ORAL HISTORY TRANSCRIPT

JOSEPH G. "GUY" THIBODAUX, JR.
INTERVIEWED BY KEVIN M. RUSNAK
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RUSNAK: Today is October 20th, 1999. This oral history with Guy Thibodaux is being conducted in the offices of the Signal Corporation in Houston, Texas, for the Johnson Space Center Oral History Project. The interviewer is Kevin Rusnak, assisted by Carol Butler and Sandra Johnson.

I would like to thank you for joining us today, Mr. Thibodaux. I'd like to start with while you were growing up, did you have any interest in chemistry or aviation engineering, anything like that, that might have led you into the career that you went into?

THIBODAUX: Well, I happened to get in by accident in a way. Yes, I had a great deal of interest in chemistry as a young person and always had chemistry sets stinking up the house like children usually do. I thought I wanted to be a chemist. But I went to the Career Day, actually, at high school there, and the head of the chemical engineering department at Tulane University [New Orleans, Louisiana] convinced me I ought to be a chemical engineer. He said it was a much better field than being a chemist. So I studied chemical engineering.

I had very little interest in model airplanes or aeronautics at the time, although I had very, very close friends who were very avid model builders. I used to watch them, but I have little patience to do that type of detail work, so I had no interest in building them myself.

I did become what they called a gopher. I went to the model airplane contest and you had to fly a number of different events, and I used to be the one who went for the models while the guy was getting ready to enter another contest. So that was basically more interesting than model building.

RUSNAK: That seemed to be a popular pastime for many of the people that we've talked to, having something to do with model airplane building.

THIBODAUX: Well, it turns out back in the thirties, all the way through the fifties, I guess, that there were an awful lot of people who worked for NASA who were model builders. [Robert R.] Bob Gilruth was a big model builder, in fact, Caldwell [C.] Johnson, Max [Maxime A.] Faget, Owen [E.] Morris, a lot of them.

RUSNAK: How is it that you ended up at Louisiana State [University, Baton Rouge, Louisiana]?

THIBODAUX: Well, I was born in Louisiana, actually, and I grew up in New Orleans, actually. It was a state university and my folks had a little money and were able to get me what they called NYA [National Youth Administration] scholarship, which paid for a part of my education, so I had to work while I went to college. It was the cheapest place you could go to get a college education at the time, and not too far away from New Orleans.

RUSNAK: I understand that you had a roommate who also went on to have a storied career in the space program.

THIBODAUX: Yes, it took me four and a half years to graduate because of my job. I had one required course in chemical engineering that I was unable to schedule. It was only taught at one time during the week, and that conflicted with my work schedule, so I had to go to summer school and then to go an extra semester.

See, originally all the other time I was in military, and I always roomed in the military barracks at the university. Max was not in advanced military, [Maxime A.] Faget. I don't know why, he and I got to talking and decided to room together during that one summer session, so we lived together during that summer session and then continued to live together until mid-term when I graduated that year, 1943, January of '43. I don't know, we were two similar students. We got along pretty good.

I used to visit his home. His father was in the [U.S.] Public Health Service. In fact, he found a cure for leprosy. He lived in a leper colony thirty-five miles south of Baton Rouge, and we'd go there sometimes on the weekends. It was nice because all of the Public Health doctors had a small golf course and swimming pool and tennis courts, so it was fairly nice.

RUSNAK: Sounds like an interesting experience for a college student, to be able to go to leper colony and have these types of activities there.

THIBODAUX: It was.

RUSNAK: Do you remember any particular stories or events from your college experience, with or without Max, that you think were typical of your time there?

THIBODAUX: Stories with or without Max. Well, I don't know of any particular stories. We were just kind of normal students in school, and I don't think either of us were honor students at all. Both of us were very good, I think, in our major courses, but the rest of the stuff we didn't pay much attention to. I never studied very hard, for some reason or other. I've said to a lot of folks, I was born lazy, and I always look for the easy way of doing things, so I had decent grades in school. Better than average, but certainly not what they expect people to have in order to get hired at NASA these days.

RUSNAK: That's true, but you seem to have done very well for yourself regardless.

THIBODAUX: Well, the insistence on hiring straight-A students sometimes misses an awful lot of good folks, like Bob Gilruth and Max and me. [Laughter]

RUSNAK: I wouldn't argue with that at all. So when you graduated you went into the Army?

THIBODAUX: Yes, I went in the Army. In fact, even before I graduated, I was two weeks short of graduation, the Army called me up, so I went and talked to all the professors and they all gave me my grades and wished me well and sent me off. I started in the Corps of Engineers. I had a commission in the Corps of Engineers, and I started out in Fort Leonardwood, Missouri, training troops up there at an engineer replacement training center, right in the middle of the winter, and up in the Ozark Mountains, which was kind of tough for a flatlander like me, who grew up in the swamps and over in the hot weather. But it was a very interesting experience.

I was there for about four or five months, and then shipped overseas to China-Burma-India theater, actually. I finally wound up in India-Burma border, in fact, and was assigned to work in an engineer general service regiment, which was building a road from India into China, actually to supply the Chinese against the Japanese. At the same time we were building a lot of advanced fighter strips to support the Air Force. So it was rather interesting.

I was a white officer in a segregated Negro organization at that time, and that was a kind of interesting part of my life. It was kind of fortunate for me that my father always treated everybody with dignity and respect and I never had any prejudice that I was taught. In fact, I learned an awful lot from those folks, things that I can apply to my daily life right now. I still have some very wonderful friends from those days, even after over fifty years.

RUSNAK: That's great. How was the general mood in the Army at the time towards segregated units?

THIBODAUX: I didn't notice that the white solders and black solders intermingled and got along fine with each other. I saw nothing of any indication of friction. There was an awful lot of turmoil going on in the United States at that particular time, though and, of course, some of the black solders would get these rebel-rousing newspapers to kind of get them a little bit angry at the system, but we had a wonderful chaplain who took care of all that. He's a very dear friend of mine. He's eighty-five years old and still an active chaplain.

RUSNAK: Were any of your experiences in the Army particularly relevant to your later career in the space business?

THIBODAUX: Well, a little bit. Actually, I did an awful lot of explosive demolition work in the Army and, of course, I ended up handling all the pyrotechnics and explosives for all of American manned spacecraft. So doing blasting and stuff like that in the Army was some of the experience that I got. Other than that, yes, I got—you might say I grew up, is what happened to me when I was in the service. I learned responsibility, learned how to deal with people. So it was a very, very valuable part of my experience.

I was a little concerned that it wasn't particularly technical, it was all very, very crude engineering, and being a chemical engineer, I didn't have an awful lot of experience in that type of construction work, but with the help of all the soldiers there, I learned pretty quick. It worked out pretty good for me, because I didn't get hurt, although I did come back and I think I had recurrent malaria for a number of years.

RUSNAK: Did you have any exposure to military solid rockets at that time, like with the bazookas or air-to-ground rockets?

THIBODAUX: Yes. In fact, I knew a good bit about bazookas. They were actually invented by a guy in the Corps of Engineering, actually. Then later on, towards the end of the war, we had airplanes that used spin-stabilized five-inch aircraft rockets, they were B-25s, which we used against Japanese shipping up in the Irawaddy River at that particular time. They were heavily armed airplanes. I got exposed to those on the air bases that we built. I used to walk around and look at a lot of things. I'm very inquisitive and I like to look at things that I'm not too familiar with.

RUSNAK: When you got out of the Army and returned to the States, what did you do then?

THIBODAUX: Well, I didn't have anything to do for a while. I was on leave. They gave me, I think, six weeks to see if I came down with malaria. Well, I didn't come down with it because I'd been on a malaria-suppressive drug. They gave you six weeks after you came off the drug to see if you got malaria. Well, I had the recurrent type, which recurred once a year, actually, for a while, so that I didn't come down with it.

In the interim, my father worked for an automobile company, and his boss hired me to do a little public relations work. They had no cars to sell, but they had to keep people off their backs, so part of my job was to take orders and try to tell them that they were going to get a car sometime soon. [Laughter]

RUSNAK: So how is it that you ended up hooking back up with Max Faget and ending up at Langley [Memorial Aeronautical Laboratory, Virginia]?

THIBODAUX: Well, Max and I had talked about if we both survived the war, you know, we'd get together maybe and go look for a job. Well, I had no idea I'd wind up in an aeronautical research organization. I thought I'd work for some chemical organization, you know, chemical plant, pharmaceutical plant, something that I knew I could apply the experience that I had. I never dreamed that I could apply all of my experiences to an aeronautical research organization. In fact, in the particular job that I had, it turns out I've been able to utilize everything chemical engineering taught me in my work. In fact, it was an extremely valuable type of education for the work that I did.

RUSNAK: Great. So the two of you ended up at Langley by—

THIBODAUX: Yes, Max called me and he said his dad had a little car that had airplane tires on it at that particular time, because you couldn't get tires for civilian automobiles. His dad told him he could have the car and we'd go look for a job. So we went back to the university and talked to various department heads about where we might look for a job.

Max had gone to talk to Fritz [Ernst E.] Maser, who was head of the aeronautical—it was an aeronautical engineering option. They didn't have a degree in aeronautical engineering at the time; it was a mechanical engineering with an aero option. Fritz headed that up, and I had come to know Fritz, too, pretty well. He was a very personable type of guy, easy to get to know. He gave us Paul [E.] Purser's name, who had been a student at LSU a few years, two or three years ahead of us. Paul had worked at Langley all during the war. He'd gotten a deferment basically for an important civilian job and he was inducted in the Air Force as a private, but his duty station was at Langley Field and he was never called to active duty. He was an inactive reserve, I think, of course, discharged at the end of the war.

We were given his name so we—Max was very familiar with the Norfolk area. In fact, he'd gone to high school in Norfolk because his father was in the Public Health Service and he had been transferred around from Marine hospital to Marine hospital. So Max had been there and he had a friend named Woody Blanchard there, who he used to build models with and go to Langley Field and fly the models. So that's basically how we got up there.

We went over the Langley and asked to talk to Purser and had an interview with him. It was kind of hard for Langley to get people, because colleges really weren't turning out an awful lot of engineers. They were interested in getting a couple of engineers for this new outfit that was going on called Pilotless Aircraft Research Division [PARD]. It wasn't quite a division yet, actually, but it was going to become one.

Paul was very enthusiastic about hiring us and he had something specific for us to do, it was very new and very fascinating and it looked like it was right up our alley. He wanted Max to work on ramjets and wanted me to work on rockets, actually. In fact, they hired me to work on liquid fuel rockets because of all the work [Wernher] von Braun and the Germans did were all liquid fuel rockets. They thought that's where the future was, but it didn't take me too long to figure out that wasn't anything we needed at all, in fact. I saw that propellant rockets were really the way to go for the type of stuff we had.

In fact, this kind of turned out to be the way to go for everything except launching off a big thing. All the military rockets have shifted over from liquid to solids for the same reasons that I understood why we wanted to use them, in fact. Instant readiness, reliability, ease of use. There were no big ground facilities there. They had a lot of good reasons why rockets, solid rockets have some particular advantages over liquid rockets.

RUSNAK: Well, certainly you're a pioneer in that respect, even ahead of your time. Do you recall what the first project was you worked on when you joined?

THIBODAUX: Well, I didn't have any projects to work on, actually. You see, I provided a service for everybody else. The main job we had to do was aeronautical research. The way we did that, we used free-flying rocket-propelled models. It was my job to get the propulsion systems and get them lined up and talk to engineers about how they wanted to use the thing, so that my job was really a service job.

Occasionally I did a little research work, actually, I think, for this research job, as they were concerned about the effects of the rocket exhaust on the base pressure models, and I did a program on that. Then another job we did, we did research on jet vanes, because the Germans had used jet vanes in their V-2 rockets, actually, so I was involved in that. I had a few research projects, but most of my work was a service to the engineers. We'd discuss what they wanted to do, actually, and what their goals and objectives were.

I found a lot of different ways that I could alter rockets, for example, to improve the type of data they got, or to help them get types of data that they weren't sure they could get. I also started changing from buying standard rockets, but modifying those rockets to give the right characteristics to help the research program.

RUSNAK: How so?

THIBODAUX: Well, you could change the thrust-time characteristics, for example, was one of the things you could do. You could get them where they'd create torques, certain torques of moments, actually, to apply to the models, and study the model's response to those things. You'd get the type of aeronautic data you wanted. We were always interested in increased speeds, you see, so it was my job to go find bigger and better rockets all the time, and then write specifications for them and contract for their design production, you see. At first we used all service military rockets.

I remember, I think my first year's budget for rockets must have been four or five, three or four years after I got there, it was I think, I believe I had $30,000 to spend for a first year's budget. I had two brand-new rockets developed for that $30,000.

RUSNAK: Which ones were they?

THIBODAUX: It was a T-44 and a T-45 made by Thiokol Chemical Corporation when they were first in business. See, there was two things happened to solid rocket world that put them on the map. The original solid rockets had no place to go. They had no future whatsoever. The manufacturing, where they were manufactured limited the size that you could build. They all burned both on the outside and the inside. Consequently, all the hot gases were on the outside. The heat would heat the cases up and you couldn't run them very long, because the cases would melt, you see, or they'd lose their strength and blow up.

The first thing that happened in solid rockets was the internal burning charge, where they burned strictly from the inside out, you see. Consequently, the only parts that get hot is the nozzle. As soon as the propellant burns through the wall, why, then, there's no more propellant left to burn, so it can't heat anything, you see. So that was the first step, actually. Then the next step was to make them where you could cast them like concrete.

There was some work at the Jet Propulsion Laboratory [JPL, Pasadena, California] probably early 1948, '47, '48, that instantly caught my eye. They were able to make castable rockets, where you could pour them. That meant you could make them any size you wanted to pretty much. I was so impressed with that, that I convinced Bob Gilruth that I ought to go to California and talk to those folks. I think I was the only one in NACA [National Advisory Committee for Aeronautics] that went to California that year, because very few people did an awful lot of traveling in those days.

I remember going out to JPL. It was still in a bunch of barracks buildings on top of a hill. The fellow who was running that was a fellow named Morton Summerfield, Dr. Morton Summerfield. "Morty" became one of the real giants in the solid rocket research. In fact, he wound up for many years at Princeton University [Princeton, New Jersey] after he left JPL. The guy who did the research work was a fellow named Chuck [E.] Bartley. Chuck Bartley and Larry [Lawrence] Thackwell and Jack [John] Shafer had written a report, actually, that I looked at. That and the internal burning charges is what opened up the whole future of solid rockets. Without those two developments, they were going nowhere.

RUSNAK: Around that same time, didn't they come up with the idea of adding aluminum to the propellant?

THIBODAUX: Yes, that's serendipity, actually. They put aluminum in the propellant fundamentally because they thought it would help on combustion stability problems. They really hadn't done their homework very well, and when they put aluminum in the propellant, it did something they didn't expect; it improved the performance of it, you see.

The way aluminum improves the performance of it, it acts like what you call a [unclear] that used to be used in electron tubes, actually. It wants oxygen more than other things want oxygen, so it kind of immobilizes the oxygen. The thing which gives rocket performance is the average molecular weight of the exhaust gases, so it increased the temperature, for one thing, and it reduced the average molecular weight of the exhaust gases. In the process of doing that, that's what drove the performance up, you see. The aluminum is a solid, it's a molten solid, it eventually condenses into a solid in very, very fine particles, and it's carried along with the gas, so that it provides part of the thrust, too, you see, as ejected mass.

That was another big thing. The propellants, the early propellants, some of the early propellants had pretty fair performance, in fact. One of the earlier propellants was a double-based propellant, about half nitroglycerin and half gun cotton. It had a pretty good specific impulse, pretty good performance. It took quite a while, in fact, for the composites, what we call cast composites, which are generally a liquid fuel, which polarizes along with an oxidizer. It took a while for those to catch up, in fact. Now you have double-base cast process, which will probably give you the highest of a specific impulse these days.

I had the opportunity to work with all the rocket companies. In fact, while I was growing up in the industry, a lot of my friends were growing up and had become presidents of a company or the chief engineer or the head of a research department, so I knew those folks real well. We could do things on the phone that you can't do any other way.

See, that's one of the things about the NACA, what made it such a great organization, that you never had to write anything down. All you had to do is talk to folks and it got done. It's probably the most unique organization the government ever created and probably the finest one ever created. It's not likely we'll ever see it again.

RUSNAK: I wouldn't be surprised if that were true. So you had a generally positive experience with industry then at the time?

THIBODAUX: Oh, yes. I was very well traveled. For some reason or other, they let me go wherever I wanted to go, actually. The big thing in the solid rocket industry at the time was that they had an annual conference, this Solid Propellant Information Agency, and it was an annual conference every year. That's where everyone got together and presented the results of some of their work, actually, and in the process of doing that, you got to attend a social occasion and other things and you got to talk to all the people and find out what they were really up to, you see.

The military was there, too. The military, the whole academic, military, and industrial complex usually attended these, and it was the big thing in the solid rocket industry. I don't know if they still have those things going on, but they were—I enjoyed going. They published big documents there, which I used, actually, to study a lot about the things going on in the business. Later on it became the Chemical Propulsion Information Agency and it included liquid rockets, actually.

It turns out we had a couple of liquid rockets up at Langley, in fact, and I eventually got to play with some of those, but not as rockets per se. They became chemical jets that produced high-temperature gas that I could test materials in. I had one little British rocket which was modeled after one of the German rockets, called a Typhoon, which was one of the simplest little liquid rockets I've seen. I bought some of those from England and played around with them, but it still never convinced me they were going that well.

RUSNAK: Well, certainly at the time you were sticking with solid rockets and launching them from Wallops Island [Virginia], right?

THIBODAUX: Yes, we pioneered a lot of stuff there. Multi-stage launching, for example, you know. When you couldn't get the real high-performance rockets where you got better performances, you just stacked them, stacked as many as you thought you could stack, you see, and then as each one of them burned out, the other one would light. It was brute force, but it wasn't particularly efficient at that time, but by brute force we were able to get some very, very high velocities with our models. Later on, you know, we got to where we could get specific rockets and kind of match them up a little bit better and get much higher velocities.

We pioneered all sorts of techniques, all the things you see, strap-on rockets there, where you put a bunch of them together on the outside and they fall off, like you see on the Delta launches. That was all pioneered by us. So many of the techniques you see used these days were actually pioneered by the group in the Pilotless Aircraft Research Division. It was an organization where no one worried about getting credit. You don't know where the ideas came from, it was just bull sessions, and the bull sessions would come up with ideas and then the ideas would just happen, you see, somewhat, the teams of people would get together.

