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INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is August 5, 2014. This oral history session is being conducted with Chris Kraft in Houston, Texas, as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project, sponsored by NASA Headquarters History Office. Interviewer is Rebecca Wright, assisted by Sandra Johnson, and we thank you for agreeing to come and talk to us this afternoon.

KRAFT: My pleasure.

WRIGHT: We'd like to talk to you about your time at NACA, and actually, even before that, when you were still at VPI [Virginia Polytechnic Institute, (Virginia Tech), Blacksburg, Virginia]. I remember in your book that you talked about using and learning from those NACA reports during that time. Can you talk to us about that experience of what you learned from those reports, and actually, what you learned about NACA and why you thought that might be a place to work?

KRAFT: What I said there was an overstatement, so I have to back up a little bit. When you study aeronautical engineering in 1944, when I graduated, what we were studying was basic aerodynamics, basic physics, basic things that you would eventually use in aeronautical engineering. Our capabilities, our teachers, were not very versed in the problems of aeronautics of the time. There was a lot of reasons for that. A lot of the research was confidential, so they couldn't know it. NACA reports were confidential, so we couldn't know it. As an example, it was obvious that the use of wing sweep was going to be a major part of the future airplanes of that age, and all of that was confidential. The Germans were doing it. You could almost get as much data out of the German reports as you could NACA reports because if you had their reports, it was okay, but to have the NACA reports, you had to put them under lock and key.

Things like turbulence, the first NACA report was about turbulence, flying through turbulence, believe it or not. The early reports were basic. A lot of things on structures, a lot of things on wing shapes, on air flow shapes. There were a number of books that were published by NACA that were just hundreds and hundreds of cord shapes, of wing shapes—cusp and flares and all that kind of fancy stuff that you could do to change the basic stability of the wing as opposed to the lift of the wing. They were published, and those were almost catalogues, and they were fundamental to the designer of the times—the designers, remember now, being late 30s, early 40s. That's what I was referring to, really.

As I was trying to design a small, light airplane, which was one of my classes, I wanted to get what the wing shapes were, the effects of aspect ratio, things like that, which I knew little about. The types of airfoils that were the most efficient and give you the least drag, and would couple generally with a light airplane. That's what I was referring to. On the other hand, structures, as an example, was something that was coming on at that time, and we had a heck of a good airplane structures prof [professor], and I learned a lot from him, and so did NACA when I got there, because I had had that background.

The books were pamphlets, big, thick pamphlets put out by a couple of the aviation companies. They were very, very informative. That's where I got my early knowledge. I really have to say, I was not very smart. I didn't know how NACA went about doing their work. I didn't know how they went about publishing their information. I didn't know that their product was reports, basic writing. I'm pretty glad I didn't, as a matter of fact, because NACA reports were very dull. One of the first reports I wrote at NACA was on the [Republic] P-47 [Thunderbolt] airplane. It was a lousy airplane from a flying qualities point of view and, really, a control point of view, and I said that. In the beginning of my report, I wrote that down. The editorial office says, "You can't say that."

I said, "It's a classical example of what you should not do to design an airplane."

They said, "You can't say that. You can say what it is, but you can't say what you said." They were kind of dull, but fundamental.

All the fundamentals were catalogued by NACA. As you know, the cowling, as an example, was an invention of NACA, and it changed the look of modern airplanes in those days. It changed the drag, eventually, and increased the efficiency of the engine, as a matter of fact. NACA was very prominent in anything that aviation did. Later, after I got there, and airplanes started to go high subsonic, supersonic, those airplanes were actually designed in the NACA tunnels. The [Vought] F8U was a classic example. That airplane, which is the first Navy supersonic airplane, which I was in charge of for NACA in flight test, not as it came through the tunnels, but every nuance on that airplane was an NACA nuance. That was quite impressive. Go someplace else—talk about the [Bell] X-1, as an example.

WRIGHT: NACA wasn't your first choice to work.

KRAFT: No. I was going to work for Chance Vought [Corporation], and I don't know, can't tell you why. I was a young student out of college; I didn't know where I was going. I said, "NACA is right next door to me, I've been living here all my life, why don't you go someplace different?" I couldn't get hired. You read that in my book. They wouldn't hire me. I had two job offers. One was with NACA, one was with Chance Vought. I could have probably had more, if I wanted them, but I just took Chance Vought, and so I didn't even mess around with it.

I did take the one from NACA as a backup, and fortunately, that was a good thing to do. When I got to [NACA] Langley [Aeronautical Laboratory (now Langley Research Center), Hampton, Virginia], most of the people knew who I was, as a kid. Where I grew up, everybody knew, it was a small town business, and everybody knew everybody from everybody else. They knew my name, and so they were very kind to me. I remember when I went to see the personnel guy, he was very nice to me, and he said, "Well, I think you'd fit best in flight research." He said, "What are you interested in?"

