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INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is July 17, 2014. This oral history session is being conducted with Kenneth Mort at NASA's Ames Research Center in Moffett Field, California, as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project, sponsored by NASA Headquarters History Office. Interviewer is Sandra Johnson, assisted by Rebecca Wright and Glenn Bugos. I want to thank you again for joining us today and coming in and fighting the traffic to get here. We really appreciate it. I want to start off by asking you about your background and your education, and how you first heard about the NACA and decided to come work here.

MORT: It's not very exciting. I came here in July, 1957, and I'd worked the prior summer. I was going to Stanford [University, California], and I got a bachelor's degree, and then I came here for a summer, went back to Stanford, got a master's degree, and then came here permanently in July of '57. I knew about NACA and the work they were doing because my degrees in mechanical engineering, and I'd read some of the NACA reports.

I hate to tell you, I really came here to avoid the draft, but then I liked it so much, it was fortuitous, it was really a great place to work. I had friends that were going to work for airplane companies and they were getting paid a lot more, but the work they were doing was pretty dull. I was glad that I had avoided the draft by coming to Ames. It was a lot of fun. It was one of those things that I think I was really fortunate to be able to come to work for NACA. NASA was okay for a while.

JOHNSON: Were you recruited at Stanford?

MORT: No. They were in a hiring mode, though, and I came in with probably on the order of 50 kids that had graduated, and were distributed all about the Center, so they were ramping up the staff.

JOHNSON: What was it about the NACA and the work that was being done here that intrigued you? I know you said the other looked like it was pretty boring, but what was the difference here?

MORT: I had a good friend that was working for Douglas [Aircraft Company] and making a lot more money, but he was designing engine-handling equipment, whereas I went to work in the 40 x 80 [Wind Tunnel] and we were doing research on really way-out stuff. It was a lot of fun. There was a big push earlier, probably in maybe early '50s, for vertical takeoff, so the 40 x 80 was heavily involved in vertical takeoff [research]. We tested a lot of interesting aircraft. Some by backyard inventors, and some by airplane companies, and a big variety of vehicles. That was really a great experience.

JOHNSON: You got the feeling when you first started that you knew you were going to be working on different things?

MORT: After about six months, I did, yes. I didn't really know what was happening [at first]. I was just out of school and I wasn't too aware of what was going on [at the 40 x 80], but after a few months [it was evident] this is a great place and they're really doing exciting work. I could contribute to that, so I was happy to be here.

JOHNSON: Let's talk about those first days when you first got here, maybe a little bit about the way the lab looked and where you went, where you first worked, and if they provided any training? Maybe who your mentors were at that point?

MORT: They did have some training, and it was by world-class people. R.T. [Robert Thomas] Jones, do you know who R.T. Jones is? He was one of the lecturers. John [R.] Spreiter was, at the time, the foremost expert in transonic airflow. Morrie [Morris W.] Rubesin, [Clarence A.] Syvertson, Sy Syvertson, [and many others]. They would give us a lecture and we had to take notes, and we'd give them our notes and they would give us a grade. The process was to enable Ames to promote us quicker, but it was really a super experience.

Most of the time, young kids, like I was, worked in a group or two or three or four. We had a mentor, and we took instructions, "Do this, do that, and collect the data, and run the tests." It was a mentoring thing for probably the first few years. It wasn't formal. Several years later, they did have formal classes on writing. Engineers tend to be poor writers, so they had a clear writing course and a report writing course, I think. They were very good—much better than what you get in school.

That was kind of the training. Ames was—and this is overstated, but they tend to do their own thing. Smith [J.] DeFrance was the [Center] Director when I hired in, and this is oversimplification, but [NACA] Headquarters essentially gave Smitty a pot of money, "Do what you want to do" kind of thing. He managed things fairly closely for a few years. If you had run a test to collect the data, you didn't just sit down and publish it and write a report. You wrote a memorandum to the Director, and it went to Smitty and it got reviewed. If it was for an airplane company, then it would go to the airplane company, but it wouldn't leave the Center without that memorandum to the Director.

The secretaries would type these things out, and that was the days before Xerox machines, so they'd have half a dozen carbon copies of these memorandums to the Director. So, there were a lot of them. It would get approval, and that's how the data went out from Ames. The data collection was primitive. In all the [NACA] wind tunnels at that time, I think all the NACA wind tunnels, [at least] the large tunnels used a Toledo Scale [Company] system, which was a mechanical system. The airplanes or models were mounted on struts, and there was a big balance platform [under the floor], and this was supported by levers. It was all mechanical.

There'd be a big Toledo Scale [head], like you used to see in the grocery store, and a dial. There'd be a paper tape that would be printed out at the scales; there was seven of them. [The scales] would print out the data and then we would collect these paper tapes at the end of a run, and then take the paper tapes to a room full of about half a dozen women that would read them. It was typically young women, and we called them "computers." You've probably heard this before. They would average the data and do the calculations that the engineers wanted. The engineers would then get the data and plot it up. The computers rarely did plotting, so that was the way it was, early on, when I hired in.

We had really world-class [Technical Services] shops in those days. Quite often, if the boss said, “We ought to test so-and-so, do a sketch and go over to the shop,” that’s what you’d do. It would essentially be a back-of-the-envelope design [that you would take] over to [one of] the machine shops. We had a machine shop, structural fab [fabrication], sheet metal, model shop, instrumentation lab [laboratory]. Quite often, the research engineers were responsible for the research from cradle to grave. From an idea, you would be responsible for the design of the model, and then the construction and installation, and then eventually, you’d write a report about it. It was a good process and a great learning experience for young engineers, so you knew what the whole process was. You knew pretty well why you were doing what you were doing.

We would have probably, I think every other week, division chief meetings—meetings of the branch for the division chief, Harry [J.] Goett; he was really a cool guy. We’d all get together and Harry would come down, and he’d have a list of all the projects. He would go down the whole list. That was a good way to know what the branch was doing; for new engineers, [it was] especially good.

