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ORAL HISTORY TRANSCRIPT

JOHN V. BECKER
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is May 3rd, 2008. This oral history is being conducted with John Becker as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project sponsored by the NASA Headquarters History Office. Interview is being held in Hampton, Virginia, during the NACA Reunion Number XII. Interviewer is Rebecca Wright, assisted by Sandra Johnson. Thanks again. I know you're taking time away from your reunion activities to talk to us.

BECKER: There's not much left actually. There's dinner tonight. I've seen everybody, I think, and said most everything. I saw four of my old secretaries, and I recognized all four of them. One of them has stayed very slender, but the other two were considerably more obese than they used to be. It was fun seeing them.

WRIGHT: Well, that's good. They recognized you too, I bet.

BECKER: Oh, yes. I'm 94 years old. So what you see is what you get.

WRIGHT: I think that's impressive by itself. Well, it was quite a while ago when you first started at NACA. How did your interest in aeronautics start?

BECKER: I guess with the sight of [my] first biplane. It was in 1919, and it landed on a horse-racing track, at Chatham, New York. They pulled it off the track, and it was sitting there on the grass, and wind was blowing, and the wings were moving. It looked like it was ready to go. Ever since then I was hooked.

WRIGHT: That's when you were about six years old. How did you learn more about airplane development as you grew older?

BECKER: For the next ten years, I got into model airplane building. My folks bought me one. It was a wind-up thing. It had no possibility of stable flight. I didn't realize it at the time. It would either climb too much or descend too much. You couldn't make it [stable]. But eventually I got some books, and William B. Stout, the guy that designed the Ford Tri-Motor, was big in the model airplane work, and he wrote some really good articles. So I got busy with that. Then when I was in high school, I was fascinated with the possibility of hang gliding.

At that time [most] hang gliders [were] biplanes, and there were pictures of them that you could find in the literature. In front of our house—we faced a huge pasture that had a long sloping [field]—just about the right slope. I can picture myself in the hang glider going down that slope. I remember there was a magazine article in *Popular Aviation*, I think it was, and it was called *The Machine Volant of the Correas*. That's a French word that means—volant means [flying and] you fly by yourself, I think that's what it means. It showed this guy going up to the top of a long hill and then floating down just right, and it was like my hill. So I decided I was going to try a hang glider.

About that time I got a very light case of scarlet fever. You could hardly see anything. But my hands started to peel, and the doctor said that was a sure sign, and they quarantined me for 30 days in a spare bedroom of the house. It was on the first floor. It was about as big as this room—well, not quite as long. It was long enough so I could make a wing 14 feet long. So I made two of them and then we hooked them together. That made 28 feet. But the problem with it was when I picked it up, it was all I could do to pick it up. I didn't realize how heavy it was going to be. So I made a skid underneath it, and I asked my dad if he would pull me in the family car.

We lived in a rural area. There were big hayfields, and we had some [newly-mowed] fields with about a quarter of a mile length, and he had no idea that the thing would fly. But it was basically a hang glider configuration on a skid, and the post on the skid where I could attach a towline was made from a cue from a pool table. It was hardwood, nice and slick. I should have wrapped the towline around that so I could release it. But the end of the towline had a loop in it, which I'd been used to pulling around. When I got it on that post, there was no way for me to readily release it. I hadn't figured what I was going to do.

On the first attempt to fly there was very little headwind. It didn't get off. We turned around and went back to the other end of the field. The wind had come up just enough as I wrapped my line around the post. But it had this loop on the end, which I held with my hand, and sure enough [we] got about 300 feet or so down the [field]—and up I went. I was so surprised—it was the first time I'd ever left the ground—that I let go of the line. It pulled loose, but the loop caught on the post, and the [glider] jerked and kept going on up. I had a control stick that had only up or down, only elevator. I pushed it down. My father looked back in his

mirror, and all he could see was the line going up. I was up about ten, 12 feet. So he stopped the car abruptly. But I had good flying speed, and I glided on up. Landed right up beside the car.

[I] remember he got out and he said, "By George!" And then he said, "But I don't want it to go any higher." He said, "Promise me that you'll keep it within safe height," which was like about three or four feet in his mind. So we made two or three other flights. But I could see that I needed all three controls [instead of just elevator]. So I decided to take it home and build a more conventional [glider]. Using the same fabric and the same wing structure, I built a conventional primary type glider, and I taught myself to fly with it.