I said, "Well, I'm not quite sure. I'm not sure I know what I want to do. I'm interested in stability and control and things like that."

He said, "Well, I think you'd be best fit in airplane flight test." I didn't know what that meant. He said, "We'll put you over in the Flight Research Division. They got a bunch of smart guys over there that'll teach you how to be an aeronautical engineer, and I think you'll make out well over there." He was absolutely right. That was a wise thing for me to go there to work because they were working on all the modern airplanes. They could work you on the P-47, the [North American] P-51 [Mustang], and the [Bell] P-39 [Airacobra]. They had those airplanes flying. It was in the old hangar—they built the new hangar while I was there for the Flight

Research Division. It had a Japanese [Mitsubishi A6M] Zero. It was a great place to learn, for me.

WRIGHT: I'm sure it's a great place for those pilots that got to fly all those different airplanes as well. You worked close with the pilots.

KRAFT: Yes. Back in those days, at NACA, test pilots were not very prevalent. NACA taught several of their aeronautical engineers to be test pilots. Jack [John P.] Reeder, who was one of the best test pilots NACA ever had, was just an aeronautical engineer in the full-scale tunnel, and they made him into a test pilot. Mel [Melvin N.] Gough was an aeronautical engineer. He had graduated from Johns Hopkins [University, Baltimore, Maryland], had a job at NACA and saw what was going on, resigned from NACA, went across the river, across Hampton Roads, to join the Navy to learn how to fly Navy airplanes, and came back to NACA as a test pilot, and eventually became their chief test pilot.

Those guys were not too unlike me. I was a young, wet-behind-the-ears aeronautical engineer and didn't know tiddlywinks about airplanes. I was a baseball player. I wasn't an engineer, and airplanes didn't interest me. I'd been there all my life, watched them fly over me and go to the air shows and so forth, but I never was interested in airplanes. That professor at Virginia Tech got me interested in airplanes.

It was a great place to learn. NACA was a really great place to learn, and I'd learned about flying qualities that [Robert R.] Gilruth had written [*Requirements for Satisfactory Flying Qualities of Airplanes*, NACA Technical Report 755]. I went to class at night, at Hampton High School, had classes at night in various elements of aeronautical engineering. [William] Hewitt

Phillips, who was my boss, taught flying qualities. I would go there two or three nights a week and learn what flying qualities was. I realized how great a man Gilruth was. He and Mel Gough had developed flying qualities, and that was the Bible for how to build an airplane from a standpoint of flying it, and it was the Bible of how to test an airplane.

The Army [Air Corps] and the Navy adopted that document as the basis upon which they were going to buy airplanes. That put me in the know on what all airplanes were. The significant fact that was a part of your teaching, and was knowledge, in NACA in those days, was everything was static. When you talked about the forces that operated on an airplane, you were talking about the static conditions. What were the balancing forces? You measured those forces in as static a condition as you possibly could. This was before anybody even thought about doing a dynamic analysis. You'd write the equations of motion for an airplane, it was just the static forces you were dealing with.

By a year or so after I got there, the biggest thing about the dynamics of an airplane was what is the time to damp to half-amplitude? You do a step input with the stick or with the rudder or with the ailerons and see what happened. You would see how it damped. In other words, if you pull back on the stick, if it damped in 1.5 cycles, that was the test. That was the only dynamic test.

Within a year, I was writing equations of motion in the airplane. It was all dynamics because it was obvious to NACA, particularly, that as you approached higher speeds, the dynamic aspects of the airplane were equally important as the static aspects of the airplane. All the wind tunnels, all the flight tests began to look at the flight dynamics, as opposed to balancing forces. Of course, that changed all the mathematic analysis, also.

When I was in college, I'd never heard of a Laplace transform. I took operational calculus, which is all I needed when I got to NACA because that's how you wrote the equations of motions of the airplane, but wasn't too long after that, that the dynamic parameters which you left out of the equations of motion had to be considered. They became as predominant a force on the airplane, naturally, as just doing it as a balance stick. The wind tunnels, everything started becoming interested in dynamics instead of balancing.

The other thing, however, was that, as I alluded to, it was obvious that in order to get to the problems that you were having at high subsonic speeds—as the Mach number began to increase, most airplanes at that time period, the high-speed airplane was about 550 miles an hour and Mach 7/10. If you increased the speed, increased the power, and the jet engine was not going to allow you to do that, get a lot more power and efficiency out of a jet engine than you could with props, and propellers were limited in their aerodynamic design as well, it was obvious that you were going to be flying at higher Mach numbers. What is the airplane going to have to do to deal with higher Mach numbers?

They began to look what the Germans were doing. The Germans were sweeping their wings. Immediately, NACA started sweeping wings and seeing what that did to the wind tunnel and how that changed the dynamics of the machine, how that changed the drag, how that allowed them to prolong, put off a swept wing. All you're doing is you take the angle of the sweep, and multiply that times the speed. You find that, well, the force is coming along the cord, and so that allowed you to design the airplane to fly at Mach 9/10 while the wing was going to 0.75. Just that simple aerodynamic feature. The Germans figured that out pretty quickly, I guess by rote, by trial and error, because I don't think they had the wind tunnel capacity NACA did.

