# NASA JOHNSON SPACE CENTER ORAL HISTORY PROJECT
# ORAL HISTORY TRANSCRIPT

JACK FUNK
INTERVIEWED BY JENNIFER ROSS-NAZZAL
HOUSTON, TX – 30 OCTOBER 2003

ROSS-NAZZAL: Today is October 30th, 2003. This oral history with Jack Funk is being conducted for the Johnson Space Center Oral History Project in Houston, Texas. Jennifer Ross-Nazzal is the interviewer, and she is assisted by Sandra Johnson and Rebecca Wright.

Thank you for joining us today, Mr. Funk. We really appreciate it. I'd like to begin by asking you what led to your interest in aviation and engineering.

FUNK: Well, my high school grades led me to engineering, because in high school I was at the top of the class in all sciences and math, and near the bottom in everything else, in other words. And so I graduated from high school in '39, right at the beginning of World War II. I was looking through a *National Geographic* magazine one day, and I looked and here was an advertisement for a school that taught aeronautical engineering.

Well, I was born in a little town called Mercersburg, Pennsylvania, and there was only about a thousand people there. So all my other brothers and sisters had grown up and gone away, so I was wondering maybe that would be a good subject for me because of my high honors in math and physics. And so I asked my father whether I could go to Tri-State College [Angola, Indiana] and then go to Indiana to study aeronautical engineering. Well, he thought that was a pretty big subject for—I had six other brothers and sisters, and only the two girls went to [college for] two years to—what was it? Well, to college to get a teaching degree, and the rest of the boys did not do that.

So he sent me to the Mercersburg Academy [Mercersburg, Pennsylvania], which was a famous boys' school that he was superintendent of grounds and buildings at—so he could have access to all the instructors to find out how good I was. Well, to make it short, my final exam in physics, I made a ninety-eight.

ROSS-NAZZAL: Fantastic.

FUNK: And they took off two points because I couldn't spell the word brine [salt solution], once for each time. And that's my low subject, spelling. I can't spell worth a darn. I can spell the word once sometimes, you know, and the next time I can't spell it.

But anyhow, I went off to school to Tri-State, 1940, and it was on the quarter system. Well, I went for the fall, the spring, went home for the summer, come back in '41, and it was so hard getting started, that I asked if I couldn't just go—they were on the quarter system—I'd just go straight through and get it over with, which was a good decision. And in 1943 sixty years ago, last March, I graduated.

As I was walking down through the administration building, somebody said, "National Advisory Committee for Aeronautics is in there interviewing for employment." So I stepped in the door and walked out with a job, just that simple. I packed up my clothes and left for Mercersburg.

Of course, I thought I was going to get drafted, even though I checked in with the draft board and they said, "Yes, all your buddies from school [are over there]," from high school.

High school was real small. We had about 120 students in the whole school, of which thirty-six were in my class, and they were over there. So I sent a telegram to [NACA (National Advisory Committee for Aeronautics)] saying, “Well, I’m going to get drafted.”

And they sent one back and said, “Come to work immediately.” [Laughs]

So I ended up there in March of 1943, and they assigned me to the Loads Division, Aircraft Loads Division. The first three years was not very much to—well, it was something to talk about, but young engineer, you know, it’s a gofer thing. They assigned me to fairing pressure distributions that we were testing [from] Navy fighters, the loads on the wings of the Navy fighters during pullups. And we had a group of assistants, girls mostly in those days, because everybody else was in the Army, and they would plot all the points from the measurements from pressure distribution, pressures on the wing. But they said the engineers should do the fairing, because they knew more what the pressure distributions should look like.

So I took some little French curves, little curves, plastic curves, and starting going around there and making these curves through the points. And I thought, “You know, that’s going to take forever and a day. I wonder if I can do it freehand.” And so I tried it, and sure enough, I could just go “shhhttt,” like that. I must have done two or three hundred of those things.

And then they assigned me to loading the wing cameras for taking the flow. We had tufts of yarn on the wings, all over the wings. The pull-ups [would show] flow patterns through the tufts of yarns.

From that I went to the gust tunnel, to run the gust tunnel where we were shooting models through a vertical wind tunnel, and to get their response to what we called gusts, and I did that. Then I was assigned, when I was doing that, to what we called the Gust Loads Division, which is atmospheric loads on aircraft.

Well, in the spring of ’46, that went three years through that, Mr. [Philip] Donely, my Branch Chief, came in and said, “We want you to go to Orlando, Florida, to support the Air Force and the Weather Bureau on flying airplanes through thunderstorms.” We were flying five P-61s through thunderstorms at five different altitudes all at one time, tracking them with radar.

The American Glider Association showed up there. They had convinced the Weather Bureau that they ought to let them fly some gliders through the thunderstorms, and they had three Navy gliders that were made for training pilots during World War II. And one of the pilots was Paul Tuntban, who was a stunt pilot for the movies. He did some early movies on early flights of the Wright Brothers and some movies on hot-air balloons and things like that. I told him I wanted to go for a ride in a glider. So one day he come along and said, “Well, let’s go. Do you want to go for a ride in the glider now?”

And I said, “Yes.” So we pulled the glider out, got the Stearman biplane hookup to it, got in the seats, and it was still being instrumented for flights at high altitudes.

He said, “Wait a minute.” He ran into the hangar, brought out the compass, and said, “Here. Hold this.” [Laughs]

So we took off and look[ed] around for a cloud, we turned the Stearman loose, we were going around in a circle underneath the cloud. And he says, “This doesn’t look too good. That one over there looks pretty good,” so we went over there, started circling, and we went up into the cloud. And I was sitting there, and he was saying, “Call out compass headings,” because he wanted to go in a circle.

So I’d say, “North, north, north.” And then I looked down at the rate-of-climb indicator. It said 4,000 feet a minute, and that was the top. I looked at the altitude indicator, and it was going [demonstrates] 10,000 feet, 13, 14, 15,000 feet. I said, “Paul, we don’t have oxygen. We’ve got to get out of here.” [Laughs]

He says, “Oh, no. I think we can beat the world altitude record.”

I said, “No, we can’t. No, we can’t. Let’s get out of here.”

So he said, “Well, you know this country.” We pulled up. It had spoilers on it. He pulled up the spoiler, and we dropped down through what was beginning to be a thunderstorm, and he said, “Look. You can see down there, and you’ve looked at maps. Pick a good landing spot so we can land.” And the idea was to land, take the glider apart, haul it back to the Air Force base.

And then I said, “Oh, I see something great.”

He says, “What?”

I said, “I see a runway.” [Laughs] So we slipped down through [the clouds] and landed at the same runway we took off [from], and he just pushed it down and put it in the parking spot and we got out.