When I got down to college at New York University [NYU], they had a newly formed gliding club. They had raised enough money to buy a professionally-built glider from the west coast. To my surprise, I was the only one that had ever had any [gliding] experience. So I got to make the maiden flight with this new glider. We [had] put it all together. I had a problem, because in my glider I had adjusted the [control] wires to the rudder [so the rudder bar was like that] on a sled. In other words, to make a left turn I pushed the rudder [bar counter-clockwise], whereas you probably know on a real airplane if you want to make a left turn you push your left foot. It's just the opposite from what I had. So I had to unlearn the direction that I pushed the rudder [bar], and I unlearned it on the ground before we took off.

[For launching] the club had 300 feet of shock cord, which is about a one-inch diameter rubber [rope]. It's made of hundreds of little rubber strands. You get two or three guys on each side to pull it out, and you keep your glider anchored to a post with somebody behind holding it. When they get the rubber all stretched out, then you say let go, and the guy behind lets go. In three or four seconds you're up in the air flying. That was the first time I'd ever been launched that way. But I got up about 15 feet, and as luck would have it I made a perfect landing. The

other guys in the club came running up. "You've got to teach us how to do that." Which I did, actually. We never had any bad accidents. We had one guy that went too far out over Newark Bay where we were flying one year, and he turned around and he couldn't quite get back, and he landed in the water. But we were able to take care of him. [The] glider had a lot of flotation [in the wing].

WRIGHT: Did you choose New York University because of any type of aeronautics program that was there?

BECKER: Yes. They had one of the three aeronautics possibilities. Georgia Tech [Georgia Institute of Technology, Atlanta, Georgia] [also had one of the] Guggenheim aeronautical schools. [The Massachusetts Institute of Technology, Cambridge Massachusetts (MIT) was also considered. NYU was the least costly for me.] Alexander Klemin was the head of the aeronautics department. He was British. He was educated in London. I don't know how he got to come to this country. But he started in Wright Field, did some design work. He had a good background in aeronautical theory of the time.

WRIGHT: Why did you choose NACA and Langley?

BECKER: I graduated in the middle of the Depression, and I was the only guy in the class [who] had any [aeronautical] job possibilities. The first one was at Grumman [Aircraft Engineering Corporation]. Grumman was a little one-horse company at the time. They offered me a job in the engineering department. But the way they did, they started you in the shop, so that you'd

learn something about how airplanes are actually put together. I thought they liked me because I was small enough to get inside the fuselage with a flatiron and buck rivets. In other words, when they were making the fuselage, they would put the rivets in from the outside, and they had rivet guns that would bang on these rivets, but they needed somebody on the inside to back up the rivet. I figured that was one reason why they liked me. It didn't sound too appetizing. I probably would have lost my hearing.

I fortunately had another job offer from the Naval Aircraft Factory in Philadelphia [Pennsylvania]. That was a design job. The head of the group was a woman by the name of French, and she was the daughter of the guy who had written my engineering drawing book, French's Engineering Drawing. So it was fun to work with her for a while. But we had assignments like changing the ripcord handle on parachutes. The Navy didn't like what they had. It was a lot of fussy little design things.

After about a month of that, I got an offer from the NACA. They were hiring [a few] new engineers [in the summer of 1936]. The only place they had was Langley. Langley Aeronautical Laboratory [Hampton, Virginia] they called it. They got two guys from the east coast and two [exceptional] guys from the west coast, [H. Julian Allen and Francis Rogallo. Rog became noted for his research on delta wings with flexible or “floppy” fabric surfaces. This] work eventually led to the hang gliders that we use today. Somebody else contributed the [structural] idea of three [tubes], which could be lined up and hinged at the nose, [forming the leading edges and keel], and you could wrap that up and put it on top of your car. It made it structurally really [simple and practical].

You have to wait for just the right weather down in Nags Head, [which is just south of Kitty Hawk, North Carolina]. You soar on the upflow over the dune, [but] there's only a [small

region] where you have enough upflow to actually [soar]. The Wright Brothers found that. Orville went back in 1909, and he had an improved biplane, and he had a couple of days when the wind was just right, and he got up and he set the world record at that time for soaring, which was nine minutes and forty seconds. There are pictures of him doing that. That record stood until 1925 or 6 when the Germans started fooling with sailplanes.