As soon as we started mucking around with sweep, we found out that was a very prominent thing that was going to be in the new airplane. The other thing was that [Richard T.] Whitcomb—Whitcomb was an aerodynamicist of my age, maybe a little older, not much, he just died recently. I think it was the [McDonnell] F-101 [Voodoo] and the [Convair F-]102 [Delta Dagger], one was a delta wing and one was a straight wing, and they found that the doggone drag was a lot higher than they thought. They had drag interference between your fuselage and the wing, interference drag, they call it.

Whitcomb figured out that if he slimmed the fuselage down where the wing came in, that the drag went down. They didn't understand that, but it worked, and then he figured the math that went with it. That's how he became famous. At that time, those two airplanes would not go high subsonic, into the transonic range, and they put the Coke bottle shape in there, and lo and behold, they reduced the heck out of the drag and they were able to get a lot, almost 1/10 of a Mach number higher, speeds out of it. That's the kind of gimmicks we were getting to, to try to get close to the transonic range. We all knew that the one block that we were facing was, what did we call it? When you get to Mach 1?

WRIGHT: The sound barrier?

KRAFT: Sound barrier, right. Everybody said, "Well, we got the sound barrier right there," and all the math said that drag became infinite at that speed, and of course, it didn't. We knew that bullets would go through the speed of sound, but we didn't understand the math. We didn't understand the design. The wind tunnels that we had would not measure the forces in the throat of the tunnel because they got these shockwaves coming off of the wing, and that screwed up the flow. The flow became turbulent. If you know anything about aerodynamics, at any speed you're flying at, the air is not only worried about what's there, but it's sending out signals in front of it. It literally sends out a signal that I'm coming, and tells the molecules of air what to do. The closer you got to transonic and high supersonic speeds, the signal wasn't there anymore. It was just a flat shockwave coming off the flat plane of the wing.

That fact, you put a configuration in the wind tunnel and the shockwave would come off and the flow would be destroyed. All the measurements you would make on the wing or the tail, on the whole airplane, were in error because the flow was wrong. It wasn't what you were going to see when you actually got flying in free air. That's where John Stack became very famous at NACA. John Stack invented the throated tunnels. What he did was put a force outside the tunnel and suck that air off, suck the turbulence off, so that you could measure things at transonic, supersonic speeds.

We people in flight tests were trying to come up with, well, what can we do—Gilruth was thinking about those kinds of things—to measure what's happening to the airplane at transonic speeds? He came up with these two techniques that we were using, which is what I started working on, almost from the time I got to flight research. That is, we would take a high fineness ratio body and put a wing on it, and sweep the wing to various angles, put the wing at various locations before and after the max [maximum] diameter of the fuselage, take it up to 35,000 feet, drop it from a [Boeing] B-17 [Flying Fortress], initially, and we had a balance inside the fuselage to find the high fineness ratio body, and we put telemetry in there.

Nobody had ever used telemetry before. We tracked it with radar as we'd drop it, as it fell. That way, you got into free air to measure the aerodynamics, and with the radar, we could measure the velocity very accurately. We could do all kinds of configuration testing. We could express our measurements on the body, on the high fineness ratio bodies, and you could locate the wing at different positions back and forth. Lo and behold, there was the Coke bottle.

We found, hey, we put the wing behind the maximum diameter of the body, the drag decreased because it reduced the interference drag between the fuselage and the wing. That was a big invention of the time. Sweep, high fineness ratio bodies, and location of the wing relative to the fuselage. A lot of airplanes then started using that to get through transonic speeds. The theory said that what happens as you approach those Mach 1 is that you get separation of flow. If the air separates from the wing or the tail, either, it loses its effectiveness, and you don't know what that is. You can't measure it in the tunnel, and we can't predict it. We can't write the equations which predict what's going to happen. That promoted the thickness of the wings and the tail.

Up till that time, their initial thought was, "Well, you got to have thick wings because it's got to be really strong to withstand these forces that you're going to get;" high Q forces that you were going to get at high speeds, but that was wrong. What we needed was thin wings, so that makes sense, doesn't it? Thin wings are going to have less drag, and that's what happened.

We didn't know what kind of controllability we would have, or whether we could predict [the controllability] in order to design the stability and control of the airplanes. We didn't know what that was going to be. We're all searching for how to measure the forces that acted on the configuration you had at transonic speeds. Gilruth came up with body over a P-51 and the drop technique, and then he took the rocket, put the bottle on the end of the rocket, and then you could get to Mach 3 easily, by firing it on the end of a rocket.