But that was my experience of flying into a thunderstorm. Fortunately, it had not built to the point of having lightning, because we had a wooden airplane. If we’d got struck by lightning—and that was the biggest thing at the thunderstorm project, except one other item of interest, it was just mundane, just load the instruments, take them out, and develop the film, fifty feet of film about two and a half inches wide, and send it back to Langley [Aeronautical Laboratory, Hampton, Virginia]. Well, when I walked out of the darkroom, I looked and said, “Well, now where am I going to dry this? They don’t have anything to dry stuff like this.” I was in a building. It was an old open-frame building they’d built during World War II to simulate conditions overseas, about twenty-five feet up to the rafters. And I thought, “I wonder. I wonder." Just like New Year's Eve, you know, you throw the things. I grabbed an end of [the film; it was] squishy stuff, and I went "zoom!" It went right up and down over [the beam]. [Laughs] So that's the way I dried the film.

Donely said—later on, I told him how I did it, and he said, "We were wondering why in the middle of this thing there was dirt."

And I said, "Well, it was [the] only [way to dry it]."

And he said, "Well, it went away after a while."

I said, "Yeah, I tried to put it over the same place all the time so that there wouldn't be too much dirt on it."

So in the fall of that year, of course, I left the thunderstorm project there in Ohio and went back to work. And then Donely came in again, and he said, "We want you to go to New York."

And I said, "What am I going to do in New York?"

He says, "The Navy has a project up there to develop airborne radar for airplanes. We want you to go up and take measurements of loads and see if they can spot the turbulence with the radar." And there again, same instruments. I forget how I dried the film up there. I don't remember that.

I do remember that I went back to Langley and then home for Christmas. On the day before New Year's—the day before? Two days before, maybe, I got a call from [Richard V.] Rhode. Dr. Rhode was my Division Chief. He says, "They're moving the airplane to Seattle [Washington]. How soon can you be ready to go?"

I said, "Now."

So he said, "Okay. Go down to Washington [D.C.] and get all your tickets and things like that, and go out to Seattle." So I went down there, got my tickets, and all of a sudden a big snowstorm came into Washington, D.C., and they shut down the airports. Well, I always—I don't stop. I just went down to the railroad and got on it. That was the wrong thing to do. It took two days for me to get out [to Seattle] on the railroad. But I spent New Year's Eve on the railroad going out to [Seattle]. This railroad was local. It stopped everywhere to pick up the mail.

So I got out there in Seattle, and the airplane was a cargo airplane. It was instrumented. And we flew back and forth between Seattle and Anchorage and Fairbanks, Alaska, and back again. When we got to Fairbanks, of course, and Anchorage, at least in Fairbanks, it was so cold that we couldn't turn the engines off. They'd freeze, you know. So we'd gas them up while they were running and [came] back.

One time on the way back, we had one of these storms like they're having right now, they're talking about, solar flares, and the airplane was flying through some of the ionization of the air about 10,000 feet. We were running around 15,000 feet. I looked out the window, and there was a ring a fire around the propellers.

But there was a lot of interesting things there. One time we got into the airplane to go up there, and this cargo airplane had no regulations like the commercial airplanes. The pilot picked up a tool, little tool holder, and he said, "I wonder why they left this in here. Take it on out." So they took it on out and put it in the shop there, and we went on up to Alaska.

[On the way] back, we found out why the toolbox was still there. They had been working on the de-icer system, and they pulled the tubes off of it, and evidently they weren't finished exactly and the guy went somewhere. We got into some icing, so the pilot turned the de-icer on, [demonstrates], alcohol [came] down in the cockpit. Alcohol. He turned it off. So much alcohol in there that they had to put on their oxygen mask to fly. And I thought, "Boy, all's we needs to have is a spark from one of those switches and the cabin's going to blow off."

Well, when we got back to Seattle to land, we had one inch of icing on the windshield. We had to land the airplane. [The pilot] had to land the airplane looking out the side window.

Another time we come back and it was fogged in in Seattle. We flew at 500 feet. Well, first of all, the pilot told me to get into the seat with the radar that we had, that we were using. We had two beacons on the end of the [runway] so that I could maneuver the radar to pick them up, and they could pick those two beacons up. We flew right down the runway at 500 feet, couldn't see it at all. We were headed for the Rockies, and the guy was getting a little bit nervous, so he pulled up and told the tower that we'd have to find someplace else to land. So, again, we were flying up there and the clouds parted and there was another runway right down there. So we pulled around, [slipped through the clouds], and landed on that runway.

[The pilot] called the tower and they said, "Well, we're on the ground. Find out where we are."

[The tower] said, "Near as we can figure out, you're on the new airport, which is under construction." Thanks. Nobody left the bulldozer in the center of the runway. [Laughs]

The rest of the program run was just like the thunderstorm project, load the films, develop and send them back, except the instruments started to malfunction. We have an instrumentation lab. They always sealed them up. You're not supposed to touch them, and so I took my—I had a tool chest full of tools by that time, and I cut it open and looked [at] what was in there, and they had put a steel gear against a gear made out of—it wasn't copper. It was an alloy of copper. Brass is what it was. And the steel gear was chewing up the brass gear. This is while we were in Seattle.

So I called them up and told them what was happening. They said, “Send the instruments back. We’ll repair them.”

I said, “I can’t send the instrument back.” I said, “Send me a handful of gears. They last about two or three flights. I’ll just run two or three flights and take the old gear off and throw it away and put a new one in.” So that’s the way we completed that project.

When I got back after that project, then the thunderstorm project was to go to Wilmington, Ohio, and I spent the next summer out in Wilmington, Ohio, doing the same thing, flying airplanes through thunderstorms. I had the distinction of spending the summer in Florida and the winter in Alaska. [Laughs]

After that, I was telling Mr. Donely that I thought we could get some good information out of taking two airplanes and making one of them laminar flow over the wings and one of them just the regular type of wing. He thought that was a good idea, and he interested the military at Wilmington to fix up two F-80s to do that. By flying them through low-level turbulence, you get lots of load [data].

So I went back out [to Wilmington] in ’49 with two F-80s, and we were going to fly in low-level turbulence. [The F-80s] were all instrumented just about like the other airplanes for taking [load measurements], and I had two pilots, Colonel Weaver and Captain Armstrong. Colonel Weaver, we became good friends because of this. They liked the project because it got—I said, “Now, the way we [make the runs], there’s a twenty-mile-long road and track between Wilmington, Ohio, and Washington Courthouse, Ohio. You set up, and one of you start on one side down and the other one start [on the other] side so you pass each other, and that way we'll get the same type of turbulence and load history on both airplanes." Well, they liked that.

Let's see. So I spent the summer out there doing that kind of research, and it worked out just like I had figured. You could plot the load history. The loads [show] the lift coefficients, and you could see a definite difference between the two airplanes because of the flow characteristics of the wind.

I [came] back from that in '50. The year of 1950 [was the year] I started writing reports for the first time. The first report I wrote was based on a rumor that I heard that jet airplanes are now starting to fly at high altitudes, and their lateral stability was [a problem. The planes] were starting to fishtail. One day I just sat down at my desk and said, ["What do I know about the problem?"] Well, this is where I started with the mathematics. I wrote a simple harmonic motion [equation] and then took aerodynamics and modeled the vertical tail. This does not have to be an accurate model [for] any particular airplane, because I'm going to do a study of the characteristics of the tail, to get the [required] damping coefficients and spring constant.

