NASA HEADQUARTERS NACA ORAL HISTORY PROJECT
ORAL HISTORY TRANSCRIPT

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SAN JOSE, CALIFORNIA – 30 SEPTEMBER 2005

WRIGHT: Today is September 30th, 2005. This oral history session is being conducted with Hubert M. Drake as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project sponsored by the NASA [National Aeronautics and Space Administration] Headquarters History Office. This interview is being held in San Jose, California, during the NACA Reunion XI. The interviewer is Rebecca Wright.

Thank you again for taking time to meet with us. I really appreciate you sharing your information about your days at NACA. Let us start by you sharing with me how you first became involved with NACA and then, of course, how your role evolved there.

DRAKE: Okay. Well, I first became aware of NACA when I was in college, the University of Michigan, [Ann Arbor, Michigan]. In my senior year I worked in the wind tunnel at the Aeronautics Division, and we used a lot of NACA reports in our activities, so I was familiar with their research efforts and the aeronautical research that they were conducting. Consequently, when their recruiters came through, they were one of the ones that I was very interested in. They were in competition with Lockheed [Aircraft Corporation] at the time, as far as I was concerned, and my wife-to-be said, "No way are we going out on the West Coast." She was from Michigan.

So I accepted an offer at Langley [Aeronautical Laboratory, Hampton, Virginia]. This, of course, was right in the middle of the war in 1943. I had been working at the Ford [Motor Company] Willow Run [Bomber] Plant [near Ypsilanti, Michigan], building, or actually repairing, B-24s that were being built there. The people that were building them, most of them came from, apparently, Kentucky and Tennessee and had never seen an airplane before, so they were making somewhat radical mistakes in building. The senior class at the university had been contracted to Ford to design repairs for these mistakes, and some of them were pretty gross. So I worked there for several months before I graduated.

We were in an accelerated program because of the war. We graduated in January instead of June. I went to work at Langley that February. I was assigned to the free-flight tunnel, which was a unique facility, in that we used models, most of them about three to three-and-a-half-feet wingspan, flying in a free environment within the tunnel to determine the stability and control of these vehicles. I was there three or four years, I think, most of the war. Most of the aircraft we tested were tailless. In fact, our saying was we took more tails off airplanes than anybody else did. We also worked on several of the glide bombs, the remote-controlled glide bombs that were under development at that time.

My Branch Chief was Johnny [John P.] Campbell, and he was a very good leader. Have you talked with him?

WRIGHT: No.

DRAKE: He should be a good man to talk to.

WRIGHT: Okay.

DRAKE: All of our work was stability and control of these various vehicles, and I wrote several reports. They're all listed in the stuff I gave you. When the war finished—well, we tested also a number of interesting aircraft that did become flying vehicles, and the X-1 was not tested in the free-flight tunnel, but it was tested in the spin tunnel, which was right alongside. At the time we looked at that and said, "Oh, hell, another rocket-powered airplane," because we'd seen all the German rocket airplanes. This one, of course, was a lot better looking than theirs.

At the end of the war, everybody had a tremendous amount of annual leave stored up, since we hadn't taken any during the war, so I took about a six-week driving tour of the United States. Went to all the various national parks that we could go to in this sort of thing; took my wife and her mother and one of the fellows from the free-flight tunnel. We hadn't been able to buy any cars during the war, of course; nobody could buy a car. So we bought one as soon as the war was over and proceeded to drive it all over the countryside.

One of the places we stopped at at the time was in Mojave in California, and Lou and I—Lou Tosti was the man we were with—and I went out to Muroc, [California] and visited the [NACA] Muroc [Flight Test Unit] that had been established for the X-1 at that time. They had flown the X-1 in Pine Castle, Florida, for glide fights, and after the rocket system was installed, they brought it out to Muroc to run the acceptance tests and to do the research flights. They had not started any of those flights at that time.

The whole program sounded real interesting to me in all, and I was quite enthusiastic about it. When we finally got home, I'd been back to work for about a week and Hartley [A.] Soulé, who was in charge of the Research Airplane Program at the time, called me in and he had an offer of two jobs that were particularly stability and control oriented. One of them was at

Langley, being investigation of basic stability and control, theoretical investigation. The other one was a job out at the facility at Muroc on the flight test of the X-1.

Well, I wasn't particularly enamored of doing theoretical research, and flight tests sounded much more interesting, so I accepted the position at Muroc. When I got home, or back to the apartment, and told my wife I had accepted a position out at Muroc, and she called her parents up in Michigan and told them that I had a job out in California, and they said, "Well, what are you going to do?"

She said, "I'm going with him, of course."

They said, "Way out in California?" Of course, that was an infinite distance from Michigan.

When we were at Muroc, it was just the start of the program. The demonstration flights by Bell [Aircraft Corporation] had just been completed, and the airplanes were being prepared to initiate the research program. As I recall, the NACA staff at that time numbered about fourteen or twenty people, somewhere in that range. The unmarried folks lived in a barracks out on the Air Force base on Muroc. The married people lived in the Navy housing in Mojave. This was Quonset huts for the enlisted personnel, our mechanics and so forth, and officers' quarters, which were square houses that didn't keep the air out any better than the Quonset huts did during dust storms.