Everybody was looking at those kinds of techniques. Then, that further exacerbated the stability and control problems, the dynamics problems, and that's when everybody started working on automatic stability and control. How can we provide stability and control when we know the configuration, but even if we don't know the configuration, we could measure what's happening and we could use that information to provide forces to correct the forces which are about to send you off somewhere.

Now, about 1951, '52, '55, that's where, in that period, automatic stability and control became prominent. You mix wing sweep, new ideas about drag, and artificial stability and control together, that's the modern airplane of the time. That developed at the NACA from '46 to spaceflight. Everything that we were doing at NACA was going in that direction. It was slow.

The other thing I've forgotten, did not talk about much, is the jet engine. With the jet engine, you'd get reduced drag, and so, therefore, how can we get high, efficient jet engines? That's the other thing that NACA began to get into, was how to reduce the fuel consumption of a jet engine. The first jet engine airplane was a Bell [XP-59A Airacomet], Bell Aircraft built one, a low-wing monoplane. I think it could only fly about 30 minutes, was highly inefficient. Everybody jumped on that, to try to improve the jet engine.

We had all these bi-flows and air flow changes and prop design inside the jet engine, the vanes inside a jet engine. That became also a requirement of the NACA, to improve the efficiency of the jet engine. All those things were coming, being driven by the jet engine; you could get to the speeds you wanted to get to, but you couldn't fly there. The F8U airplane would fly at supersonic speed for 6 minutes and run out of fuel, so you needed to improve the efficiency, needed to improve the drag, you needed to improve the stability and control, and you needed to invent automatic stability and control. Those were the design requirements that developed in the time period when I grew up in that age.

WRIGHT: Can you explain to us how all of you that were working on these projects exchanged information?

KRAFT: We had this exchange of information between the divisions, we had meetings, we had high-level management meetings, but the other thing we did was our product was reports. Everything we did, we had to write about, describe in detail how we got there, what the math was, what the measurements were, if we had any, and then write the results of it, the effects of it, in the English language. What we did then was if I wrote a report, then it was sent to the other organizations at NACA that were similar to what I was in, flight research and stability and control, and it went to the wind tunnel people, it went to the structures people, and they would appoint an engineer to be a reviewer of the report.

You would send them the report, they would read the report, they would comment on it, both technically and the writing, and improve not only what the writing was, but make sure it was technically correct. You would have these meetings and they would come in with their comments. You, the author, had to take those comments and justify what you had said, or correct it on the basis of what they said. Everybody was feeding off of that information, continuously. That's where I learned that the easiest thing in my life I ever had to do was criticize somebody else's work, which is true. You learn a lot that way, a heck of a lot.

WRIGHT: Were you working with Ames [Research Center, Moffett Field, California] and people with Lewis [Research Center, Cleveland, Ohio] as well?

KRAFT: We didn't have that close a relationship as I just described with the reports, but there was, yes, a similar transfer. Not as great because of the distances, but yes. We had an NACA conference, eventually twice a year, where the whole universe of the aviation industry would come to NACA. We'd have it one time at Ames, one time at Lewis, one time at Langley, rotating, and we had these vans that would take all of these people, and we'd be about maybe 500 people. We had to set up these stations around all of Langley, and we'd have an engineer who's responsible for making a presentation at every one of those stops.

They'd divide these people up into groups of 25 or 30, and you would have to make that spiel about 10 or 12 times a day. That would take about three days. We did that every year, with the NACA conferences. Start out in the morning, where they would go to the main auditorium and the head management and the head of the various divisions would make a presentation, probably the most significant of the presentations, to the gathered group, and then we'd split them all up and send them out in these little groups to review the work.

That, plus the reports—now, here comes all these NACA reports—the time scale on those was lousy. It took too long to get it through the editorial office and get it through this process, but eventually, and we had several classes of reports. If it was for the services, we had these preliminary reports that we would put out, which were not edited, that just presented the data, and sent them out. They sent us an airplane, and we would give them the results of those tests that we had done, various phases. We'd send them a report, which was undisciplined, I suppose, so they would have the data within several weeks after we had obtained that. The actual report itself took another six months. That was just a confidential report, and then we wrote technical notes. To get a technical note took probably four or five years to write because it was fancy graphics and fancy writing and highly centered and highly edited. The editorial office was as important as the engineering offices were. It was a good product.

WRIGHT: You had some support with math, as well? I think you've mentioned this before, it's your first dealings with the computer was during that time, but you also had all the math equations to work out.

KRAFT: My highest math was operational calculus when I was at Virginia Tech, but as we got into dynamic analysis, as we got into an analog computer, which you mentioned the REAC [Reeves Electronic Analog Computer], then that was a whole new world to get into. Because none of us knew that kind of math and none of us knew that kind of analysis technique. That was cumbersome and it took a long time to evaluate because if you write the equations of motions and you do all this math, you get one single point at one single point in time. What you want is a time history of what happened to the airplane for the next 25 seconds, as an example. To do that, you had to compute what the airplane was doing maybe, probably initially, every 10 seconds in time. Whereas if you had a computer, you got a continuous readout of it. We didn't have a computer—what we had was a Marchant and Friden machine. Have you ever seen one of those?