[The] report demonstrated that the higher you go, the less damp the oscillations, and the more difficult it would be for the pilot to fly [the airplane] unless you put in some kind of mechanism that adds damping to the system. They were then talking about trying to put artificial damping into the airplane, and they had some ideas of how to do that, but this report gave them the information that said you have to do it. They just weren't going to be able to fly up there. It's going to drive the pilots nuts trying to keep it from going [unstable. Funk demonstrates.]

I guess Donely was impressed with that report, because years after that, when I was down here [in Texas, I] went back to Langley and went down to see him, he says, "It's funny you dropped in right now."

I said, "Why?"

He says, "Remember that report?" He says, "I'm just mailing one out to an aircraft company." And he then said, "You know, nobody does work like that anymore."

Well, the next report I wrote, Donely came by desk and said, "I want you to do an analysis of the impact dispersions of the Polaris missile due to winds." He said, "They need to know how far the winds [are] going to be blowing it off course." And this was the submarine, Polaris, a missile that they fire from [a] submarine. They probably don't use it now. They've probably [have] upgraded [by] now.

And I thought, "We've got three statisticians here. Why doesn't he ask them?" Well, I went to Jerry Engle, [who] was a friend of mine. He had two statistical books that had more about statistics than you ever want to know, and I picked them up and looked through them. I was looking for information on sums and difference of numbers because I had a group of average wind profiles that had been done, which were statistically one for like a 99 percent, 80 percent, 70, one, two, three [sigma profiles]. And I was wondering whether I could use those [wind profiles] to do [the] analysis, and I figured out, "Yes, yes, they will work."

Then I went to Donely and said, "Well, can you give me the aerodynamics?"

He said, "No."

So I went back to my desk and sat down and said, "Well, I'm supposed to be an aeronautical engineer. I guess I can produce my own aerodynamics." So I went back and said, "Well, can you give me the shape and the weight?"

He said, "Yes, I can do that." So I took the shape and the weight and the wind profiles and what I thought I knew about statistics, and wrote a report. About six months [or] nine months after that report was out, [Dr. Robert R.] Gilruth called me over to his office, and he had that report in his hand.

And I said, "What do you want to do with that?"

He says, "Well, the Navy ran a thousand wind profiles through a model and they had all the aerodynamics and everything, and they got the same answer you did." He said, "Well, how did you do it?"

And I said, "Well, it tells you in the report how I did it. What you do know now, though, is that you can use that report during the design phase to do real fast analysis to get how the different design requirements affect it, and then when you get design finalized, you just do the thousand wind profiles to verify it, and it will speed up your design characteristics really quick." And that had some effect on what I did when I was working for Gilruth [during the space program].

Let's see where I'm at [refers to notes].

ROSS-NAZZAL: I think we're around the early fifties. You started talking about writing reports.

FUNK: Nineteen fifty to '55. That's when I was just talking about.

So the next thing is 1955, U-2, five years, 1955 until I went to work for the Space [Task] Group. And there's not a great deal that I can tell you about that, I guess. What we had there was an airplane that could fly three times as high as any other airplane in the world. As a matter of fact, it produced some of the sightings about UFOs [Unidentified Flying Objects]. There was a sighting about a UFO, a pilot that saw this cigar-shaped vehicle flying around about his altitude. He went over there to see what it was, and it went "zoom" [up and away. The pilot knew that no airplane could fly that high so it must be a UFO.]

And what my part and Tommy—what's his name? I've got it written down. Where is that? Tommy Coleman and I were assigned to the [U2 project]; we were cleared for top secret. It was fairly easy for me. They already had a complete history of me from the time I was born until that time, because they cleared me for confidential and secret while I was working for NASA. They went back home, and I got a call from my father saying, "Hey, the FBI's [Federal Bureau of Investigation has] been up here. The bank's called, and the FBI's been up here asking questions. The bank just told me to tell you so you'd know what's happening." [Laughs] In a little town like Mercersburg, everybody knows what everybody else is doing.

But we were cleared for top secret as the cover story. Every what you call covert—is that a covert action, U-2s?

ROSS-NAZZAL: I believe so.

FUNK: Every covert action has to have a cover story to cover it, especially when it's going to be obvious to the Russians that there's an airplane up there so high up they can't get to it. But we did actually do research. We were assigned to do research on atmospheric loads. At that time, airplanes were getting to altitudes of 30, 35,000 feet, and we had absolutely no information on what kind of turbulence [was] up there, you get some pretty severe turbulence, there's just no clouds or anything, because of wind shears. You're getting up to the altitudes where the winds are pretty high at times and they have shears [that] you can't see. As a matter of fact, there was at least one news item here recently about hitting some pretty high-altitude gusts. But we were actually doing research and writing reports.

As a matter of fact, on the report side, from the thunderstorm project, I got a citation for writing the most reports of anybody at the Center. And I told Donely, "That's not fair.

He says, "Why?"

I said, "All's I had to do, it's data reports we were sending to the Weather Bureau. All's I have to do is write a cover letter, which says about the same thing every time, and put it on there and send it out. I said, "Like the other reports I'm writing take years to do, it takes maybe six or seven months to do it, and it takes at least a year to get it through the editorial process."

He said, "Well, that's the way things go."

But that's about all I can say, I guess, about the U-2 project,. I left the project, '58, '59, after about four years. About six months after I left, they shot one down, and I realized that that airplane could outmaneuver that rocket if they had seen it launched. Well, after about four or five years, things get, like everything, complacent. The pilots that go up there, they set the compass and the automatic pilot, and it just flies for eight hours, takes pictures. And they probably were reading a book or something.

ROSS-NAZZAL: That's unfortunate.

Why don't you tell us a little bit about your involvement with the Space Task Group.

FUNK: Okay. The Space Task Group was what I started in '58. And let's see [refers to notes]. I joined them because at the time that I joined them, they were going to go up to a facility near Washington, D.C., the Goddard Space Flight Center [Greenbelt, Maryland]. But when I joined them, they wanted me to go to the Operations Division because of all my experience with the Air Force and everybody else and going around like that. They were going to have a lot of contact with those people. But I told them, "I've already flown about a million miles."

I took a trip to Turkey, over to Germany and then from Germany to Turkey because they were having trouble with the fogging of the windows. I actually carried some instruments with me, and when I landed at the airport there at the Rhein Main Airport in [Frankfurt] Germany, I had to go through Customs. And I had these two instrument things all packed up like that, and they wanted to open them up. And I said, "Don't open them up. They have instruments, and we're going to have to pack them up again," and everything. And I was arguing with him.

And all the people finally said, "Oh, let him go. What's the difference, you know? This is just—."

So I pulled a little card out of my pocket and said, "Here." And it said I'm a major in the Air Force. I was carrying—they gave me another card for the U-2 project, so that if I'd get captured, they'd have to treat me as a military man.