Actually, there's a saying in Muroc was that the wind starts blowing in the middle of January and stops blowing at the end of December, so that you have two weeks of no wind. We had periodic dust storms. On Washington's Birthday—I think it was in [19]'49—the dust storm was so bad, actually we put duct tape around all the window corners and all, and the sand would blow right through the duct tape, even with the sealing. The curtains, you could see them moving with the wind, and in the morning after a dust storm, you could vacuum up and shovel out little mounds of dust. In a bad dust storm you could see about twenty feet. We could just barely see the Quonset hut across the way. But, of course, although it was windy all the time, you only had dust storms periodically.

We—and when I say "we," I mean the engineers; I'll speak about the wives in a minute—the engineers, most of us were extremely fascinated by the work. You never knew. You were anxious to get to work. There was always something exciting going on. In the X-1 period the flights were, of course, generally exploratory, since we were investigating transonic flight at a time that no one had been in transonic flight before. Consequently, planning the flights and evaluating the information afterward, discussing it with the flight crews and so forth, was all fascinating work.

The way the program was set up, there were two airplanes active. One of them was being flown and maintained by the Air Force, the other by NACA. The data recording and reduction for both aircraft were done by NACA. The difference of the two airplanes, the NACA airplane was thicker winged. It had 10 percent wing, 8 percent thick tail, and the Air Force aircraft had an 8 percent wing and a 6 percent thick tail, the reason, of course, being that the 8 percent wings were very exotic for the time period.

Most aircraft in that period used 12 to 15 percent thick airfoils, so they had the two thicknesses because they weren't quite sure how the newer sections would behave in flight. They felt that they would behave better in the transonic speed than the thick ones, but they didn't know. At that time, as I recall, wind tunnels could not cover the speed range between about eight-five [.85] Mach number and one-point-one-five [1.15]. Testing in that speed range was being done by very small airfoils, wings about four inches high, [installed] on the wings of P-

51s, which would then dive, and the little airfoil would be tested in the high-speed flow over the surface of the wing.

We did our flight tests on a program of gradually increasing speed and gradually increasing lift. A flight test would go to an indicated four-hundredths [.04] Mach number faster. The X-1 was air-launched, of course, from a B-29, and being rocket-powered, the only control that the pilot had over power were there were four levels of power with the four cylinders of the motor, and the duration of power application. So it was not a very fine control of speed and altitude. We would increase speed by about four-hundredths, and then pull up to several-G [gravity] level to increase lift. Then after that flight we would evaluate the data that was obtained and interview the pilot as to how the airplane was flying to his feeling.

The main problem at the time, of course, was the air speed calibration. The increased Mach number would be indicated on the pilot's Mach meter. However, because of the transonic aerodynamic effects on the pitot-static tube sticking out in front of the airplane, we knew this was in error, and we would make calibrations of this error by doing radiosonde measurements of the atmosphere and [also] measurements using the B-29. As it would climb up through altitude, we'd measure the atmospheric conditions, and also with the radiosonde at altitudes above the level that the [B-]29 flew to. Then we would take a survey as the X-1 climbed up through these altitudes and obtain a pitot-static recording that would enable us to calibrate the error in the Mach meter.

Well, this procedure involved a lot of data reduction and was always a little bit behind the flight testing, the data reduction process. Our fastest computers at the time ate lunch with us and—

WRIGHT: Of course, you're referring to the mathematicians.

DRAKE: —consequently, computation was not as fast as it is these days, so the calibrations were always one or two flights behind. The Mach meter on the flight one time indicated nine-tenths [.9], and the next one was supposed to go four-hundredths faster, but it went to nine-two [.92], and we stopped there, because we still had no calibration for that condition. We paused flight testing till we got calibration on the Mach meter. The calibration indicated that we were at nine-nine-five [.995], which was probably a little more accuracy than we could expect for that calibration.

So the next flight, we told them to go to nine-four [.94], and he went to nine-two [.92] and the Mach meter abruptly jumped to one-point-[zero]-five [1.05]. So we knew we had sonic speed, and it was probably at supersonic speed, the Mach meter reads correctly. The pitot-static error was zero, because the shock waves had traveled right past the head, and it was in complete supersonic flow.

The pilot's comment at the time essentially were he had been experiencing—this is my recollection, and the reports probably would argue with me, but as I recall, the pilot had experienced buffeting, continually increasing as the speed increased and as lift was increased. Between nine-tenths [.9] and one-[zero]-five [1.05], buffeting stopped, but the controls, normal controls, were continually losing effectiveness. This was particularly pronounced in the longitudinal control. Fortunately, or not fortunately, but, of course, designwise, we had anticipated loss of conventional elevator control and had provided that the entire stabilizer be movable, as well as the elevator, so that the pilot was not without longitudinal control, it was just that it was considerably weakened, his conventional feel on the elevators. The elevators required more force and had considerably less effectiveness.

Our speed program, accelerating program, continued up to, as I recall, about Mach one-point-three-five [1.35]. I think that was about what it was. We had a conference. Of course, the day that we knew we were supersonic, the project went immediately from unclassified to secret, and went from being totally unknown to the front pages of the *L.A.* [*Los Angeles*] *Times*. Since it was unclassified, as soon as we went supersonic, somebody called up a friend down in the industry and told them about it, and then it, of course, went right into the newspapers.