Caldwell Johnson, I think you've interviewed Caldwell already. Well, Caldwell was one of the guys head of the Dynamic Model Engineering Section, and he'd come up with ideas plenty of times, too. We'd kick around what we wanted to do and he'd offer suggestions on how you ought to do it. In the process, there was no holds barred. No one told us we couldn't do anything, you see. We were encouraged to come up with ideas. If you couldn't come up with an idea, you had to work on somebody else's, basically. But if you came up with your own, you could get heard, actually.

I got involved in many other things, too, at the center, at Langley, for example, and the other centers, in fact. I first got involved with Lewis Research Center. They had ramjets which they were flying out of Wallops Island, actually, staging out of Wallops Island, because we had the instrumentation.

The guy who used the fly the airplane was a guy named Warren [J.] North. I don't know if you've interviewed Warren. Well, Warren was a pilot. He flew a twin Mustang with two P-51s that were two fuselages that were joined together. He and—you probably haven't interviewed Scott [H.] Simpkinson, because Scottie's dead. I don't know if you caught him before. But he and John [H.] Disher, one of the fellows up in headquarters, were flying these ramjets out of Wallops Island. Well, they'd drop them, but they couldn't get going fast enough, you know, because you have to get going supersonic for the inlets to start and to measure the performance of them.

They came down and asked me what they could do, so I put rockets inside the ramjet bodies. Then we designed the firing circuits and looked at all the safety when they were carried on the airplane. We actually would then fire the rockets before they were released, believe it or not—I'm sorry, fire them after they were released. Through a trailing wire you'd release it and then that would come back and fire that rocket. It was a very simple system. These were ten-inch-diameter JATO [jet-assisted takeoff] bottles, actually, that the Air Force used in jets in takeoffs. The ramjet was about twelve inches in diameter, so it just fit right up the back, and it had a little shearpin to keep it, and when the rocket fired, it would shear the pin, and then when it finished, or got up to speed, the air pressure just blew it back out, you see. We were always doing little simple—trying simple things like that.

One of the things I liked, I liked what they called passive systems, you see, systems that require nothing active in them in order to accomplish a task. Most people don't realize that passive systems sometimes are far more difficult to design than active systems, because you have to understand all the rules that govern how they behave, you see, and you can't tweak them. You can't throw a switch. You can't open or close a valve. You design the thing so whatever happens is preordained, you see, and it will happen in a normal sequence of events.

We used to do a lot of things like that. It makes for simplicity, fewer moving parts, less things to go wrong. That's pretty much how we operated to a large extent. I don't think we ever had a model that had any articulating surfaces, for example, or anything like that. We used other techniques to cause them to move around.

RUSNAK: Describe what it was like being on Wallops Island and around Langley at the time, just the general atmosphere and physical—

THIBODAUX: Well, when I started at Wallops Island, Wallops Island was a mosquito- and sand-fly-infested beach that had a bunch of wild ponies on it running all over there. The Chincoteague ponies were at Wallops Island, ran all the time. We used to have to shoo them off when we fired rockets. If we fired rockets, they'd almost stampede. We lived in Quonset huts, actually, and we had no flush toilets or anything. I think we had a one-lane strip of concrete. I'm not sure if we even had it then.

You see, Wallops is an island only in the sense that it's separated from the mainland by a marsh and some little inlets. It's about like Galveston Island would be, you see, in a sense. We'd get there by boat. We'd fly, usually flew up to Chincoteague Naval Air Station and we'd catch a bus and then go and get a boat to go over to Wallops Island. Or else we flew up in an amphibian airplane that could taxi up on shore and let us out.

Basically we had a very, very crude system there. We had what they call a split shed. It was a big shack that you could put the models in. When you got ready to launch them, you'd split it and move it aside. And one little launch pad and a little block house, actually. We stayed there overnight until we did our work. We had shops to work in. We worked till ten every night. I mean, it was about the time we quit. No one ever got any overtime for that, either, by the way. It was just you worked because you liked to do it.

I did a lot of things that had nothing to do with rockets, for example. The engineers had to do what they call a CG [center of gravity] weight and balance on the rocket models, loaded and empty with propellant in them and propellant out. I used to work with them. It had the term moments of inertia, for example, and I would help them with moments of inertia determinations. Everybody pitched in and helped, you know. It didn't matter whether it was your responsibility or not. Of course, in doing that, you learn other things, too, which is nice.

The food wasn't that great. We had two or three old Coast Guard guys, retired Coast Guard guys, who were cooks and watchmen and did every other kind of job up at Wallops Island. We used to get good clam fritters. There's a lot of clam diggers up there, and sometimes we got what they call blackened red fish. These guys were good at blackened red fish before Paul Prudhomme was ever heard of, in fact. We'd sometimes catch what we call them drum up there, red drum, but typically it's the same thing as we call red fish down here. So we ate pretty well. We ate in cafeteria with the trays, military trays. They'd hit the spoon and it'd splat on the tray, you know, typical military type of barracks.

Later on we got, as things improved and we got a little bit more money, we built some decent quarters for us to stay. We had a kind of lounge and kitchen. One room was a dispensary and then we had bunk rooms where there were a couple of bunks to a little cubby hole. So it was nice, actually.

We'd sit around and play cards afterwards and read *Amazing Science Fiction* magazines, *Amazing* and *Astounding Science Fiction* were the two things that people used to read a lot of back in those days. Even Bob Gilruth, he used to read those and come around wanting to know if anybody had seen the November issue or something like that. [Laughter]

RUSNAK: Did you get any good ideas from those magazines?

THIBODAUX: Not really. Not in a sense. They were kind of interesting in that the people who wrote those things were pretty good scientists, you see. Most people don't realize that Bob [Robert] Heinlein, Bob used to work up at, I think, [U.S. Navy Aviation Medical Acceleration Laboratory] Johnsville [Pennsylvania]. He was a chemist, as I recall. And [Isaac] Asimov. In fact, I don't know if you've interviewed Dick [Richard H.] Johnston, but Dick had worked with Bob. They started writing science fiction, and then later on they started making more money on the science fiction thing than they were working for the Navy, so they made that their full-time profession. I remember they asked me to take Bob out one time. He and his wife were down here for some sort of soiree we had. Kind of interesting talking with him.

They have groups of people who meet and they get to know each other pretty much like we did. I remember reading one story in one of the magazines, and I never saw the guy's name again, and I asked Bob about it and he says, oh, yes, he knew that guy, and said he committed suicide after he had written that story.

RUSNAK: I know that Heinlein was always a big supporter of the space program.

THIBODAUX: Well, yes. Like I say, he'd worked up at Johnsville, at the Naval Research Laboratory at Johnsville, as I recall.

RUSNAK: What were some of the other projects that you worked on there? For instance, the blast research project at Wallops, do you remember that?

THIBODAUX: Yes. Oh, yes, that was a kind of interesting one. They were getting ready to do the H-bomb tests at Kwajelein, and what they planned on doing was they planned on flying the F-80 drones, the F-80 jets, the drones, into the blast wave to do sampling and get data, actually. They called Langley and asked for some people to help. Well, the people who were in charge of that were the people involved—we had a gust loads, we had a gust tunnel, but fundamentally that's when you're flying into a wave, you fly into a gust. They were in charge of that.

Hal [Harold B.] Pierce and I forget Morrow's name, the two guys that I recall working with. They came to me and asked me what can we do about getting a blast, you see. So I said, "Sure, we'll get you a blast." They said it had to simulate a bomb, a H-bomb or A-bomb, you know. So we took a look at all the various parameters, and I said, "We'll get you a big explosive charge and we'll set it up at the right height and everything else, how big you want it."

I think we settled on 200-pound, maybe a couple of thousand-pound charges. Of course, I couldn't handle anything like that, but the Naval Mine Warfare Depot was up at Yorktown about fifteen miles away, and I knew they knew something about that. So I went and talked to them and talked to them about building these charges for us.

So we detonated one of the charges one day, and we didn't understand some things about sound refraction in the atmosphere. It turns out if you have an inverted temperature above the cloud layer and the sun is shining on the top, and it's hotter on the top than the bottom, that it bends the wave like water refracts light. We set this charge off, and I think we rattled Philadelphia that day, for some reason or other. We also knocked a bunch of antique plates off walls of the local population and a few other little things like that. It looked like that wasn't really what we wanted, too. You had to hire a preacher to go assuage the anger of all these folks around, and get you a lawyer and the whole bit.

We came up with the idea of doing it with basically a big shock tube, where we'd build a big ten-, twelve-foot tube, and pressurize it and then put a big diaphragm that we could rupture instantaneously and let the shock come out. That way we could use stationary models like in a wind tunnel or we could even fly them through it, actually. The first ones we actually rocket-launched, timed and rocket-launched them through the blast wave, you see, but later on we could do it with fixed models and measure the response.

My job in that was to design the pyrotechnic devices. They had to pop the big ruptured diaphragm so it would rupture instantaneously. And to be an advisor generally on that program, but it was mostly the gust load folks that did that. It was the first big bomb simulation test facility, actually.

Rather interestingly, I got involved in a huge one here after I retired, as a result of my experience with the Nuclear Defense Agency, since they can't do any above-ground testing, wanted to build a great big nuclear blast facility. It's a thing that's probably thirty meters wide and a couple hundred meters long and has great big old shock tubes up at one end, and then they wanted to do thermal simulation. Somehow or another Henry got them in touch with me because he knew of my background and I was the architect engineer and reviewer of that entire design, along with the help of some people here from JCS and White Sands, actually. That was one of those other duties as assigned, sort of, you know.

We did all sorts of strange things like that. I had got involved in a lot of wind tunnel testing. In fact, we actually fired little rocket models off of airplanes in wind tunnels to see the transient response to the flow field disturbance, things like that. For a long time we were doing research in wind tunnels on airplanes that had big turbo jets on them, but it was rather obvious to me that that wasn't a good simulation, because the jets weren't coming out, you see. So I got involved in a project with John Stack, who was one of the assistant directors at Langley, and we started building turbo jet simulators which you could put inside the wind tunnel model, actually, to give you good simulation of the turbo jet exhaust.

I worked with a couple of guys over at the sixteen-foot wind tunnel there. John Swiehart [phonetic] there, who later on became a big international vice president of Boeing, and Jack Ronkle [phonetic], who stayed at Langley and headed up the wind tunnel section there. So we got involved in things like that.

Then I got involved in building rockets about as big as the end of my finger that we put on little models in the spin tunnel, actually, so they could study some of this, really understand more about the spin characteristics of spin recovery. They were able to understand.

I did a lot of pyrotechnic things for everybody. There was one project where they had a bunch of old C-46s with varying amounts of flight time, and they were interested in structural damping in the wings, to see whether the cycle life had anything to do with how much structural damping we had. What you had to do is you wanted to get the wings to flap, and started the response in the wings, see. So what you do is you put a couple of ton weights on them, and you release these weights instantaneously and then the wings flap and then you started the transient response to the wings. So they asked me to build them some pyrotechnical release devices.

I got involved in more than just PARD. I was involved in dealing with a lot of organizations at Langley and also at Lewis [Flight Propulsion Laboratory, Cleveland, Ohio]. I got to know a lot of folks at the other centers, actually, as a result of working with them.

One other job I remember doing for Lewis, they had slurry fuel ramjets and they needed something to expel the slurries at a given rate. So I built basically a positive gas expulsion system which used propellant, actually rocket propellant then, to generate the pressure to expel the things, and we tested that back at Langley for them. We put solid rockets propulsion technology to work in a lot of places, not only in PARD.

You see, you had opportunities to do that there. I mean, the word got around, people had ideas, they'd want to come around and talk to you, or if you could help. There was no constraint on me working on any project that I wanted to work on back at Langley. In fact, helping anybody who asked for help or asking anybody for help that I needed to.

RUSNAK: Sounds like an ideal setup.

THIBODAUX: Well, it was the greatest place to work in the world. I mean, I think you can talk to anybody that worked there and they'll tell you the same thing. It was what you'd call a bottoms-up outfit. All the ideas started at the bottom and came up to the top, and the managers had enough sense to kind of steer you or guide up, but never interfere with your work. Just 180 degrees from the way things are right now.

RUSNAK: Do you recall who some of your managers were, that type of thing? Some of the key people at that time?

THIBODAUX: Yes, my first guy I worked for was a guy named Paul [R.] Hill, a real weirdo. Oh, boy. But wonderful person. Oh, he was the greatest guy to have as a boss. He was Max and I's first boss, in fact. He'd go see a movie he'd like, you know what he'd do? He'd buy two tickets at the movie, and when he'd come to work, he'd give Max and I tickets to the movie. If he had a book, a technical book he liked, he'd go buy each of us a copy and give it to us, you see. He had a sailboat and he'd take us sailing on his boat. He was very frank and open in discussions, you know. We used to have job reviews and had various interests, you know, so many things to be graded on and we'd sit up there and talk about it. It was really neat.

He had some strange habits. He traveled light. I'll never forget, I went on a trip to Huntsville [Alabama]. See, you could drive your own car when you went on a trip. They'd give you the airplane fare is what they'd give you and whatever, and you could drive your car. Well, I was taking leave and going back to Louisiana there, so I went down, and since I was driving my car, Paul rode with us. Well, we stayed in the same motel there. In fact, we had a kind of suite where they had two rooms.

Paul got on the car and all he had was a little kit about so big. That's all the clothes, that's everything he took with him. He had a bar of soap, a toothbrush, and a razor in the kit. He brushed his teeth with this soap, he washed with the soap, he washed his underwear out with the soap, he shaved with the soap, you see. So that's what you call traveling light.

He wore a tie, and he believed ties changed your luck. So as long as his luck was good, he wore the same tie, catsup on it, mustard, whatever, same tie. When the luck changed, he changed his tie, you see.

He was a great, great boss. I mean, very encouraging and very supportive in everything we did, for both of us.

Then Bob Gilruth, of course, was our first boss, and he's one of the most modest guys you ever saw, you know. The greatest asset he has, he recognizes what people can do and he gets them to do it. [Laughter] He knows who to pick and who not to pick. He knows when things aren't going right, but you have to know him in order to recognize he's showing that, you see. He never gets angry at anybody or anything like that.

He did have one way of letting other people know things weren't going—when a project didn't work out too well, his only comment was, "We didn't have enough talent on that job," was his way of saying that someone wasn't doing what they were supposed to do. Those of us who knew him, we'd see him fidget and we knew the guy who was making the presentation was in deep trouble, you know. He was always a gentleman all the time.

And he was super supportive. There was a time when I had some ideas back in the early to middle fifties on some fancy new rocket designs, and I wanted to get a rocket plant where I could manufacture the rockets there. Of course, Lewis Research Center was a propulsion center, actually, and we weren't supposed to be involved in doing that kind of work. So I think we were kind of told no, that we couldn't build one of them, because Lewis was the place where that was supposed to go on.

Well, Bob told me, says, "Well," he says, "my signature authority is $999.99," and he says, "if you can buy all the equipment you need where each piece is under $1,000," he says, "go build it." He says, "The building's okay, but," he says, "that comes out of a different budget and we can call that anything we want."

So I built me a rocket plant by Gilruth signing a bunch of purchase orders all for under $1,000, you see. That's the way I got my first rocket plant, and that way I was able to try out some new ideas for rocket designs and manufacturing techniques which I had some patents on. Bob was always like that. He always could find a way to get things done. We'd call that a lot of bootlegging. Well, it really wasn't bootlegging in the sense of the word, because headquarters is aware that all this stuff was going on. They never told you no, you know. They'd let it go on. They wouldn't stop it, because they understood that in a research organization you have to have a lot of freedom to explore things, explore ideas before you really want to do something with them. They kind of turned their head. They acted like they didn't see what's going on, but they really knew.

The next year after I did that, built that one there, they gave me $225,000 to build me a nice new plant. Of course, $225,000 was big money back then, nothing like 225 now. Now it won't buy anything.

RUSNAK: Well, if we could pause for a minute so we can change the tape.

THIBODAUX: Okay.

THIBODAUX: His title was engineer in charge when I first went down there. Later on they gave him a big fancy title called director. I don't know, that didn't change what he did.

Then he had a chief of research was the number-two in the outfit, a fellow named Floyd [L.] Thompson, who was another great guy, you see. Underneath Thompson there were three directors, Bob Gilruth, John Stack, and Hartley [A.] Soulé. Stack handled all the wind tunnel, supersonic wind tunnel stuff, and Soulé handled generally the low-speed stuff that dealt with airplane stability. They were all tremendous individuals, actually, all different personalities. Gilruth was very quiet, very modest. Stack, explosive. Soulé was acerbic. He prodded everybody and probed you all the time.

Turns out, most of those people came out of the flight research organization, believe it or not, there. It's kind of interesting to make observation that generally the people who were at the top of the organization are the people who deal with the complete product, you see, the whole airplane, not just the wing or the tail or the engine or anything else. That's the way it worked.

The number-two guy in the NAC, [John W.] "Gus" Crowley [Jr.], and he was the flight research. Gilruth was flight research, actually. Soulé came out of flight research.

Thompson came out of flight research, you see. Stack's the only one who didn't come out of flight research, actually. And all these guys were national and internationally known people, you see.

John and I got along real well. I don't know why he liked me. Langley had a funny way of you going up the ladder of success and you never knew what it was, necessarily, but we used to hold what they called research meetings once a month, where one of the divisions would give and present their results to the center, to the center staff basically. You kind of had arrived when you started getting invited to every resource meeting. One of the things I noted. It didn't matter—Langley had a classless society, by the way. If you had an awful big ego and let it hang out too much, it didn't go very far, you know. It didn't encourage that kind of behavior. The other thing about it if you did what you were supposed to do, it took care of you. I mean, it worked that way.

I worked at Langley for, what, seventeen years. No one ever got an award at Langley. No one ever received an award. And that's the best thing that could happen in an organization, by the way. All this stuff that they do these days on awards is the most ridiculous thing. In fact, I think it causes more grief than it does anything else. I've gotten I don't know how many awards and I wouldn't give you a dime for any of them. There's only two awards I ever got meant anything to me. But they're generally given to people because of the position you occupy, or that you've got to go round up something, or call to give an award, so you got to go [unclear], but Langley never did any of that. You got rewarded by getting to work on the best jobs. You got rewarded by getting promoted. You were rewarded by being given more responsibility.

They used to always ask Caldwell Johnson how come they see all the military people getting their work for military installation. My God, they had hundreds of them. They're getting all these awards in the newspaper. I asked Caldwell, "How come no one ever gets an award out there?" He said, "Hell, you've got to be superior just to work there." That was his kind of comment.

The guy at the shop, the lowest guard, the technician, everybody had equal status in the organization. They were all members of the team. They could all express their opinion and be heard. There was never any type of direction where you direct people to do anything. We'd walk in the shop, I knew all the shop people. Everybody knew everybody back there. It made for a very, very nice type of environment to work in. You never had to worry about stepping on anybody's toes. You could say anything you wanted to say. Nobody ever had hurt feelings.