And I need to then tell you another thing about the U-2. At the very beginning, they took us up to the CIA [Central Intelligence Agency] Headquarters in Langley, Virginia, myself and our test pilot Mel [Melvin N.] Gough, and they took us into a room and they set us down. Of course, I knew what project it was. Mel didn't know what was going on. All the way up there on the airplane, he was punching me, "What's this all about? What's this all about?" And I didn't tell him a thing, didn't tell him a thing.

And what they did is they had a curtain up there, and they pulled it open. There was a big picture of the U-2, and they said, "What can you tell us about this airplane?"

Mel Gough looked at it and said, "That airplane was designed by Kelly Johnson of the Lockheed Aircraft Corporation."

And they just stopped, and they said, "Is it that obvious?"

He says, "Yes, anybody that's in the research or anything doing with airplanes will recognize who designed that airplane."

They said, "Well, I guess we can't disguise it, so we'll just let it be a [NACA] research airplane." So it carried the NACA logo all the time it was flying.

So when I joined the Space Task Group, they wanted me to go to the Operations Branch, but I wasn't interested. I told them, "I've been flying for years. I've rolled up at least a million miles," and I wanted to stay home for a while. Besides, I wanted to go back to the report writing type of work, the technical and mathematical type. So that's when he assigned me to Bob [Robert G. Chilton] in the [Dynamics Branch, Flight Systems Division]. …

Well, I didn't really have any assignments to begin with, so to speak, and they were in the process of going to fire a test vehicle to test the escape system they were putting on top of the Mercury and, I think, the Gemini. I don't know if we ever used it on the Apollo or not. But then they had a vehicle there called the Little Joe vehicle that was going to fire [the Mercury capsule] out over the water and then they would pull the [capsule] off of [Little Joe].

Rod [Rodney G.] Rose—I think he did one of these—was one of Gilruth's assistants, and I asked him one day, "How are you going to know how to set the right azimuth and elevation so that you put that downrange where you have some idea where it's going to go if you've got winds out there at Wallops Island [Virginia]?"

He says, "I don't know."

I said, "Well, I think I can develop a set of charts that you can use to read off the wind profile where you get the average winds, and it will tell you the right [azimuths] and the declinations to set the launch system with."

So I spent about, I guess, about six weeks, produced a set of charts, and I gave them to Rod Rose. One day Chilton came in and said, "You know, Rod's getting a lot of notoriety out of those charts," as if he was taking all the glory for it or something like that.

And I said, "Well, you know, Gilruth knows that Rod doesn't know how to do that, and if he doesn't know that I did them, you can tell him." I said, "I could go out there and sit underneath the tree like thunderstorm project where I spent a whole month doing nothing but going to the Officers' Club and having a good time and drink Scotch and do that for you, but there's too much work here to do for that. I don't care whether Rod gets all the notoriety for that. I just care what you and Gilruth know."

Let's see. Then we moved to Houston. But before we moved to Houston, I guess Chilton came into the office, said, "Gilruth wants you to look to about going to the Moon and back." So by that time, I was a Section Chief. Had a section, pretty good people, and called them together, and I said, "Well, we've got a new project to work on, but we're not supposed to tell anybody, because there is rumors about such a project, but all kinds of controversies, you know." Like there's somebody that's against everything.

So I called in the staff and Victor [R.] Bond, Tom [Thomas F.] Gibson and a few of them, and Ellis Henry there, told them what we were going to have to do, and they said, "We don't know anything about the Moon."

I said, "We don't know anything about going into orbit either." I said, "That's our job, to find out how to go to the Moon." And so that's where we started out.

Let's see. How did this go? The next thing I did was the entry analysis, the next thing I did of real importance. I was always doing little things like taking a simple equation and doing the propulsion requirements for the Atlas vehicle reserve propulsion that you needed to fly it because of the dispersions in the system. The rocket motor is not going to be the same, and everything's not going to be the same. You've got to have it. I'd do things like that.

But the next important thing I did was the entry analysis. Bob [Robert O.] Piland, who was another of Gilruth's assistants, every time he saw me in the halls he said, "Nobody knows anything about the entry, whether you can come down and then you'll skip out and not come back or whether you'll skip in." At that time, we had a land-landing requirement, had a range requirement of 5,000 miles from entry point to landing, a variation of 5,000 miles.

Max [Maxime A.] Faget was saying we want to go with a capsule-type system. Everybody else was saying, "You're going to have to [have] wings." Max knew that if you put wings on it, it was going to be too heavy to fly, and there was a big controversy about that.

Somebody told me, "Well, they're going to have a symposium of all the people in NASA and anybody else that is doing research on entry," and he did that time after time. We [had] an Aerodynamics Section.

So I just sat down and thought, "Why don't I see what I can do." So I took one of my assistants, and I started laying out [entry] simulations. By that time, I realized what digital computers could do that nobody else could do. And I said, "They're never going to solve this analytically with equations, at least not by time enough to choose a [configuration]." So I set out a series of runs for him to make on a program that we had, which did three-degrees-of-freedom launch trajectories. [I] parametricized the lift-over-drag coefficient, the other [aerodynamic] coefficients that [produced] force for entry.

And I knew what I was looking for. I can't explain to you what it [was]. It was a curve that would come over here like this and then go shooting up that way [gestures], because that would give an entry corridor of so many miles that would allow us to do the range characteristics. And I plotted up a function of range and other characteristics, corridor widths and things like that.

I had a staff. We had a staff of people that could make good presentation slides. They were professionals, and they drew up all the graphs, made all the slides. All's I had to do was look at the information coming back, the graphs they were showing back to me, make decisions to what other runs to make, until they had a presentation that I knew was accurate; that is, it showed you the characteristics of the entry, the type of aerodynamics you need to do what they said to do. And then I went and told Piland, "I will give a presentation at this conference," what you call conference presentation they were going to have. It was going to be a public presentation.

They always have a rehearsal, all right. I'd done this before. I'd given presentations before.

Okay, let's jump back. Okay? We used to have a presentation at Langley Research Center [Hampton, Virginia]. They were the best school I ever went to. My education came really from Langley. Every month, [a] division had to give a presentation of what they were doing, just like you went out and give a presentation at one of these symposiums. And you had to go through that. You had to go through the whole thing like that. So I'd given presentations before. That wasn't any trouble.

So at this one, they put me last on the program. I know what's going on. So I got up, gave a twenty-five- to thirty-minute presentation, and sat down. And there was no questions, absolutely none. The symposium leader, which was one of Langley Research Center's Division Chiefs, said, "Where did you get all that information?"

I said, "Well, I [ran] it off on the computers here in the last month."

And that's all he said. But Max Faget came running over and sat down beside me, big smile on his face. He says, "You know they're not going to let that in the public presentations, don't you?"

And I said, "Yes." I said, "I didn't have to give it here. I could have give it to you privately, just put it over there." I said, "I did it so everybody else knew that they couldn't argue with your decision anymore. Whether it goes in public presentation or not, you've got the information. Let's go do the job."

Lunar descent trajectories. Chilton came into the office, said, "We need a descent trajectory to give to the contractors." I think that was after the project, and we went through the contract selection. Well, I had to do that, too, but I don't know what you'd say about that. That's all read the manuals and see which contractors say do the best one, you know. How do you pick it?