There was a conference—let's see. The first supersonic flight was in October, and we had a conference at Ames [Aeronautical Laboratory, Moffett Field, California] in December to present the data, such as it was, from these first flights. There were several, quite a few, papers given by people from the High-Speed Flight Station.

I'd say a bit about the families there. We, as I mentioned, were living out on the—the married folks were living on the Navy housing at Mojave. That was a Navy training base during the war, and we were living in their housing. The unmarried folks lived out at Muroc. The men—mostly men, since most of the computers, they were all female; this was sexist, of course, but they were all female—they lived out on the base, and the wives that lived in Mojave formed their own little club, so to speak. There weren't very many of them.

The entire crew drove out to work every morning in one station wagon. In fact, the station wagon, one time we went out there, and one of the engineers who drove—his name, as I recall, was [William H.] Barlow. We didn't know why we were driving so fast, and he said, "Well, two of the tires were very low this morning, and I inflated them all to a very high pressure, and I want to get out there before they go flat again."

So, anyway, the wives made do as well as they could. This was just like being in wartime, as far as they were concerned, because it was war housing, and Mojave was and is pretty remote. When one of the R-4Ds would come back (one of the C-47s) to Mojave from Langley, they would bring oysters and lobsters and so forth, and we would cook them over a fire in a ditch there by the housing and have a party. I remember those. Christmastimes, we'd drive up in the mountains outside Mojave and cut Christmas trees down. Cutting a Christmas tree on a mountainside, you have no idea how big the tree is until you get it home, and it's usually about four feet too high for the room.

Back to the flight testing, the X-1 Program led to the next program was a D-558 built by Douglas [Aircraft]. The D-558-1 was a jet airplane and was able to go to high transonic speeds; [but] could not exceed, I believe it was, about nine-tenths [.9]. This was the first airplane that NACA had ever lost and killed a pilot. Howard [C.] Lilly was a test pilot, came from West Virginia, and was very well liked by all the staff there. He was the number two pilot behind Herbert [H.] Hoover, who was our lead pilot. Lilly had flown the X-1 and was the lead pilot on the D-558, Phase 1. I don't recall how many times he flew it, but the engine of the airplane blew up on takeoff on one flight. I mean that was a total crash. We had, as I recall, two of the Phase 1 airplanes, and the flight test program continued on the second one.

But D-558 Phase 2 was a rocket-powered, initially jet- and rocket-powered, airplane. It was supposed to take off from the ground, and it did, but during the development, the design was changed so that it could be air-launched. Some of this may be—this is my recollection, and there may be error there. The jet- and rocket-powered airplane could just barely exceed Mach one. It was limited performance-wise to somewhere around one-point-two [1.2]. It only made one takeoff, jet takeoff and rocket flight so that it could be the first ground takeoff airplane to exceed Mach one. But that was, as I recall, the only flight that it made from the ground.

The D-558 Phase 1 airplane program was completed. The D-558 Phase 2 program, the second airplane in that program was all rocket-powered, did not have a jet engine, and it was the first airplane that went twice the speed of sound. That was very close to the extreme performance for that airplane. It had to have a very carefully worked out flight plan and flight profile to make that speed.

There was a bit of a competition there, because at the same time period, a later X-1, the X-1A, was being flown in its initial flights, and it was a considerably heavier and higher performance X-1 than the initial ones. It used the same power plant, but it had much more fuel, which consequently gave it considerably more performance. So it was in its initial flight test at that time, and since the Air Force was flying that, there was a bit of a competition between the NACA and the Air Force, a competition that nobody admitted existed, to get to be the first ones to Mach two. As luck and the scheduling would have it, the D-558 did it the week before, as I recall, a week before the X-1 did. They were both using the same launch aircraft, but that didn't affect the scheduling.

The remainder of the D-558 Program went smoothly. The X-1A Program eventually got to a speed of about two-point-six [2.6] Mach number, two-point-five, two-point-six. Chuck [Charles E.] Yeager was flying it at that time. The flight instructions, we had made analytical estimates based on what we knew from our measured characteristics and the wind tunnel tests. Of course, wind tunnel tests were made at supersonic speeds, so we had a pretty good idea of the aircraft's characteristics. The estimated behavior of the airplane at zero lift was, as I recall, that it would be neutrally stable directionally. And we had warned the pilot that he should decelerate by maintaining zero lift after the engine quit; he should maintain zero lift until the airplane slowed below Mach two.

It's very difficult for a pilot to fly at zero lift continually. Consequently, when the engine went off, Chuck inadvertently, or advertently, pulled up to some level of acceleration. The airplane became unstable, and diverged in three directions at one time. In fact, it demonstrated the aircraft's strength in all three directions. The record of that showed extreme loads, extremely violent maneuvers. In fact, it was a supersonic spin, and finally came out of it at subsonic speed. Fortunately, the airplane was still within gliding distance of the dry lake, and he was able to recover on the dry lake.