We used to get in some big knock-down drag-outs all the time and some people would think that we were very insulting to each other, you know, but that was not the situation at all. I'll never forget one day, for being a chemical engineer and having no talent for drawing, when you talk to a guy about something you want them to design for you, you make little sketches. Well, Caldwell used to keep my sketches. One day I came in there to get him to design something, he pulls one of these, he says, "Thibodaux, what the hell is all this chicken scratch here? How in the hell do you expect anybody to know what you wanted?" [Laughter] You'd go in the shops and the guys would say, "Why do you want to make this thing? That's a dumb way you want to make it." And we'd negotiate right there. That's the way we did things and that's the way we got things done.

I don't recall ever writing—you don't write letters to people. The management was weird. The only person who could write a letter that went out of the organization was the guy in charge, the number-one man, you see. No one had any letterhead stationery. If you wanted to write a letter to someone, you wrote a letter requesting that a letter be sent to someone, and then they had an organization called a Research Staff Office, and the Research Staff Office would research it, you see. Then they'd come back and they'd give you a draft to see that if it didn't change the tone or expressed things the way you wanted. Then the director would sign it and send it off, you see.

If you wanted to work a guy overtime, there was only one man in the center could authorize you to go on travel or work overtime, that was the budget officer. He controlled the money. You'd say, "Hey, I want to work two guys two hours overtime tonight." You'd have to pick up the phone and you'd call Rufus House [phonetic]. He was the budget officer.

Well, he had this great system for tracking it. He had a roll of adding-machine tape about that wide that came down and was flattened out, and he had the top right desk drawer pulled out, and he'd write down there he authorized two hours of overtime. That would roll up in there and then each day he'd clip that off and he'd write the day on it, and that was his records. So if you wanted to check him, you wanted to make sure that he authorized that, he could tell you that, you see.

If you had an emergency at night, didn't matter what time of the day or night it was, one guy you call, he was there, [W.] Kemble Johnson. You'd pick up the phone at two o'clock in the morning, "Kemble, I got an emergency. I need some people from the shop out here." And that's the way things happened. Instant response. And big organization, too. It was 3,200 people, you know. It wasn't any small organization, by the way, but that's the way things happened, you see. You never had to document anything, you know; you just called people and did it. And that's the way Langley worked.

Another way you kind of realized you were getting ahead is you were asked to present papers before a national audience, you see. When you did that, you had to rehearse for everybody from the director of the NAC and his staff to all of your center staff and then through every other center staff. So you'd been rehearsed over and over and over again, and they had certain rules about how you did things, you see. Your charts had to be just right.

I'll never forget, one time I had a chart that was kind of busy and I was kind of getting—I was having difficulty dealing with it. I remember Floyd Thompson's comments to me, he says, "Son, it's permissible to lie to them, but never to confuse them." He says, "You'd better get that chart straightened out."

Some of the guys and their comments, you know, looked like they were attacking you personally. Like Soulé had a habit of doing that, Hartley Soulé. He was one of the other directors, like Gilruth. He'd begin to attack your integrity and everything else there and question you about things. I realized he didn't know a damn thing about propulsion, so I stomped on him a couple of times, you know, and he quit bothering me after that, you see.

I'll never forget many, many years later I was out in California, it must have been '57, I guess. We were eating dinner one night and I says, "Hartley, you know, I used to think you were the most vituperous old SOB I'd ever met in my life. You used to challenge me all the time, you used to insult my integrity and everything else." I said, "Now, that I've gotten to know you, you're the kindness, most fatherly old duck I've ever met."

"Hell, yeah, I was just trying to find out if you knew what the hell you were talking about," is what he said. He said, "Once I found out, I never bothered you anymore, did I?" And there are people like that.

Henry [J. E.] Reid there, after he stepped down as director, he became a kind of human being again. I guess as long as he felt he had the responsibility for running the center, he felt like he had to remain kind of aloof to a lot of things.

We had light gas guns that were developed by LX Charters Hopkins of Aberdeen Proving Grounds, actually. LX had come up with these shock compressed guns, and we were doing micrometeoroid research. And these things would shoot up to about 20,000 feet a second, you see. Henry Reid had been president of the National Rifle Association, you see, for quite a few times, I believe. And because I had these guns, he was interested in these guns, so he used to always come, and he'd sit down and eat lunch with me and we'd talk about all sorts of things that went on. Some of the things went on back there before were—I don't think he'd ever permitted during his time as being head of the organization. We had a lot of very interesting discussions.

Floyd Thompson used to come and we'd all sit down and chat. Floyd's main claim to fame was he designed the attack submarine, not the fact that he was involved in aeronautics at all. He tells a story about, that he was on a bunch of committees with some of the Navy admirals. He was chastising them about the way they designed submarines for a number of years. He says, "You designed this thing as a surface ship, but it spends most of the time under the water." He says, "Why'd you design it as something that spends most of its time under the water and it comes up to the surface every once in a while, sort of like let it fly through the water like an airplane." And when I first got down there back in '46, they were doing research on submarine hulls in the full-scale wind tunnel, actually, and the first attack submarine was design was done at Langley. That's the type of caliber of people we had down there.

There were a lot of others, I mean, you know, some of them—a lot of them had some very, very outgoing personalities, too. [John V.] Johnny Becker, compressibility, was one. Most of Stack's people were that way for some reason or other, I guess. People are like chameleons. They kind of take on the characteristics of their boss, I believe. I don't know.

RUSNAK: It sounds like there was a very high caliber of people there.

THIBODAUX: Yes, I think the people were all—well, they had something different. I don't know what it was. It's kind of hard to put your finger on it. Well, not everybody we had was that great, by the way. There was a time we had difficulty attracting good people. Probably in the early fifties the colleges weren't graduating an awful lot, and the American industry had began to convert back into a peacetime economy. The government couldn't compete salary-wise for a lot of folks. So we were getting some people there who I didn't consider really outstanding during that time frame, but they were about all we could get, actually, to fill the spaces.

RUSNAK: I wanted to ask you about another development while you were still at Langley, of the spherical solid rocket motor. Where did the concept come from?

THIBODAUX: Well, it's a fairly simple concept. A sphere actually has the least amount of surface for a given amount of volume, and what you're interested in, in any type of rocket, is you want the least amount of inert weight in order to contain the greatest amount of propellant, you see. Well, a sphere then would be the lightest weight for a given volume, you see, and for a given pressure, so that's fairly simple. That's pretty straightforward. The big question is, okay, now, how am I going to design something that fits inside that, that's going to meet the general characteristics that I want? Generally on a rocket, you like it to operate at a constant pressure, you see, because that way it's always operating at maximum efficiency at the highest pressure ratio, you see. So you like it to do that.

This other thing is you'd like it to hold 100 percent environment would be propellant. Well, of course, in a solid rocket you can't do that. You've got to have some initial area for the gases to flow out and you have to have the right type of surface initially in order where you start out where it's a sphere, but as it burns—if the propellant burns from the inside out, that the area doesn't change, this burning, you see. In a solid rocket there, you can design them to do almost anything you want by going through an exercise in geometry and trigonometry, you see. You're only limited by the imagination and ingenuity of the guy who does the charge designs.

I wanted to have the lightest weight container and operate it at a good high pressure, and I wanted to fill it with the maximum amount of volume is what the requirements were. So I had just gotten this young kid out of Georgia Tech [Georgia Institute of Technology, Atlanta, Georgia] there, [Robert L.] Bob Swain, and he was an Air Force ROTC [Reserve Officer Training Corps]. Somehow or other they told him that he didn't have to go in the Army, but he'd have to go to work for some government agency to serve some time before, and then he'd be through, discharge his obligation for that. They assigned him to be at Langley. I kind of talked to him and told him a few of the ideas that I had, gave him a lot of different options, various things to look at. I told him to go ahead and design me an internal burning charge, which is just a general description of all the things that work. And he went and designed one of them, you see.

I reviewed a lot of different designs and we picked one of them that obviously looked like it was going to be the best, but we had a problem. How you going to make the thing? Because in order to make it, it has this really intricate internal shape. You don't have a great big opening there at the bottom of it, you see, because you have to put a ring around it, adds to the weight. So you have to be able to take this great big old thing out of the inside and pull it out through a small hole. We came up with the idea that we were going to make it out of woods metal there, which melts at about 160 degrees Fahrenheit and we'd cast it. We'd put that in there and then we'd weld the case around while that was on. That's one of the things we did. That way when we got through we'd just melt it off. That was one of the novel ideas, techniques that we had.

Later on, I got the idea, well, why take the—this internal fairly intricate shaped is called a mandrel, actually. I said, well, why should we even take the damn thing out? Let's shoot the rocket, fire the rocket with it inside of it. We made the mandrels out of the same stuff that cuff's made out of there, you see. That weighs about three pounds per cubic foot. I made that mandrel with the central core where you could slip the pieces in. What you do then is you build the igniter around the outer rim of the mandrel, you see, and you pull that out. You shoot it with—you just leave the mandrel inside the rocket.

Well, I made a number of rockets, and back in those days which twin has a [unclear]. I'd fire one and fire the other one and asked you which one had it and which one didn't have it, and you couldn't tell the different in the records, actually.

Somehow that never caught on, though. It was an ideal way for me of making rockets. It had all sorts of tremendous advantages, but the closest thing came is Aerojet doing Polaris rockets. Actually use foam, some foam parts in the mandrel, actually.

Then later on we got into filament-wound cases actually. The ideal filament-wound thing is not a sphere. [Laughter] It turns out to be that you have to have an opening, for example, to put the nozzle on and let the gases flow out. Well, as soon as you put an opening there in a filament-wound case, the volume of the case and the size of the opening will uniquely design the shape of the case, actually, so that we had to come up with some rather unique designs to go into these things that looked like ovoid, spheroids, actually. It's like the Earth, an oblique spheroid; they're kind of flattened. It's not exactly that, it's the solution of a fourth-order differential equation is the shape of that case, but you can very closely approximate it to a spheroid. We did a lot of work on that.

In fact, the guy who did most of the analytical work for me on that is a fellow named Jerry [L.] Modisette, who was one of the early division chiefs in the science organization here. Jerry worked for me back then. We found easier ways of building that. What we did is we assembled a mandrel like a ship in pieces. You'd put it in and you could slip, they have little things that slip, the external things would slip in the grooves and you'd put it in there, and then when you'd melt it, then the core would be small enough to drop out of the hole. We did a lot of those, a lot of those designs.

So the spheres evolved into these ovoid spheroids. The whole thing has evolved, actually, over the years into things which don't look exactly like that. There's still a group of them used, spheres used. I wanted to build big ones and I couldn't build—the biggest thing I could build back at Langley would probably be around a couple of hundred pounds was as big as I could cast at Langley very well. If I wanted to build a big one, I had to go out to industry. So I let a contract with Thiokol Chemical Corporation up in Elton. They picked up on the idea and they started marking a large number of spheres for a lot of the Air Force projects and for a lot of the satellite and assertion programs. Then the Japanese copied some of the designs earlier.

RUSNAK: Well, moving on a little bit, when was the first time you heard of any serious inquiries into putting a man in space?

THIBODAUX: Well, that all started—I don't know. I have no idea when it really started. The official start of it was the week Sputnik flew. [Laughter] The Air Force had a project. You see, the Air Force, we had the research airplane projects there that we'd come up with ideas. Like Max says, toys we'd want to play with, and we'd get the Air Force to fund them and build them, you see. The Air Force was looking for something past the X-15 and they weren't quite sure what they wanted to do.

I think Bennie [Bernard A.] Schriever had the idea for a big space WPA program there, where he was going to have observation satellites, surveillance satellites constantly flying around the earth looking at things. It's rather interesting where that grew out of. That grew out of the V-2 work in Peenemunde, actually. It turns out that von Braun's boss was a guy named Walter [R.] Dornberger and he was a general, one of Hitler's generals. Walter had proposed to build, I forget whether it was A-4 or A-10, and that was supposed to be able to bomb New York from Berlin, actually. It was going to be a great big winged V-2 that would glide in.

The Army got hold of von Braun, and the Air Force wasn't a separate branch of the service, but it became a separate branch of the service, and here's all these Germans building all these things in competition with their bomb airplanes, you see. They didn't particularly like that, so I think they engaged in a little what I call one-upmanship and brought von Braun's boss over here. The boss was a Ph.D. engineer, a real great guy, in fact.

The Air Force sponsored two very top-secret studies at that time called RoBo [Rocket Bomber] and Brass Bell. One of them was more interested in the structure and the design of the spacecraft, and the other was interested in the aerodynamics than the other thing. The Air Force was proposing to do some of these things, but they weren't sure about the technical feasibility of it, so they asked the NACA to convene a group of people out at Ames [Research Center, Moffett Field, California] in October of 1957.

I think we were out there the week Sputnik flew. Gilruth wasn't there; he was off in Europe somewhere. Floyd Thompson was there, and me and Max, Paul Purser, Walt [Walter C.] Williams, who was running—Ed was at the time. Neil [A.] Armstrong, a young X-15 test pilot, was there. Al [Alfred J.] Eggers [Jr.], Harvey [H. Julian] Allen, the guys who came up at the ballistic, blunt body ballistic missile nose cone reentry thing, which—I don't know all the other folks that were there. Adolf Bussenbaumer [phonetic], German supersonic scientist that we got from Germany right after the war.

We all took a look at this "thing." It eventually became Dyna-Soar [Dynamic Soaring], is what the thing became, but we all took a look at it, and our conclusion was it doesn't violate any of the laws of nature as we know them, you know, but there's an awful lot of tough engineering problems we have to solve.

Max and Al Eggers started kicking around the idea of putting a man in space. There'd been some talk about it, in fact, quite a bit of talk about it before that time, in fact. They had arguments whether it ought to have wings or what shape it ought to look like. I guess I wanted to do what they called a Scout Program. I had ideas about—Vanguard was the only authorized satellite launching program we had, and it wasn't doing too well at the time. I wanted to do the Scout. I'd been to Denver in July of that year on this one of these SPIA conferences I was talking about.

Aerojet was building a 40-inch-diameter rocket, solid rocket, which is known as the Jupiter Junior. It was something for the Navy basically, and it would be launched either from a ship, an aircraft carrier, or else they'd put it down where it floated down in the water and launch it directly out of water. Then Thiokol had been building a rocket called the Hermes, which was a 31-inch-diameter rocket. Then Grand Central Rocket Company was building the Meteor, which was the top stage of the Vanguard.

It didn't take very long, I got home and I started doing a few calculations on this, and I had three stages and found out that just through—not serendipity or whatever you want to call it, but I found out that if you took a 40-inch and a 31-inch and you put that at first and second stage and you made a four-stage out of that one, that you could design a third stage where all of them ideally matched, where that you had a constant mass, constant velocity per stage and a constant mass for action per stage, which is almost a perfect match. And decided that, hey, that would be an ideal way of putting something in orbit, you see, and it'd be a real cheap way, because I already had three stages pretty much well developed. I only had to develop the other stage.

We kicked that around with Dr. [Hugh L.] Dryden and we were told, no, the Navy's the only one. [President Dwight D.] Eisenhower said the Navy's the only one going to put a satellite in orbit. Of course, they didn't; they let von Braun put it up. By the way, using two stages of solid rockets is the upper stage. Those little rockets that I had developed, T-55s, actually, they were done by Thiokol.

So we couldn't do Scout. Well, we went back and we started working on Scout, and Max wanted to talk to them about putting a man in orbit. He made his classical statement that kind of almost cost him being head of NASA. That's kind of like shooting a girl out of a cannon, you know, and that almost crucified him for the rest of his life when he said that. It came to haunt him any number of times. Max decided, well, we're going to put a man in space, and we went back and starting "bootlegging," I think, from that point on, you see.

The other thing is that after that time, Eisenhower or someone had decided they wanted a space agency in response to Sputnik, you see, and they had to choose some outfit that was going to kind of be the nucleus of it. Well, they happened to pick the NACA as the nucleus of it. I'm convinced that most of the reason for that is that the ones who are really space people were the guys at PARD who were going up, 100-some miles up into space and getting some very, very high velocities with simple launch vehicles and doing a lot of work like that.

Crowley decided that what he wanted to do was he got—someone got wind of the fact that we might be the nucleus of the space agency. I'm not sure what, but in March of 1958 they formed a little committee to study the NACA's role in space for budget purposes, you see.

That we met, I think, first meeting at Langley, but later on they made a permanent group and we met up in Washington. From about April through almost October, we spent four days a week up in Washington, four to five days a week up in Washington, trying to work out what the NACA would do in space, you see, what programs we'd do and budget for them, and we got people from Lewis, some people from Lewis. Ames was pretty far out; they didn't send [unclear]. They had a guy assigned, but he didn't come out and stay with us the whole time.

We worked up NACA's program. We also went through the Pentagon, DoD [Department of Defense], like we owned the place, you know. We could attend every top-secret meeting they had. We had a list of those and we'd go to them, or go to the Atomic Energy Commission [AEC] to see what was going on, what these people were thinking about of space. The idea maybe since they were not going to be part of it, that we'd bring this program into the new NASA organization, although we didn't know it was going to be NASA at the time. So we did that.

I need to stop. [Tape recorder turned off.]

RUSNAK: Okay. We were, I think, just talking about—you'd mentioned Scout and the idea of using that to put something in orbit.

THIBODAUX: Yes. Okay. Well, Max and Eggers were the ones who kicked them around, you know, putting a man in orbit. There'd been some things going on, actually, in the country. We had a delegate to it, a guy name Bill [William J.] O'Sullivan. They were all sworn to secrecy and Bill wouldn't say anything about it, but Al kind of got fed up with it and started kicking this thing around with Max and telling Max what they'd been looking at. Hey, if the Russians could put a man up, you know, if they're going to put a man up, we've got to try to beat them, since they put this thing in orbit.

We went back and Max started "bootlegging," you know, doing something without asking authorization for it, like we always did anyway. We were doing that back at home and we were bootlegging, taking a look at all the concepts for the Scout, formulating what the thing would look like, what type of control system we'd have, and about how much we thought we'd put it in orbit, and about how much it would cost, doing that type of planning. We started doing planning for what eventually became the Mercury Program.

But during this time frame, Max and I and Paul Purser and Bill O'Sullivan and Bob Gilruth were on this committee that, you see, one little division constituted at least a third of the committee, actually, for some reason or other. We were going through the Pentagon, finding out what went on in DoD that related to space, with the idea that if they weren't going to be allowed to do it, we'd pick those things up. Like I say, a lot of things, military space they were going to keep, actually, but at least the space program, per se, Eisenhower determined it was going to be strictly a civilian organization.

We came up with the idea of budget. Pretty soon we realized suddenly that we were going to be it, you know, that there wasn't going to be anybody else in space, that this was going to be *the* space agency. We kind of finalized what we wanted to do. I wanted to do Scout, we wanted to do Mercury, I wanted to do big solid rockets. They had some big liquid rockets there, one of them which became the F-1, was the E-1 Program. Del [Adelbert] Tischler from Washington was the guy who was looking after the liquids. I was looking out after the solids at that time.