WRIGHT: I have seen one, yes.

KRAFT: You sit there and crank that damn machine. Took me three months to get one time history of an airplane response. If you pull back on the stick on a fighter airplane, it will go like this and damp out and get to 2 Gs [gravitational force]. In order to do that mathematically, it would take you maybe three months on a Friden calculator or a Marchant.

On a REAC, you could do it in real time, but you had to convert the forces in your equations of motion to electronic terms, as opposed to force terms. In other words, you had to interpret your equations of motion into an electronic circuit, which then would respond to the way your airplane would respond. Does that make sense? REAC was nothing but—it was a big panel, about that big [gestures], with hundreds of knobs on it. Those knobs would change the resistance, the capacitance, and what's the other term, in the electronic simulation of your equation of motion.

When I first started doing that, I didn't know what the hell I was doing in the first place, but I got an electrical engineer to help me construct this equation of motion into electronic terms, but then I need somebody to teach me how to use the various parameters that I had on that computer to simulate what that equation of motion was. You'd have to go away, and in order to show you the answer was right, you'd have to do it mechanically, first, do a time history of the response of the airplane, and then compare that to what you were putting into the REAC.

Of course, if you got to be an expert in that, after a while, that was the right way to do it. Fortunately, the card machines came along, digital machines came along instead of analog, and instead of recreation, electronic recreation. That's what NACA had bought, was card machines. Didn't take long to get rid of them because it was a monster—the whole damn base was covered with cards. These big banks of cards, do you ever remember that?

WRIGHT: I've seen those, yes.

KRAFT: Banks and banks of cards would have been describing this equations of motion of what you were dealing with. That was driving me nuts—I hated it because I couldn't understand it. When I wrote an equation on the airplane, using quadratic equations and eventually, operational calculus, that was pretty straightforward for me. When you got to Laplace transforms, that was not the case. Laplace transforms is a fancy mathematical technique—it's really a logarithm, like a logarithm, so you could describe the airplane's response, together with its automatic pilot—it had its own dynamics—and Laplace transforms allow you to do that.

MIT [Massachusetts Institute of Technology, Cambridge], they were the godhead of automatic stability and control, and they wrote these books. I remember the math book. I tried to teach myself that math, I'd work at it and work at it. My boss, Hewitt Phillips, was a genius, and I'd work for weeks trying to figure out the derivation of my work. I'd do it and then I'd take it in and show it, discuss it with him. He'd look at it for 5 or 10 minutes, and he'd say, "Why didn't you do it this way?" It used to really piss me off. He was that smart. He knew how to do that damn high math. He had graduated from MIT, it's where he got his teaching.

Bob [Robert G.] Chilton, I don't know if you know that name or not, but he came into our stability control branch about a year and a half, two years after I got there. He taught us all how to do it. He came directly from MIT. They called it the Instrumentation Lab [Laboratory] back then. Now, it's called the Draper Lab. That's the reason we hired Draper to do all of our analysis, eventually, as we got to spaceflight.

WRIGHT: I know that the NACA did a lot of evaluation of the aircraft that was being built by contractors, and at times, I'm sure the NACA found some issues that they had to discuss with the contractors.

KRAFT: Eventually, we got the third airplane of any new airplane out of the military. It was sent to NACA, the third airplane built, for flight tests. The aircraft company themselves were doing the initial flight test, then the Army was doing the flight test, and the Navy, and then NACA was doing similar flight tests. We got together with their flight test people, with their flight test engineers with their test pilots. They would come to see us or we would go to see them. They were at various places.

The Navy had a flight test center on the Patuxent River, called [Naval Air Station] Patuxent [River]. We would do these flying qualities tests, various maneuvers that you do to measure the airplane response, and we put instruments inside the airplanes, and got the answer on film. It was a mechanical process, light-driven sensors connected to a potentiometer, as an example, to tell you where the drill surface was, or the stick is. Then, we had devices for measuring rates and accelerations. We'd measure all that and put it all together. That's where the girls came in to play. When I first got there, I was doing it myself. You would actually take the film and a plastic scale and measure the deflection of the trace, and from that, you could tell what the numbers were. This is where the elevator is, this is where the stick is, this is what the rate is, as you do this maneuver. NACA was very efficient at that, very good at that, and very accurate at that.

That's where I spent half my time, calibrating instruments. If you got a vane or the position of the surface, I go there and have to put it here and see what the film said. I'd calibrate the potentiometer that was on the surface, just a rotating thing, electronic signal going to my film box and film measuring, tell me what the deflection was. We knew what the calibration of the measuring device was, and then we'd calibrate the surface itself, or whatever it was. It appears to be that that's pretty tedious, but it wasn't tedious at the time because that's the only way we could do it. Now, you wouldn't do it that way, but nevertheless, that's the way we did it.