[Charles W.] Bill Harper was the branch chief [when I was hired], and then Woody [Woodrow L.] Cook, I think, and then Mark [W.] Kelly. In those days, our branch, the 40 x 80, pretty much—this is overstated—did what they wanted. They would propose something and talk to Smitty and say, “We ought to really do this,” and Smitty would say okay, if it was a good idea. I know Bill [Charles W.] Harper became division chief, and Mark Kelly was the branch chief for a lot of the time [I was there]. Somebody’d call up Mark Kelly and say, “Hey, you guys ought to do such-and-such, do you think it’s a good idea?”

If Mark Kelly thought it was a good idea, he said, “I’ll look into it,” and so, he would talk to Smitty and if Smitty said okay, then we’d do it. That was pretty well the way the branch ran for a long time, which was pretty good. Those were the early days, and it continued in the early days of NASA, the same way. I didn't see a big change until the Apollo Program started up. The 40 x 80 did a lot of parachute work. At one stage, they were going to do a land landing of the Apollo, so we tested all kinds of parachutes.

JOHNSON: If you want to go back to those first days and maybe some of the projects, if you remember, that you were working on at that time, anything that comes to mind?

MORT: Writing this history [referring to pages], I'm reminding myself.

JOHNSON: That's true—you should have it right there.

MORT: The very first project—in fact, I worked on it the summer before I hired in permanently. I worked with a guy named Bill [William T.] Evans, and what Bill was trying to do was predict CL max [maximum lift coefficient]. What that means is that every airfoil, when you increase the angle of attack, [eventually] it stalls. Just prior to stall, there's a maximum lift coefficient that it achieves. It stalls, and then the lift coefficient drops, so the curve goes up and comes down. The key thing is when does that point occur? I tell you, nobody's really done that yet. Predicting the CL max is still a [challenge].

That was a project. It was theoretical, and then the result of the theory was to build a model and see if we could really test it by [optimizing] the geometry. It correlated the geometry of the airfoil and CL max. People had done that just on a geometric basis, but we were using a theory to predict the pressure distribution on the airfoil, and to use that as a correlation with geometry, and then predict the CL max. That was the very first project I worked on, so I remember that.

The next stuff I did was related to vertical takeoff. Ducted fans, I tested several ducted fans. I worked with Paul [F.] Yaggy then, probably until he shifted over to the Army. I've forgotten when that was. I have it written down somewhere. He was the lead of the group I was in, so while I was in Paul's group, we tested ducted fans, parachutes, and propellers. One of the tests we did was a paraglider for the Gemini [Program], and if it worked for the Gemini, then they were going to apply it to the Apollo. It had inflatable booms, and again, it was for a land landing. North American [Aviation Company] had built this model—or prototype, almost—with inflatable booms [attached to] a capsule. We tested that in the 40 x 80 and did quite a bit of testing on it. It turned out it was kind of a turkey.

Some of the testing was good, and some of it [wasn't, and sometimes] you stopped the project. One of the backyard inventors that I got to meet several times was a guy named Harlan [D.] Fowler, who invented the Fowler Flap. [Nearly] every commercial airplane [uses] the Fowler Flap. He invented this thing in the 20s or 30s or something like that. It was really early on—it was when airplanes were still covered with fabric. Harlan Fowler invented this thing, the Fowler Flap. He was a funny old coot. He would come around with different ideas, to try and promote testing.

One of the times, he came around and [it started us] thinking of an idea of Harlan's, and so we built a model and tested it for him. It was kind of a generic test that was useful. That was fun. He was really hard of hearing—he wore a hearing aid, and he was really old. He'd sit down to meet with the branch chief, and he'd always start the meetings off with a bad dirty joke. You just knew it was coming. One of the times, he even brought his wife because he didn't drive. I was only with NACA for one year, so most of my stories are in the early days of NASA.

JOHNSON: Let's talk about that transition. You came in July of '57, and then October, of course, Sputnik [Russian satellite launch] happened and things changed. Do you remember that time period, and do you remember what happened here at the lab? You said you were working on things that were more theoretical and they would think of something to work on, or somebody would come up with an idea, and then things kind of shifted after that, as you said, you were working on some things for Gemini?

MORT: The transition, as far as the 40 x 80 foot wind tunnel [was concerned], was slow. It wasn't a dramatic change. We didn't [suddenly change shift after Sputnik]. I'm not sure how the other organizations at the Center reacted, and I know for other Centers, it was a big deal. For the 40 x 80, we continued pretty much the same procedures and process. We did get a lot of Apollo money, so we were flush. It was mostly related to recovery—the parachutes, primarily, and the paraglider related to capsule recovery. Then, later on, we did a lot of work on the lifting bodies. This was prior to the [Space] Shuttle.

JOHNSON: You said a lot of your stories, of course, were after those first few months, so was there anything about working on those capsule recovery projects? Any stories you want to share?

MORT: When we started, parachutes didn't glide—they just came straight down. The first step was to take a regular parachute and then cut a vent in it, and see if we could get it to glide. We cut two vents and had a flap to get it to glide. They weren't very good. L/D [lift to drag ratio] was only about 0.5. L/D of 0 is when it comes straight down. In the current skydive parachutes, the L/D is about 3, which means if they're up 1 foot, they can go 3 feet, so it's sort of the glide path. You can think of it that way. If I say L/D, you can think of it as the glide path. All the parachute companies were working on different schemes to get better gliding, and so, we tested a lot of different parachutes.

I think one of the things that we learned early on was that some of these gliding parachutes were actually kind of dangerous—that if you tried to glide with them, the leading edge would collapse and the thing would collapse, and it was hard to recover. One of the things we did was to define where that collapse was and, I think, scare off people trying to do it that way. You don't want to try these schemes. Eventually, we did develop pretty good parachutes, but it was way past Apollo. It was too late for any of the space recovery. The current skydive parachutes are very good. Their L/D is, like I said, close to 3. Currently, the Air Force is considering a major recovery system using scaled-up parachutes. The tunnel is being used for [testing] bigger gliding parachutes.

JOHNSON: How do you test a parachute in the wind tunnel? I'm trying to think about it physically.

MORT: You have a single strut and then, it's coming out behind with all the lines, and at the top of the strut, you have a thing that's got spools and the cables, and you can control it remotely, the lines, using this control-head, and you measure the forces [on the strut]. Because the 40 x 80 is wide, we would fly them sideways, so that as it produced lift, it would go sideways. That was kind of the process.