We did a fairly good job at it. But what they did do is pick a contractor, and this is what they should have done, on the base of its past performance. They picked the contractor that did the X-15 job, because they took an airplane, designed one that could fly faster than the speed of sound, and were successful.

But anyhow, Chilton said we needed lunar descent trajectory to give to the contractors. I said, "I don't have anything that can do that yet."

He said, "Well, we have to have one."

So I said, "Well, here again, what do I know about it? What do I know about an optimization of a trajectory optimization anywhere?" I know what it would be like in a free gravity, out where there's no gravity; it would be a straight line. The lunar gravity is pretty small. So I picked up my slide rule. I didn't do exactly a straight line. I did more like a parabola, so it would go down this way, you know, [gestures] and computed a lunar trajectory and gave it to him, knowing full well that this is now a race to get the information before they needed it for design. And that sufficed until I took Don [Donald J.] Jezewski and told him, "Well, we're going to produce a study of lunar descent trajectories," and I think we did that, I don't know, it was around the time we moved. We had some information. We had a report that gives some optimization theories for this type of mission, and Don programmed it up on the computers.

By that time, we had FORTRAN. That was another big thing that came along while we were at Langley. We were using the Langley Research Center computer, but IBM [International Business Machines Corporation] came in with FORTRAN. At that time, I was given the job to interview computer people like IBM and Unisys. Unisys had the best computer. IBM had the best software. I had to tell the Unisys people that I was not interested in what was in the black box, only what I could put in and what I could get back out. And IBM had FORTRAN, and that is essential to the project, so we bought IBM computers, of course.

But anyhow, Jezewski came back after he programmed it and said—I think we picked up part of that program from Huntsville [Alabama, Marshall Space Flight Center]. And he said, "You know, I'm having an awful time coming down. It either ends above the Moon or below the Moon, you know." He said, "I don't know how I can do this study."

So I thought a little bit and I said, "Well, you don't have any trouble doing lunar ascents, do you?"

He says, "No, that's pretty easy. We can convert it to different altitudes pretty easy."

And I said, "Well, why don't you do an ascent and start with empty tanks and then put the fuel back on. Instead of putting it in, put it on, just do a reverse, like they do in the movies, you know, they back everything up." That worked very well, and we did get sufficient information for producing what was required, was a maneuver budget, a budget of the velocities required for the various systems, the maneuvers they were going to have to make to go from the Earth to the Moon.

At that same time, we were going to have to produce a budget for the service module to get out of Earth orbit, the Saturn third-stage to get out of Earth orbit, go into lunar orbit, and come back to Earth. We had no program to do that. So I talked to Tom Gibson. I said, "Well, we have the lunar solution for two bodies, two-body solution for the Moon and the Earth when the Moon's in a circular orbit." I said, "That has a first integral that's solved that produces velocities, a velocity profile work for those two things, that had been published, that can let you look at something of the velocities." I said, "Take that and it consists of two parts, the one about the Moon, the one about the Earth. And there's a spot in there that if the velocities are too low, they don't either go to the Moon or they don't leave the Moon, come back." I said, "Take where that opens up, so you have trajectories from the Earth to the Moon."

And he did that. That wasn't too hard. He could do that almost at his desk. The equations were that simple. He came back and said, "Well, I have it. What do I do with it?"

I said, "Add 10 percent to it and publish it as a requirement."

ROSS-NAZZAL: Is this what was known as the matched conic trajectory?

FUNK: No. That's the next—[the equations were from a solution of the equations of motion of the Moon orbiting the Earth which was published years before the Moon project].

ROSS-NAZZAL: That's the next step?

FUNK: That's the step. That step. That's what we needed. That's not what we had. But that's the Delta-V budget that we published, and that's the one that was used. I figured that 10 percent [would provide mission flexibility]. We did a few other things. We did a few other things [with] the descent trajectory. I had them put in some safety measures about what should you do if you had to abort, some safety measures which increased the Delta-V budget. I wanted to leave them there so that I could take them back out sometime later [if necessary].

Well, that was the start of the matched conic program. We weren't in the Center; we were up in some building on Telephone Road when I assigned Tom Gibson and Ellis Henry to do a lunar trajectory program, you know, the mission program.

Now, there's another story about this. Dr. Gilruth took me up to Washington, D.C., on his plane, I think, to—oh, yes. He was going up. He asked me to go in his plane, to go into a review of a contract they were going to give to Bell Laboratories, the great research outfit of AT&T [American Telegraph and Telephone Company], to do mission studies to the Moon. There was a big controversy with [NASA] Headquarters [Washington, D.C.] and the [Manned] Space[craft] Center as to whether we had the technical capability to do [the mission studies].

There was somebody up there that wanted to take over and run the [program], actually, up there. It was nonsense, really nonsense.

And so I went in and listened to this proposal that they were making [f]or the assignment for the contract. [Headquarters was] giving [Bell Labs] a million dollars to do a study for one year sixty parameters of trajectories between the Moon and back so they [could] piece them together, using a program that they had written, Bell Labs had written. In those days, we talked about real-time programs. The computers were so slow, that doing a launch trajectory for a launch vehicle took more time than it took to launch it.

Anyhow, I listened to the whole presentation, a couple hours, I guess. I don't know how long it was. And then I said something that I thought was, you know, just stupid, but I said it anyhow. I said, "It's my estimate that it would take you 365 years to complete that study, [it was] exact estimate." One year later, they had done one day of trajectories.

Well, that was [during] the time of when Tom and Ellis were working on the trajectory program, and Tom was an expert mathematician. When we'd go out to lunch together, he had a book of math problems that he would sit there and solve as a hobby. Well, Ellis used to run a restaurant before he became an engineer, a very practical kind of guy. So I stuck them together, and they went in.

In that day, we had punch cards that we used to input to the machines. And so one day Chilton came in and said, "People are complaining about Tom Gibson."

I said, "What for?"

He said, "He comes to work late, sometimes goes home early."

And I said, "I really don't care much about that. The progress they're making is so good, even if he stays home, it's all right with me."

He said, "Well, why don't you [speak to Tom]."

So I went and said, "Tom, whatcha doing? [I] said, "They're complaining about you showing up late."

"Oh," he says, "I go down to the computer system," that was then in the University of Houston's computer or something like that. It was down [on the UH campus] somewhere. He says, "And I take the guys a couple beers and sit down and talk to them and somehow my work comes out first." [Laughs]

I [said], "Well, don't make it so obvious."

So I was looking for what they were doing. I went in to see what they were doing, and they were really very shrewd. They had these punch cards labeled. They were taking each subsection that they needed, subroutines like that, they would program it, they would write a program to test it, which is what I do when I wrote programs now, they stick it in there and go down and do like that. They were making huge progress. I mean, just, you know, like take months to days, even that fast.

But they came in and said, "You know, we're having a lot of trouble."

I said, "With what?"