You can do it by all kinds of crazy ways, now. Electronic devices can allow you to do that. The tunnels had certain sets of instruments and manometers to measure the pressures and velocities and flow. They had their own set of instruments. They were highly accurate, highly expensive, and very difficult to calibrate, but that's what they did. Then, of course, they began to get computers. We called them "computers," but very pioneer devices, that eventually, they had high-speed information also, but it took a long time to get there. That was what NACA was doing when the space program came along.

WRIGHT: Sputnik [Russian satellite launch] changed a lot, didn't it?

KRAFT: Yes, it did.

WRIGHT: You want to talk about that? Your thoughts when you first heard about the satellite?

KRAFT: I remember I was in Washington [DC], at an automatic stability and control meeting, and we read about it in the paper, I think. Talked about it at the meeting. A couple of weeks later, I was up there when Vanguard [rocket] flew. I remember being in a taxi, and the taxi driver said, "Hear what happened this morning?"

I said, "No."

"Had a rocket blow up down there at Cape Canaveral [Florida]." That was the Vanguard. Came up off the bed and exploded. That was the country's first attempt to put up a satellite. It immediately got everybody thinking, well, in the first place, about orbital mechanics. I didn't know a damn thing about orbital mechanics. Immediately got people thinking about high-altitude pressures and temperatures and ways of measuring the environment and ways of putting instruments into zero gravity in a perfect vacuum. Of course, people immediately—I wouldn't say immediately, probably close to it—started thinking about, well, can man survive in that environment? Can he survive at zero gravity? How do you keep him alive? That's a whole new ballgame.

Our high-speed airplanes that were going to 50-60,000 feet were using g-suits and pressure suits already, but not space suits. They had means of supporting the body from an acceleration point of view, and namely means of providing oxygen at the upper altitude, upper levels of where they were flying, up to 60-70,000 feet, but they didn't have a space suit per se. It initially started out being g-suits as compared to space suits. The initial test pilots sucked on a tube to get oxygen before they had masks. Back when I first went to work, if you flew at 35-40,000 feet, you did have a mask, but you also had a backup tube of oxygen. All the early test pilots used a tube in their mouth, but that progressed pretty rapidly, too.

The Army Air Corp was in the lead at Akron, Ohio, where they had a test facility. All that kind of equipment was being developed there, in consultation with the flight test people, where I was. That was a whole industry in itself. We began to think about what the problems were of stability and control, of reentry, of thermal protection that we hadn't ever thought about before, and Gilruth and several of his engineers started thinking about how to put a man into space.

I remember that Chuck [Charles W.] Mathews—whom I worked with very closely in different things, and eventually he was in charge of what I was doing—four or five of them that started thinking about putting a man in space. The Air Force was thinking about it also; they called theirs the MIS Program, Man in Space Program. I was not in that group.

Chuck Mathews and Max [Maxime A.] Faget and Caldwell [C.] Johnson and Aleck [C.] Bond were all working for Gilruth in the Pilotless Aircraft Research Division, and he got a bunch of those guys to start working and thinking. Mathews was a very smart engineer, and so, Gilruth got him to come work on it. I knew that they were working on something about space, but I didn't know any of the details until he asked me to go to work on it. As you discerned here, I was not very happy with being a mathematician, as compared to being an aeronautical engineer, or eventually, an aerospace engineer. That was a welcome request. I'd get the hell out of that math. I was good at it, but I didn't enjoy it. I did not look forward to doing that the rest of my life.

As soon as I went to work there, I had at least 10 things to do every hour. Different requirements that dealt with tracking or communications or geography or control, and how was the body affected by it, how do we get information back, and how do we do that in real time? What could you do? That was where my thoughts immediately went.

WRIGHT: Were there significant changes with the transition from NACA to NASA that affected your job during that time period after Sputnik?

KRAFT: No. The changes were different in that our relationship with the aerospace industry changed, yes, but I don't think that in terms of internal to NACA and the organizational aspects of NASA; NASA was still the NACA. They had just had additional duties called "Space."

Now, it didn't take long for us to be different because our jobs were so different. Now, we were responsible for the hardware like the Air Force and the Navy was responsible for their hardware. That was a different relationship for us, and that may have been one of the reasons that Gilruth wanted me to go to work in that area, because I did have that kind of experience, where I worked with the airplane contractors pretty closely in the flight test of airplanes, and I was pretty good at it. I was pretty rough with them, pretty straightforward with them. I don't know whether he knew that or not, to tell you the truth.

As in any organization, back in those days, they sort of protected themselves. They were always trying to protect their ass, so to speak, and it was my job to ferret that out. That particularly came true on the F8U airplane, where I found out a hell of a lot of wrong things with that airplane. Sometimes it took a lot of doing. I had the airplane grounded, and they didn't like that. Grounding an airplane was the last thing anybody ever wanted to do because it took it out of the hands of the operators.