Well, anyway, I was able to [hang-glide again in the ‘70s], to conclude this part of the story. I decided when I retired to do something foolish so I wouldn't age so fast, so I took up hang gliding. I bought a hang glider, and Rog had one just like it. We had several learning sessions down at Nags Head, and two years later in '78 my wife and I were down there one day when it was perfect. There was about an 18-knot wind coming in, which was just right. I had one flight of about four minutes. This is [gliding along] the dune, then you had to turn fairly quickly, come back, doing like that. I've got some pictures of that.

WRIGHT: Was that a good time?

BECKER: Yes, that was '78—that was from '28—that was 50 years after my first [flights]. It got to be a little too much for me. You had to carry the thing. The thing weighed 80 pounds, and you had all your gear to carry. Slogging up through the sand was a real effort. I sold it in '83.

WRIGHT: I'm glad you had some good landings with it.

BECKER: Oh, the landings were always easy for me, because in my early flights—they were fairly short—I was always landing. You get up about 20 feet, and then I would let the rope go

and glide down. I would practice turns. My brother was two years younger. He wasn't interested in flying, but he was interested in driving the tow car. He could do that legally out on the hayfield.

WRIGHT: Sounded like a good partnership.

BECKER: Except he never contributed anything to the flight part of it. In the end we had an accident that I always felt was partly his fault. But it really wasn't. We had a slight crosswind, and the glider was just about ready to take off. It would bounce up and come down. With a crosswind you'd come down with a slight drift. It came down with a pretty good drift, and it jerked me sideways so that I fell out of my seat. My safety belt was only a converted skate strap. The strap broke, and I fell out. He kept on going with the car. I figured he should have seen that and stopped. But anyway, he kept going. The glider went up [without me] to about 75 feet, I guess. It looked way up there. Finally he had to stop the car at the end of the field, and it fell off and stalled, and came down on one wing. It just telescoped, it was a complete wreck.

WRIGHT: What were your parents thinking about your thoughts about flying?

BECKER: My father helped me right in the beginning. Most of the [later] flying I did was during workdays [while he was at work]. My grandmother was visiting one day that we had a good fly, and she took some pictures. Those are the only pictures I have of the glider or the flights.

WRIGHT: That's pretty exciting.

BECKER: Yes. My father was very tolerant of everything I wanted to do. I used to use his watch to time my model airplane flights, and I lost it. One flight went fairly far down the flats, and in chasing after it, I lost his watch, and was never able to find it. But he accepted that without getting too mad about it.

WRIGHT: It was a pretty good move from New York to Virginia when you moved to Langley. Had you been down to this part before?

BECKER: No, no. I went down on the train and boat. The streetcar turned around at the Chamberlin Hotel at Old Point. I asked the driver if he would let me off at the Langley Hotel. There was one hotel in [Hampton]. There were no motels. It was an old broken-down thing. It was terrible. I was depressed. But the next morning I took another streetcar out to Langley [Field] and checked in, and I mentioned that in the book. [*The High-Speed Frontier: Case Histories of Four NACA Programs, 1920-1950*]

WRIGHT: I remember you mentioned that, and started to work on the eight-foot-high tunnel, right?

BECKER: I had written my thesis on a towing basin in New York University. I was interested in flying boats and the aerodynamics and aquadynamics of hulls. Langley had a beautiful towing basin. It was almost [half] a mile long. In New York University our basin was only—I think it was 75 feet long. So you had to do everything [very quickly]. You had to get the carriage up to

high speed and take your data and then slow it down. It was almost impossible. It was too short. So I looked forward to working in the towing basin, which was managed by Starr Truscott. He's one of the old Langley pioneers. But Mr. [Elton W.] Miller put me in the high-speed tunnel, which was the best thing that ever happened.

WRIGHT: Could you spend a few minutes and talk to us about that tunnel, working in that tunnel, and then how the experiences that you got from those days moved you on to the next ones?

BECKER: At the end of our aerodynamics teaching in NYU, I remember there was one lecture where Klemin tried to look at the future, and he had taken data from Dr. [Hugh L.] Dryden's paper of 1926. Dryden and [Lyman J.] Briggs. That very crudely showed what might happen to lift drag, and moments at speeds up near the speed of sound. That was the first picture I'd had of any of that. That was the only background I had. I had to learn everything from actually doing it in the eight-foot tunnel.