He says, "The libration points on the Moon." You know they move around. The Moon does like this [demonstrates] because it's in an elliptical orbit, and therefore it does not keep the same side to the Earth all the time. Well, I mean, it's the same side, but it moves around. "And we can't get more than three-place accuracy on that." See, they were testing and everything.

I said, "Well, there's a telephone."

He says, "What?"

I says, "There's a telephone. Call the [U.S.] Naval Observatory in Washington [D.C.], and they've got the person up there who did that program. Tell them who you are and ask them what the problem is."

He says, "Oh, they won't talk to us."

I said, "No, they will. You tell them you're Tom Gibson with the Apollo Program and you want to talk to [E. C. Hubbard], he will come to the phone right away," which is what he did.

He said, "Oh, you're getting three points of accuracy?" He says, "That's pretty good. Those equations aren't that accurate." [Laughs]

So we produced the program, and the program was a mission design program. It was not a kind of program that anybody else had written. You put in the characteristics that you wanted, flight time out, flight time back, stay time at the Moon, and that part was just that part. That wasn't the descent. Stay time at the Moon and inclinations, all kinds of characteristics that you wanted to fly, and it printed out the characteristics of the trajectory.

Later on, we hooked it up to an integration program, so it could produce the time history also. So we did a study of lunar trajectory, which we put also the descent information with it and a bunch of landing sites, longitude and latitude, which produced the mission characteristics for landing on [the] Moon in parametric style, you know, showing what it was going to cost in propulsion to get to all these different landing sites.

I had some contact with Gilruth that told me that's the kind of [data] he wanted, so I put the report together, and that report had in it the fact—by that time we were in the design phase, and weight statements were coming out, and the weight statements were problems. That report had in it an estimation of the weights that we could deliver to the surface of the Moon, which I immediately stamped "confidential." Took the report over to Gilruth's office, give it to his secretary, told him, "Give this to Mr. Gilruth. Nobody else, nobody else, is to see it but Mr. Gilruth." I didn't tell him anything else, I hope, because it had in information that allowed him to manage the weight statements, knowing full well he could allow the weights to grow by a certain amount before he'd get into any real problems.

I even went up with him on his plane to Headquarters, to give a presentation, because now, he could tell Headquarters, "Go away. Leave us alone. We don't need any of your help at all to do this." But I didn't include that weight data. That part of it was not in there. The part of the weights was not in any other report, and I don't know that there were any other copies to that report that I gave Gilruth.

We showed them what we could do, and when I got back, one of the Headquarters people, I think his name was [Joseph F.] Shea—I'm not sure about that—he showed up. And we were still on Telephone Road. He wanted a copy of the program. I said, "Fine, I'll give it to you. Just go over and get Gilruth's permission." [Laughs]

He never returned, but he did offer Tom Gibson a job at Headquarters. Tom came in and asked me, "Do you think I ought to go up there and take that job?"

I said, "Well, you'll get more money, but it will not be a permanent job. Once they get what they want out of you, and that's the program, then you'll probably stay there, but when the project's gone, you'll be gone for sure. But likelihood that you'll have a long service up there was pretty small." I said, "All they want is that program." So he didn't go.

ROSS-NAZZAL: I also understand you did some other work with NASA Headquarters. I understand that there were some disagreements between North American Rockwell [Corporation] and NASA over lunar orbit rendezvous.

FUNK: Well, that wasn't my disagreement; that was the project disagreement. [John C.] Houbolt had written a report on lunar orbit rendezvous. The whole project was slated toward direct landing and direct launch on the Moon, rate in, rate out. Gilruth came down and said, "Look. You owe this a look." He said, "You owe this a look." So the whole project dropped what they were doing on the design direct lunar landings, and went into a, I guess a couple-months' study of lunar orbit rendezvous.

Well, again we went back to what are the portion requirements. But this was done before all this stuff that I was telling you. This was done when we were at Langley.

ROSS-NAZZAL: Okay.

FUNK: And we started putting down the different propulsion requirements the size of these vehicles that we were going to have to build. And even then, we were talking about just building the C-2, which was much smaller than the Saturn. And these vehicles started to be gigantic. We'd get to look at them and look at them, and everybody's saying, "You know, that's pretty maybe impossible. So the size of the vehicles are growing to the size that we're going to really have troubles designing, so we'll have to chance it."

And I can just jump into another—jump back later on when we had the Gemini Program running. We put the Gemini Program together to do something while we were getting the

Apollo system designed and built. Chilton stuck his head in the door and said, "Nobody knows what to do with the Gemini Program."

… I said, "Do the lunar orbit rendezvous. Give us some support that we really can do that." That became [the] main problem. "Just show that we can rendezvous." Then we can put that part to bed and have more confidence on what we're doing.

ROSS-NAZZAL: If you don't mind, we need to actually take just a quick break to change out the tape. I don't want to miss any of your stories.

FUNK: Well, I didn't do a lot of work on the Gemini Program, because that was [during] the Apollo phase. That was just to keep the public interested in space. Management thought if we just went into—what did it take? Seven years before the first launch? Congress and everybody would lose interest and it'd be gone. So they did the Gemini Project, which, well, it did do a very good thing; it provided the engineers and the people who were doing this with some information that, yes, we can do lunar rendezvous. We won't get somebody up there and lose them.

ROSS-NAZZAL: I understand that you had some experience working with the Apollo guidance computer.

FUNK: Yes.

ROSS-NAZZAL: Would you like to talk to us about that?

FUNK: Okay. They sent me up to MIT [Massachusetts Institute of Technology, Cambridge, Massachusetts] to listen to a start-up of the program that MIT was going to build and program the guidance computer. Their instrumentation laboratory, which produced radar, very famous radar, was going to take over the job of building the computer and programming it.

Now, way back at the beginning, somehow, I guess during the phase, Bob told me that the computer was going to have about 5,000 storage locations for instructions. I told him, "That's not enough."

He says, "Well, [MIT] says that that's plenty."

I said, "It's not enough, not beginning to be enough, no matter what they say."

But anyhow, they sent me up there to review the startup of the contract, and I don't know what all was in that contract. It was a pretty big contract, but the section for programming guidance computer was about that long [gestures].

I got back to Bob and I said, "Boy, they got a great contract."

"What do you mean?" he says.

"They don't have to do anything." [Laughs] I says, "They have no schedule, they have no requirements, they have nothing."

So he says, "All right. You write up the Apollo Guidance and Software Development and Verification Plan for MIT."

So I sat down and produced fourteen pages of what I thought would instruct [MIT] what to do, and [gave] it to Bob. This was just about Christmas vacation. He came back and says, "It's not enough."

I said, "What do you mean, it's not enough?"

He says, "Well, I'm going to get TRW [Thompson-Ramo-Wooldridge Inc.] to rewrite."

And I said, "Well, I'm going to go on Christmas vacation."

He says, "[Okay], you can go on Christmas vacation. They're going to come over and spend the day talking to you, and then they'll go back and rewrite the [document]."