It was a whole new relationship then developed between the NASA government and the aerospace industry. The Air Force and the Navy did not have the technical competence that NASA had. The NASA engineer was a very different animal than the people in the services that were telling the aerospace industry contractors what to do and what they wanted, what the requirements were, and how they measured the contractor's performance. It was a very different relationship. We knew technically as much about what they were doing as they did, and in some instances, more. Some instances, less, but our background, having been in the aerospace industry ourselves, was pretty good.

That was a new relationship that had to develop as we wrote the RFPs [requests for proposal], evaluated the RFPs, and then contracted for it and then built it. Building it and testing it and proving its systems evaluations, etc., that was a whole new set of engineering circumstances that we at NACA did not have. We had to learn how to do that. We had to develop a working relationship with the industry, which I thought, it was difficult, but after we got it going, it was extremely profitable, from a hardware performance point of view, I thought. I'm right—I think that was unusual, but it was beneficial to both sides, both the contractors and ourselves, and I think we probably saved lives, the truth of the matter is. That created a new set of engineers, which are called flight controllers.

WRIGHT: You never had a hesitancy when Bob Gilruth asked you to come to the Space Task Group? Did you think twice?

KRAFT: I thought about it for at least a couple of hours. I didn't know what I was looking for, but I knew it was a new opportunity, and I knew that I would be given an opportunity to lead, or to be a leader in that field, and I guess that's what I wanted. I'm not sure I knew I wanted it at that time, but I liked doing that when I was a kid, so I guess that's where that came from. We needed some leaders, and we got a lot of good ones. Got a lot of bad ones, too, but they get sorted out, after a while.

WRIGHT: Do you feel like a lot of, or some of, the lessons that you learned, or some of the experiences that you had at NACA followed you on through?

KRAFT: Hell, yes. I knew what an instrument was, and I knew what a good instrument was, and I knew how you measured the response of an instrument to know what you were measuring on the thing you were trying to measure. I know that's a lot of nonsense I just said, but I knew how to do that, and I learned that at the NACA. I learned that from a guy named Piggy [Alfred E.] Alexander, who was an instrument mechanic, but I learned how to do all that. I saw when people started talking about the response of an instrument, or the responses and the inputs that had to the design of a system, they came up against a guy like me that knew what the hell he was talking about. That was unusual.

The Air Force guy that had that responsibility in the past to them didn't have that capability. He didn't have any technical background. The NASA engineer had a heck of a background of various kinds that he had accumulated as a pure engineer. It was hard on McDonnell Douglas when they first started working with us, but after a while, we became a pretty good team.

WRIGHT: When I've read about people working within the NACA, what I always found very interesting, and I think very—"nurturing" is not the right word, but it's the best one I can come up with the moment—was that the exchange of knowledge you got was not just from your bosses; it was pilots, it was machinists, it was, as you mentioned, the technician. Everyone very much collaborated in sharing that information to a large degree.

KRAFT: Very much so. When I designed a model of the X-1 and put it in the shop to be built, I designed it on the drawing board. I put the device in it to make it work, to get the data I wanted. I designed that and then I took it to the drawing board and designed that, and then I took it to the machine shop, and they built it. I was the guy that interfaced with the machinist, the chief machinist, not the guy on the lathe, but the guy that was their boss, making that piece of hardware. You get that in the industry, too. If you were building a spacecraft at North American and you were a North American engineer, you interfaced with the shops. I had that experience as an NACA engineer. Then I had to flight test it. I had to think about what to do to test it, and then I had to do it, and then I had to write about it. I was sort of cradle to grave about the operation of some piece of equipment or some flying piece of hardware. We had a lot of experience. I don't think we were any smarter than anybody else—we just had the experience, and we knew what questions to ask. After a while, we started figuring out a lot more about it.

A mission rule, as an example; we would run these simulations and we'd come up against some problem that a system had, and we'd say to ourselves, "Well, we better write that down. If this thing happens in space, then we better write it down, and we also better write down what the hell are we going to do about it." If this breaks, if this thing deviates, if this becomes a problem, what are we going to do about it? Why do we want to do that? We want to remember it, in the first place, and the second place, we want everybody else to know that's what we're going to do.

You started sending this stuff up to the management, and they'd get this stuff—"What the hell is this?" Eventually, they wanted to know, what are the mission rules, what are you going to do? If the fuel cell fails, what the hell are you going to do? As I looked at that myself, I said, "Hell, I want everybody to know what I'm doing. If I'm going to bring this thing down, if I'm going to bring it out of the air, if I'm going to abort it, I want them to know this is what I'm going to do if it happens."

We started writing all that stuff down, and that led you to asking more questions. Here's an instrument measuring the flow of oxygen out of this tank, and you look at the answer, and it doesn't look normal. It looks abnormal. Why? Is it something wrong with the tank, or is it something wrong with the instrument? If it's not real, it's as bad as the damn thing failing. If you want to know that that instrument is telling you the truth, and if it isn't telling you the truth, why isn't it telling the truth, and if it is telling you the truth, what is happening in the system that's causing it to give you that answer?