The canopy—now, the original X-1s had a completely faired canopy. The X-1A series had a more conventional Air Force type canopy, in that it was a bubble-type canopy, so the pilot had room to move around in it. The canopy in this case after the flight showed markings all back and forth, side to side, on the inside of the canopy, from the pilot's helmet. You could track it from one side completely to the other. I think the pilot was demonstrated at the same time as the aircraft was. We were very fortunate we did not lose that aircraft and pilot at the time. Further flight tests on the airplane and on the X-1B, which was another of the same series, were, of course, restricted to slower speeds.

The X-2 airplane came along in the same time period. Now, the X-2 was designed for Mach three. The initial program was for three aircraft. One of them was lost at Bell before it was given over to the flight test program. The second airplane came into the flight test program at Edwards [Air Force Base, Edwards, California], and it was ready to operate.

Oh, I should go back. We lost one of the X-1A Series airplanes. The X-1B was lost before launch from the B-50 at the time. I forget the exact conditions at the time, but we got warning from internal instrumentation that we had a problem, and the pilot climbed out of the airplane and back into the B-50, and the aircraft was dropped and, of course, crashed. We lost one of the X-2s on a ground test of the rocket engine.

The next X-2, the manufacturer demonstrated the airplane to subsonic speed and the usual high lift conditions. The flight test program started with the Air Force flying it. They were to fly a certain number of flights and demonstrate the airplane to its high-speed condition, and then it would be turned over to the NACA for the continued research program. The pilot, “Mel” [Milburn G.] Apt, was the Air Force flight test pilot. The airplane flew, as I recall, thirteen flights, and the speed was increased. We did the usual flight test program of gradually increasing speed and increasing lift to explore the conditions at each speed level.

Again, we had supersonic wind tunnel data to check against our flight test data. The wind tunnel data indicated that at Mach numbers above two-point-five, as I recall, the airplane would become neutrally directionally stable or unstable, and at lift it would certainly be unstable. We instructed the pilot in the development flight tests as regards the behavior to be expected from the airplane. The flight test program continued up to, as I recall, about two or two-point-two [2.2], something like that, Mach number.

On the next flight, which was to be the last flight of the Air Force program, the flight test program indicated for the pilot to go to maximum speed, which was estimated to be close to two-point-eight [2.8], and—now, these are recollected numbers, so I may be in error there—and decelerate again at zero lift or slightly negative lift, holding zero-G. Again, we had an experience, the same as we’d had with the X-1B. The pilot, through his probably natural instinct, pulled up to 1-G at the maximum speed when the engine went off, and the airplane diverged in, again, three directions.

The aircraft might very well have recovered at subsonic speeds, but the X-2 differed from the X-1 Series, in that it had a capsule in it, erroneously termed an "escape capsule," that could separate from the aircraft in case of emergency, and then the pilot would later leave the capsule in a safe condition by normal ejection. The pilot actuated the capsule. The capsule went into an extreme maneuver, that probably incapacitated the pilot, and crashed at the same time as the aircraft crashed, the aircraft, of course, totally destroyed, and the pilot killed.

In the same time period as we were working with the rocket-powered airplanes, we had a series of conventionally powered aircraft investigating configuration effects and various type of aircraft improvements. The X-4—well, I should mention the X-3 was intended to be a supersonic aircraft and to fly at supersonic speeds, jet-powered. It was a Douglas Aircraft, very extremely long airplane in length with small wings. The aircraft was designed to fly at Mach two, but unfortunately the aircraft was developed much faster than the engines were developed. Consequently, it never flew faster than low supersonic speed. The airplane had roll coupling, the same as several of the transonic fighters had at the same time.

As I started to say, we were investigating a number of configuration effects, and had the X-4 for a tailless airplane, and the X-5 was a variable-sweep airplane. The X-3 had a low aspect ratio wing, straight wing, similar to what was developed on the F-104 later. At the same time there was an agreement with the Air Force, or rather with the Department of Defense, that NACA would obtain one of the initial production of every one of the advanced fighter aircraft.

The F-100, as I recall, was the first of these aircraft to come to the Flight Research Center. The F-100 was designed for about one-point-four [1.4] Mach number, and consequently was the first airplane capable of sustained supersonic flight, and consequently was the first airplane that could get into trouble at these speeds. One of the things that occurred during flight test of that airplane, initially during the manufacturer's flight test program, was a phenomenon called roll coupling, which would show up as a matter of—well, initially initiating a roll above certain roll velocity would introduce a directional divergence condition and put the airplane into an uncontrolled maneuver, which could develop high loads, particularly in the directional mode.

It first, as I recall, first demonstrated itself as a real problem on the F-100 where it occurred during one of the manufacturer's flight tests. The vertical tail was destroyed, and the aircraft crashed. We had then a F-100 at the time; it was number nine of the production. So we started a development program to investigate this. I recall at that time, this was the first time we did a real dynamic analysis using computers. All the computing that we had done before was just whether or not a maneuver was stable or unstable. It was not a detailed motion analysis.

So Joe [Joseph] Weil and I went back to Langley and used their computer to plan the flight test program for the F-100. It was the first time either one of us had actually worked with a—you might say—a good computer. The program then was essentially to test three different vertical tails on the F-100, and it ended up with a considerably larger vertical tail than it had had before. This became characteristic of the supersonic performance airplanes, that on a basis of a pure performance standpoint, they would have a lot less vertical tail than they have for stability.