Well, the thing I didn't say, this is coming on later anyway, we started doing all this work back at Langley. We'd come back at Langley and kick things around, you know. We'd leave instructions for the people back at Langley what we wanted to do and then we'd go up and spend all that time up in Washington. We worked a pretty tough schedule, actually. We'd get up kind of early in the morning and go eat breakfast and then we were always at the NACA headquarters building right around the corner from Lafayette Square. We'd go there every morning and we'd work all day.

We'd go to the Pentagon and spend time over at the Pentagon. I spent time up in Germantown at the AEC on occasion, talking, looking at nuclear rockets and stuff, things like that. We'd work kind of late and then we'd go out and eat. We'd all get together and we'd all go out and eat supper.

In fact, everybody—we lived together pretty much. We stayed at the same hotel, a kind of an old flea bag called the Francis Scott Key over there. It's part of Georgetown University. Now I think it's dormitories. It was a kind of low-key place. Nice hotel, by the way, I'm not running it down, but it had no bellhops. You carried your own baggage. Every room had a stove and a refrigerator. It had a unique air-conditioning system. It had a big shaft they put blocks of ice in and they blew over it, and if you opened your window, it let air blow out of your room and keep your room cool. It was cheap, because per diem was very low back then. We didn't have a lot of money to spend.

But we always went out. Restaurants, taxis were cheap in Washington and the restaurants were fairly inexpensive, good food, so we always used to go out and eat all the time together. Then we'd come back and go back to the hotel. Stop by a liquor store and get a bottle and we'd sit around and have a few drinks and sit up and we'd go over the day's activities with Bob and Abe Silverstein, actually, who was kind of a—made himself to kind of number-one guy of the space program. He was director of Lewis at the time. So we'd go over and kick around the ideas about what we thought we wanted to do and get some feedback. That's pretty much what went on.

At the same time, we were doing all this work back at Langley on Scout, what Scout would look like and what the Mercury would look like. Then Max was kind of going around to the military and various other installations trying to sell the program a little bit, I believe. By the time the Space Act was passed and we became NASA on the 1st of October, I guess, of '58, that we'd done most of the groundwork actually for what NASA started doing, actually. We started the Scout Program shortly after we became NASA. That was one of the big funded programs. We started a big solid rocket booster program. We started some liquid rocket programs. We started the Mercury Program, basically the man-in-space program. We were ready. By the time '58 came around, we were hot to trot, we were ready to go, you know, no problem.

Then shortly after that, they formed the Space Task Group because they kind of felt it was kind of too big or it didn't belong in a laboratories, per se, as part of a laboratory; it should be a separate operation. They formed the Space Task Group back in probably October, November, of '58. Three months later, I think we had the Mercury under contract.

RUSNAK: A very short time indeed.

THIBODAUX: Well, yes. You see, that shows you, you don't go out and do things like that if you haven't done a good job of planning. We had a design. We had everything pretty well laid out. And we'd been doing some other type of testing, actually, in anticipation of that.

RUSNAK: How did the rest of Langley feel about the creation of NASA, being part of that organization and an emphasis on space flight?

THIBODAUX: Well, Langley I don't think it bothered. It didn't seem to bother Langley that much, because NASA is National Aeronautics and Space; it's not only space. It wasn't going to diminish any of the work they were necessarily doing in aeronautics. In comparison, I mean, if you want to compare one or the other, one's a little bit more new and more exciting, but other than that, I don't think it changed anything, because the space program was really PARD in there and we always had—we were always the fair-haired boys and there was always some resentments for some reason or other, not anything serious or deep, but I'm sure there was slight resentments in other organizations that felt maybe we were getting treated better than they were. I don't think it changed anything, because that little difference, the difference between us and the rest of the organization, was always a little bit there.

Max is a fairly outspoken person and some of the guys over on the other side of house were fairly outspoken, so we used to have some knock-down drag-out arguments all the time. Some differences about approaches and things went on, but I didn't see any change, actually.

The space part, you know, I think all the space smarts, not all, but most of the space smarts went with Gilruth, actually. It don't take an awful lot of people to do the inspirational work or to kind of keep things moving, you know. It needs a few idea people and some people who will stick their neck out and sell things. The rest of the people, if it's worthwhile working on, they all want to work on it, you know. You don't have any troubles. Like even today out at NASA, a lot of kids want to come to work out there because it still seems exciting to some of them.

I didn't see a lot of difference in the way people reacted. There were a few people that wanted to get into the space part of it, by the way, from the other organizations. I think the flight people, Chris Kraft and some of the guys over there. In fact, some of the guys, actually some of the branch heads over in flight actually had asked us in PARD to come up and give them lectures in space and in propulsion and various other things to give their people training. Most of those guys wound up down here, by the way.

Langley folks were that way, too. I mean, they weren't envious. Like Thompson has no resentment of the fact that here was a big chunk of his organization going somewhere else, you know. That was a feather in his cap, as far as he was concerned, and he wanted to do everything he could to create or to enhance the thing, you see. The people are much bigger than they are these days about things like that.

RUSNAK: Although you worked with the Space Task Group, at first you weren't an official part of it.

THIBODAUX: No.

RUSNAK: How did that come about?

THIBODAUX: How did I become—

RUSNAK: How is it that as someone who had been a part of PARD and had quite a bit of time with those types of activities, that you weren't an original member?

THIBODAUX: Well, no one ever told me why, but I understood why, I think, that they needed someone to keep continuity of experience in the organization who was still there, you see. They couldn't give everybody up. I think Thompson must have cut a deal with Gilruth that I wasn't going there, that he wanted to keep me down there. So I wound up getting Paul

Purser's old job as head of the High Temperature Branch, which had a lot of—those were nice—those were things, too, which helped us get into the space program, too, a lot of work Purser had done, you see, in his branch. It turned out that we were trying to get data using these free-flying rocket-propelled models at very, very high speeds, but these things were getting bigger all the time and a little bit more complicated. Gilruth assigned Purser the job of saying, "Hey, can't we do this on-ground stuff, do materials research, heating and stuff like that, with reasonable test conditions on the ground?" So Paul had been pulled out for six, seven months to kind of look into this.

I was in Paul's branch at the time, I guess. They made a shift about in October, I believe, it must have been about the time we were out in California, as I recall, that they wanted to get into some high-temperature materials research, ground research and stuff like that, and high-temperature structures and various other things, because we were talking about flying things real fast and you were going to have to protect from the heat.

Purser was assigned that job, and when we decided we were going to do something about it, we came up with the idea of ceramic pebble heaters, actually, where we used little small pebbles that we heated to about a little bit over 5,000 degrees Fahrenheit, you see, and they wouldn't melt. We picked zirconium, which would not melt at that. We designed and built these pebble heaters, and then we came to the arc jets. We got into the arc jet thing. We got into space radiation. We had an electron beam radiator which would produce about what you get from an electron beam radiation in space. We had light gas guns that would shoot things up approaching meteoroid reentry. You see, all that in one little branch. [Laughter]

We were kind of pioneers in all that stuff. Those are all the things you need to worry about when you get into space, too. We'd already been doing it. See, we were kind of in the forefront of all those things. We got all those things going, actually. I had all my rocket work and I had all the high-temperature materials that worked, too, actually. I headed two sections in Purser's branch.

The first thing that we did to get high temperatures was to activate this old liquid rocket we had. I gave Bob Sweeney [phonetic] a job. He's one of the real great guys I had, very talented young engineer here. He eventually wound up as director of engineering and technical services at Langley there. I gave him the job of converting this old liquid rocket there. It was, I think, going in a Gorgon missile, I believe Gorgon or Locken [phonetic], one of the early Navy pilotless aircraft things. We converted it over from acid analine and I told him we wanted to make it an acid ammonia thing. I had some specific reasons why I wanted it to be ammonia, because there are a lot of things I liked about ammonia. It's different from hydrocarbon, but things are much stable in general and they were very, very easy to work with. You can make them hypergolic, where they ignited by themselves.

We converted this old Gorgon thing to acid ammonia and then we put little models in it, in the exhaust of it, and it was spectacular, watching them burn up. We had *Life* magazine come down there one time and they shot a bunch of pictures of it. I'll never forget this *Life* guy, *Life* photographer running around with his tam on, screaming, "Fantastic! Fantastic!" [Laughter] I even remember his name. Yale Joel [phonetic], his name was. He was a character.

So we were doing all that kind of work anyhow. Matter of fact, that was probably the most experience I had with liquid rockets till the time I came down here was dealing with, working with that. We had a lot of redesign we had to do, you know, and stuff like that, make sure it cooled properly with the new fuels. So I had to get involved in liquid rocket design because I wanted to use a liquid rocket, it was a high-temperature jet.

Anyhow, Paul got that started. We had a lot of other things. We had some facilities to make protective oxidation-resistance coatings there that we did, made the small models and protected them with some very, very sophisticated coating technology. In fact, we made borage, nitrites, carbides, and oxides and various types of materials here that we used for coating the little models for oxidation resistance.

We were doing a lot of work on ablative technology, too, by the way. We did an awful lot of that. In fact, pioneered in a lot of that work.

So I got Paul Purser's old branch, anyhow, that had all that stuff. My branch was as big as the division I had left out here. In fact, I had about 120 people in the branch, and we were doing all sorts of weird stuff. Then the center started setting up other people in competition with me, I mean, duplicating these things everywhere around. I was getting kind of fed up, not because I didn't like competition, but none of us had enough people to do anything with it, you see. Got a little group of people, well, why not combine all of them? I told them I'll give up all this stuff, just take it. [Laughter] And they couldn't understand that.

That's about the time I was starting to get fed up with Langley. They had no more space people. Most of them were aerodynamics people, you see. While they were pretty good in their own field, they knew nothing about any of the things that I was involved in. I always had a saying, "If you can't pick a fight with your boss, you'd better get a new one." I mean, if he's too dumb to understand what you're talking about, you don't want to work for him.

That's the other funny thing about Langley, too, you know. It didn't matter where you worked, you always had an opportunity to pick your own boss. You worked for who you wanted to work for, by the way. You worked with who you wanted to work with. Things work out that way, anyhow. I had that freedom anyhow, you see. I was always given that freedom. If I wanted to work for John Stack on a project, I worked for John. I worked for Soulé. I worked for Mel Rothwell [phonetic]. And no one ever said I couldn't do that, you see.

The same thing when I was up in Washington, even though Silverstein was in charge, I worked for Bob Gilruth. I didn't pay any attention to Silverstein at all. That's the way we worked. Gilruth was assigned to work for—at Goddard [Space Flight Center, Greenbelt, Maryland], Harry [J.] Goett was his boss. He never talked to Harry Goett. He wouldn't have anything to do with Harry. I don't know how that happened, but that's fact, you see. That's the way it worked. It's unusual. That's the unusual thing about the organization.

I forget where I was exactly, but we were talking about me coming down here, actually. Well, they had some changes. They put a guy in charge of the division I was in. Joe [Joseph A.] Shortal retired, actually, and Joe was a nice person, but he was an aerodynamics type. Even though he was chief of the division, he really didn't know any awful lot about the things we were doing that well. He was a very pleasant person. He was an Aggie, by the way, Texas A&M [College Station, Texas] graduate from way back, I guess. The nice thing about Joe is he never stopped you from doing anything. You did your thing. On occasions he was helpful.

It was the next echelon of people up above Joe that I had difficulties with. Well, you would talk to them about things you wanted to do and they had no comprehension at all about it, you know. It was blah, you know, like drawing a blank stare. I just got tired of that. I like bosses who are worthy adversaries, you know, who you can talk to, who can challenge you and who you can trade back and forth with, you see, and where both of you walk away and feel fine about it. That's the kind of people I like to work with. That's the kind of people I got used to working with.

When that changed, I decided I had to get out. I had gotten some offers from a small rocket—I guess medium-sized rocket company out in Arizona. They wanted me to be chief engineer out there and talked to me about stock options, all that other stuff. I was almost ready to go there when I got a call saying they'd reorganized the center here, just started it, and they were going to create a more functional organization there, where the project office would be supported by a line organization across the board. They created an engineering directorate and flight operations. They split the center up into about three or four different places.

They needed a propulsion power organization and then wanted someone to run it, so they called me and asked me if I'd do that. That's how I happened to come down here. It sounded real great to me to get back with the old guys again, people I knew, anyhow, and I could talk to. So that's how I happened to come down here, I guess.

I wasn't first choice, by the way. Headquarters had somebody they wanted out of headquarters to come down and run it. Somehow he didn't want to come down to Texas. So it worked out great for both of us.

RUSNAK: How very convenient for you that they were opening this job in your area of expertise.

THIBODAUX: Yes, yes, about the time I was getting ready to leave. The timing was right, because if I'd gone it, I probably wouldn't have come down here.

RUSNAK: Well, while we're on the topic of people moving, I think it was in '59 where a group of Canadians and some Britons came down from AVRO in Canada.

THIBODAUX: Yes.

RUSNAK: Did you have a chance to work with them, particularly in the early period?

THIBODAUX: Yes, I worked with some of them, yes. In fact, the big guy, the boss of the outfit, Jim [James A.] Chamberlin, I worked with quite a bit. I didn't really work with a lot of the others, no. I knew them socially and personally and I knew what they were doing.

Well, yes, I did get to work with a few of them, too. Yes, Bob [Robert E.] Vale was chief of structures division later on. When they came down here, I did get to work with a few of them. Most of them went up in flight operations, as I recall, and I didn't do an awful lot of work with flight operations. My guys worked with them, but I didn't. I sent a lot of folks over there to work with them, but I didn't specifically interface too much with the flight operations people.

Chamberlin was a real strange, very super strange person, actually. He was a brilliant guy, I thought, but he was the poorest communicator I've ever seen. No one could understand him, but I decided I was going to give him a chance, you know, and I was going to try and understand the guy. After I figured out how to understand him, we did really great. His mind didn't work the same way as most people's minds work. He shot-gunned things. He never did things in a sequence, in an orderly fashion. If you'd listen to everything Jim told you and you'd go back and you'd try to reorganize in your own mind the way you would logically think about it, it came out perfect, you see. But no one else was willing to make that effort to try to understand the guy. He was always in hot water because people couldn't understand what he was telling you.

I had a lot of people working with Jim, basically, when we did the early Shuttle work over in Building 32, actually. I had a bunch of guys assigned over there, and I used to call them and try to tell him how they have to go about understanding Jim, you know, to give them lessons in how you understand his thinking. Once they did that, it all worked out pretty good, actually. So I knew Jim and I worked with Jim after he went to work at McDonnell-Douglas down here, too. So Jim I knew better than the others.

And Vale. I knew Vale. While he was the division chief over there, I didn't have an awful lot to do with structures. I think I'm one of the few people who minded my own business and I only was concerned about what I was supposed to do. I never had any ideas about everybody else had to—technically what they should do or how to go about their business. There are an awful lot of people who want to design rockets around here, though. [Laughter] Everybody I ran across seemed like they were rocket designers.

RUSNAK: Well, let's talk some about the rocket design on Mercury. What were the areas that you were responsible for?

THIBODAUX: Well, they had the retrorockets and launch escape rockets and they had the little reaction control rockets, actually, there. I wasn't too heavily involved in that. I was heavy in the original conceptual study, choices we were going to make, you know, but I didn't get involved. I did handle the solid rocket part, but the reaction control thing was actually turned over to engineering. It was more of a mechanical design type of thing and fluid system design. The only thing we did is we chose who was going to use peroxide and what type of catalytics can you use and help with the selection of the contract. Other than that, I didn't get involved in the day-to-day activities of that, but I was involved in the day-to-day activities of launch escape and the retrorockets and all the pyrotechnic stuff on it, too.

See, most people don't understand, there are hundreds of little pyrotechnic devices that go off. The only ones that ever hear about it are the astronauts, when they go off they get a bang and they understand that all these things go off. You never hear anything, any discussions about that as a system, but it's a very large system. It's one of the most important systems on a spacecraft because it's the only thing that has to have 100 percent reliability in the fact that—in two ways. Number one, it can't function when it's not supposed to function, and it must function when it has to function, you see. Both ways. It has to be totally out of the way until it's called off. It can't inadvertently function. There are hundreds of these devices all over, and there's a vast number of different designs. In fact, from initiators to tension tie cutters, to explosives, to bolts this big to release the SRBs [solid rocket boosters].

RUSNAK: With the pyrotechnics on Mercury, did you have problems with electromagnetic interference setting them off?

THIBODAUX: Well, no, but we had one big problem within Little Joe, actually, that we had an inadvertent launch because someone had overlooked something in the design of the system. They were charging the batteries up, in fact, and when the battery voltage got high enough, it fired the system and it took off by itself. It wasn't the design of the pyrotechnic—it was the design of the pyrotechnic circuitry, actually, that caused that particular problem.

I did have one bad accident up at Wallops Island as the result of some weird pyrotechnic stuff there. See, I had to design all the pyrotechnic stuff back in the early days. There was no pyrotechnics industry. So whenever we needed something, we had to come up with the design of it ourselves. This particular case, a guy was working on a rocket, actually, was up on the launch and he was checking the telemeter route, and when he touched the pin to the rocket motor case, it went off and it cut his hand off here. Fortunately, it didn't do any more damage to him than that. But that was the only accident we've had that I know of. It was a very weird sneak circuit that we finally psyched out what it was.

Like I say, we did our part. In fact, I even went and gave a paper over at AGARD [Advisory Group for Aeronautical Research and Development] over in Holland on all the pyrotechnic stuff there one time many, many years ago, in the early sixties, I guess.

My chemical engineering experience, being a chemist and chemical engineer was very, very helpful in all that stuff, too, by the way. I did some other weird things at Langley, too, which involved getting really into explosive design, actually. I had an idea that we wanted to shoot things bigger than these little BBs about a sixteenth of an inch in diameter, fairly high velocity, but I wanted to get something big going. So I conceived the idea, well, let's get a gun to shoot a bullet through a hole, but then explosives actually have detonation, can go up to detonation velocities up to about thirty, thirty-five thousand feet a second, you see. If I can put this bullet through this barrel and then I could squeeze the barrel around, see, peristaltic action, sort of having to squeeze, that it would accelerate the bullet. And if I had an accelerating detonation wave in here, that then that would accelerate the bullet to a higher velocity.

Well, when I finally got one of them going 10,000 feet a second, I think, one day, but then the explosive charge would get big and they were rattling everybody's cage, for one thing. And then I couldn't do it every time. But I had a tremendous amount of research I had to do as to how you can build something which has an accelerating detonation wave that starts out at the velocity that it enters the barrel and comes all the way out to the end, then make sure that that thing, that that wave is traveling normal to the axis so that it squeezes uniformly. Well, the barrels used to come out and they were nothing but a solid rod afterwards, you see.