Some of the times, we had tried having a probe so that we could check stability by forcing it off the trim position. We'd do that sometimes. It was pretty simple. Other times, we would test them. There's a picture—it used to be in the head shed [Administration Building]—of the control head down close to the floor, and then we would fly it vertically. If we thought weight was a factor, if the thing was heavy, then we'd fly it in the normal way, but the control head would be close to the floor, so we gained as much height as we could. In fact, we tested the paraglider, the one with the inflatable booms, that way because it was heavy. It would distort the shape too much to fly it horizontally, so we flew it vertically. It still was a turkey.

I was heavily involved with lifting bodies, and that was a lot of fun. That was pre-Shuttle. In effect, it defined what you could land un-powered—what you could land as a glider. They had poor aerodynamic characteristics, high drag and not enough lift, but they were flyable. The L/D on the lifting bodies was 3-4, so they were not much better than parachutes. It was a really interesting program. All of the ones that flew at Dryden [Flight Research Center, (now Armstrong Flight Research Center) Edwards, California] were tested in the 40 x 80.

JOHNSON: Did you work a lot with universities in the area, or in the tunnel?

MORT: On-again, off-again. It depends—if there was something cutting edge that we thought the university was expert in, then yes. I was trying to think of examples. I personally did work with Stanford. [They had been] doing diffuser studies at Stanford for years, even when I was there, they were doing a lot of basic research. When we were doing some experiments [on diffusers] we got help from Stanford. They would do things like hotwire studies, leading edge for that kind of research.

Nowadays, some of the universities are dreaming up airplane configurations, and so proposing models to be built, and then they'll have a NASA cooperative program, and maybe the military, to build a model. The funding is so low now in aeronautics that NASA is always looking for multiple partners, sponsors. There's been several configurations proposed by universities, and they've done some cooperative work. Some of the airplane companies, too, have chipped in. That's kind of the current approach. It's a lot different than it used to be.

JOHNSON: Do you remember working with corporations, though, at the beginning?

MORT: Yes. That would happen for vertical takeoff [aircraft]. The Department of Defense [DoD]—mostly the Army, early on—would send out an RFP, request for a proposal, to the airplane companies, "We're interested in an airplane that'll do such-and-so," so they'd get proposals from the airplane companies, and then the Army or DoD would pick one, and then they'd pursue it. The company would build the aircraft and then we would [usually] test it. We [would always recommend] testing the prototype [before flight]. Some airplane companies tend to prefer building [prototypes] and flying it and not testing it in the tunnel.

There's politics involved, and there still are. They would build this thing, and then if the DoD or the Army was smart about it, or the Air Force, they would say, "Make part of the contract [a requirement to] test in the 40 x 80." We'd do the tests and then fix it, do some improvements, then they would fly a prototype. Sometimes, they would rush in to build the prototype, fly it, and crash, and then we'd test it. We did all the sequences. Sometimes, they would fly it and it'd fly great, and then they would say, "This is really cool—we ought to explore doing such and such," so we would put it in the tunnel and do some extensive [tests]; we would change the model or the aircraft and do further experiments on it. There was a big variation in the process.

JOHNSON: What about classified work? Did you have any involvement?

MORT: I have a funny story about that, I'll tell you. Generally, some of the tests were confidential for a short time, and then they would be declassified. When the tunnel was first operational, they were doing airplane tests on prototypes [that were] classified, and generally, it was confidential. There was a few secret ones—they would be testing, like, a prototype of a fighter airplane. Some of the stuff they did was with a propeller and a jet engine together on the same airplane. They would put that in the tunnel, and that would be classified. You [need] a lot of thrust to get to high-altitudes. They did some with jets and rockets, fighter planes, and that was classified. It was confidential, typically. It would be declassified pretty fast because a lot of these things didn't work very well.

My favorite story about classification is we tested a flying saucer in the 40 x 80—and I've forgotten when this was. This was in the days of [a lot of] VTOL [vertical take-off and landing] testing. Everybody had a VTOL they were proposing. The local papers said, "Ames to test a flying saucer," and it was built by a Canadian company.

BUGOS: Avro [Canada].

MORT: Avro, yes, Avro. It was the [VZ-9] Avrocar, yes. You remember this story—it's a good story. They built a couple of these things, and they had pictures of it at their plant in Canada, and the pictures were terrible. All you can see is a couple of round things on the ground. They brought the thing to Ames. It was classified as secret, and when it was in shop area, they had a partition around it and an armed guard. You couldn't look at it. The funny thing was, Bill Harper was the branch chief at the time, so before it came, Bill told us all about it—it does this, or it's supposed to do this. It was a clever idea. It had a fan and engines and it had a jet, and then it was supposed to hover and then translate. The thing was really terrible. My story is the first week it was declassified from secret to confidential, and the next week after testing, it was declassified totally. It was not a good machine.

We did put a tail on it—it was unstable in just about every direction, but primarily in the pitch direction. As it started to translate, you couldn't [control it, and it was unstable]. It didn't have enough control power for control, so we put a tail on it to take care of that. It would probably translate in ground effect, but it was unstable. When it was in ground effect, the nozzle was around the periphery, and as the thing lifted up, then you'd get focusing, the air would come into the center, and that was an unstable process. So there was a problem getting very high. You [could] stay in ground effect, but that's not very practical. We had done small-scale experiments in the 7 x 10 [Wind Tunnel] of a flying saucer, and it's not a good shape for aerodynamics.

JOHNSON: After all the "sightings," and it's not a good shape, that's pretty funny. I was looking at some of the things on your résumé, and this may have been later, but you managed a team responsible for aeroacoustics, and you accomplished a 50 percent increase in 40 x 80 Wind Tunnel airspeed? When was that?

MORT: I've forgotten the [exact] dates.

JOHNSON: Yes, I don't see a date.

MORT: In the 70s. The 40 x 80 was the world's largest wind tunnel, and there were shortcomings doing vertical takeoff [and landing] testing because we were getting wall effects from the jets, and then we couldn't get data if the model was very big. We needed a bigger wind tunnel, and that's part one. Part two is that with helicopters, the goal was higher speeds. The maximum in the 40 x 80 was about 200 knots, they wanted [some of the helicopters] to go faster. The three OAST [Office of Aeronautics and Space Technology] Centers did studies on building a new wind tunnel, [NASA] Langley [Research Center, Hampton, Virginia], Lewis [Research Center (now Glenn Research Center), Cleveland, Ohio], and Ames—Langley, Glenn, and Ames.

