Just taking advantage of the fact that you've got 64 processors was a big problem. I was never involved in any of that. Marcie was, because she was actually in charge of the ILLIAC IV for a while, and she knew the people who were doing the programming to resolve these problems and find ways to make it do what you wanted it to do to get some decent results out of it.

This picture that I mentioned, of the airflow around the airfoil, may have come from the ILLIAC IV. The ILLIAC IV didn't make the picture—no, the data was given to another computer, and I think that was an IBM 1800. I can't remember for sure. Whatever computer it was, that computer then did the computations to build the picture. Either that computer, or yet some other computer, did the work necessary to actually produce the picture. That picture was produced in a grid of 4,000 by 4,000 pixels.

Recently, Sony has come out with a new television product, 4,000 lines. A super high-definition, top of the line, wow, this is so super, it's going to be the coming thing—maybe it will, but this was in the '70s. We were doing some really far-out stuff. Not just computational fluid dynamic stuff, but computer stuff. We were using computers in ways that were beyond the wildest dreams of the people who built the computers in the first place. Today, and certainly way back when, when I got involved in the automatic data processing equipment acquisition planning process, time and time and time again, I would get this deal from a scientist or an engineer who was buying some kind of computer or capability of some kind. "Why am I involved in this? This is not data processing. This is science. Why do I have to do this? This has nothing to do with ADP."

You still hear that today. The budget planning that I do, we call it IT planning now, we don't call it ADP planning anymore. We call it IT planning. They still think IT is business. "Why do I have to be involved? I'm doing specialized research." And it is way out there, no questions at all about that. It's an extremely wide range. It's not just computational fluid dynamics anymore. If you go out to Building 258 and walk their walls to see the pictures of what they've been involved in and what they've done, they have a picture of galaxies colliding. The number of data points it takes to simulate something like that is mind-boggling. The computational requirement to track all those billions, trillions, quadrillions of data points, is mind-boggling. That's not the only thing they do—they do all kinds of stuff out there.

To me, it's the quintessential scientific computing operation. They have a state-of-the-art computer, and they keep it at the state-of-the-art. It's been in the top 10 of the top 500 [supercomputers] most of its life. [A new list of supercomputers is released every six months.] For one brief, shining moment when it was operational and its competitor was still in test mode, it was the number one. That was a long time ago, when it was not nearly as powerful as it is today. Today, it's starting to lose ground, I have to tell you. They do what they can. They keep it going, more and more powerful all the time, because the requirement is there. People are thinking of more and more complex computational jobs. They still have jobs that have to run for months. At that speed, can you think what it would be on a computer like the PC on your desktop? You're not talking about months, you're talking about decades, maybe even centuries, just not even thinkable. You just can't think about it.

They illustrate a lot of what they do there. That's one of their PR [public relations] strengths, from my point of view. I'm glad they do it, I'm really glad they do it. Yet, throughout the Center, there are computers, there are PCs, there are mini-computers, there are big computers—we don't even call them mainframes anymore; the whole concept of a mainframe has died, even though the main frame of the super computer is humungous. What they mean by "main frame" is if you have a requirement of a computer so large that you have to build a big cabinet of framework and fill it with electronic stuff—they also call it a chassis—this big box, then, is your main frame for all this stuff that has to go in it. Over in Building 258, they have so much stuff that some of it's over in Building 233-A. I'm sure they'd take over Building 233 entirely, if they could. They have the need for that much territory, that much real estate, if you will.

They recently had to start paying for their own power because they draw 6 megawatts when they're operating full-strength, and probably 2 or 3, even, when they're not operating full-strength. If you multiply it by 8,000 hours per year and you figure out how many trillions and trillions of kilowatt hours you're talking about, it's up there. It's way up there. Our utility folks said, "Sorry, we don't have this much money anymore. You're going to have to pay for it yourself."

BUGOS: Thanks.

JOHNSON: We appreciate you coming in today to talk to us, we really do, and sharing the information with us.

PEARSON: You're very welcome.

[End of interview]