WRIGHT: A few years after you got here you were moved to be in charge of the 16-foot wind tunnel.

BECKER: Yes, my first boss was Russel [G.] Robinson. He [later became] Chief of Aerodynamics out at Ames [Research Center, Moffett Field, California]. He was very meticulous. It was helpful to have a guy like him right at the beginning. He was very critical of how you wrote reports. He died a couple years ago. But then our second boss was exactly the

opposite. John Stack. He was by comparison really wild. I tell a little bit about him [in the book]. His son has come to these events here.

WRIGHT: You worked in the wind tunnel area for a good piece of your career with Langley. Can you share with us about some of the changes that you helped make with those wind tunnels?

BECKER: The problem with our eight-foot tunnel, my first place, was that it didn't have quite enough power. You could get into the region where the airfoils start to get into trouble, and if you tried to go further you'd get trouble with choking, because when the flow was choked in the tunnel, all the results were completely invalid. So you never knew how far you could go and still get valid data. The drag [curves] that we always measured on wings would be starting to get vertical. But we didn't know where we could cut it off and say, "That's no good out there," and, "It's okay down here." It didn't have power enough to really become transonic.

So one of Stack's first promotions was to give the eight-foot tunnel enough power to reach Mach 1 [speed of sound], full choking. Then he pictured that once we had that much power, we could fiddle with different ventilated test sections that let us eventually get valid data in that speed range. So a big part of what we did was we tested models up about as far as we thought we could get valid data. We had the only big facility that could do that, the complete models. We tested a model for Howard Hughes. Did I mention that?

WRIGHT: You did. 1939, I think.

BECKER: The nice thing about that model, the interesting thing, was it was not an exact model of what he had in mind building, because he was afraid that we would learn all about that, and we would all have access to his data, and we would profit by that, and we could tell other people about it. So he designed a model which would teach him what he wanted to know, but that didn't look like his final airplane. The airplane was one that he crashed in and [was] almost killed, that was what the final airplane was.

I don't know whether I told about it in there, but the guys from the Hughes plant, of course had met Hughes and knew how he worked. Virginius [E.] Clark—he knew Hughes very well. Said Hughes would call him up in the middle of the night with an idea and make him come over. He would be in bed with one of his starlets and she would be asleep, and they would talk in whispers so not to wake her up. They were talking about aerodynamics and all.

WRIGHT: I think you left that out of the book. That's a good story. I'm glad you shared it with us. What about the different types of materials that you used during the years that you were at Langley in the wind tunnels? Did those change quite a bit?

BECKER: The materials?

WRIGHT: The materials, like what the blades were made from or the actual—how you did the tunnels. I guess the first ones, you started out, you built the tunnels—they were wooden?

BECKER: No, I didn't really have much [concern about materials]; our models were [usually] made out of solid [metal], and we didn't worry about the structural problem. Another one of our

big projects was a four-foot propeller [investigation]. The four-foot propeller testing machine was called a dynamometer. We made the first tests at really high forward speeds with the propeller with this diameter. This was one of Stack's pet projects. The propeller was a prime problem for airplanes because it got into shockwave problems before any other part of the airplane because it was whirling so fast. You could whirl them so fast on the ground not moving forward at all and get sonic speeds on the tip. You could tell when you got it because it would start a very funny roaring noise. The performance parameters of the propeller would get very poor [at high speeds]. So that was an interesting project. Then the [eight]-foot tunnel was pretty well adapted for that.

But then the thing that really saved everybody was the jet engines came along. So we quit thinking about propellers. But we first tried to get tests [of complete models] as close to the speed of sound as we could get and still get valid data. We did it by using small models. Models that wouldn't choke the tunnel until you got up to about 0.95 Mach. You could get valid data up to Mach 0.9. There's a picture of it in here. [referring to book] One of the schemes we had was that center plate. The center plate was essentially—every wing that flies, right in the middle of the wing the flow is two-dimensional, and it's not affected by the end conditions of the wing. So by putting a center plate in there [to support the wing] and using a small wing of about [3-] foot [span], we could get [valid data] up to 0.96 Mach number. There's some data I showed in there. [referring to book] That's the best you could do in a fixed-wall tunnel.