That was a very different perspective. Apollo 13 is a classic example of that. A guy sitting at the console at Cape Canaveral, he looks at this damn thing and he doesn't have a measurement. He says to himself, "The instrument must be broken." It wasn't broken. It was off-scale high. He didn't realize that; he thought it was the instrument. Had he realized that the problem was in the instrument or the system, then he would have taken the right action. That's what we wanted to prevent in real time.

It turns out every time we had a problem, it's like the first time we launched, the rocket down on the pad, and all the plugs had pulled out, and first thing you know, the spacecraft is interpreting what it sees on its instruments. It's wrong, but it acts. The first thing you know, you got a rocket going out here and a parachute coming out here. You say, "Well, now, why did that happen?" We didn't have a permissive circuit in there that said, "Well, you've gone through 20,000 feet," and now you can arm that circuit, but until you go through 20,000 feet, you don't want to arm that circuit because you don't want those things going off when they shouldn't be going off. Those are the kind of things you just don't think about. You don't think about that when you design it. I think all kinds of things paraphrase that.

As on Skylab, they had these solar panels that were inside a cover on the side of the rocket, where the damn thing is ascending. First thing you know, the goddamn cover blows off and so does the solar cell. You immediately say, "Why the hell did that happen?" You start looking into it, and you didn't vent the goddamn thing. As you went up, all this pressure builds up inside the cover, gets up to a certain altitude, and it damn blows the covers off. They didn't think about that. The mirror on the Hubble [Space] Telescope, they didn't do that well. They didn't ask the right questions. They didn't make the right tests. We only learned that by experience.

WRIGHT: You remembered a whole lot more than you thought you were going to.

KRAFT: I didn't get into NACA very well for you.

WRIGHT: No, you did. Are there other thoughts?

KRAFT: NACA was a very unique place, and it was a different relationship among people that I don't think anybody understood that was inside of NACA. The local people didn't understand the NACA people. They wondered, "What kind of nuts are those? They want to know things about this washing machine nobody ever asked before," when they were downtown buying things. They say—I'm not sure this ever happened—they'd see these guys walking around the street and they looked like they were in a different world, thinking about what they were doing at work. I never saw that for myself, but they'd accuse us of being brain-busters that way.

WRIGHT: I understand some people, when houses would go up, they would watch the unique way of building it because engineers were building houses to be very efficient.

KRAFT: I built my own house in Virginia—I was a contractor. I hired the bricklayers and I went to a handbook to figure out how to build a chimney, the fireplace, so I wanted to make sure it was the right kind of draft there, sucked flow instead of coming in the house. I designed the way in which the walls of the fireplace were to be built—this way, that way, how that got into the flue. I hired the bricklayers to do that. I gave them the drawing because I did it myself, and I walked in there and they'd finished it. They'd done it like they'd always done it. They didn't do it like I put on that piece of paper. They said, "What's wrong?"

I said, "You didn't do it like I told you to do it."

"Well, this is the way we build fireplaces all the time."

I said, "Well, it's not the way you're going to build mine—tear the damn thing out." I didn't see those bricklayers for about a month. That's an NACA engineer, right there.

WRIGHT: You were a close-knit family on the Center itself because you all knew each other so well. Your families did things together as well? Did you have activities and things that you did?

KRAFT: Not so much, no. I don't think that we were that close. Me being a local kid, I didn't have that problem, but now there were a lot of NACA engineers had come from other places, and so, they did become very close. They lived together in apartments and things like that, the engineers did, and then they built houses close to each other, and they became very close friends, yes. That's very true. I wasn't one of those. I did get to know a lot of guys that way, but I was fortunate enough to be a local rat. NACA people were known to be very strange people. They liked them, though, because it was very important to the economy in Hampton, Virginia.

WRIGHT: I'm sure many became community leaders as well, getting involved with the local towns and doing some things within towns.

KRAFT: Yes, of course. School boards and things like that. We changed this place [Clear Lake, area] when we came.

WRIGHT: Yes, you did, definitely.

KRAFT: I'll tell you that. The education system improved 100 percent the day we walked in here.

WRIGHT: There's this local story of how at that time, the group of old men that always sat together at the bank and talked were trying to figure out just how many years NASA was going to be here before it left, and then they were going to have to deal with all these problems, all the buildings. It's kind of funny now because people thought five years—until you got to the Moon, and then everybody would just disappear.

KRAFT: Unfortunately, that's beginning to come to roost, isn't it?

WRIGHT: So many years later. We're glad you're a—what did you call yourself, a local rat?

KRAFT: Yes.

WRIGHT: I'm glad you consider yourself a local person now in Houston. We appreciate you coming in and talking with us today.

[End of interview]