They came over and, you know, they had read the document. They asked a lot of questions and things like that, and they went back. And when I came back from vacation, they handed me a document about that thick [gestures]. And I opened it up and looked at it. I've still got the copy. I opened it up and looked at it. It's double-spaced, not regular text. It's double-spaced. It's on matte paper. You know how thick matte paper is?

And what they did add is a lot of flow graphs and a lot of figures and schedules and a lot of other things in there that looked good. Actually, you learn pretty soon in this business <u>how a presentation appears is almost as important as what it says</u>.

But then they said, "Okay, now you're Project Manager," see, just like that. And so I went up to MIT to kick off the project. And Dr. [Richard H.] Battin, who I already knew his name because he had written some pretty interesting solutions to orbital mechanics problems, and he was sort of their lead person. He wasn't the Project Manager up there, but he was the lead person.

We sat down and talked for, you know, about half a day, I guess. I told him then, I said, "Well, I'll be back in a month. I want presentations of what you people are doing, I mean symposium-type presentations, except you don't have to make it slides and stuff."

And he just smiled and went back.

I came up the next time, and they started in the morning and they said, "We're going to go all day. We're going to have sandwiches sent in, and we'll be here all day." So they started through. They had these blackboards full of math and stuff like that. About two people got up and went through what they had done and everything, and the third guy was in there. He was doing some kind of optimization study of doing this, and he went through his whole presentation.

After he got done, I simply said, "Let's go back through that again." That stopped them all.

So he'd go, and I said, "Yes."

And there? Yes. There? Yes. And there? A big mistake, a real big mistake, I mean, a big mistake. And he said, "Oh, yeah, you know, that equation's not right, is it?"

I said, "No, it isn't." [Laughs] That changed the whole relation between me and MIT. As soon as they knew I could read their equations and understand them, I can do that better than text, the whole what I got when I went up there [was] what the real problems were and what solutions they were trying, and they gave me presentations on one—every time I went up, we spent the whole day, and we went out together in the evening to dinner, the whole staff, I mean everybody that was in that room that did the presentations. And we had dinner and discussions. Sometimes they'd bring up technical things. I ate in every good restaurant in Boston [Massachusetts]. [Laughs]

But they asked me where I got my—what do they call it? My sunshine pills or something like that. Because, I was relaxed. I said, "Well, you know, this is a very interesting project, but if we fail, it's not going to be that big a problem. If we get too uptight with it, we will fail."

But the one presentation on descent guidance, they were having a real problem. There's one statement the guy says, "These set equations are better than optimum."

I said, "Say that again." Optimum's the best you can do, you know.

He says, "Oh, I see what you mean." [Laughs]

So I went back home and I said, again, like the descent trajectory, what do I know about optimization that I had to do a long time ago? That popped up immediate. My mind is not easily to recall, but it will bring things up like that right out of nowhere. A linear program should work. It'll work. It will do the descent. The only question is how much fuel will it use over an optimum solution? We already have run the optimum trajectories. We know what those fuel requirements are.

So I sat down one day and sketched out a linear guidance set of equations with a technique of iterative solutions. You start down, you calculate the direction and thrust level you need. We had variable thrust engines, because you had to land. And then you calculated the direction and the thrust level you needed to make the landing site. And I plotted a plot showing what happened, and I gave it to [Floyd V.] Bennett, and I said, "Here, go program this and see how much it costs in propellant." And it was pretty close to [the] optimum. But if you got off the optimum, you don't lose much propellant, and that's what was happening because of the light gravity. It wouldn't work on the Earth, but it would work on the Moon.

Next meeting, I went up and I give them this, and I say, "Here, try this." And they looked at it and then they went and they started trying it.

The next meeting, they said, "Well, yes, we're going to go with that, but with a different sort of set. We're going to put a what we call a jerk on the end. We're going to redo the whole thing, going to end up with the parabola again, so that we come down and we put the pilot upright, so that they don't have to do a maneuver to get [vertical] for landing. That's going to be pretty difficult [for the pilot]."

And I said, "Yes, well, that's a good idea," and so we went with that. They programmed it. I mean, it can't be a linear equation; it had to be some kind of quadratic equation. And it's going to look like a parabola. I got a little [certificate of recognition] that says that I, with the MIT, produced the lunar guidance equations that put the man on the Moon. But that's the way that happened. I mean, I began to tell them, "Look. We don't need the best. We just need something that works." [Laughs]

ROSS-NAZZAL: It's an interesting perspective.

FUNK: Well, that's another little item which comes up all the time. You can use derivatives. The difference between two numbers, like a launch trajectory, the difference between a launch trajectory that is this here and the one up here, if you differentiate them, then you can calculate [the initial conditions to achieve the desired end point]. You can also calculate the performance requirements and how much you're losing for various improvements or changes. It's differential calculus. That's second year of college?

ROSS-NAZZAL: That's above my knowledge. [Laughs]

How long did you serve as the Project Manager for the Apollo guidance computer?

FUNK: Two years. Then I called Chilton in. I stayed [until] the first launch to verify it. It was an unmanned launch of a vehicle to verify it. Battin called me on the phone a couple days before the launch, said, "We've got an error."

I said, "What's the problem?"

He says, "Well, depending on the trajectory, we can get into gimbal lock on the gyros," which means you're going to lose the vehicle.

But I said, "Where?"

He says, "Well, about 90 degrees."

I said, "What's the probability that we're going to have a trajectory that far off course?"

He says, "Practically none."

I said, "Well, then we'll launch anyhow. Just don't tell anybody." [Laughs]

Well, I called Chilton into my office and said, "The program's written, and it's about time that you [get] somebody else to do this job. I don't want to sit around as a manager. I've done the kind of management that I like to do." They had offered me a job in Washington years back, and I turned it down. It no longer exists there anyhow.

So they decided to make Tom [Gibson project manager]. I said, "Besides, it's going to take the whole branch to [manage it]. The problem is that the input and the output to the program takes a lot more [computer code] than the program, and the astronauts are going to get involved from here on in," because one of them had already [came] to see me.

By that time, we even had 32,000 storage locations. We went up to 16,000 along the way, and then one day Chilton put his head into the door and said, "If we spend a million dollars, we can double it to thirty-two." He says, "They have a smaller little ring." This is a hardwired program. Once it was produced, it couldn't change. "And they can double the space."

I said, "Boy, that's the best million dollars you can ever spend." I said, "You'll save more than that on cost of programming." So they had 32,000 words of storage, and they still started trying, "What do we need? What don't we need?" We had onboard navigation in there. I think they decided to eliminate it. We did go with getting the positions from the ground and sending them up to the computer to save storage. But it finally grew to a whole division that was doing that.

And Tom, he got tired of sitting in meetings trying to figure out what you can—no technical stuff. I mean, Tom wasn't a [manager], he was an engineer.

So then, let's see. What did I go off doing at that time? Let's see where I am on this list. Skylab. Floyd Bennett was the name of the one with Skylab.

Oh, there's one other thing that happened with the trajectory program. Chilton came in one day and said, "You know, we need to produce a mission planning manual so that people can sit down and take the manual and take the outgoing trajectories and all this and put together a mission."