We had our friend Ray H. Wright, who was a really good mathematician, and he devised slots that we first tested out in the small model of the eight-foot tunnel. We did that over in 16-foot. With a small model tunnel and using the 16-foot tunnel as a suction device, when you ran the tunnel up to high speeds, the pressure in the test section drops down. We could get a drop in

pressure equivalent to 500 miles an hour or so. But by putting a diffuser on the end of this little test section, we could make the pressure drop still more. So what we would do is the big tunnel would be running at full speed with nothing in it, and we tapped off the low-pressure air, put it on the end of this diffuser, and then that way we could get pressures [low] enough for 1.2 Mach. So it was an ideal place to check out—well, it wasn't ideal. It was a very inefficient way to get the flow. But we proved that the slotted tunnel would work there. That's all in here. [refers to book]

As soon as I showed Stack what we were doing—we weren't sure that the flow with the slots was really what we wanted. But he immediately, he decided [to assure] it was. So we went ahead with the slots in the eight-foot tunnel. Then later we added the same slots in the 16-foot. It worked. We got good checks with the flight data from the X-1. There's a plot in here that shows. I haven't looked at this in so long. It's in here somewhere [referring to book]. Shows the X-1 exactly the same as our [small] wind tunnel.

WRIGHT: The work that you were doing at Langley—and then there was work being done in other places—how did you exchange information, or did you exchange information? I know that you had publications, and I know that you had engineering conferences. Is that where most of the information was exchanged?

BECKER: Yes, there was a lot of competition between Langley and Ames. Not so much with Lewis [NACA Lewis Flight Propulsion Laboratory, currently Glenn Research Center, Cleveland, Ohio], because they were on the engines. But we had helpful competition. They would try to

outdo us. The conferences were the showdowns where we were all sitting there together, and we could criticize each other.

WRIGHT: Were those held annually, or were they more often than once a year?

BECKER: No, they were at least once a year. More often twice a year, I think.

WRIGHT: Were they held at different locations?

BECKER: Yes. First Langley and then Ames.

WRIGHT: So you got an opportunity to travel out to the west coast.

BECKER: Yes. The first time I went out there was 1948. Then after that I went quite frequently. I went in '49, and after that it was fairly frequent.

WRIGHT: Did you find their facilities to be like Langley's?

BECKER: We knew all about the facilities pretty much. They were all written up. But I started the hypersonic work down at Langley, and the other guys—I don't know whether they'd even thought about it or not [in 1945]. But in 1949 Stack was invited to make the after-dinner talk up in the University of Virginia [Charlottesville, Virginia] where the American Physical Society was having its annual meeting. They had got wind—I guess maybe Dryden had told them, I

don't know—that we were experimenting with hypersonic flow. So they invited Stack to give an after-dinner talk. He turned it over to me, because he had very little to do with the hypersonic stuff. So I went up there and made the talk. Dryden introduced me. By that time there was a small effort that had been started at Ames and another one at Lewis. So Dryden was able to say truthfully, "We have hypersonic work going on at all three Centers." But that irked me a little bit, because we had started the stuff. But anyway, I got to make the talk.

We had one very critical finding that we displayed for the whole country for the first time. One of the questions about the hypersonic tunnel was that when we got the air going that fast, the pressure was very low, and the temperature dropped low enough to condense the oxygen in the air and liquefy the oxygen. We [had] realized right away, that was going to be a problem. In fact, [Arthur] Kantrowitz told me that he thought there would be a limit to the useful speed of wind tunnels because of this condensation. What we decided, "Well, we'll build the tunnel and we'll find out if condensation really happens." The theorists felt that there's such a short time of passage—the air went from normal temperature down to liquefaction, then it immediately came back out—that it wouldn't happen. They [thought they] could prove analytically that it wouldn't happen.

One of the first questions Dryden got before I actually started my talk was—some guy raised his hand, said, "Is there any consensus on whether liquefaction will really occur?" and Dryden said, "I don't know whether there's a consensus, but we're going to discuss that matter in this talk." I was able to show conclusively that it did condense. By heating the initial air enough, you could keep it above the condensation point. It was a perfect theoretical calculation, when the temperature got down to the normal liquefaction point, you got a fog in the test section.