The F-104 and the X-1, X-2, the F-100, all demonstrated this roll coupling during their flight tests. The X-5 also encountered it in one or two of the sweep conditions. This behavior had been predicted by [William] Hewitt Phillips at Langley in a report that everybody overlooked, published some years before the Research Airplane Program started. It was in a technical note, which shows the attention that's paid to technical notes, theoretical technical notes.

About that time, about the time the X-2 airplane was coming up into its flight test program, Bob Carman, one of our engineers, and I had considered what would be an extension program beyond the X-2's performance of Mach three. We did a hand calculation of an air-launched swept-wing airplane; actually, it looked a bit like an X-2—just as a basis for the performance estimates, and what would be required in terms of power and size for an airplane that would go to Mach numbers substantially higher, perhaps about twice the speed of the X-2. So we wrote an unofficial proposal to NACA about that, and eventually, after being massaged and distorted and calculated in committee, it ended up with a specification for the X-15 airplane.

I might mention, as an aside, the jump from the X-5 to X-15 in the X-Series is full of odd vehicles. The X-Series included in that series a lot of airplanes and missiles and so forth that we were not involved with. There was a nuclear-powered airplane. There were a number of missiles. The X designation was not restricted to our use. So the X-15 was the next one that we had in the X-Series.

As I mentioned, though, we did have a number of aircraft. A number of F airplanes, the F-104. The F-105 was an interesting one from Republic [Aviation Corporation] and the aircraft, as I recall, had inverse taper of the wing root; the wing at the root was a shorter chord than at the tip. It didn't go anywhere but to the experimental [program]. It also had a rocket engine in it, however, and it was of interest from that standpoint.

The F-10[7] was an airplane with an aft inlet above the pilot, above and behind the pilot. The only thing I really remember about the 10[7] was that after a landing—and as I recall, [A. Scott] Crossfield was flying it—and after a landing, the tires blew up, and they were such high pressure tires and so thick that they made the airplane unsafe to fly by lumps of [rubber] penetrating the structure. So that ended that flight test program.

Scott had another interesting episode flight testing the F-100. We had our new facility down at the end of the lake at that time, and during the flight tests of the F-100, he had a power failure, so he made a deadstick landing, a very nice landing, and a safe landing; he was rolling. We had a ramp that led right out onto the lake, and he rolled up on this ramp and, unfortunately, had forgotten one part of the pilot's manual, which said that power off, gear down, you only had two, no three, brake applications.

So he was rolling up on the ramp, and figured to stop, but he hadn't planned ahead. He'd already used his three brake applications, and the 100 rolled right into the hangar. Door was open, fortunately. He rolled right into the hangar and through the wall of the hangar. The airplane came to a stop right in the wall, and there was an I-beam right above the level of the instrument shelf on the 100. It destroyed the top of the airplane, and it stopped right in front of the pilot's canopy. I remember Scott saying, "At that time, I wished it had not stopped," because he just didn't want to face anybody from having rolled his airplane into the hangar.

WRIGHT: Mr. Drake, when you were reviewing and analyzing all this data, how involved were the pilots with you and the teams?

DRAKE: Okay. In doing a flight test program, every flight test, the setup was the engineers would plan out what was to be done on it. We had a general program, which was established by, you might say, by committee. All the different specialties—stability, control, performance, and so forth—would get together and they'd lay out a general program for the entire program of the airplane from the beginning to the end. Each flight would be more or less laid out in that program. Now, some of them were very short. You might have a program of only fifteen flights. For example, the Air Force might only plan thirteen or fourteen flights of a given aircraft, if it was one that they didn't have their own program for. Our program might have a whole stability and control program laid out, and it would be merged into whatever the other programs were. So each flight then would be set up by the lead Flight Test Engineer for that aircraft. We would have a meeting with the pilot and the various Research Engineers and the Operations Engineers, who actually had responsibility for the aircraft itself, before the flight and discuss what was planned for it. Any peculiar things that might be expected, what the objectives of it were, anything that might be a problem would all be discussed at that time.

The flights in general would be performed in that manner, and after the flight we would have a meeting postflight, which we'd discuss with the pilot what he had observed and everything. This would be without looking at the data. Now, later on—and I can't recall—right from the beginning, we had telemeter data. Even on the first—on the X-1s, we had data telemetered, so that during the flight we could see what was going on. Not that you could do anything, because by the time you tell a pilot, "You're in trouble," he was already in trouble. And it was initially not presented—there wasn't very much of it being—the initial telemetering was almost an accident investigation tool. As time went on it got more and more complex, and finally we were telemetering very large amounts of data.

After our flight, we'd discuss with the pilot what he had observed happening. A lot of the flights, the pilot would go up, make the maneuver, and as far as he was concerned, it was just another maneuver. We would be measuring the lift distribution, manometer data, and so on. Then after the flight data was reduced, and that might be a week, two weeks, because hand reduction of data was very lengthy, and you would generally get a first approximation of the results, enough so that you could then plan the next flight, but it wouldn't be the detailed full reduction of data that you need for reporting in a final report. But we'd get enough to plan the next flight.