I kind of got out of that business, because I didn't have anyplace to work anymore. I was going to try to get it up, go up past 10,000 feet a second, which would have meant that I'd have been dealing with, you know, quite a few pounds of super high explosives. That people didn't like to hear, the glass rattling around there.

So I developed a pretty good insight into how explosives work and how I could build things where I could get actually an equilibrium detonation velocity which is as low as—it's very difficult to get it as low as 5,000 feet a second, actually. We worked out all the techniques for that, and that was one of the places that I learned an awful lot about explosives, about high explosives there, about the theory of high explosives and things. That's the pyrotechnics story.

RUSNAK: With the Mercury solid rockets, you mentioned the launch escape system. Where did the concept for that using a tractor rocket to pull the capsule off?

THIBODAUX: Woody [Willard S.] Blanchard [Jr.] is the one who came up with that. Actually, the kid who Max used to fly model airplanes with over at—he came to work for us. Woody had been a B-54 pilot in Italy, I guess, during the war and he finished school after me and went to VPI [Virginia Polytechnic Institute & State University], I guess, and finished school after the war. He came to work for us as soon as he got out of school. He had the idea for a tow rocket, you see, put the horse before the cart, you see, because one of the big problems you have trying to boost things which have wings is that you have to have very, very large fins on the booster, you see, because that's like canards on the airplane, you have to have some oversetting moments in the back so it doesn't flip.

We were getting some kind of models that looked like they were going to have large lifting surfaces in one plane, you see, which is real bad. Symmetrical things are much easier to handle than non-symmetrical things, just like the Shuttle. Although we actually had flown similar configurations to the Shuttle back in the fifties, early fifties, probably, that particular piggyback configuration is not foreign to us, but it was very difficult, much more difficult to do than just flying with the fin. But we almost had to do it because the fins were getting so big and heavy, it was cutting down on performance.

So Woody got the idea of towing it. So what he did is, I got a couple of rockets and we tied them together and I cannoned the nozzles out and I built him a little pyrotechnic of a cable to pull it and then the cable had a little pyrotechnic device that would separate it, you see. So the tow booster was Woody's idea and that's where the idea came from. We flew some of those at Wallops Island. By the way, they worked. It's one of those ideas, fad didn't catch on real well, but it did come up when the Mercury Program came up and we wanted to get the Mercury capsule off. How are you going to get it off? So the tow booster that Woody conceived of is where the idea came, where Max got the idea for the launch escape system.

RUSNAK: Fortunately we never had to use it during manned flight, but tests of it—

THIBODAUX: Well, we did have an inadvertent firing where it worked great.

RUSNAK: That's true.

THIBODAUX: Out at White Sands, as I recall, I believe, on one of the Big Joes, I think. We had a Big Joe came apart, I believe, and the launch escape system worked like it was supposed to.

You know, that's another system, too, by the way, that has to work all the time. It can't work when you don't want it to work. It has to work all the time. If you don't get it off, you lost the whole thing, the crew and everybody. So pyrotechnic device is something that you have to be real careful with.

RUSNAK: Well, it's interesting because on Gemini they didn't have the launch escape rocket.

THIBODAUX: That's because Chamberlin didn't want one and he got his way.

RUSNAK: Could you tell us about that story?

THIBODAUX: Well, I don't know the whole story. All I know is Jim wasn't buying into a launch escape system, though, and I think he—I'm not sure whether they—I think they had ejection seats on Gemini. Yes. He's an old aircraft type, you see, and all the things he designed had ejection seats. I'd rather take my chances with the launch escape rocket, but Chamberlin's the one who insisted it be that way. And he was program manager, I guess, at the time, so he got his way on that one. Fortunately, never had to use any of them. I just don't think I'd like to have to bail out of that thing with one of those. Jim's the one that—that's why Gemini had ejection seats.

RUSNAK: Did you have a lot of involvement with Gemini in general?

THIBODAUX: No, not that much, actually. You see, Gemini was a transitional thing and I was still back at Langley pretty much when Gemini was conceived. I did have an awful lot to do with the reaction control systems on Gemini, actually, because they were both being done on two separate—Apollo reaction control system on the command modules very similar to the Gemini and they were diverging. When I first came down here, Gilruth said, "Get those things back together again."

Then I had a lot of trouble. Walter [F.] Burke wanted to start the McDonnell Propulsion Company up there, because he didn't think Rocketdyne was doing a good enough job, you see. Walter was one of the real great guys in this business. He was just as honest as the day is long. Wonderful person. He'd also been in charge of manufacturing, vice president of manufacturing for McDonnell-Douglas before he got to work on the Mercury Program. So he had the idea he could build these things better than Rocketdyne, actually. I don't doubt he could have, but I couldn't see getting another new outfit in the business. I thought we could get Rocketdyne to straighten up and fly right.

So I did get involved in that to some extent, the cryogenic storage and supply system, but those things were pretty well—they were done in an adequate way. The fuel cell was in terrible shape, and we did get involved in a little bit of that activity.

Gemini was really run, it was a totally different program, it was run totally different from any other operation we ever had around here. It was not a team effort. They wanted to be across the street. You could take the Gemini Program and move it across the street, as though the center didn't exist, is the way they wanted to run it.

Later on, I guess, it changed a little bit when [Charles M.] Mathews started running it, I guess. I saw some changes in the operation from the way Chamberlin was involved with it when Mathews took over.

RUSNAK: Any thoughts on the paraglider or parasail for Gemini?

THIBODAUX: No. I know a good bit about parasails, actually. I say "a good bit." About the history of them. They were invented by Francis [M.] Rogallo at Langley. Frank and his wife used to manufacture these kites in their attic, and they used to roll them up in cardboard baling tubes and they used to take them over to Virginia Beach during the summer and sell those things as kites to kids, is what they used to do. That's how the Rogallo thing got started. He was head of the seven-by-ten-foot wind tunnel, you see. I guess he got involved. They decided they wanted something that wasn't going to come straight down, but that they'd be able to kind of maneuver a little bit.

I don't know. We did some tests on it at White Sands. I got involved in retrorockets and things like that on some of the things that set down when they were running some tests out at White Sands. I don't have any opinions about it. I mean, it was a good kite for kids to fly. First-class kite, in fact.

RUSNAK: Well, obviously it never made it to the flight stage on Gemini.

THIBODAUX: No, it didn't. There are a lot of things, you know, they're not bad ideas, but they're just not good enough to last long. I think of all the things that—so many of the things that people started that sounded good at the beginning, some reason or another didn't fly. Some things we've been working on for fifty years and we still haven't gotten there yet.

RUSNAK: That's true.

THIBODAUX: Sort of like Dyna-Soar, you see. Dyna-Soar eventually got—the Shuttle's a Dyna-Soar. Max don't like me to say that, but the Shuttle is nothing but a big Dyna-Soar, exactly. It's launched into orbit and it glides back in and it lands. That's what Dyna-Soar is supposed to do, except this is bigger.

The thing that happened, while we were doing Mercury, Gemini, Apollo, and all these other programs, we developed all the technology that Dyna-Soar needed, that we knew Dyna-Soar was going to need, you see, in order to be able to fly.

See, you get all these wonderful ideas. I see a billion of them, not a billion, but millions of them, hundreds of them, coming around all the time. People have these wonderful ideas about how they're going to do things. Well, most of them are scientist types, they're far-out thinkers, but in order for things to happen, there's a lot of "collateral technology." There are a lot of things down beneath the surface that have to constantly come up and they all have to be ready when you're ready to do this thing.

You see, I see this astronaut over here talking about these super space propulsion systems, you see. Well, he's working on the wrong end of the machine, you see. We know those things ought to be able to work, but where's the energy coming from? See? You can't do anything at all. You can work on those things until you're blue in the face, but until you have the energy system, which can provide the energy to make the thing work, you can forget about it. You're just whistling *Dixie*, you see. It's very premature. You ought to be putting your money on the other part. We do that all the time, you see, in response to ideas people have.

You can't force technology, see. The big mistake, the reason things cost so much is people—there was a mind-set in this country started by, I guess, Benny Schriever some of the people back in the early Air Force days is that you take on an impossible task, you see, that you know ain't going to work the first time, but what you're going to do is you get it going and then you find out that you're missing all this technology. So what you're going to have to do is develop the technology to make that work. So in order to do that, you got to get more money, you see. You got to authorize more and more, so you kind of use that kind of as bait. Then you bring the technology along that allows you to do the thing you said you could do already that you know you can't do. There's too much of that goes on.

It's one of the reasons why at least my part of the Shuttle was so successful, there was nothing new in it. It's all the same stuff we had in Apollo. A few new things, but they are things, by the way, that I had had money to do research on, not with the pressure of a program saying it has to happen now, but that I could do research on, so that by the time the program says we need it, I had it ready, you see. That's the cheap way of doing it, you see. I don't have to worry about all the paperwork and I don't have to worry about all the inspections, I don't have to worry about all that stuff that all costs a lot of money. I do it off-line and then what you do when the technology is ready, then it lets you do the things you want to do. That's one of the big mistakes we make all the time, I think, is not doing things that way.

The best example I know is the fuel cell on the Shuttle compared to the Apollo fuel cell and the OMS [orbital maneuvering system] engine there. We talked about reusable rocket engines. Well, OMS engines have been flying forever, you see, and it's totally reusable. The fuel cells are at least ten times as good for the same size as the Apollo fuel cells were, ten times as good in many ways, you see. The first thing is they can produce ten times the amount of power. The lifetime is about ten times as long as the Apollo was. The other thing is, from an operational standpoint, if you want to use them, you turn the switch on, and if you don't want to use them, you turn the switch off, you see.

Well, Apollo it took twelve hours to start and twenty-four hours to shut down, you see. Then for every three of them you built, you only flew two, because you always screwed one of them up in the process of either starting it up or shutting it down.

I had a very interesting conversation with Pratt & Whitney, who developed the fuel cell, a guy named Stu Conley [phonetic] over at my house one night telling him what he was going to have to do and what was important about fuel cell design, because, to me, the Apollo, while it worked and it did its job there, it was certainly very, very deficient in many of the things I thought they ought to have. One of them was ease of operation. Turn it on, turn it off. You don't get instant results. You turn it on, it takes a little while for it come up, but it's a very short period of time, but you don't have to worry about it, about it failing when you try to start up. When you turn the switch, you don't have to worry about it failing when you shut it down.

Then the OMS technology we developed, we paid for, was what they call platelet technology and that's where you make the injectors by a series of little thin plates that you do photo etching all the little things and then you diffusion bond the things together, actually.

See, I might not have known an awful lot about liquid rockets when I came down here, but it didn't take me long to figure out what was wrong with them. The biggest problem with the Apollo liquid rocket design was the fact that the rocket injector is like a carburetor on an internal combustion engine. It's got to mix the fuel with the oxidizer and it's got to do it in a very efficient way. In a rocket engine, the injectors are something which you bring both the fuel and the oxidizers down from the tanks and they go over into some circular passage, and then it finally winds up in little holes that are usually pointed at each other or something like that, you see. It should be a precision piece of hydraulic machinery, that it should do exactly what the design intends it to do.

What they kind of overlooked is that fuel comes down, you see, and it changes direction, it accelerates the flow and the pressures change so that the difference in pressure distribution so every hole in there is going to see something different in the flow velocity. If the holes are this way, the velocity going in the direction, or this way it's the opposite direction of it. So none of these—hardly any of these things behaved the same way, you see.

Then to worsen that, they drill the holes—they weld the injectors up, and in the process of welding you get weld splatter all over and sometimes you drill a hole that's got a little weld bead right next to it. Then you drill the holes with drills and the drills have burrs, you know, as they come through they get a little lip on it or something like that. So it's not a precision piece of hydraulic machinery at all. It's kind of catch as catch can that each one of these things behaves a little different.

So we used to go through injector after injector design. Fundamentally I think what we were doing is we were finding the particular injector pattern design that was more resistant to all the flaws in the flow. Well, we changed all of that, actually, in one program. It's when we had the LM [lunar module] program, we had a problem with the LM ascent engine, and we decided we were going to go get a backup, and we started manufacturing in a totally different fashion. We used electron beam welding to weld the things, get rid of the weld splatter. The other thing we used is the process of electro machining, actually, where you use a high-frequency electric arc to nibble away at the materials. They're an electrical discharge, discharge machine. You get very consistent holes with no burrs on the end or anything else, so that you do have a—and then you take a good look at the other aspects of the injector design. Consequently, you get something that behaves the way the designer designed it to behave. Then you'd have a baseline to start doing your studies with.

Those are some of the improvements that we made in the way you manufacture things to get them where they are a little bit more precise, actually. That was a big difference, actually, in the whole philosophy of how you go about things, designing and manufacturing things.

RUSNAK: Well, if we could, we'd like to pause so we can change the tape.

THIBODAUX: Okay.

RUSNAK: We were just talking about the manufacturing on the injectors, and you had mentioned that there were some problems with the ascent engine on the LM. Could you tell me about that a little bit?

THIBODAUX: Well, of all the rocket engines we had there, the one, I think, we made—I don't know if it was an arbitrary decision or not, but the one that was going to have to be the most reliable was the LM ascent injector, you see. I don't know whether you call it psychological or political, but if we got all the guys down on the moon, the worst situation, scenario we could come up with is that you can't get them back, or you wouldn't get them back, you see.

One of the things that plagues a lot of rocket engines is combustion instability, you see. It's the ability, if you get a little disturbance for that thing to damp, it will return back to its original conditions in the engine. A lot of them, if you disturb it, then they go into cyclic combustion, and that generally results in a lot of problems. Sometimes it results in more throat erosion, sometimes it eats the walls of the chamber up, sometimes you get excessive heat in areas where it melts parts and things like that. So it's the additional high heat transfer rates that go along with that.

Bell was having some problems. We had a very, very strict criteria that you had to meet. What you had to do is you had to put basically a little explosive charge, which would give it this certain type of disturbance of a certain intensity and shock, basically. Then you started this response to that disturbance. Well, if it doesn't damp, or it doesn't—very quickly there, that's some indication that you could have some problems with it. Bell's injector there was still having problems pretty late in the different types of instabilities.

There are two types of instability. One of them is you get pressure variations in the manifold that actually feeds back and that's low-frequency instabilities, and then you have high-frequency instabilities, which are characteristic of the dimensions of the chamber and the properties of the gas in there. It's sort of like an organ pipe, basically, or like an oboe or something, where you change frequencies. On oboe, you finger. You don't change the frequency in rockets. Although we used to do that in solid rockets, by the way, is that's the way we handled it, we'd drill radial holes in it. We had a lot of other techniques to handle the solid rockets that you really don't have available in liquid rockets, actually.

We couldn't get Bell's attention. We couldn't get Grumman or Bell's attention on this, and it was getting late in the game. Since we couldn't get their attention, we didn't know what we were going to do about it, you see. They wanted to dismiss the thing, and we decided, well, we really need to get something we have absolute confidence in, so we decided we'd go out and get a backup.

George [M. Low], Bob Gilruth and I, and Joe [Joseph G.] Gavin had been up to Bell, I guess. We flew a Gulfstream up to Grumman and had a meeting up there and then we went up to Bell. I didn't think we were getting anywhere as a result of all these conversations. George asked me what should we do about it. I said, "Well, let's go get a backup." He wanted to know how long it was going to take and how much money it was going to cost, and I gave him my best estimate right on the airplane. I said, "We can do it in about a year and it can cost us about a million dollars a month," because I'd taken a look.

The way you make estimates is based on your experience. You know about how much it costs for a man year, for you to hire a company with their overhead, you see, and you've got a pretty good idea how many people you're going to have to put to work on it and stuff like that. I knew about how many man years that would buy me, for example, at that particular time. So I told him a year and 12 million dollars. He says, "Well, when can you have someone on contract?" I said, "Well, we've got to go through the whole source board, you know." I said, "Give me thirty days to run a whole source evaluation."

I wanted to have bidders' briefings, and, number two, write a set of specifications, send them out to the bidders, come have bidders' briefing. Get proposals in. You evaluate the proposals and then go up to headquarters and be ready to get the administrator's decision, since it was enough money that he was going to have to decide.

That had never been done before and it's never going to be done again, because we had Cecil [R.] Gibson wrote me a set of specs. It took him about two days to write those set of specs. By the end of the week, I had a bidders' briefing down here for the contractors. Picked up the phone, called one of my buddies in the rocket business and said, "Hey, this is what our intention is, this is what we intend to do. Are you interested in bidding on it?" He said, "Yeah." I told him, "We're going to have a bidders' briefing, if you want to come down to the bidders' briefing."

We gave them a week to prepare the proposals and told them, "You're going to limit your proposal to seventy-five pages. That's all we're going to allow you and we're going to take a week to evaluate it, you see."

So we did that whole job. George got me on [James E.] Webb's calendar, says, "If you're going to do it, I'll get you on his calendar." I went through that whole process from cradle to grave, getting a decision from the administrator, in probably about thirty-two, thirty-three days, actually. You don't do things—there's no one ever did that and no one will ever do that again.

Number one, I had the entire support of the entire system there. Number two, I didn't have to report to anybody. You see, source boards, they used to have a little blue manual to show you how to manage a source board, and they gave the source board chairman, he was almost God. You had all sorts of freedom to do—the sole purpose of the source board or source evaluation is to get the best deal for the United States Government, not what someone tells you is going to be the best deal, but what you feel is going to be the best deal, you see.

Some of those proposals were some of the best I'd seen, by the way. Seventy-five pages. There wasn't a lot of boilerplate or anything else, and it told you how they were going to design the thing, how they were going to go about it, and who was going to run the job, you see.

The interesting thing is that the president of Rocketdyne came down. He said, "We have a question-and-answer period." The way you ran a source board back in those days is, after you viewed the proposal, you made a bunch of notes about questions you had about it. I don't know whether I did it the way everybody else did it, but I felt that what you do is you tell the people, "These are the questions we're going to ask everybody, identical questions for everybody who proposed, you see, so you'll know that. Then there's some specific questions we're going to ask you, which is strictly related to your proposal." I said, "We want you to bring whatever key people you want who are going to answer these questions."

Well, Sam Hoffman came down as president of Rocketdyne, the only one of the guys that high up in the organization. When Sam walked by, he said, "You know, this is really refreshing." He says, "You know, I been bidding on jobs for the United States Government as long as I've been at Rocketdyne, and this is the first time I've ever had an opportunity to talk to the people face-to-face about my proposal and what they're concerned about." He said, "I really like that."

So that was something that I thought was kind of unusual, that we did business that way. By the way, I didn't think Rocketdyne was going to get it. I thought Aerojet was a shoo-in for the job, by the way. Aerojet didn't think we were serious, I believe, because I remember Aerojet's the only one who wanted a bidders' debriefing, you see. Bob Young was president of Aerojet. Bob had been in charge of the Saturn Program at one time, and he went back to Aerojet. He said he wanted to know how come they didn't get the contract.