We did studies, and then Mark Kelly, our branch chief, says, "Ken, we could modify the 40 x 80 and do most of the requirements for a lot less money—probably an order of magnitude less." The idea was to soup up the 40 x 80, [that is] increase the power. The total power in those days was 36,000 horsepower, soup it up to 135,000, and then add on the 80 x 120 [Wind Tunnel] test section.

I led the aeroacoustic group to do that, and we did experiments and studies and demonstrated that we knew what we were doing. We kept the same fan diameter because of the existing structure. The idea was to make it cost-effective. You had to put in bigger motors and fans that would absorb more horsepower to give the higher speeds. The other thing was we were successful at making it quieter than the existing drive. It was pretty easy to do because the existing drive, they didn't understand, in the [19]30s and 40s—and even the 50s—the effect of wakes on propellers.

There were airplanes that never were successful, like the [Convair] B-36 [Peacemaker] that had pusher props. They were in the wake of the wing and that's hard to deal with from a performance and acoustic, especially an acoustic standpoint. The old drive had the fans downstream of the struts, so we put them upstream, and that was a major advance. We also reduced the rotational speed by putting more blades on, and that helped reduce the noise. I think aerodynamically and acoustically, it was very successful. It's a good facility.

JOHNSON: The wind tunnels, all of them drew a lot of power. Was a lot of the testing at night when you first came?

MORT: Yes. Unitary [Plan Wind Tunnel] was the other heavy user. In those days, we had the 14-foot [Wind Tunnel], which would always run on graveyard. I'd come to work in the morning and the thing would be running. It had a distinctive rumble. I was sorry to see them tear that down—it was a cool wind tunnel. Yes, we would schedule the power with PG&E [Pacific Gas and Electric]. Ames, in those days, had a good agreement, a special agreement, with the utility about power usage, so we would schedule with the Unitary.

JOHNSON: When the things were running, at some of the other sites, early on, they said a lot of the times the engineers would stay through the running, and then later on, they would let some of the technicians run at night instead of the engineers staying all night. If it was running at night, you stayed, or did technicians stay?

MORT: The procedure with the 40 x 80 was always to have an engineer and two mechanics. That was the minimum. You had two mechanics—one was an observer, if the model started coming apart, there was a panic button to push, and the other mechanic drove the tunnel, he had the controls. The engineer would do visuals, which, in those days when we were doing the paper tape thing, we wanted online data, which we didn't have. You'd read important scales and write down the numbers, and we had a slide rule, and you'd calculate the coefficients that you were interested in during the course of the test, so that you could run the test more intelligently. That was typically on graveyard. Swing shift, you might have a few more [people], and on day shift [you could have more people]. The idea was that on swing shift and graveyard, you would try to just run the tunnel and no do [major] model changes. Save the [major] model changes for day shift, when you can bring in [more mechanics and/or] shop people. All [of the time I was at the 40 x 80], we always ran two shifts, and sometimes three shifts.

I have another story about running the graveyard [shift]. Junior engineers got to run graveyard, which was 12:00 [a.m.] to 8:00 [a.m.] or something like that. It was really tough. We were testing a vertical takeoff [airplane,] and I hadn't been here too long. It was probably like '58 or '59. It was a vertical takeoff [airplane] built by Ryan [Aeronautical, VZ-3RY Vertiplane] that had two propellers and big flaps. The idea was to deflect the flaps to turn the air down for the vertical takeoff, and then retract the flaps to translate. The thing was like built out of Reynolds Wrap [aluminum foil]. They tried to save every pound to get vertical lift. We'd finished the test. It was about 3:00 in the morning, on Friday, and the test pilot, Pete [Peter F.]

Girard—isn't that a cool name for a test pilot?—he died of old age, not in a plane crash. In those days, that was kind of rare. He came over to me, and he says, "Ken, have them put the flaps all the way down and then we'll slowly bring the airspeed up, until something breaks." We did that, and the flaps started to deform, and so then we knew what the limiting airspeed was. I think nowadays, "Could they do that? Would they let the engineers do that, run it till it breaks?" We did that to find the boundary, in realistic terms.

JOHNSON: No kidding, all the way to breaking. It looks like you were in the 40 x 80 a good part of your career. Is that correct?

MORT: Yes, I think generally, about half [my career was in the 40 x 80.] Then Mark Kelly asked me to lead the [aero/acoustic] group to modify the tunnel. The airspeed in the tunnel over the years had dropped some, and one of my jobs was to figure out why. I don't think I ever did figure out why. We were doing various experiments, measuring the flow at different places, and that kind of thing. When it came time then to study new wind tunnels, I did that. Mark Kelly was still my boss. Then, we came up with the mod [modification] to the tunnel. That project [was then done]. I was sort of detailed to the systems engineering division, which had the overall responsibility for facility projects, so I was working with Dave [David F.] Engelbert, who was the division chief. At the time, he was head of the project, and then I became head of the project, and we had an accident, and I wasn't head of the project anymore.

After the dust settled, Ames had successfully proposed modifying the Unitary and the 12-foot [Wind Tunnel]. Dave says, "These are wind tunnels—I could use aerodynamic help on both of these," and so, he talked me into working for him as his assistant. That was essentially to the end of my career. I stayed there, and we did the 12-foot and the Unitary.

Probably about several years before I retired, NASA was again interested in building a new facility of some kind because all the NASA facilities have relatively low productivity. They weren't designed for high productivity, ever. They were designed for research. Boeing [Company], especially, was critical of that—"We need a big throughput for when we have a new configuration; we're competing with Airbus and we've got to get data quicker." Boeing had started studies on a new facility, and then Boeing decided NASA ought to do this, which was probably right. I was part of that for a while, and that's when [Daniel S.] Goldin was the head of the agency. His thing was reinventing government, and so he gave Boeing, I think it was $400,000,000 to study [new tunnels].