Then you would sit down with the various engineers, decide what you wanted to do for the next flight, get together with the pilot and the Operations Engineer, and lay out the actual flight. Depending on the kind of flight, now, flight tests, for example, of the F-100 or F-104 would be fairy conventional. They'd be tracked by radar, but that wouldn't be a primary thing. We had a local radar station that was used, for example, in air speed calibrations and so forth, but the general flight tests of conventional airplanes, they would track, but that wouldn't be a data reduction requirement.

Most of the flight testing of conventional airplanes was conventional. We were not doing anything extreme. The airplanes were not designed for exploratory flight tests. We might be doing something, for example, on the F-104, it had somewhat different characteristics than the other airplanes; had a extremely high horizontal tail, where other airplanes of that time period, the tails, horizontal tails, had gone very low. This, again, was a result of some of the odd behavior at transonic speed. They were the two engineering responses to it. Consequently, the F-104 had somewhat different characteristics than the other airplanes had. It was a very interesting one, and it was also higher performance than—it had the highest performance, in terms of speed, of any of the airplanes of that series. So we had quite a lot of flight tests of that airplane.

WRIGHT: About how many people were on your team?

DRAKE: Oh. By that time, initially, on the X-1, as I mentioned, I think we were around twenty people, something like that. By the time we were doing the F-104 and so forth, we were up to probably about eighty people, I'd say, somewhere on that level. It might be maybe a little more, I'm not sure. We might have gotten up to about a hundred. We were in a new facility.

We had initially been, when we were only twenty people, we were sharing a hangar down in this what they called the South Base with the Air Force and contractors. Everything was right there. We were in an office alongside the hangar, part of the hangar, and the airplanes were right there. You could look out the window, and there were the airplanes. Then we had our own hangar. That was for the X-1s, were all we had. Then we started getting more airplanes, and we moved to our own hangar a little further down the flight line. It was a smaller hangar, one of the old, really old hangars. Still had the offices right alongside.

The thing I remember about that, gas leak. They were putting in a service line; had nothing to do with us. It was out in front of the hangar, something to do with a—and a man got caught in one of their ditching machines there. Killed him, I think. Anyway, a terrible thing. It had nothing to do with us, but everybody had a shock from it.

But then we had our permanent facility built. They had decided by that time that this was not going to be a Station, it was going to be a Research Center, and they built us a very fine facility up at the far end, the north end of the base. Not the far north end, which was another Air Force facility even farther. All of this was distributed along the dry lake, which was, I think, about ten miles long, so it was a very big facility. We were midway, halfway up. So we had a nice facility built just for research. We had an office building with an attached hangar for the research airplanes, and on the other side, had another hangar for the loads work, where we would install instruments and do loads applications for calibration and so forth. Had a Flight

Operations Room on top where we'd have our telemeter data put on visible recorders for us and all the other support requirements. It was a very nice facility, or is a very nice facility down there. It's even bigger now, I understand.

WRIGHT: Did you see a lot of change in operations when you switched from NACA to NASA?

DRAKE: To NASA? Well, when space became a common word, so to speak, we were, of course, interested in it from the standpoint that our airplanes had been going to higher altitudes and were becoming closer to some of the requirements of space. The actual going into space, we were very interested in that, and when they started working towards manned space flight, we had a lot to say about that.

We felt that, for example, the capsule approach to our—I'm speaking now as representing the Flight Research people—we thought it was wrong not to give the pilot something to do towards controlling his destiny rather than just an on/off switch. So we were involved in all the discussions in planning these operations. We even put together a proposal, that didn't get anywhere, for the Manned Space Flight Programs to be operated out of the West Coast instead of the East Coast, because we felt that operating over land, you would have a lot better chance of observing the initial portions of the flight and so forth. We were shot down on that, of course. They said, "It's better to have the damn things crash in the ocean than to have them crash on land." As a result, they did crash on land.

We were involved in the manned space—we weren't involved in the unmanned activities at all, other than later when the X-15 had virtually completed its flight test program, they had a program where the X-15 was to be used as an observatory, so to speak. They installed sensors, one of them a telescope, as I recall. This was about the time that I left Flight Research Center and went up to OART [Office of Advanced Research and Technology], so I was involved in setting up the program, but I wasn't involved in it when it was actually under way. And they also had sensors on the wing tips to accumulate meteorite dust and so forth, since we were flying at altitudes above 200,000 feet.

The Manned Space Flight Program, we got involved in that from the very beginning, particularly as it developed into the [Space] Shuttle Program, into programs that required more pilot input than just the capsule. We initiated a program of flight test of the wingless—the lifting body program, and we had several—in fact—I'm trying to go through this as it occurred. Well, let me do the X-15 first.

When we got the X-15, there was three airplanes, and one of them, as I recall, one of them crashed, not destructively, but broke in half during the demonstration program. One of them crashed after I left the Flight Research Center. The entire program was a very successful program. We did it pretty much as it was planned. The one thing about it, the initial part was somewhat delayed because the intended engine, the LR-99, was slow in development, actually was behind the airplane. We always had the comment, it was quicker to build an airplane than to build an engine. So we flew the X-15 initially with four of the X-1 engines, which were four-cylinder engines, so we had sixteen cylinders, so we had considerable control over how the airplane operated. We could do a graduated flight test program very easily and very well. That program went very well. We had no difficulty, as I recall. Stability- and control-wise, the airplane behaved the way it should.