So I was talking to the local rep, and I said, "Well, ask him how he wants it. Does it want the lawyers, does he want a group of people, or how does he want to do it? Would he like to come and talk to me with a cup of coffee? He can bring one other guy with him."

Finally, Bob said, "Yes, a cup of coffee and one other guy would be all right."

We came down and we talked about the contract, and it turned out he didn't think we were serious and he didn't put very good people on the job. He finally got to where he looked at me and the light shined, he says, "Well, you know, I'm the one who misread it."

So we had the whole rest of the day, he couldn't fly out till that afternoon. I said, "Well, what can I do for the rest of the day?" I said, "Let's go play some golf." So we went out and we played a round of golf. He had a little putt about that long and he missed it. I said, "Bob, that's how come you lost the contract. You misread the green." [Laughter]

Now when you have a debriefing there, you know, you've got to have a battery of lawyers and a million people. It's the biggest mess I ever saw. Fortunately I've never had to handle one of those, but I've been on some source boards where someone else was chairman, where they had to handle all this stuff. Two of them in particular, I guess, the two biggest we had, the SSME [space shuttle main engine] and the SRB were two of them that had a big protest. But no one protested this little job, this job we had. I've never been involved in a protest on any of the boards that I've personally chaired.

RUSNAK: In this particular example it seems like an amazing amount of time to get that type of job done, but looking at the bigger picture, of course, getting a man to the moon in the amount of time given before the end of the decade was certainly a remarkable achievement. Do you remember your thoughts when President Kennedy made the announcement that we would put a man on the moon before the end of the decade?

THIBODAUX: No. Well, I'd seen challenging things done before. You know, I didn't have any doubts we could do it. You just let us go at it, you know. I didn't think it was stupid or anything like that. He just pulled that out of a hat, by the way, you see. It's the thing to do. He didn't have any idea. He shook up Gilruth a little bit, but, you know, Bob was going to be responsible for the whole thing.

Well, we did it. What else can you say? And we did it on time. We had the kind of support from the administration and the public that I had back at Langley as a young engineer working at Langley is what we had, you see. I always had that kind of support. The reason I retired is I no longer had that support here at JSC [Johnson Space Center], you see. For me, it quit being fun anymore, so I decided I might as well go out, you know and do my thing, whatever it is, or do nothing, if I choose to do it. But I was spending more time trying to get authorization to do the things I knew needed to be done than I was doing them, and that was kind of a negative reaction on my part. I couldn't keep that up indefinitely.

RUSNAK: What do you think the biggest technical challenge you faced in getting Apollo to the moon?

THIBODAUX: The biggest technical challenge.

RUSNAK: From the perspective of your division specifically.

THIBODAUX: The biggest one. Well, they all seemed about equal. I don't know. I didn't think there was anything as being bigger than any of the other ones. We kind of rolled with the punches. We did things and we saw which way it was leading us and then we took action to get it back on track again.

The biggest challenge? Well, the biggest challenge was in getting Apollo to the moon. I would think of the biggest challenges are the type of people problems you have, not within my own organization, but dealing with people out in the industry, actually. Then the local project office on occasions, too, by the way. Not Apollo, no. George Low was super. I can't say about [Joseph] Shea, but George was no—there was no problem in dealing with Low. Low was great and Gilruth was great. But there are some people, some people who I dealt with out in industry and other people who were problem areas, that's all.

RUSNAK: Did you find that a particular company was more difficult to work with than another?

THIBODAUX: Well, the best company I ever worked with was McDonnell-Douglas, actually, period. They were kind of unique in that they were in St. Louis and they were kind of the only aerospace outfit in St. Louis, you see and they had a kind of loyalty. The people that they had had been used to working together for long periods of time. When they cut down, they'd drop people off, but they were always the first ones who hired the experienced people, you see. They had a camaraderie that was different from the other companies, actually.

Walter Burke was a prince of a guy and he was accessible to everybody in the organization, you know. Anybody could talk to Walt. Walter knew everybody. He wasn't just sitting in the little ivory tower there, where everybody had to go to the secretary and get permission to talk to Walter, you see. Anybody in the organization, Walter knew who they were, you know. In fact, he knew everybody in the manufacturing line. I never went to McDonnell-Douglas where Walter didn't grab me and say, "Let's go out in the shop." He'd go out in the shop and he'd say, "Hey, Joe, how's Mary doing?" Joe would get the old wipes and wipe his greasy hands and he'd shake hands with Walter. Walt said, "Let me see [unclear]." He'd talk about things and he'd walk all through the whole plant. They had a very, very—looked like a very efficient manufacturing operation, you see.

When you'd talk to some of his people, he'd say, in essence, if you questioned them, they'd say, "Well, I can't tell you that. Walter told me that we're not ready to talk about that yet." They wouldn't give you any facetious answer or try to lead you around. That told me, hey, maybe if I don't like the answer, I'll go talk to Walter about it, you see, which was real nice, you see, that they were honest people, is what they were.

Other people, other organizations, there was no company loyalty at all, you know. On the West Coast generally, there was a bunch of job jumpers there. The contracts were changing all the time and people moving around, and I didn't see any of that type of loyalty amongst some of the employees, actually.

Some outfits were pretty good. I liked Rocketdyne. Mr. Sam Hoffman was one of the finest guys I know. He was honest as the day is long and he was a straight-shooter, period. You could trust Sam. You had no problems trusting Sam, period. Sam always did what he agreed to do, whether you had a contract or not, at least when I was working with him on the ascent engine. He also told me the ascent engine was the finest contract he ever had with the United States Government, too. Said he made every cent of profit he bargained for, and he said it was the finest contracts he ever had. Dollar-wise, he said, it was piddling. He said he had billion-dollar contracts, that's just a little 12-million-dollar thing. A drop in the bucket. But he said, "It's the best contract I ever had."

RUSNAK: Certainly something for you to be proud of, having put it together.

THIBODAUX: Yes. Well, in fact, the whole organization should be proud of, because the reason the program succeeded is because everybody in the program office thought it would fail, by the way. [Laughter] Not George. I'm talking about the other people in the program office. Consequently, I never got any help. It turns out that they assigned one guy, I guess, in the program office who stuck with me, and he was the kind of planning and program type of guy. He was an Air Force guy. Dennis Johnson.

We did things different on that job from what anybody else does, in order to make it work. We have all this paper, more or less, we have a change control board, you see. So the first thing we had is we had to have a change control board. The account was so small there that nobody in program office wanted a manage change control board, so they told Max, "You run the change control board." Knowing Max, Max don't mess with stuff like that. He says, "Guy, you run the change control board." I says, "Great."

So we ran the change control board on the floor of Rocketdyne, is where we ran the change control board, with the two guys who were responsible to me were getting the job done. There was a guy name Bill Liston [phonetic] and Cecil Gibson down here. They were the change control board. They got together with Rocketdyne and they decided the changes and they authorized the changes.

The other thing Sam told me, he says, "You know, you technical guys take a lot of credit for a contract, but what you had, you had one of the best contracting officers I've ever dealt with." He says, "The reason he was a great contracting officer is that the paperwork always followed the decision." You see, "I could decide, but I knew the paperwork would follow and I would be covered." He said, "I could trust." He says, "My own people didn't trust him, and they told me, 'Hey, Sam, you can't do this. You don't have the contract. You don't have the change.'" Sam says, "We're going to go ahead and do it anyway."

So he trusted us and he trusted the contractor. Unfortunately, because of what [William M.] Chastain did, they put nasty letters in his 201 file about him doing things that he wasn't supposed to do here. I had to write glowing tributes to Chastain to put in his 201 file to counteract some of that stuff, you see, because he was doing things as we wanted them done.

I'll never forget the first—oh, there was a couple others, some other interesting stories. First day I went out after we got the contract underway there, Rocketdyne came by, says, "You know, there's one thing we overlooked. That facility we have out at Reno, it's just not capable of testing an engine this big." He says, "We're going to have to do something about that."

So we asked him, "Well, what are you going to have to do about it?"

They told us. "How much money you want for it?"

"Go ahead. Start now," you see. And those types of decisions they got were really great.

The guy running the program was an Air Force general, Bolender, Carroll [H.] Bolender, "Rip." Rip was a kind of interesting character. He had been running Air Force projects. All during this process of making decisions on the ascent engine with Rocketdyne, Rip came to every meeting we had.

The other thing, Charlie [Charles M.] Duke was the astronaut who was assigned to work with us, and Charlie's one of the real great guys I admire as an astronaut. So Rip used to go to all these meetings, but Rip never attended the meeting, you see. Rip would get out there, and pretty soon the phone would ring and Rip would be on the phone here. He's coordinating twelve-, fifteen-page telegrams and he's doing all this other stuff on there. And he's missing the meeting, you see. He calls me and says, "Hey, I want to go to the plant at two o'clock. I want to see how it goes." I says, "Go." [Laughter] I wasn't going to go with him. I wasn't going to look at no plant at two o'clock in the morning.

Every meeting he was in, he was on the phone all the time, so he was riding back on an airplane one night, coming back, he says, "Hey, how do you think I'm running the program?"

I said, "I'm glad you asked me. I'm the only guy who would tell you."

He says, "What do you mean?"

I said, "How in the hell did you ever get to be a general?"

He said, "What?"

I Said, "I thought a general knew how to delegate responsibility and authority." I said, "You know, you come to all these meetings and you're supposed to be paying attention to what's going on, and you're on the phone all the time coordinating all these damn papers and telegrams and letters. Do you ever hear me get a phone call?"

He says, "No."

I said, "Good reason for that." I said, "I've got a guy you call the deputy back there, and when I'm out of town, he takes care of everything. So why don't you go get you a deputy."

Well, he went back, and Owen [E.] Morris became his deputy. After that, he never attended another meeting. So there are things that are kind of funny little things that happened like that, kind of amusing to me.

RUSNAK: You mentioned Charlie Duke. What sort of input did the astronauts have, and how valuable did you feel their contributions were in your area?

THIBODAUX: He was very supportive. We needed his support, actually. They were going to fly the thing. George Low was looking for astronaut office support. They gave Charlie—Charlie goes talks to them about what's generally going on. He was the spokesman for the whole crew. Likewise, he had a desk in my division, in fact. He was over there all the time. He attended every meeting we had, participated in the meetings. He was the only astronaut we had.

[Charles C.] Pete Conrad [Jr.] used to come to meetings for social occasions there three years ago when he was assigned to it, but I don't recall Pete ever spending an awful lot of time at that. But Charlie participated in the meetings. He asked questions and he was a real participant. He was pretty much living with us as much as he could during that time frame, but never missed a meeting, always there. He's just a nice person. In fact, he's one of the few guys still married to the same wife, I believe, he and Dottie. He's got a house overlooking the first hole on Orlando Park Golf Course up in New Braunfels right now. Charlie, like I say, was a really—lived with us. He was part of the project. He was a member of the team, a real member of the team, not just lip service to it.

RUSNAK: Fantastic. In the beginning of 1967, obviously, they had the Apollo 1 fire. What was the impact of that on your division? What are your thoughts on that particular incident?

THIBODAUX: Well, the impact on the division was I had the only facility capable of running all the tests there to figure out what went wrong and how to fix it. I wasn't in charge. By the way, structures and mechanics division was in charge of it. Dick [Richard W.] Bricker was the guy in charge of it. He was section head over in structures and mechanics.

But we had the only facilities you could do the work in, you see, so all the work on Apollo 5, the fireproofing it, to check out the theories as to what caused it there were all done out at the Thermochemical Test Area. A group of my people are the ones who did all the effort, we set up the tests. They were in charge and they told us what they wanted, but we did fundamentally all the hands-on types of operations there for that.

We also were doing some work for the FAA [Federal Aviation Administration] on fireproofing airplanes. There used to be an old Boeing 737 fuselage out there in the Thermochemical Test Area at one time that we used to do fire tests on, which was a follow-on, actually, as a result of the work we did on that Apollo fire test. We weren't in charge of it. We were just the agents of the organization that did the operations and the testing for them. They wrote the requirements and the data they required, and we ran the tests for them.

So that's the basic impact on me, anyhow, and, of course, it slowed things down. But in retrospect, it didn't come as a big surprise. I think a lot of folks knew that was a problem. I think Joe Shea had a problem understanding what he was being told. He took it very hard, which he should have. Joe was the type of person that he had to make every decision himself; he couldn't let anybody else make any decisions. He had such a big ego about it.

Gilruth and I and a few others have some opinions about people's backgrounds, for example. Bob calls them electronickers, you know, and they're used to dealing with little—not big hardware, but they're used to dealing with little circuitry and they're very highly mathematical and everything else, but they don't really know a lot of about mechanical things, fluid systems, or thermal protection or anything else.

Joe was warned, I believe, but he just wouldn't listen. He was a tough guy to work for. I could never work for him, in fact. Fortunately, I worked for Max. Like I say, you could pick who you worked for. I worked for Max, I worked for Gilruth. I picked who I wanted to work for, whom I'm going to report to. Joe [Joseph N.] Kotanchik went over. Shea wanted Kotanchik to go over there, and Kotanchik couldn't take it. He left after a short period of time. I know other people, the more competent people I know, who couldn't work for Shea.

A young fellow, Harry [L.] Reynolds, he brought down here to run the LM program. Well, Harry lasted a few months and went back to California, saying his wife didn't want to move down here. Harry was a very good propulsion man, too, by the way. He was good all-around. He'd been in charge of the nuclear ramjet program, which was a real beast to try to work on and develop.

RUSNAK: In 1968, Apollo finally got off the ground with Apollo 7, and the next mission, Apollo 8, where they were sending the command service module to the moon without the lunar module. How did you feel about that decision, without having that backup?

THIBODAUX: Didn't bother me a bit. I'd have flown Apollo myself. Shuttle, I don't think I'd ever get anybody, and I don't know why, but the Apollo I'd have flown. I'd been willing to ride it as a passenger or anything else. I had absolute confidence in the Apollo Program.

The Apollo was done pretty much with what I call us, you know. The NACA people were in charge of the thing, Gilruth, Max, a bunch of guys who grew up in the environment that we grew up in. I had absolute confidence in the decision-making capability. I can't say I had that in too many other programs.

Of course, we didn't have a lot of financial constraints either. That's the other thing. We went down a lot of—I won't say blind—we did a lot of things we didn't need to do. We weren't sure of what we had to do, but we did a lot of things we didn't need to do. Like these ideas you get to where you go off on tangents, you know, and you find out, no, that's not what you really want. So we did a lot of work, collateral work, off line, that didn't show up in the program, and for good reasons. Everything wasn't perfect. We made a lot of mistakes, but we didn't make any of them that kept us from getting to the moon, except once.

RUSNAK: That's true.

THIBODAUX: Yes, we didn't make any mistake that kept from getting to the moon.

RUSNAK: Do you remember where you were when you finally did make it to the moon, when Apollo 11 landed?

THIBODAUX: Yes, I was in the viewing room over in Mission Control. I had a lot of folks living over there. I had some reporters living over at my house, the spillover there, asked us if we could furnish a room for some of them, I guess. Me and my wife had a lot of guests over there, I guess, relatives who wanted to be close to the action. I don't know. [Laughter] But I was in the viewing room when Apollo 11 landed.

RUSNAK: What were your feelings at the time?

THIBODAUX: Well, I was just happy as hell. I mean, why not?

RUSNAK: Sure.

THIBODAUX: We finally did it, you know. Gilruth said after 11 he wanted to can the program. He was finished with it. He said, "It's all just a waste of time and money to keep on going back to the moon." He might be right, I don't know, you know.

RUSNAK: Did you sense that there might be some waning interest in Apollo after the first landing?

THIBODAUX: Yes, it was downhill from then. Yes, it had to be downhill from there, you know. That's the pinnacle and everything. It's a tough act to follow. You do it once, and, like Gilruth said, why do you want to do it again? He was ready to quit Mercury after John [H.] Glenn [Jr.] flew. He said, "Why would you want to do it again? Let's get on with something else."

I don't know if you've ever read his interviews on the National Air and Space Museum. He had six of them there that he did. The last ones, I can see Alzheimer's coming in in his last few, you know, as he progressed there. I can tell that he was a little bit slower on the uptake and he's got to be questioned a little bit extra about a lot of things. But I think they ended in '87, as I recall, they did the last interview.

We did things out of sequence, is what happened. Kennedy got us in trouble by getting us to do the big thing first, and then, you know, it's kind of a tough act to follow when you get down to it, as far as manned operations go. If I'd have been doing it, in charge, the next thing we'd have been doing is we'd have been building a colony on the moon, you see, not going back to a Space Station. That would have been a big challenge, you see, because we—well, I'm not too sure the agency could do that.

The biggest problem is the mind-set people have these days. You see, when we colonized the country going from the East Coast to the West Coast, we didn't have to have Howard Johnson's and hotels, you know, the Holiday Inns and everything else for those people to stay at. Well, the way we want to run this program is we have to do that way.

See, the astronauts should have been in the infantry in World War II and they'd have learned a little bit about some things that people don't understand. In fact, everybody should have been there. You learn things about trust and camaraderie, and you learn how privacy is so damned unimportant, you know. [Laughter] When you live in adverse situations there, you get down to the fundamentals, you see.

In my wartime experiences I lived amongst a bunch of very primitive people where survival was the name of the game, when you really get down to it. It was different when I went to school, in fact. When I went to school, you see, my total college education cost my father $1,040 out of his pocket, the entire thing, and then I earned whatever it was to make up the difference, you see. The difference is I never had less than three roommates and I lived in a little room that was about this big and it had two double-decker bunks, it had a table and four chairs, it had four lockers on the wall, it was bare. It didn't have air-conditioning, it didn't have none of that stuff. We had a communal bathroom across the hall, because that's—and there's absolutely nothing wrong.

Everybody wants their own room and they all sorts of stuff that goes on these days. They've got to have a car. They can't do anything these days. They got to have all these—

it'd be real nice to put everybody in the same situation as I grew up in, and they'd a learn a few things about the fundamentals of life that I don't see anybody learning these days. You learn what's important and what's not important real quick.

RUSNAK: Sure. Well, getting back to Apollo, during actual flight operations did you have any involvement with the flights at all?

THIBODAUX: Not personally, no. I had a whole group of people in Building 45. That's where the brains of the operation is. It ain't in flight operations, by the way. All the subsystem managers and the people who started out with the thing from the concept of where it started through all design, development and know everything about every test phase and operation, all the real knowledge of the systems rests right there. Whenever anything happens there that nobody else can handle, they have to call up 45 and they talk to the people in 45. So that's fundamentally what—so I had relatively little to do except to go relax in the viewing room and watch the operations for a while until they got boring, and then go do something else I was supposed to do.