Larry [Lawrence J.] Ross, who was displaced at Lewis—Glenn—was the official head. Larry Ross got the money and then gave it to Boeing. The statement of work was one paragraph. Imagine that. Boeing then set up a team and had a lot of other airplane companies, Lockheed [Martin Corporation] and Douglas and a lot of good people. They did a good job on the requirements but not on the design of the facility. I decided after that, that Boeing was good at designing airplanes, but not facilities. That went on for several years, and it was going on when I [retired]. The handwriting was on the wall. It was obviously not going to go anywhere.

They had work done by the A&Es [Architecture and Engineering] that were architect engineer firms, that had done wind tunnels, were also part of this team. They were good, and we did talk them into doing a risk assessment study, which was like a preliminary design. That was really the only design work that was done. There was a lot of studies that were kind of siting studies, where should it be located? Should it be at Tullahoma [Arnold Engineering

Development Center, Arnold Air Force Base, Tennessee]? I argued for Ames, and Langley wasn't interested in having it, for some reason. They probably were smarter than we were and knew it wasn't going to go anywhere, but that's where [it was] when I left. [They studied both] transonic and subsonic [wind tunnels].

JOHNSON: You were talking about the lifting bodies that were studied in the 40 x 80, and you said all the ones that were actually tested out at Dryden were studied here, first. Did you ever go to Dryden for any of those tests?

MORT: Yes. I didn't go for the tests; I just went for other things. We'd have meetings about what should we do. I have another story. The M2 was really the first one—that was the M2-F1 that was a lightweight. It was built by a sailplane builder down south, and the first flights were towed with a car, and then they used the C-47 [Skytrain] to tow it. That was successful. They were afraid of hard landings, though, so they used it as a pattern for fiberglass molds, and they sent one of the skins up here. I'll come back to that.

Then, they built the M2-F2, which was heavier, and it was to be launched from the B-52 [Stratofortress]. That was flown quite a bit, and then, one of the pilots, Bruce [A.] Peterson, crashed it and that was the film on the *Six-Million-Dollar Man* [television series], the preliminary [part] where he tumbles. It was amazing he survived, but the fact that he survived is a tribute to a guy down at Dryden that designed, essentially, the cage. The vehicle [M2-F2] was built by Northrop [Corporation], and I can't think of his name. Bruce Peterson survived because of the cage that they had specified around this thing. It tumbled and it actually went end for end.

It had a lateral directional stability issue that you had to be careful [with during flights]. The pilots were always being careful of that. The L/D was low, and it was really low when you dropped the landing gear. The procedure was kind of like the Shuttle landings, it sort of dives at the ground, levels out, and then drops the gear. The pilot was distracted and didn't drop the gear soon enough, and so that was the cause of the crash.

Northrop was in the process of building another lifting body, the HL-10. That was a Langley design, and the Air Force was paying Martin to build another lifting body. There got to be three of them, and so the question was, "Why should we fix the M2-F2?" We were having a meeting about that, to discuss that, and we had done some experiments on a model that the shops had built using the fiberglass skin that we'd gotten from Dryden.

The key thing was, for the lateral directional problem, to put a center fin on it. We did experiments on that, and demonstrated that really made the thing a lot easier to fly. We were advocating [a center fin] as well as rebuilding it. John McTigue was a cool guy down at Dryden; we were having a break and I said, "John, this is really terrible. They may not rebuild it."

He says, "Ken, don't worry about it—we've got it half-rebuilt in the shop." They were doing it. Dryden was pretty creative with [Center] Director discretionary money. So was Ames, at one time. The thing was built and it was a lot better flying machine. We had tested the HL-10 and the Martin [Marietta] one, the X-24[A], I guess.

JOHNSON: Since you spent so much time at the 40 x 80, and you were talking about the early technology and the tapes and then sending it to the computers, and as you were there, throughout your career, the technology changed quite a bit.

MORT: Substantially.

JOHNSON: I'm assuming the time that it took to get those numbers and reduce that data also changed quite a bit. If you want to just talk about some of those changes?

MORT: It's a good story. Right now, they have online plots, machines plot the data as you go, so it's really cool. The steps that I remember is the paper tape was a real pain, and the next step was the computers would read the tape and then punch out IBM cards. The cards would go down to the Computing Center. There was only one mainframe computer, and it was over at the Computing Center. They would run the cards and run the program that the engineers had written. We'd get the printout sheets, IBM printout sheets, in a day or two. It was more accurate but not any faster. That was the next step, the [IBM] cards.

My first boss, Bill Evans, he was tasked with figuring out a better way to do it. They put load cells on the scales and then hooked them up to a machine that produced punched paper tape. I don't know if anybody ever remembers that, the punched paper tape. We would run the test, collect the tape, and then we had a line to a timeshare in Palo Alto [California]. We would take the paper tape, put it into a reader, and send it over to timeshare in Palo Alto. Then, they would send the data and we'd get a printout from timeshare that was quicker than going to our Center.

We did that for a while, and then they improved the scale heads. After a while, the Center decided this is crazy. All the tunnels had to send their data over there, and if they were running a theoretical study that took days, tough. It was not good. There was a lot of pressure. We're not getting online data. In fact, when Paul Yaggy left the 40 x 80 and started the Army activity, the first thing he did was buy a computer, a mainframe computer. He calls me up and he says, "Ken, you got to come over and see my new toy." That was almost the very first thing he did, when he started that operation. It was really a problem.

The 40 x 80 got a computer and started doing [online data reduction], and slowly, it got better and better. They had a guy that did the systems engineering for wind tunnel computers, so there was a lot of commonality—he designed the system for all the wind tunnels to have the same kind of components, which made a lot of sense. It evolved in the 70s, I guess, somewhere in there, maybe before—60s? I forget. It was a big improvement, and so we got online plots while you were running the test and didn't have to read the visuals and do [estimates] with the slide rule.

There's a story about the slide rules. Nobody had computers in the early days. It was the mainframe computer, and that's it. You had a slide rule. HP [Hewlett-Packard] came out with the HP-35 for $400. Now you can buy a [pocket calculator] that does the same thing for about $5. Mark Kelly comes into the office that I was sharing with a guy named Bill [William T.] Eckert, and Bill was interested in computers. He says, "Bill, do some research and see if we can get a cheaper computer than this $400 for HP."

Bill did the research and bought—it was a place in Oregon he bought them from, and they were $400 for the basic computer, and if you wanted a programmable one, that was $800. Bill bought one of each and took them apart. It turned out that both had all the same components, it was just a couple of wires that weren't hooked up for the $800 version. We bought about 10 of the $400 versions. That was the first computers.