It was designed to fly, or to operate, in a vacuum, so it had reaction controls, little rocket controls. We had built a facility for testing the reaction controls in the loads hangar. It was an iron cross affair. Had the same inertia characteristics as the X-15, but was built out of I-beams, and the pilot sat in a seat on one end of one I-beam, and it had the reaction controls at the various locations, so we could give the pilot a little bit of training in how the reaction controls would work in the absence of an atmosphere.

The X-15 required a lot longer range facility than we had had before, and as a result there was a three-station radar research range established that led all the way up to the northern part of Nevada, and on flight tests, depending on the performance required, the B-50 would fly up to the northern end of the range and launch there, and the X-15 would fly, I think it was, 200 miles and land at Edwards. There were several dry lakes selected on the way down from northern Nevada. In case of malfunction or problems, the X-15 could land on one of these dry lakes, so this was a considerably greater facility requirement than we had had on any of the other research airplanes.

We never had any real trouble with the range. A few times we tracked unknowns from Area 51, and we had a few reports of unidentified flying objects. We probably established a few.

I'm diverging, but as I think of these things, I'll mention them. On an F-104 flight that Milt [Milton O.] Thompson was flying, the interconnect between the [flaps] malfunctioned, so he had a situation where [to maintain wings level flight he had to keep] increasing speed. He increased speed, but then he could see that he was going to be flying supersonic, and there was no cure for his problem, so he ended up ejecting before he went supersonic and bailed out and landed out in the desert. Picked up his parachute and walked out to the highway and met some people there that were yelling, "Hey, there's an airplane crashing around here."

He was carrying his parachute at the time, so he said, "Yes, I know."

The development of the lifting bodies, we had been watching the lifting body tests that Ames was running on M2-F airplane or vehicle. We designed one that was exactly the same as theirs. Our Director at the time, Paul [F.] Bikle, was a glider pilot. In fact, he held the altitude record at that time for sailplanes. When we got interested in the M2, being a flight operation, we figured, "Gee, how does this thing fly? After all, it's designed to make a lifting entry, which infers that it would be piloted. Would you be able to land it? Would you be able to fly it?"

So Paul says, "Well, let's build one. We'll built a lightweight one and test it." So he had some friends, some sailplane builders, in the area, and we went and talked to them, told them what we wanted, described the thing. As I recall, it cost $8,000 out of our [petty cash] to have them built this thing out of plywood with a fixed landing gear, designed to be towed by an automobile. It had a flying speed, I think, of about fifty miles an hour, something like that. So we towed the thing, and it flew. Not well, but as well as one of those things can be expected.

About that time—and we were in the process of proposing a flight test program for a high-speed one of these things, powered with our old faithful X-1 engine. About that time, as I recall, there were our usual hearings at [NASA] Headquarters [Washington D.C.] for budget purposes, and someone of the congressman's staff or congressman asked the Air Force man about lifting bodies being flown out at Edwards, and he didn't know anything about it. So naturally it was a question for, "Does NASA know anything about it?" And NASA did know something about it, except NASA Headquarters didn't know anything about it. So, fortunately, I was not in that flap. Paul had to take care of that part of it.

But we did get some attention from it. Our proposal for flight test vehicles was accepted, and we had a competition for the design and construction of these M2-F2—that's what we called them—vehicles.

We had a competition with the various aircraft companies for the construction of this vehicle. Northrop [Corporation] won this competition. In fact, it was—I guess at this time I can release some of the few odds and ends—North American [Aviation, Inc.] really wanted the—they felt they were in the research airplane game, since they built the X-15, understandably, and they proposed to do the whole job for a dollar or something like that. Their proposal was a good proposal and the price was right, but Northrop came in with a proposal that was technically better than North American's. They proposed using all the first-line control system equipment that was being used on their latest fighter, so consequently it was a much more technically acceptable proposal. Of course, accepting that proposal, which was, as I recall, $3 million, meant I had to go to Headquarters and discuss the fact that we weren't taking the minimum price thing. As I recall, that's the only time I had to argue technically an advantageous proposal.

Northrop built some very good equipment. The only problem that we had with the M2s, as I recall, was one of the flights, the landing, the airplane was [an] uncontrolled for rolling oscillation as it landed, and it rolled over. The airplane was essentially a round airplane, flat on top, and it just rolled over and took all the projections off, and the pilot—I have a bad memory for names, and I'm embarrassed that I don't remember his name. Well, the pilot was injured. He lost an eye, I think.

WRIGHT: Was it [Bruce A.] Peterson?

DRAKE: Yes, Peterson. Why did I forget that name? Everybody around is named Peterson. [laughter] That accident probably had more publicity, inadvertently, than any other one, because the bionic man program [*The Six Million Dollar Man*], every program showed the movie of that accident. It was very embarrassing, and it continued to be embarrassing.

Okay. About that time, I was transferred from Flight Research Center to OART, where they had set up at Ames a branch of the Headquarters Aeronautical Research and Technology Office. I was the Associate Director for Aeronautics of this office.