RUSNAK: What about during Apollo 13? Did you have involvement with that?

THIBODAUX: [Laughter] Yes, I was very heavily involved in that, yes. I was up in Washington when that happened. We were doing a source evaluation board on Phase B Shuttle studies, I believe, and I was up there when that came apart. I got a call and came home immediately. Bob Gilruth was—first thing I did, I got involved in taking a look at everything that went wrong and why it went wrong, you see. They had a great big investigative board, I guess. Ed [Edgar M.] Cortright, I believe, was in charge of that. They had every center director on the board, Hans Mark, Jack Clark. Who else? I'm not sure. They had this big board to try to figure out what went wrong.

We were the ones that figured out what went wrong and fixed it, actually. We were responsible for it. They had people from all over, Marshall people down here. William Rosnick [phonetic] was kind of chief engineer at Marshall, he came down. He took one look, he says, "We don't need to be here." He says, "You've got everything you need and you know what you're doing." He said, "We're going home." He never came back.

I'm convinced if I had sent my subsystem manager, Shelby [L.] Owens, down there that would have never happened. I'm absolutely convinced. We always had a struggle with the Cape people. They never wanted us around for some reason or other. They said, "We're big boys and we can handle everything. We don't need you down here." We were getting a little tight on travel, and we were trying to handle all these problems over the telephone, actually. I think if I had insisted that George send Shelby down there, he'd have picked that up and could have figured that whole thing out.

It was a funny sequence of three or four things we stacked up that were not very probable that it would happen. It goes back to a long time ago when somebody made a decision on putting the kind of heater they put in there and then putting the kind of thermostatic switch there to make sure that they didn't get overheating in the tank. Plus some real bad decisions by the Cape, actually.

The biggest problem I have with the Cape, particularly [G.] Merritt Preston, I guess, always wanted to run tests on everything, flight test everything down at the Cape, you see. He wanted to pre-flight them, take the Apollo command service module and have it tested, and fire that one before it launched. Well, the first one he tested, he screwed up. When it flew, it had a failure, actually, as a result of the testing, by the way. He wanted to test all the lunar modules. I finally got my way when I told him, "No, we ain't gonna do that." So we didn't do that.

The reason the thing failed is that they had to run one of these tests they call a countdown demonstration test, where they roll the whole thing out to the pad and then they put all the fluids in and check everything out, the loading and the servicing and everything else. Then they detank it and roll it back, you see. Well, I guess that's all right to check out the GIC, but the basic problem was, there was a flaw in the tank, one problem we had, and the other problem was procedural errors there.

The tank has a standpipe that comes down to the bottom and there's a little elbow in there that's made out of Teflon that goes into the fill and drain thing. That little elbow was loose; it didn't fit tight on there. Normally the cryogens in the tank normally are set up to what they call a super critical state. They're basically a very, very dense gas, is what they are. As soon as you drop the pressure, it all reverts back to a liquid. The way you detank is you put pressure on the top of the liquid and you push it down and you push it out that pipe, you see. Well, if the pipe is broken up at the top, you don't push down on it, you push it out the pipe.

So the only way they had to get rid of it is to boil it out. They start to boil it out in the normal procedure, 28 volts, and it's taking too damned long to boil it out. So they said, oh, well, let's go look at the specs on the heater. The heater says it will take a lot more voltage than that, so if we double the voltage there 56 volts, that gives us four times as much as energy in the tank and we're going to get it out four times as fast.

They had this switch in there that was going to tell it to shut the heater off if it overheated. Well, they didn't realize the switch wouldn't take 56 volts. So when they put 56 volts on it, it fused the contacts shut, so that they kept on supplying heat, and long after the thing had gone out, the temperature in the tank probably got between six and nine hundred degrees. I'm not sure exactly what the number is. It damaged the insulation on the wires, is what it did.

What happens is the wires have Teflon around it, Teflon-coated, and they have oxygen in the tank. Well, one thing most people don't understand about Teflon, they think it's inert. Teflon's a very powerful oxidizing agent, you see. I used to use Teflon as an oxidizing agent in rocket ignitors, for example. Magnesium and Teflon is a great explosive mixture. Same thing with freon, by the way. People don't understand that either, you see. They normally think of these as inert. Well, they're not inert in certain circumstances.

What happens, Teflon and copper will react, for example. Teflon, at elevated temperatures, will react to things like Teflon and magnesium blows up, you see. It's a pyrotechnic mixture for setting off rockets, in fact. They have fans in the tank, actually, to circulate the fluid, because in zero gravity there you get what you call stratification, and when you put heat into the heaters there, if there's no swirling around, it just travels through conduction, you just get a hot gas bubble that moves around, you see. So in order to get everything the same temperature, you turn the fan on, it swishes it around and mixes it, you see.

What happened when they turned the fan on, basically, they got a disturbance and some of the wires shorted out, and then that started the fire. The wire came up through the wiring bundle and into a little pipe we have that all the wires came through with a connector on it. All that did was actually overheated that in-canal tube to where it couldn't stand the pressure. The tube is what burst. There was no big explosion.

The problem was a little tube about half-inch in diameter, five-eighths inch in diameter is what burst. Of course, with full vacuum on one side and the fact that here it is spewing out this hot gas in there with oxygen, we had a lot of combustibles inside the bay, and it just blew the door off, is all that happened. So there was no huge explosion.

We were able to go in a ground test, actually, and almost duplicate that time line perfectly, actually, and prove beyond a shadow of a doubt what happened as a result in ground test. It was a brilliant piece of detective work, with that little information, to be able to come back and know exactly what happened and why it happened and how.

The way to fix it was to spend a few bucks on changing one procedure: don't ever do that that way again. Number two, put a new switch, you see. Well, of course, it happened before God and everybody and it's politically incorrect to just make a little fix like that. You've got to make a big hullabaloo about it, spend lots of money. So we went and redesigned the guts of the tank, which to most of us technical folks was totally unnecessary to do that much redesign.

I saw an interview Chris [Kraft] did recently, he agrees with that, too, and George Low agreed, said, "Ain't nothing I can do. We're going to have to do more than that to satisfy everybody."

RUSNAK: Did you get involved with any earlier studies to use the LM as a lifeboat?

THIBODAUX: Yes, not studies there. We always knew that was a mode, using the descent engine, for example, the way we did, and the descent engine was qualified for all that. We did all the tests on that in anticipation of something like that happening. So that was no big deal in my way of looking at it, because we really had planned for the system to have that capability. It's just nothing new to us, for example, that it was going to do it. We never thought we'd have to do it.

There are a few things I never thought I'd have to do either. I never thought I'd have a LM entering the Pacific Ocean, you see, because I had a big battle with the Atomic Energy Commission over that radioisotope thermoelectric generator, you see. We got tied up with AEC on development of that, and they still wanted to do more tests on that thing, you know, and we were getting ready to fly Apollo 11. We went round and round over that. I remember talking to some guys, telling them, "Well, you know, we're not going to go those tests."

"Well, you ain't going to fly it."

I said, "Well, we'll see about that." I says, "You tell me that Seaborg [phonetic], the head of the Atomic Energy Commission, and Webb are going to get together and Seaborg's going to say, 'You ain't going to be able to go to the moon, because I don't think this thing's ready to fly'?" I said, "It'll be ready to fly." And it was.

But I could never conceive of that thing ever coming back in the Pacific Ocean, either. Of course, it didn't break apart or anything. The whole thing they were worried about was it coming apart and doing all those extra tests. So that was one big surprise I got.

Then we had another LM landed in Colombia, I guess, where some of the natives brought some of the cryogenic storage tanks back of the Colombian jungles there one time.

But those modes of operation had all been thought up beforehand. I don't know why everybody was surprised about that, and we'd actually tested the system for that. One of the reasons I told Merritt we're not going to do any LM testing, I see an awful lot of wasted stuff, you see. Turns out it's kind of funny, but from an operational viewpoint there, as soon as the LM is buttoned up on the Apollo, until they come back ain't nobody touches it. Nobody services it. You can't do anything about it. You're stuck with it. Since you can't do anything about it, you have to accept the fact that's the way it is and it's going to work.

So from the time we loaded it up at the Cape until the time it came back, there's no operations people, there's nobody can do anything to it. It's either going to work or it's not going to work. So why do you have to fire it? I mean, you know, once you fire it on the moon to see whether it's going to lift off, I mean, it may never get off. There is certain testing that you have to do, but there comes a time where you have confidence that what you've done is the right thing and that you don't need to do anything more.

I know Chuck Mathews never wanted to do any testing on anything after a certain point. He had a good reason for that. I didn't always agree with him, but from a programmatic viewpoint, if you find anything that causes a problem in a condition that it's not going to operate under, that you don't think you're going to see in a spacecraft, it causes him problems to try to answer all the press and everybody else who creeps up out of the woodwork, you see. So you have to be very careful about the testing you do after you think you're ready, because you should have found all the problems up to that point.

Chuck never wanted us to do any testing. Kenny [Kenneth S.] Kleinknecht is that way, too. Kenny was a real guy, too, by the way, to work for. He probably was one of the best and most experienced program manager we ever had. He started work on the X-15 and he was involved in Mercury, Gemini, and Apollo, you see. I don't know why he never got the job of running the Shuttle Program. He'd probably have been really ideal to do that. He's a guy I liked to work for. I could choose him to work for, you see, because he's an old NACA type, too, and he understood the ways we did business, you see.

RUSNAK: With one of the other flights, Apollo 16, there was a little bit of a problem with the service propulsion system engine, I think a gimbal problem while they were in lunar orbit. Do you remember anything about that?

THIBODAUX: Apollo 16.

RUSNAK: There was an oscillation with the SPS [service propulsion system].

THIBODAUX: What kind of—I don't remember anything about that. If it was the gimbal, the gimbals weren't my problem.

RUSNAK: Okay.

THIBODAUX: See, if for some reason or other there that I—that was the guys in navigation control, since that took all its input from everything they had, and their net result reacted to do it, but I didn't do anything about that, the gimbals. All I did is I had a hard point between the spacecraft and the engine for it to work, you see, and I always try to separate things out into where they belong. [Laughter] There's no reason for me to get involved in that gimbal thing.

RUSNAK: Okay.

THIBODAUX: There are other reasons, for example, in some systems that I do have to get involved in, like on the Shuttle, the hydraulic system, the actuators, I had to worry about that, because we had the entire hydraulic system, you see, or things like that. The important thing is to design what I call clean interfaces, you see. If you don't have a clean interface, it causes nothing but problems, you see.

One of the worst interfaces we ever had is that on the Shuttle, Marshall has the external tank, you see, and Marshall has the SSME, the Shuttle main engines. The Shuttle main engines are bolted to the Orbiter and the tanks bolt to the Orbiter. But all the pieces and everything which connects those two together is part of the Obiter, and I was responsible for it, you see. So I got interfaces on both ends. I got the SSME.

We had to have a big committee of us and Marshall and everybody involved, and all the contractors sit up here and they haggle about who's responsible for what and who's going to do what and who's going to make what work, you see.

The main propulsion system, we treated the entire thing as the main propulsion system. I was kind of caught in the middle between the two Marshall things, actually. Then all the contractors, by the way, too, so that was a big bag of worms to try to deal with. You know, dealing with the physical interface is having to deal with the people. You asked if I had any problem. Yes, the people, just trying to get people to work together and to recognize their responsibilities. That's what the big problems are. The technical problems are fairly simple, in a way, in a lot of ways. You just don't stick your neck too far, that's all.

RUSNAK: When was the first time you heard the idea of having a Space Shuttle? What was your involvement with that?

THIBODAUX: Well, I guess '68 or '69. You're talking "the Space Shuttle," yes. It was something that was going to be manned and NASA was going to do it. Yes, probably '68. Well, Max was trying to decide what we were going to do next, you see, and then in '69 he set up this bunch over in Building 32, I guess, where we all furnished people to. I furnished some propulsion people and some design and structures, and they all got together with Chamberlin kind of masterminding the whole thing, trying to come up with a first cut at what the system might look like.

Later on, the Air Force got into the act and put all sorts of requirements but no money into it, and then backed out in the end. That complicated the deal.

The Shuttle is a more difficult thing to do, by the way, than the Apollo was, believe it or not. Far more difficult for me, anyway.

RUSNAK: How so?

THIBODAUX: Yes.

RUSNAK: In what regards?

THIBODAUX: Apollo, we knew exactly what we had to do. Shuttle was everything for everybody, you see. The Air Force wanted cross-range. It had to be able to land all sorts of different sites. It had to do all sorts of different missions. Apollo, the whole goal was to put a man on the moon at this spot and get him back. That was very distinct and something anybody could understand. The Shuttle, no one knew what it was going to be used for, so it had to be everything for everybody, you see. You had to provide all this capability anybody wanted, no matter what mission they wanted to do or where they wanted to land. That's particularly the Air Force's requirement for 1,200 miles cross-range is what really drove a lot of the design, actually.

Then the other thing made that difficult was, I don't know who was responsible for it, I hate to give anybody credit for it, but the most important thing that happened in the Space Shuttle is the fact that it was managed in a very, very weird, totally foreign environment. All the other programs we ever did there we started out and we build up to a great big peak during development and then we dropped way off towards the end. The Shuttle was done with fixed-year dollars over the entire part of the program, and that makes it where you have to fit all the little pieces of this jigsaw puzzle together. The most efficient way to run a program is everything has to run at a dead heat at the finish line, you see. If you'd have one system that's way advanced of everybody, you have to shut that down and wait for the others to catch up, and then you have to reactive it. A big waste of money and time and effort, you see.

So that from a management viewpoint, the most efficient program you can run is to plan everything so it runs a dead heat to the finish line, and that way nothing gets out of kilter. You don't have to put more money in to accelerate something, or you don't have to slow it down.

There are a lot of things that I don't know whether you could have done it any better or not. You'll never have another chance, see, to figure it out, so there's no point—that's kind of a moot question whether you could have done it any better, because you'll never get a chance to see. You can only have opinions about that.

But the Shuttle, if I say the most significant thing about the Shuttle is the fact that it was done in that environment, you see. But I'm sure that it caused—I know it caused a great deal of inefficiencies, particularly after the program was over. The biggest problem they have is spare parts, you see. No one ever wanted spare the thing. When one of them breaks down, they would cannibalize the other spacecraft all the time. See, that doesn't make it a little bit better.

Then there are a lot of things that were wrong with the basic design, wrong in the sense that, no, it doesn't mean it's going to kill the program or be super dangerous, but that there are some simple things you could have done earlier in the program that would have cost less to have done then than it costs to do later on. There were quite a few deficiencies or things that they couldn't quite do as well as they should have done. The brakes were bad and they had to go and fix the brakes. There are other parts of the system there that it's been a little bit more money to break down, but it resulted in less cost in the long run.

RUSNAK: You said earlier from a propulsion prospective you used a lot of the Apollo hardware for that?

THIBODAUX: Well, the technology. You use the same propellants systems, for example. They all use hypergolics in there and we used hydrogen and oxygen fuel cells that were very similar. They're basic fuel cells. We had two options, in fact, for fuel cells. We had GE under contract as well as Pratt & Whitney, and they were both running neck and neck, but GE had a damn failure, a critical failure, in the later development testing, or crossover there, where the hydrogen and oxygen got together and started a fire within the system. Then that kind of was a black mark for GE.

I remember Joe McNamara was heading the Apollo Program at the time for Rockwell, I believe, and we went out there and they were kind of waffling about what decision, whether we wanted to go GE, one of the people we had under this. This R&D contract we had, actually, that I had out of the center was GE and Pratt. They were kind of wanting to put the screws to me in helping me help them make that decision. I told I wasn't about to, that was their responsibility to make the damn decision, and they were going to have to live with that decision. I wasn't going to tell them who they ought to pick.

So they decided, well, Pratt & Whitney was their contractor, there wasn't a problem and they'd pick Pratt & Whitney. That was perfectly all right with me. It didn't make any difference. I thought GE had a little bit more potential, actually, in the long run.

I believe in that sign Joe Shea used to have on his desk, the motto of the Russian Academy of Sciences, and something that everyone should believe in, and I'm sure Kleinknecht believed it. "Better is enemy of good." It only has to be good enough, it don't have to be the best, you see. That's got to be good enough where you have confidence in it, but you don't have to keep on throwing money after something to make it better than it needs to be.

RUSNAK: For the reaction control system or the OMS engines, were there alternatives that you explored?

THIBODAUX: Yes, we had R&D contracts. R&D contracts, actually, with people. We had some with, I think, TRW. I had a number of very small offline things, R&D-type contracts. One of them in particular was with Aerojet, was for this platelet technology. The way it turned out was nothing what they intended it to be. We had to kind of straighten them out on that. It turned out to be an injector with much more conventional mode of operation than they anticipated, actually.

The only problem I had with Aerojet and with this platelet technology, what's so great about platelet technology is that since you use a photographic process where you draw pictures on a piece of paper and then you photograph that and then you project that on sheets of metal and you etch it and then you diffusion-bond it, you can get an entirely new injector design built in two weeks' time. See, the others, it took months and months. The biggest problem I had with Bell, they could never turn an injector out. It took months. Well, this way you can do that.

My problem was to keep them from trying too damned many different designs, you see, I mean to settle on one, because before, you had to try different designs, because so many of them didn't work. Well, now you could do this instantly, you see, or with a very short turnaround time. It was one of the best things I saw about the technology, in fact, from a development standpoint. It's so simple to try different things, you know. It doesn't take you a lot of time. And that's what appealed to me with that technology.

TRW had a—they tried to ride the LM technology for a long, long time, actually. The LM technology is kind of interesting in that the LM is a throttlable engine, you see. It's the only engine we had throttlable and it throttled over a very wide range, ten to one, you see. It's difficult to do because all these things happen in a rocket engine there as you throttled. You could run into all sorts of problems. So it's a very difficult job. You pay a penalty in performance for that, you see.

TRW kept trying to sell that for fixed thrust injectors all the time, you see, and it just couldn't meet the performances. A few seconds of specific impulse can make a big difference, actually. So I had problems convincing TRW that they shouldn't use that technology, and they were pretty adamant that wanted to continue to use that.

I don't know who else we had contracts with, but the Aerojet contract was obviously the—the R&D contract we had with them surfaced this technology, which looked from a reusability—people said you couldn't reuse the thing because the plates would separate after a while. Well, we didn't believe that, so we ran enough tests where that didn't happen.

The other thing on the Shuttle, for example, was the zero-G technology we developed in the tanks, you see, for the propellant tanks. That was probably a big deal, actually, because in Apollo we had part of the displacement tanks, we had bladders that you just crushed the bladder around a pipe and it squirted everything out, you see. Well, the bladders are not reusable. You use them two times and they develop cracks, and pretty soon you're in deep trouble, so that we had to have a reusable technology.