So all the hypersonic tunnels from then on all had to have heaters in order to avoid the condensation. We were not going to put a heater in initially, because we thought maybe we'd find out that we didn't need it. It was Russ Robinson who asked me what we were going to do, how we were going to handle that problem. He said, "Why don't you put in a heater?" I said, "Well we've looked at that, and they're very expensive. It would add another $80,000 or so." He said, "Well, I think you ought to do it. You can deal with the whole problem." It was good advice.

WRIGHT: Did limited budgets have an impact on what you did?

BECKER: Oh, yes. A lot of what we needed to do in those days didn't cost much by present-day standards. That's why the Administrator [Michael D. Griffin] yesterday told us that he doesn't decide what should be done, he just executes. Somebody else, Congress or [President George W.] Bush, Bush's so-called vision that has a man landing on Mars [Vision for Space Exploration], that's what dictates what we do now. Whereas in those days everything started ground up. Guys who were actually doing it would promote what they needed to do. It was an ideal situation. You can't do it anymore, because what you need to do is so damn expensive that it takes a Congressional act.

WRIGHT: Was the process if you had an idea you took it to your supervisor, and then got the okay to begin working on it? Is that how it worked?

BECKER: Yes, that's how it worked. I was supposed to go through my assistant director, who was Stack, and I became a division chief in 1947 and took over the High-Speed Division. Stack never turned down anything that we wanted to do, either me or my other guys. So it was really dependent on the individual [researchers]. Occasionally the advisory committees, they would run into trouble designing their airplanes, and they would tell us what their trouble was, and sometimes we would act on their suggestions. That was really a minor way we did it.

WRIGHT: You mentioned a project that you worked on for Howard Hughes. Were there other companies or other types of those projects that you were involved with?

BECKER: Yes, there's work we did for the companies, but it was never that kind of hush secret that Hughes had. In most cases we could tell what we had learned from tests [for] somebody for like say [Ling-Temco-] Vought or whoever. We could talk about that with other companies. They would always tell us if they didn't want us to do that. "This is company secrecy; I hope you won't tell anybody." But they never made a big issue about it. A lot of times we had many [researchers] who got their ideas—I shouldn't say many, but some of them—from the company people who would come in. Then later they would forget where they got the idea, and they would be working on a project that really had arisen from the company suggestion. That was unfortunate, because usually these were ordinary performers that didn't deserve to be promoted, say, but they would do something that looked pretty good, but then you'd find that they got the whole idea from one of the company people.

WRIGHT: I know you were there before the Second World War started, or before the United States got involved. Were there a lot of changes to your research as far as new employees coming in? Were there more people coming into NACA during that time?

BECKER: Oh, yes, there was a huge increase. Basically what we did was take everybody. Then on the job you would find some of them that were so miserable that you had to get rid of them. But we took everybody and then filtered out the impossible cases. We had one guy from Louisiana. Walt [Walter C.] Williams' school [Louisiana State University, Baton Rouge, Louisiana]. All he wanted to do was operate the boat that plied between Langley and Wallops. He applied. He said, "I would like to captain this boat." He was a junior engineer.

Well, probably you didn't want to know this, but Walt Williams did not make a good impression when he first came in. He was in the Flight Division. They debated whether or not they were going to keep him. But they finally decided to keep him, and he began to show some talents. Floyd [L.] Thompson was his boss. They finally decided to keep him. But I remember it was discussed in one of the [promotion] committees. Shows how wrong you can be.

WRIGHT: He was out at the Flight Research Center in California. Did you have an opportunity to go to the other Centers?

BECKER: Yes, if you needed to.

WRIGHT: But as far as working? Or you preferred to stay at Langley?

BECKER: I used to go out to Ames every now and then.

WRIGHT: At one point you had an opportunity to represent the Center at a meeting where the X-1 was first being discussed. Can you share with us about that meeting and your impressions of what you were learning?

BECKER: Well, Stack had gone to Europe. He'd made his first European trip, and he was a member of the High-Speed Panel. He normally would have gone. But because he was in Europe, I went. I substituted for him. I mentioned that in the [book].

WRIGHT: You did. I thought it was an interesting meeting.