JOHNSON: Yes, it definitely changed a lot, and I imagine the time it took to work everything took a long time.

MORT: Yes, it was painful. It was really painful.

JOHNSON: Do you remember, early on, they were having the inspections with NACA and then, I think after NASA formed, they still had some, where they would go to the different Centers? Do you remember any of those?

MORT: I remember one of them. I think they did one of them after I was here. It was kind of good because I was a pretty new engineer and they would shut the place down and have displays and exhibits for the facilities. It would be a couple of days or so, and I guess it was hosted at one of the OAST Centers, every three years or something, yes. I don't remember much about it—I remember it was fun, though. We'd build displays to illustrate things that we're doing, trying to do. That was a good thing to do, and then it stopped.

JOHNSON: Things changed, right?

MORT: It's all about the money.

JOHNSON: That's what we've heard before, that things were more, like you mentioned, research-driven, and then after NASA formed, then it started moving towards more goal-oriented and getting to the Moon and going further with the different projects.

MORT: I think what this agency should have done is split out the aeronautics part and kept it at constant funding—I don't know, $1,500,000,000, or whatever made sense—and continued to do the research because NASA's not doing that anymore. It's gone. Research that we did 50 years ago is being used now, so it's long-term. The airplane companies don't do it on their own. Long-term to Boeing is five years, maybe, and their research is can they add another 10 feet to the fuselage of a 737, or change the wing on a 737? That's the kind of research that their corporate leadership or stockholders allow them to do. They don't do stuff that's 20 or 30 or 40 years out. That's just the way it is.

That's the function of NASA, or should be, or was the function at NACA. They did try—and I don't remember it very well—they did have a group of three airplane companies down at Caltech [California Institute of Technology, Pasadena] that had a facility, and that was going to be run for the benefit of the airplane companies. I don't remember too much about it, except they didn't last very long. They were going to do basic research that was of interest to them. That's really NASA's, the aeronautics part, that should be their function, which they're not doing.

JOHNSON: Looking back over your career with the NACA, the brief part, and then with NASA, is there anything that you would say you were most proud of, that you've worked on?

MORT: I worked on a lot of neat stuff. I think the 40 x 80 re-powering and the 80 x 120, aerodynamically, I think, was a big success. That's probably the biggest thing, I think. There were a lot of people that contributed. We had an accident, and then I was off the job. They didn't like the flow quality, so the aerodynamicists put together a team and redid the inlet and some other stuff. The [original] project was under-funded. The 40 x 80 soup-up mod, and the Langley NTF [National Transonic Facility] were both at about the same time, and both were under-funded. Langley's better at hiding their problems than we were. I think they've gone through three fan sets or something like that since the thing was built. We've gone through one. I think we did a lot of really good stuff for a budget that was too low. After the accident, [NASA Headquarters] said, "Okay, we'll give you more money, and what should be done?"

I was working with Dave Engelbert, and so we made some suggestions about really improving the tunnel because we had, I hate to say "cut corners," but there were some things we didn't do. Like the air exchange system was really poor, and we knew that, and we told everybody, "You can only run so long or you're going to have a problem." That's okay; keep it under budget. The north end wasn't reinforced, but we could get away with it if we monitored the wall pressure. So, you had to actively monitor the wall pressure to make sure you didn't have a catastrophe. After the accident, that was beefed up, and there was a lot of stuff like that. When I was running the project before the accident, Bob [Robert E.] Eddy was the director. Did you know Bob Eddy?

BUGOS: No, I never met him.

MORT: I really loved Bob—he was great. He was in the head shed and he was Dave Engelbert's boss. Bob would come see me, he said, "Ken, how's it going?" and so we'd have a little chat. He said, "So, you're running out of money?"

I said, "Yes."

He said, "I want you to keep enough money to do landscaping."

I said, "Bob, I'll take care of the landscaping. I'm going down to Orchard Supply [Hardware] and buy 5 pounds of grass seed and we'll throw it out in front." I liked Bob a lot.

JOHNSON: You mentioned the accident—what was the accident? What happened? If you want to talk about it.

MORT: Oh yes, I'm philosophical about these things. You have to be. Things were going along great. It was in '82. December 9, 1982: my wife was in the hospital. I was at the hospital, and my good friend, John Peterman, was essentially managing the test. We had had trouble with the vane set upstream of the drive. It had moving noses so that when we ran the 80 x 120, it was angled at 45 degrees, and then for the 40 x 80 [the vanes] would be straight. It was a two-position [vane system]. The loads were high. We were doing experiments to try to get the loads down. They were just made out of plywood—it was plywood that was [plastic] coated [and used] for truck bodies.

We kept moving the nose over, and we had a sliding [bracket] so you could unbolt it and slide it over, but it failed. Then, the things shut off the flow, in a fraction of a second. People have said there were shockwaves, and there probably were, and so the whole mass, all the vanes upstream of the drive, went through the drive. [They] destroyed the drive and did other damage. It was pretty traumatic.

They had an accident board, and it was headed by a really nice guy, Bob [Robert] Swain, of Langley, and then there was other NASA people. There were people from some of the Space Centers, not just OAST, so we were all picked on for this should never have happened. That's true—it never should have. I was the project manager at the time. I'd been the project manager for about a year, after being the head aerodynamicist. I was replaced by Lee Stollar. We ordered new blades and we did that pretty fast.

Dave Engelbert and I and Lloyd Walsh, head of procurement, we went and visited Permali Gloucester [Limited] and said, “We need a new set of blades—how fast can you do it?” That part was good, and they still let me do that. After the accident investigation, and it was only a couple of months, they wrote [an accident] report. They had a list of things to do—reliability improvements as well as fixing the damage—and that stretched out for about five years, which I regret. It wouldn’t have, if I’d have still been project manager, because they were doing stuff that I didn’t think they needed to do. They had to pay duty on the new blades—I didn’t. Battles I had already fought. I had a good relationship with Lee Stollar. He was the project manager. There was things that went on that I wouldn’t have done. There was some good things, though, a lot of good things. It’s clearly more reliable now than it was in ’82.