Oh, one thing I should mention. Just before I left, this was when they were setting up the Apollo Program. I was involved in that, to some extent. I originated an idea for a—once they started talking about a lunar landing, I felt that there should be some kind of a flight vehicle so the pilot could gain some experience in making landings on the Moon, and suggested a vehicle that was not a flying vehicle, except for being supported by an engine that would provide the five-sixths of a gravity that was not available on the Moon, and the pilot would land under the one-sixth's gravity. So this proposal, or suggestion, I should say, was accepted by NASA, and the Bell Company built three of the vehicles. [Neil A.] Armstrong has been very complimentary in saying that these vehicles were very useful, which I really appreciate, since he had to eject from one of them.

I was involved in the Apollo from the standpoint in the initial planning of how do we get to the Moon, do we go by streetcar, do we go by rocket, or do we go by ferry or what. [John C.] Houbolt had one group there for evaluating the potential of the lunar rendezvous approach, and I was on his group. We made comparative studies of other approaches. One of them, as I recall, was Earth orbital rendezvous, and you end up with six rockets standing there. We called them phone booths, because they were like the facility down there. Six of them standing there. They'd all take off and they'd all get together and—no way. So we were in Washington, as I recall, what was it, a month, six weeks? Terrible in the summertime. That was a real decision, never to go to Washington to work.

Anyway, about that time I was transferred to OART at Ames. [Clarence A. "Sy"] Syvertson induced me to do it. I was doing advanced planning at the Flight Research Center, and his sales pitch was, "Why plan just for the Center?" He says, "You can do it for the whole agency," which was a slight exaggeration.

At OART, I had a great bunch of fellows. There were, as I recall, nine of them. Any one of them were better engineers than I was, and I told him that. I told Sy this.

He said, "Well, they would know that what the curve said was right, and you would know what the curve said was wrong," he says.

So I said, "Okay, as long as they don't ask me to set it up that way." This was just about the time when computers were really coming in, and I was fortunate in that I didn't have to learn to use it. I had nine guys that were good at that, and in fact, one of them is on the committee here. Tom [Thomas J.] Gregory was one; he's always bothered us. [Laughs]

ELEANOR DRAKE: Dick [Richard H.] Peterson was also one of the young men, who eventually was Director of Langley.

WRIGHT: Really.

ELEANOR DRAKE: So they were all top-notch.

DRAKE: They were all crackerjacks.

ELEANOR DRAKE: Yes. All went on to do important work.

DRAKE: It was really good. We did studies there. We would do an in-house study to guide contract studies, and we studied everything that had to do with aeronautics. We made studies of light airplanes. We made studies of hypersonic launch airplanes, hypersonic stages for lunar or orbital transport. So we had hypersonic transports using hydrogen. Everybody talks about hydrogen cars. We studied hydrogen airplanes, and the big thing there was hydrogen is great, but it ain't free. You've got to make it somehow. After all, water is a lot of hydrogen, but how do you keep it from being linked to the oxygen? So we did system studies on that. We had a contract study with Boeing [Company] for a transonic transport airplane using the oblique wing design by R. [Robert] T. Jones. That was a very interesting program.

We did some studies on light aircraft that ended up being very optimistic. We could see that the light aircraft had a potential of being a major transportation thing, but we overlooked the fact that the lawyers wouldn't allow it. You get one crash, and it kills a whole company. So we overlooked that one. But the studies of hypersonic aircraft, hypersonic launch aircraft, and that sort of thing, are still in the, you might say, visual stage, and ultimately, there will be, to our estimation, vehicles of that type.

I was in the OART function for—I forget—about four years, and they were in the process of deorganizing that, and I was transferred to be Chief of the Aeronautics Division at Ames, the Wind Tunnel Division, as they called it, which was an excellent division and I have nothing against it, but it wasn't my line of work, and I retired from there about three years later or something like that.

ELEANOR DRAKE: [19]'75. '74, yes.

DRAKE: As a surprise to my wife. I came home one day and told Eleanor that I'd retired. This was—

ELEANOR DRAKE: Well, he was thinking about it.

DRAKE: This was in the Christmas season. I had something for her, I told her. I stayed on for another three months during transition to get another Division Chief, so that was about it.

WRIGHT: Sounds like a very full career.

DRAKE: After retiring, in fact, I was in the process of building two airplanes. Anyway, I was building two BD airplanes, a BD-4, which was a four-place airplane, and a BD-5 single-place airplane. We were moving over to Aptos [California] on the coast. I had the house all designed, and we moved in, and the BD-4 was under construction. I had a room for it. We pulled the thing up on a little hoist and moved the airplane into the shop.

ELEANOR DRAKE: We built on a hill, and we had to move it up.

DRAKE: Moved up on this—and about a month or two after we moved in, I got a note from the FAA [Federal Aviation Administration] that I couldn't renew my pilot's license, so I had to get rid of both airplanes.

ELEANOR DRAKE: He was on medication for a heart irregularity.

DRAKE: Yes. They didn't mind the heart; it was the medication that they didn't like. So I sold the BD-4. The fellow, [Seth Anderson,] that I was building the airplane, the BD-5, with finished the airplane and flew it quite a while. He was a hang glider pilot at Ames, and everybody at Ames knows him.

WRIGHT: You will.

DRAKE: Okay. You don't need to know what I did in retirement, did you?

WRIGHT: No. You may not want to tell me everything you've done in retirement. [Laughs] But I promised not to take all your day, so I thank you for all the time.

[End of interview]