BECKER: That was where [Melvin N.] Gough made his unfortunate remark about never letting NASA pilots fly in rockets. I think he wished he'd never [said] that. But that happens all the time. One other thing [that] happened. He and another Langley pilot were about to take off from National Airport, it's now named after [President Ronald] Reagan. They got down to the end of the runway to take off—and maybe you've heard this—but they were going back to Langley, and Mel told the copilot—he claims he said, "Lower the flap 20 degrees." For takeoff they sometimes use a little flap. It was the Lockheed Electra, which is a ten-passenger twin-engine. According to Mel, the copilot, instead of lowering the flap, pulled the handle that retracted the landing gear. The landing gear came up, and the airplane squashed down on the runway, broke the propellers. Mel thoroughly, totally blamed it on the copilot not doing what he said. The

copilot had a slightly different story—was that the instruction that he got was not all that clear, and he without thinking pulled up the landing gear.

WRIGHT: Goodness, that's pretty memorable. Tell us how you got involved working on the X-15.

BECKER: That's pretty well covered in that and the X-15 literature. The way I really see what was important about that—if we hadn't started our hypersonic program back in 1946, we wouldn't have had the background to propose the pre-X-15 thing that we did. There probably wouldn't have been an X-15. When we started the hypersonic business, there was no demand whatever for it. It was just a matter of it would be fun—we felt it would be fun to get out, far out, and [explore] what happens. If we hadn't done that, I think our proposal for the next airplane would have been similar to Ames. It would have been a conventional fighter-type thing able to go to Mach 2 perhaps.

WRIGHT: In 1958, NACA transitioned into NASA. When did you first hear those discussions? How did you feel the transition was going to impact your work?

BECKER: NASA did quite a bit of thinking about space before that. All the Centers did. We could see the handwriting on the wall. We'd have to change some way. There was that last meeting up in Washington with Jimmy Doolittle. He was the chairman of the committee. They had [invited] young guys from all the Centers. He didn't want the old—he didn't want Henry [J.E.] Reid and Floyd Thompson and those kinds of [older] guys. He just had young guys. I was

one of them from Langley. We discussed the whole thing. But it was inconclusive what was going to happen. I don't know what Doolittle's input to [President Dwight D.] Eisenhower was, but the decision to make it into NASA was—I think the big contribution the old NACA people made was to change the C to an S. Adolf Busemann was working with me at the time down at Langley, and he said, "That means they substituted [Abe] Silverstein for [John W.] Crowley [Jr.]" Silverstein did come from Lewis down to Headquarters. [John F.] Victory was very proud of the fact that it retained all the same letters except that one.

WRIGHT: Just the one. Did you have any interest in working directly with the space program? Did you want to be a part of possibly the STG [Space Task Group]?

BECKER: Oh, yes. The way it happened, the X-15 got going before the space business started. We were busy with the X-15, working with North American. We were actually out at Ames during the Sputnik [first artificial satellite, launched by the Soviet Union in 1957]. We went out at night and actually saw the thing. So that expedited everything. My division, with its high-speed aerodynamics work, was a natural to look at reentry. We very soon did reentry. I wrote an article for the *Scientific American* that was their first discussion of reentry in January 1961. It's the lead article. It's still valid, a very good discussion, I think, of reentry. It showed what we were thinking at the time. There's a progression in there of possible configurations. Some of them begin to look pretty much like [the Space] Shuttle. They were winged configurations. Our interest very quickly shifted from the Mach 6-7 area all the way on up to reentry speeds.

The focus began to be the Dyna-Soar. If I'd had to choose between one or the other, I guess I would have said, "Let's go all out with Dyna-Soar [X-20]." But the X-15 was well along,

and it lost a lot of glamour because of the space program. Here was a device that would only go to Mach 6, when we were really thinking very hard about how to get the first astronauts back in a better way than the Russians did. Max [Maxime A.] Faget, he worked on my committee for the X-15. He was the propulsion specialist, and he very quickly shifted to the Space Task Group. But he and I were very close all along.

He asked me what I thought when they first thought of bringing the Shuttle in at 45 degrees angle of attack so that all the [critical] heating would be on the bottom of the wing. That's what we had been advocating for Dyna-Soar. The big difference was that the X-15 was strictly pilot-oriented. All the previous research airplanes had been flown by pilots, and we relied totally on the pilots to do the research and get the vehicle back down. The X-15 was the same way. In fact, the capabilities of the pilot was part of the goal of the X-15 testing. We didn't realize that these automatic systems, the computers, you could put a program in there, and you wouldn't need the man at all except for very special maneuvers like the retro firings and—well, the pilots are monitors, they watch what's going on. They listen to Houston [Mission Control Center, Houston, Texas]. But we didn't visualize that at all, automatic spaceflight the way [reentry] is today. It really needs to be that way, [for example] because the pilot can't really sense how much retro-rocket he needs to apply. Somebody on the ground has to figure that all out and then tell him about it. So there was a big change in philosophy between the X-15 and the spaceflight.