JOHNSON: You mentioned that when you first started, an opportunity to work with some of the people you got to work with, and management, are there any lessons that you learned from those men that you’ve used in your own management experiences in your career?

MORT: Yes. Be patient with new hires. I was fortunate. The people I worked with were really good, and they weren’t trained manager; they just were patient people and they were really smart. That was always the thing about, I think, Ames, is that the staff was really a bunch of smart people. I was fortunate to work with so many smart people.

One of my stories, when I was working with Paul Yaggy, he was really patient with this kid. We were doing some parachute testing, and we decided that the lines—there was one strut the lines [were attached to]—we wanted to make the lines twice as long. It had to be accommodated in the 40 x 80, which is 80 feet long. They decided to put an I-beam [on top of the strut] and get the control head further upstream so the lines could be twice as long. I was getting ready for another job, another test, and [Paul] and another one from our team were working on the parachutes.

Paul calls me up and says, "Ken, we're going to put a 10-inch wide flange beam and go forward. Check it and see if it's okay."

I thought, "Jeez, 10-inch wide flange beams, you could hold up the world." I did a quick, back-of-the-envelope calculation. I call him, "Paul," I said, "It's okay." They put this thing on there, and the control head's up there, and Paul's standing on it, 20 feet up, and I-beams are weak in torsion. Paul's moving it back and forth in twist.

He says to me, "Ken, did you check it in torsion?" We both had a laugh. "No." It was that kind of thing that I respected Paul for.

JOHNSON: A good lesson.

MORT: Yes. I think there's a picture of that, somewhere, of Paul up on [the I beam].

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

WRIGHT: I just have one. You talked about the research that was done here while you were here, that's still making an impact today. Can you give us a few examples?

MORT: Yes. I did a lot of ducted fan work, and some of these little things you see, these drones, have ducted fans based on the work that we did, probably in the 60s and 70s. The parachutes, they're still doing parachutes. That's probably mostly the basic research. I'm trying to think of other things. The V/STOL [Vertical and/or Short Take-Off and Landing] stuff with flaps, Boeing and others are still proposing STOL to shorten up the runway by extending the flaps. Make the flaps a little bit bigger, and you can reduce the runway length. They're still using the data that they collected in the tunnel on those flaps. I don't know if there are any propeller transports left, but we did a lot of work on propeller transports to take advantage of the propeller wake on producing a slow takeoff. That kind of stuff is what I remember off-hand. I'm sure there's more.

WRIGHT: Thank you.

JOHNSON: Glenn, do you have anything?

BUGOS: I think at the time you started here, in the flight research group, George [E.] Cooper, [William H. "Bill"] McAvoy, and all those were in the same division as you?

MORT: Yes.

BUGOS: Were they looking over your shoulder? What was the dynamic between the pilots?

MORT: It depended on the test. I have a good story about Fred [J.] Drinkwater. We were testing one of these backyard inventor things. [Alexander M.] Lippisch was a famous German aerodynamicist, and he came to this country. He was working for a company, a U.S. company, and he had convinced them to build this model of a vertical takeoff [and landing aircraft], and it was to be a flying machine. There was essentially a big duct with a couple of propellers inside, and then vanes at the back, so you'd deflect the air down for vertical takeoff, and then un-deflect them to translate. They ran out of money. The Army would say, "Okay, here's so many bucks, see if you can do it for that. You're not going to get any more money." They did that quite often.

They built this thing, and they ran out of money before they'd finished the fuselage. They used wood and they made it like a light airplane—wood and fabric covering. They had this thing in the tunnel, and the thing was so flimsy that we built a frame underneath to give it some rigidity. The thing's in the tunnel and we're doing testing on it, and Fred Drinkwater comes over to have a look at it, and somebody says, "Fred, would you fly this thing?"

He says, "No, it needs a few more rubber bands." That's the way it looked. There would be other tests.

They did a lot of cooperative stuff on boundary layer control on the F-86 [Sabre]. That went on for many years. They changed wings and did all kinds of work. The F-86 was a good, generic jet fighter. They would do experiments in the 40 x 80, and then they would do flight tests to [verify wind tunnel results]. That was a comparative program that went on for a long time.

If the thing was going to be a flying machine, then the pilots would come down and have a look. The Ryan [VZ-3RY Vertiplane] is an example. Pete Girard did the first flights. It didn't have an ejection seat. This was early on for ejection seats. They still were in their early development [for V/STOL aircraft]. If the plane had a problem, to get out of it, you had to get out of your seat. There was a hatch behind the seat, so you had to get out and get to the hatch and then drop out with your parachute, hopefully. This thing had a hard landing when Pete flew it, so they decided to rebuild it. One of the things they did was put an ejection seat in it. [Initially it had] a closed cockpit—they [converted it to an open cockpit]. It was really intended for fairly low speeds, probably 100 knots, maximum, or something like that.

They did a lot of flight testing with it. Pete Girard finished [the contractor tests], and then Ames pilots flew it a lot, and Glen [W.] Stinnett crashed. A lot of these things had problems, control problems, and so you had to be a little careful. It pitched up and then went over, and then was in an inverted spin. Glen ejected, he's upside-down, and he lived, and he survived, but he hurt his back. They fished the thing out of the bay and rebuilt it and did some more [flight] testing with it.

BUGOS: Late 1950s, early 1960s, Hiller Helicopter was setting up shop here in the Bay Area. Did you see much of them?

MORT: Not too much. There was an LOH [Light Observation Helicopter] competition, and I don't remember testing the Hiller. They had the flying platform thing. I just don't remember. The LOH program, that was really the end of Hiller. I don't know if you know; Hiller built great helicopters. Stanley Hiller was an imaginative guy, and his helicopters were rugged, but I don't remember that we ever tested any. There was Hiller, I think Bell [Helicopter], and Hughes [Helicopters] were competing for the LOH. It was big bucks. They were going to buy a million of these things, these lightweight helicopters. They had performance requirements—fairly high-speed.

Hughes won by being the low bidder and having high speed. They bought it. Hiller didn’t have enough business to survive, and they were sold to Fairchild [Aircraft]. We did test a model of one of the Pogos [Lockheed XFY-1 Salmon]. This was the NACA days. This was probably the first vertical takeoff testing in the 40 x 80. It was a ¼ scale model of a propeller-driven airplane that had a counter rotating propeller so there was no net torque. It was a tail-sitter, it was to sit on its tail, and the pilot would get in it and then take off. They built the model of it and they tested it in the 40 x 80. It was the Lockheed [Salmon]. They subsequently built the full-scale one because they got the money.