I said, "You're crazy. You don't want to do that." I said, "All you have to do—besides, we'd be like Bell Labs, and we'd be running just thousands of trajectories to get this manual, probably more than we'd need if we just used the mission planning [program]. When you want a mission design, you come in and give it to Tom or Ellis or anybody else that we showed how to run the program. You [input] the mission characteristics, and it will give you flight times out, flight times back [to] Earth orbit. Say you're going to leave from Earth orbit, you're going to get into, and where you're going to hit in the guidance corridor, and it will spit out the characteristics. And we have it hooked up to an integrator now, and it will spit the time histories out. And what that's going to take? We'd take it in the morning, and we'd have it out by noontime if they—well, even if they'll run it when they take it in, we'll have it out in five or ten minutes. You put these people down to that manual, and it'll take them probably weeks or months, and the cost will be fantastically different. And besides, if you don't like the one, we can go back the next afternoon and recompute it."

So he accepted that. "We won't do a manual."

We also lost [the] program. I meant to keep a copy of it, but it was on punch cards. And one day the division decided to clean up, reorganize and everything. Somebody saw all these punch cards in a cabinet and decided to throw them out.

ROSS-NAZZAL: Goodness. All that hard work down the tubes.

FUNK: [Since I retired from JSC], I did a lunar trajectory program [on contract] based on matched conics that's a little better, but it doesn't have everything in it. [It's not a duplicate of the mission planning program], but it's the fundamentals. It's a driver. It's like the engine. I can reproduce [the mission planning] now if I had to.

Now, we're back to—how did I get off on that tangent?

ROSS-NAZZAL: You wanted to talk about Skylab, but I'm wondering if this might be a good place for us to stop and schedule another interview, and you could talk about Skylab and your work with the Shuttle, if that would work for you.

FUNK: Oh, okay. Oh, yes, it's all right.

ROSS-NAZZAL: Okay, because it's about two hours, and this is usually about a good time for an oral history.

WRIGHT: If I can ask before we quit, though, could you just share your memories of Apollo 11 and what that was like for you to know that your work—

FUNK: Oh, we wrote a report about—oh, I didn't tell you about the free-return trajectories.

ROSS-NAZZAL: No, you did not.

FUNK: When we started out doing the mission analysis, Tom Gibson said, "Gosh, it's going to take us forever."

I said, "What?"

"Well, there's millions of way to get to the Moon and back."

And I thought, and I said, "Well, why don't we use free-return trajectories, and make that a requirement for the mission. The safety of the free-return trajectory, where we go out and we're in a position when we get out there to come right back to the Earth, that will cut it down a lot, won't it?"

"Oh," he says, "yeah, that will cut it down. But you know it's going to be awfully hard to fly those [trajectories]."

I said, "Well, yes, if you try to do it without guidance or without making midterm guidance to get back to Earth, you're not going to make it, of course, because it's too sensitive to errors in the trajectory."

I think I gave [Gus R. Bobb, Jr.] that job, to write a report ["Translunar LM Ops Abort Techniques for Advanced Lunar Missions," Internal Note 68-FM-189] on using the LM [Lunar Module] to do midcourse corrections in the case we lost the command module to the Moon. Now, the astronauts—we also in that report put in a lot of changes that they should make to the LM so that they could use the LM's power and the LM's life support system if the power went down in the command module in order to get back. And we put that in there about putting the cables that go from one to the other. The astronauts got pretty interested in that, but they never did make the changes, you know.

Did you see Apollo 11? … That made it pretty tough.

ROSS-NAZZAL: When did you write this report? Do you remember the year of the report?

FUNK: Well, it had to be—let's see. It was when we were here. It must have been in late about [1968]. It was something we just did because we [were] sitting there thinking about it, and I thought about it, you know, there was a possibility.

I was really thinking about what kind of aborts could you do. I mean, it looked like if you lost anything, it was pretty serious that you weren't going to get the astronauts back. And I said, "Well, we could back up the command module propulsion system with the LM," and had somebody write a report. That means that most of the work on the actual mission for me was done, and I was just sitting around thinking.

ROSS-NAZZAL: Rebecca had asked, before we close out today, what were your thoughts when Apollo 11 finally landed on the Moon?

FUNK: Oh, not particularly. I was off—I was usually at my desk doing something. A lot of it I threw away. It wasn't any good. But I was doing something. And I said, "Well, we made it."

But I got a note from the Division Chief saying, "Great, great, great doings. Off to Mars," which was some of the things I was doing then. I guess you don't want us to start that now.

ROSS-NAZZAL: I think it would be a good time for us to stop, and that way we can start at this good point. Unless you have more to say about the Apollo Program, this might be a good break for us.

FUNK: Well, I didn't do much on the Apollo Program right now. I did some studies about lunar landings, but there wasn't too good a study anyhow. [Laughs]

ROSS-NAZZAL: All right. Well, thank you so much for coming in today. We really enjoyed it.

FUNK: I threw most of my stuff away, sketches. Most of things was, "Why don't you look at this? Why don't you look at that?" Or I just decided to look at this, look at that. Way back at Langley, Donely was saying, "Hey, the V-2s are breaking up when they come down through the atmosphere."

[Wernher] von Braun was saying, "Best place to be when we shoot the V-2 is at ground zero."

So I asked him, "What altitude are they breaking up at?"

He said, "About 40,000 feet."

Well, I went back to the report on impact dispersions to the wind profiles, and I said, "Donely, you know, they're flying through the jet stream. That's where the wind profiles are the highest. They're breaking up in the jet streams. They don't have that when they shoot toward London [England]."

But that's mainly the way I got what I did, you know. Even the controversy about the gust loads data we were getting from—we had V-G [Velocity-acceleration] recorders in all airplanes. They still have them, but it's not called a V-G recorder. Every time we have an accident, they go looked for the recorder. Well, they started out as velocity versus loads on a little thing with smoked glass, actually, which had a little needle that went up like that [gestures] and did the profile. It wasn't even time history, just a profile. So it would get the kinds of loads these airplanes were going through, commercial airplanes, because we had inputs to the design specs [specifications] for the airplanes that the government requires.

As a matter of fact, that figured into the U-2 project, too. Tommy Coleman and I were in a room writing up reports and stuff, and he looked at one of the recorders. He said, "Look at the G they pulled on that airplane."

And I looked at it and I said, "Whew! That airplane's not designed for anything but level flight, practically." I said, "I wonder if we ought to call the Air Force or whoever we need to call and tell them they'd better take that plane out of service and inspect it?" Before we could get that done, it crashed.

ROSS-NAZZAL: That's unfortunate.

FUNK: It didn't crash overseas; it crashed here in the United States.

ROSS-NAZZAL: Oh, did it? Oh, goodness.

We want to thank you for sharing your stories with us today. They've been very entertaining. We've enjoyed them. I think we've learned a lot about the early days of the STG [Space Task Group] and NACA, and we'd like to hear more about your stories from Skylab, ASTP [Apollo Soyuz Test Project], and Shuttle next time we talk.

[End of interview]