WRIGHT: Did you continue working on any projects at all when Max left your committee and moved over to the STG? Did you have a chance to continue communicating your research?

BECKER: The Space Task Group pushed the idea of a [small] very blunt [capsule] for obvious reasons. That's the smallest, lightest thing you can put together. That's what was needed for the [Mercury-] Atlas. To make a winged vehicle at that time was really beyond the capability of the boosters that we had. It had to be a minimal capsule for the Atlas. But at one of the last aeronautical conferences, we compared Max's capsule with a slightly lifting [capsule]—[Alfred J.] Eggers, he took the capsule idea and just cut it in half, and the half that would be used had some lift. Then we went all the way [in my paper] to [high] lift with a winged configuration, which was the Dyna-Soar configuration basically. The original Dyna-Soar configurations were a lot different than the final ones. The final ones were just the way Langley advocated they should be.

WRIGHT: You retired in 1974. What made you make that decision at that time?

BECKER: The biggest pusher that I felt was I had an excepted position that was great. It paid you more than the civil service. But it was not automatically corrected for inflation. You remember in the '70s, the inflation got to be terrific. Got to be double-digit. If I'd stayed there, I would have kept on getting my [fixed] excepted position pay, whereas the guys below me who started out with general service pay, they very quickly with 10% [added] every year would be going [beyond me]. So it was the financial pressure as much as anything. I also had a feeling that I could do some consulting work. I didn't try to organize any, but it fell in my lap, and I made more money in the next eight years than I'd made in all the previous [37].

WRIGHT: Goodness. What were some of the jobs that you took for consulting? Were they with companies?

BECKER: The biggest one was for GASL, General Applied Sciences Lab, that's Tony Ferri's [company]. Tony Ferri was a very good friend of mine. He died in '75, I believe. I worked for them. I got them interested in this idea of a flight test of a scramjet. They're well equipped to do that. They made a large contribution to this most recent—two years ago they got up to Mach 10 with a scramjet in flight. That was a Langley-designed thing with really almost 100% contractor inputs. So my contract with them was that I would get 5% of any government work that they got, which I kept getting up to '83. Their contract was open-ended, and I kept getting this money without doing much of anything. They would call me two or three times a year. So I finally quit, I quit doing it. By that time I had qualified for Social Security and Medicare and all that you don't get from civil service. So that was a big help in my later life. But I worked for a year after I retired at essentially the same [NASA] job. I think it was called consulting. They changed the title. Then it was taken over by Roy [V.] Harris and then Bob [Robert] Jones and then Bob [Robert L.] Trimpi.

WRIGHT: Mr. Becker, what do you think was the most challenging aspect of your career when you worked for NACA?

BECKER: I never actually thought of that before, but from my experience, the way to get ahead in the kind of research we were involved with—don't do the drudgy day-to-day stuff, but let somebody else do that, and get out in front. That's the way Allen and Eggers did with their

reentry stuff. That's the way we did with our hypersonic. So in other words, work out on the lunatic fringe.

WRIGHT: I like that saying, that's a good one. Is there anything else that you'd like to add today as you think back on those years with NACA and with NASA?

BECKER: The old aeronautics guys like—Roy Harris is one of them, and there are hundreds of them all around—tend to have the attitude you're spending these billions on space, give us a few hundred million for aeronautics and we'll make new advances like we did in the past, like we'll develop slotted tunnels and swept wings and all that. But I personally think that that era is pretty much exhausted. The companies also don't need us like they used to. When we had the eight-foot tunnel, we had the only capacity for testing models with controls and propellers and so on. But now most of the companies have their high-speed tunnels. They have smart guys working for them. So it's changed. Maybe it's the era of presidents like [George W.] Bush having a vision of the future so-called, and Congress passes enough money to fund it, and the laboratories all have to do what they're told.

WRIGHT: We'll have to look and see, won't we? Thank you for talking with us today.

[End of interview]