There was another one built by [Convair, XFV-1 Pogo]. They had identical drive trains, these two [tail sitters]. The program didn’t last too long. It was intended to be flown off of ships, and the pilots had a terrible time laying on their back, trying to land this thing. They decided it wasn't practical, so they took the two engines and propellers and then gave them to Hiller, and Hiller built a tilt-wing X-18, I think.

That’s about the most that I remember of Hiller, which was too bad. I think the helicopter testing that they did in the 40 x 80, then, was mostly after I came. John [L.] McCloud and George [B.] McCullough started it. They did subsequently test a Bell Huey, I think, as kind of a generic test. It’s probably too bad. That’s what I remember. I could be wrong, though.

BUGOS: One other big change with the coming of NASA is that Harry Goett was recruited to go start up the [NASA] Goddard Flight Center [Greenbelt, Maryland]. Was that a surprise to you guys there? What sort of management change was there, in the shift from Goett to Harper?

MORT: It stayed pretty much the same. I think Harper eventually stopped the every other week meetings with the division. Bill Harper was a really smart guy, and he was good. We'd go down and in those days, if you wrote a report, it had a lot of review. You'd write a report, and then [there would] be an editorial committee of your peers to go over it. Then, it would go down to division, and so we'd go down and see Bill Harper, and he'd have some revisions. He was a good writer and speaker. Then, it would go to the head shed, and Gerry [Gerald E.] Nitzberg would review them. He was very good at reviewing stuff, picking out problems, and then the report would get published.

Those reports were really first-class. You ask anybody in the aero field about the old NACA reports, and the early NASA ones, and they're high quality. You had to describe the model, you had to define your notation, you had to do all these basic things that a lot of reports nowadays don't do, which bugs me. Nowadays, they write papers, and the AIAA [American Institute of Aeronautics and Astronautics] doesn't give a rip about what you do. That's overstated. It doesn't have the same peer review that the old reports had. They're still used because they're valuable. The interesting thing is that they've been digitized, all the NACA reports, as far as I know, by two separate outfits—one U.S. and one British. I've succeeded in downloading both versions, and they're different. After that, the reports, it's not as systematic, the digitizing. There's some I've looked for and haven't found.

BUGOS: What about Harry Goett?

MORT: Harry Goett was great. He was a really smart guy, really clear. When he said something, you paid attention. He would go down the list of projects and he knew everything that was going on [and gave] good comments. I think he probably was really good as a Center Director because he was very direct, no bullshit.

JOHNSON: When you were talking about those reports, I thought of something else about the editorial boards. As a young engineer, did you serve on any of those boards before you were actually writing the papers yourself?

MORT: Yes.

JOHNSON: Was that the learning process on how to write those papers?

MORT: Yes. Everybody got to do that, though. Even more senior engineers were on the editorial board. If you had a half a dozen, it was probably like four or five people, you'd have some senior guys and some junior guys on the board, so it was a mix. One of them would be familiar with the subject for sure, or a couple of them at least, and so you'd get a really good mix of criticism. It was a good process.

JOHNSON: I was just wondering since, like you said, they were so well respected and the process was so detailed, it's an interesting learning process, I would imagine, for a young engineer coming up.

MORT: Yes. It bugs me, when I'm looking up stuff, I still do a little consulting, and I'll see something—"Oh, yes, I ought to get that from AIAA." I hate paying for stuff. You get a paper and there's not enough information. You don't get the whole story. You need about half a dozen papers to get the whole story. That really bugs me.

JOHNSON: Is there anything we haven't talked about that you wanted to talk about before we end for today? Are there any other stories that come to mind?

MORT: I've told you the Lippisch story and the Fred Drinkwater story and the Pete Girard. Those are the ones I can remember. It was a great place to work. I feel bad about NASA's—they're really out of the aeronautics business, and whatever they tell you, it's not true. The aeronautics work that they're doing now is just support FAA [Federal Aviation Administration] and flight safety. Which is important, but that's not basic research. One of my good friends, Vernon [J.] Rossow, is still doing the consulting. He's an Ames associate. He, for years, has worked on the wake vortex.

This became a problem with the big airplanes, the [Boeing] 747. Every airplane that produces lift, you get a vortex off the wingtips. Little airplanes, it's not a big deal. Big airplanes, it's a big deal. If you're in the wake of a 747 in a Piper Cub, you're going to roll and maybe crash. There have been crashes. Ames and Langley both were doing research on that, but it's not funded anymore. It should be. They talk about, "Well, can we put more runways here up at San Francisco, closer together, shorten the distance?" All that's based on these tip vortices. We should be doing more experiments on that. It should be an ongoing thing; it's been stopped.

Doing research on this book, I find all kinds of stuff. I'm easily distracted. One is the high-speed research was the program, and when NASA quit funding it, I forgot who the principals were from NASA, they were asked by the Congress, "You've quit funding this?" They never really say why. It's because of the money. They prioritize things and then cut it off. NASA, in '03, I think, quit helicopter funding. Somewhere in there, they actually stopped, and then there was a hue and cry, but I don't know what the recovery was, if they really did. I know they have some NASA people that [still] do helicopter work, but not very many.

I did want to try to have a little story about that, but so far, I've failed. They shut the tunnel down in 2003, and the Army said, "We've got to test helicopters there," so then they started it back up. The Air Force then operates the tunnels. The Air Force is the facility operator for DoD. They operate the tunnel for the Army, for NASA and the airplane companies. When you get a list of NASA wind tunnels, that tunnel is not listed. That bugs me. It's just stupidity.

JOHNSON: After your history with it, I would imagine you wouldn't like it.

MORT: It just doesn't make any sense. The problem is that the management of NASA and the Centers is mostly concerned about going to Mars or something like that—space. Getting money for space, that's where the money is. It's not in aeronautics.

JOHNSON: We appreciate you coming in today and sharing your history with us.

MORT: Okay.

JOHNSON: All right, thank you.

MORT: You bet.

[End of interview]