The other thing is try and push it out with pistons, you know. That's too heavy and it's a very poor design. So we had to use basically what we called capillary-action tanks, where we used capillary action to suck, to get the propellant into a position where it sucks it out where you want to suck it out, where only liquid flows out and not gas. That was one of the big challenges, I think, where we did things totally different than Apollo for a good reason—I mean in the Shuttle, than we did for Apollo for a good reason.

We had zero-G cans, we called them, screens in there, but none of the sophistication like we have in the Shuttle. It's quite a bit different. So that was probably where we went out on technology.

We also got involved in the Shuttle on some turbine work, actually. All of our previous systems, we basically used gas pressurization to force the fluids out there, you see, no pumps. We had no pumps at all. We got involved in the hydraulic system and we had to have pumps, and at the same time we had to have something to power the pumps. So we got involved in some turbine technology on that and we did some R&D work, in fact, on that before we got involved in the program.

There were a number of commercial aircraft, actually. The Concorde, for example, has something to give them power enough to land the airplane in case they have an electrical failure. There are two or three things like that around, and we took a look at some of those and didn't particularly like those for the Shuttle. But we picked one concept there that's flying right now that looks pretty good and has been very successful.

The only problem with that is that Marshall uses for the SSME, for the hydraulic system, they use essentially the same system we use. Theirs has to run for 110 seconds. Ours has to run for a lot longer time and be totally reusable, you see. We had two separate contracts at Sundstrand with contract dollars and everything else, and they had one difference in what they wanted in the design. They wanted their valves designed differently from ours, and they wanted a way to put an edge to spin the turbine up to make sure it spun, which I thought was asinine.

Consequently, we went to all the expense of building two separate units with two separate project offices. There's no reason why. If I'd have been running the program, I'd have said, "Hey, you're going to use the same one, and we're going to have one do it, and that way we save an awful lot of money." I never could understand the logic for letting them go ahead. Same contractor, same identical size unit, same everything else, but two totally different management systems for such very, very small differences. Our was a driving requirement, actually, you see, and they could have very well used ours. We'd have saved a mint on that.

RUSNAK: Earlier you mentioned the APUs [auxiliary power units] and the flight control hydraulics. Was there any thought given to using an all-electric fly-by wires?

THIBODAUX: Oh, yes, yes, sure. Jim Ackerman wanted to do that in the worst way, and we even had a system operated behind guidance and control there that was actuators and motors and stuff like that. That's a long time ago, long before anybody else was ever doing that, too, I'm talking about. I've been retired twenty years now, and that must have been another seven or eight years before that. Yes, they were coming out with new electric motors, that had a horsepower per pound, [unclear] cobalt horsepower motors. You had new gear train systems like that.

The thing that killed that was Bob Gardner, who was director of electronics over at the center, and he said, "You can't find any diodes which will switch that amount of current that fast." Inability to have switching currents to switch large amounts of current as fast as you're required was the one long pole in the tent at that time. I'm not sure that was the whole reason why we didn't do it. There were probably a lot of other people didn't want to do it either, but as far as I was concerned, it was a doable thing at that time. You have to furnish electric power for it instead of hydraulic power, that's all. It would have a simple integration of the G and C. It would have been simple integration all the way around, too, I think, to do that.

We hadn't thought of it long before we came up here. That was a definite consideration. We debated that quite a bit, in fact, with the various people. So it's not anything new. I get tickled while young engineers come up with all these ideas, and I tell them, "Well, what you want to do is, there's a place, a little building over there behind Building 45, and what you do is, before you have these brilliant ideas, what you do is, you go do some research on it. The place you do your research is called the library, and you go find out maybe somebody else thought of that twenty years ago, or thirty years ago. And see what happened to it then." [Laughter]

RUSNAK: Well, we certainly spend a lot of time over there.

THIBODAUX: Well, you know, very few engineers go to the library anymore. Now we got computers and the Internet where people have a much bigger tendency to do research now than they ever did before, by the way. I mean, it's so much easier to do, you see. It won't be too long before they're going to have so many more things on the Net there that you can find. If you know your way around, you can find anything you want to find. I know I'm a big Net fan, and I use it a lot.

RUSNAK: Earlier you said you retired in 1980. What have you been doing since then?

THIBODAUX: Same thing I always did: whatever I feel like doing. [Laughter] I did a little consulting work, but my power base has died or gone away. No one knows who I am anymore or no one wants old guys, anyway. I did some consulting work after the 13 fire, actually. When Henry was running the center, he knew a lot about my background and there were a lot of things that he wanted me to kind of research for him, or at least do battle with against other folks who were trying to do the wrong thing.

I worked for Thiokol. I worked for the National Academy of Public Administration, looking at whether NASA's losing its technical edge there, with a team of people. I looked at that big nuclear blast facility, I did that. I did some consulting work for Thiokol. After the Shuttle, I was kind of the key guy on taking a look at some nuclear techniques for failure analysis there, probability risk assessment there, that we did on some Shuttle components there.

The answers were very, very interesting, actually, that we got. I learned something from it, too. I learned some very important things, for example. We spent an awful lot of time doing all sorts of detailed testing of qualification of components, you see. Our problem is, we only built fifteen or twenty or a hundred of things. You see, the Air Force does this for millions of things, you see, and they have a test base that they use, of failure rates and things like that. I found out that our failure rates are no better than the Air Force's are, no matter how much work we do on it, number one.

The second thing I learned is what I already knew: the cause of most of the failures of the systems I have are leakage. Whether it's leakage through an O-ring in a liquid rocket, in a solid rocket, or whether it's a liquid rocket or whatever, if there's leakage, things that can cause explosions. It can result in refrigeration and freezing. There are a million different things leakage can cause, so that the primary failure that I had in all of the systems that we had to worry about is things leaking.

The Apollo 13 was a leakage. We had a leak in a line, is what caused that problem. What caused the leak is different. There are three things if I could handle, that I could save the government billion of dollars, and those three things are compatibility, contamination, and leakage. Those three single items are the most difficult problem I've had to face. Those are the most difficult problems I had to face. Not solving the problem. Not only solving a problem, but setting realistic specifications, you see. It's the tendency of people to set a specification based on the minimum measurable quantity with the technology you have these days.

Like yesterday I got a thing on all the contaminants in the water I drink, and it's ridiculous when I look at it. I've been drinking that water for years, you know. When they want to measure a fracture of a part per billion of something, you know, when I take a look at all the requirements of all the ninety items that they measure in the water I have, I ask, "Well, why is it set this way?" The requirements set for the minimum thing, the lowest thing you can measure, is what you need to have. In other words, nothing, you see.

So leakage, we say is we have no leaks. It's hard for you to find no leakage. Well, it's the minimum amount of leakage you can detect is no leakage. So if I have an instrument that will test to 10 to the minus-8, well, that's what the requirement is, you know.

Then contamination is the other big problem, you see. We have quite a few. In fact, Gemini 8 had a contamination failure, actually, where the thrusters stuck, I guess, on Gemini 8. I had a lot of problems with tanks blowing up at one time due to contamination. That was a weird problem, too. It had something to do with specifications on the color of nitrogen tetroxide, believe it not. We had tanks blowing up on the East Coast and then not too many are blowing up on West Coast, and then we had tanks start to blow up on the West Coast. We did all sorts of research on that, and we fundamentally found that what had happened is that the one Air Force inspector took a look at the nitrogen tetroxide, and nitrogen tetroxide normally is brown, see, in liquid state. This particular one was more greenish than brown, so he complained to the people who manufactured it, "The color's wrong." Well, they knew how to fix the color, you see, all they do is bubble a little extra oxygen in it to oxidize that nitrogen dioxide, and it turns brown.

Well, some people were using the green stuff, some people were using the brown stuff. The people using the green stuff never had a tank failure. The people using the brown stuff had tank failures. So it's all over. One of the inspects the color, nothing else. Everything was inspected except the color. It turns out that NO, that oxide of nitrogen, is an inhibitor to stress corrosion.

Another example of little things like that, now that I'm thinking, is pyrotechnic failure. The tension tie cutters for the LM were failing. They were dudding, actually. They wouldn't explode. And we couldn't understand why. Well, we have a very detailed inspection process on these devices. Number one, we X-ray all of them, and then what we do is we use neutron radiography to inspect them. Neutron radiography looks at all the light elements and X-rays look at all the heavy elements, so you can look and see things in there.

We had neutron radiographs of every one of these detonators. I had my own engineers there, so I told them I want them to read the neutron radiographs and the X-rays themselves and tell me they're okay, you see. All you have to do is have common sense and a good eye to be a good X-ray reader and understand something about the design of the system, see. So all of a sudden they started looking at these. This one particular one, we could pick out the one that failed and in the explosive mix it was darker than the others. Then we saw some others that were darker, you see.

So we chased that back to one little lady who was building the things. She was putting this material in there, and there was a little bit, around the edges of it, there's a little powder there that's high explosive, and she knew that was dangerous, so the way she got rid of that she is got some isopropyl alcohol and a little paintbrush, and she got in there and she dipped that in. Well, some of that isopropyl alcohol was getting into the regular mix, you see, and so consequently the isopropyl alcohol in there, which has a lot of hydrogen and stuff, it was showing up as dark on the neutron radiograph. That's what was causing the dudding. She was doing something she thought was perfectly all right, you know, and she thought she was doing something to help.

Part of the problem when you go to set a specification or procedures there, is you can tell everybody what they should do, but you can't tell everybody everything they shouldn't do, you see. This is one case where this lady, in all honesty and sincerity, thought she was doing everybody a big favor making them safer to use there. What she was doing is she was causing it to fail.

I don't know, I can't recall all the things, but there are many, many things like that in the program which cropped up, that required a tremendous amount of detective work, you see, on our part.

Now, why did that pass the inspector? Well, I'll tell you why it passed the inspector. This particular plate that comes out there requires thirteen or fourteen signatures on whether it's good or not. Unfortunately, the first guy who signs the plate, the okay on the inspection, all he's doing is certifying that the quality of the neutron radiograph is good enough to read. Well, of course, the system, all of a sudden, the next guy coming in, he's kind of lazy, don't care about what he's doing. "Oh, this guy said it's fine. Hell, I'll just sign off on it, too." So thirteen other guys sign off on the damn thing, none of them ever taking a look at it to understand what the hell the problem was.

So I started having my guys read every one of them from that point and trying to get the message back to our people. So you can make errors of commission as well as errors of omission when you do things.

RUSNAK: Well, if we could stop to change the tape.

THIBODAUX: —who was also one of my bosses. He's the guy who hired Max and I, Paul Purser. He was another great guy to work for, too, by the way. He also was very talented. He was, I guess, head of the general aerodynamics branch back at Langley. It was a section, I guess, when Max and I first went there. Then Max and I got to be section heads. Max and I and Ray Watson were section heads, and Paul Hill's propulsion aerodynamics branch, and Purser had the general aerodynamics branch. Then we had a stability and control branch. Three branches within the division.

We had two or three reorganizations, I guess. One of the reorganizations, I'm trying to think which one, I think that it's when we first started this high temperature branch, actually, which was somewhere in '56, maybe, I guess. They took Paul's old general aerodynamics branch and they took Max and I out of propulsion. I guess we didn't have a propulsion branch anymore, propulsion aerodynamics branch. They combined that. Max picked up part of what was the propulsion aerodynamics branch and part of Purser's old general aerodynamics branch, and then Purser picked me up. Max was my branch head then. We've had either parallel positions or he's been my boss over the years.

They transferred me over to Purser's branch, actually, and then I brought my rocket work over, and then took over what they called the materials application section. I don't even remember the name. Materials research or application, whichever it was.

Paul, he could flood you with a lot of ideas, had a lot of good ideas and a lot of them crazy ideas, you know, but we were very fortunate that if you didn't have any ideas of your own, you worked on the boss' idea. If you had your own, you worked on your own. It was perfectly all right with him. It was always your option, for example, at least for me, and for a lot of the other guys, it was our option what we were going to work on, because I guess they sensed if you were working on things you really believe in and you want to work on, you're going to do a good job. If you're working at some other jobs that you don't like, you're just not going to produce, you see.

Paul was a very good planner. He could visualize things. He could see. He planned this whole high-temperature work and did a beautiful job on that, you see, of what we needed. He didn't do it alone, we all worked together on it, but we came up with these facilities and all the things we really thought we were going to need. Paul was a very rapid writer. He could write a report quicker than anybody I ever saw. He was very concise and he's a good editor, for example. He reads very rapidly, but he's a good organizer.

At one time he headed up the budget office. They used to rotate the engineers back at Langley, try to get some engineers into administration work. So Paul was budget officer for a while. He didn't like that, and he got a chance to go to work for Gilruth, actually, then. It turns out Sherwood Butler was an electrical engineer and he went to work over in the procurement office and he wound up being the procurement officer, actually, for Langley, but he's an electrical engineer, who was a classmate of my CO [commanding officer] in Burma, in fact. [Laughter]

Paul, as I say, he was a great planner, you see, and he also was a guy Gilruth trusted. A lot of things Gilruth didn't care to do, you know, like I don't care to do a lot of things.

Plenty of things Gilruth didn't like to do. He didn't like to mess with all this nitty-gritty stuff. He liked to work with people. He liked to work with projects, programs, and he could care less about how many pencils you needed or how much office space or what kind of building you needed. He cared nothing about that. Paul could handle all that, for some reason or another, you see.

So when we were up in Washington this whole time trying to work out the stuff between NASA and NACA, you see, Paul was kind of working on stuff for Gilruth. Whatever Gilruth wanted, Paul was doing. He wasn't doing a lot. He didn't participate in an awful lot of the technical planning, but the facilities planning and the administrative and the organization stuff Paul worked on.

When he came down here, I'm pretty sure Paul's the guy who built the center for us, you see. He's the guy, since he was able to plan all this high-temperature work and all this other stuff, I'm sure he—he didn't build it, you know, but he got the architects and the engineering and all these people together to decide all the things we need and to come up with the right answers, you see.

He did such a good job on it, that the University of Houston wanted someone to help them with this, and Paul was the guy responsible for the University of Houston at Clear Lake, in fact, that he was on loan to them at the time. He had a brain tumor, and when they removed it, it paralyzed half of his face and it took him quite a few years of therapy and nerve reconstruction before he got well again. He never did come back to the center, actually. But he was one of the key guys, as far as I'm concerned.

His title was special assistant to the director, but he was more than that. He was more than the deputy director was, in fact, as far as I was concerned, you see. If you had a problem you needed to have solved, you just picked up the phone and you said, "Paul, I got a problem." He says, "What is it?" And he says, "I'll take care of it." And that's all you had to say. It was that simple, you see. That's the way we worked back at Langley. Got a problem, call the right guy. "I'll take care of it. Forget about it." Paul was tremendous at that, in addition to being a real good research type, wrote a lot of reports, did a lot of research, but he was also a very good overall planner.

You see, when we first started out at Langley, the deputy division chief we had was originally the deputy chief of construction at Langley, and the reason he was deputy division chief is we were building Wallops Island. Gilruth didn't give two hoots about what you have to do to be—"I want an island, I want a lunch. I can't tell you all the things I'm going to need." So he got Ray [W.] Hooker to do that.

Then when we were having troubles with the instrument people because we didn't feel they understood all of our problems, the guy who was chief of the instrument research division shows up as our deputy for a while. So he can come over and live with us and understand what our problems are, and go back a little later on and fix them. Then we were having problems with the stability and control branch, so they brought Joe Shortal in, who was a stability and control type, you see.

Whenever Gilruth needed a special type of assistant, they gave Gilruth that special assistant. Well, Paul became that general special assistant. Like I say, he had more power, I think, than anybody, other than Gilruth up here, you see, as far as what went on at the center. He's the guy that I always, when I needed something done, while he was around, pick up the phone, call him, that'd take care of it.

So that's what Paul's strength was, and he was a very important person in this whole overall thing, the planning of the activities up in Washington, the big-picture-type stuff, working, trying to get Goddard going, interfacing with all sorts of people. That was Paul's real strength.

He was kind of strange, too. I mean, not in the sense like Paul Hill was, but he suffered very much from the heat. I don't know what was wrong, but he always wore a towel. We didn't have an air-conditioner. He always wore a towel around his neck and he never wore a tie. His office, his whole branch was in one big office where he could look out and see every desk in it. He was at the head of the room and he could look out and see everybody.

Sometimes he'd have this thing, when guests who came into town, and he didn't have a tie on, but he always used to keep an old seersucker coat hanging on a coat rack in the office. He'd grab that coat and he'd grab the first kid who had a tie, and said, "Hey, I want your tie." [Laughter] He'd grab his tie and he was putting it on while he ran out of the office, so he'd look like he at least dressed up to meet these big shots or whoever it was. That's one thing everybody remembers about Paul, is he always had this towel around his neck. Still had it around here, too, while [unclear]. In fact, when we did these first interviews, I got started on my own over here and Paul shows up with a towel. [Laughter] But he's one of the real keys to the whole success of the operation, too.

The other thing I didn't tell you about, I guess that I told you in the other interviews about von Braun and me meeting von Braun there, was a kind of interesting deal. Dryden had gotten wind of the fact that the Army couldn't use von Braun anymore, because the Army wasn't going to be involved in building missiles to launch into space. The Air Force won a battle between Thor and Jupiter, you see, so they were not going to be building anything past Redstone down there.

Dryden kind of got wind that he was going to have to pick up this whole operation down there, so he called me in one day and he says, "Hey, I want you to go down to Huntsville and find out what's going on. Tell me how many people they got and tell me something about the operation down there, because we may have to bring them into the fold."

I flew down there, and I was the only guy on the airplane. That's kind of unusual, you know. You normally don't have people show up where you're the only passenger on the airplane. I was a GS-15 at the time and that meant they had to have a bird colonel meeting me protocol because I flew into the arsenal airstrip. No one knew what I was down there for, but somehow or other they got the message that they were supposed to talk to me, whatever I wanted to talk about.

So I talked, went and talked to the Army people for a while, because they had a lot of weapon systems, Lark and a lot of antiaircraft things and small things, other things used in propulsion, and then they had the von Braun operation, which built a big liquid thing, the other part of it.

I had to listen. I knew I wanted to talk to von Braun, but I couldn't give it away just by going and talking to him, so I had to spend a little bit of time listening to all the stuff going on on the Army's side of the house, and so I didn't give away what I was up to. Then I went over and talked to the von Braun people. I had a good tour by von Braun's folks. In fact, I got to crawl all over Redstone, inside that top and everything. Willi [A.] Mrazek gave me a run around. I talked to Frank Williams, who was kind of the guy for von Braun, like Purser was for Gilruth.

Then I met von Braun, and I was kind of tickled as I walked down the hall, because I could hear—you didn't hear it or see it, but you sensed all the Germans snapped to attention. As soon as I walked down the hall, I could just sense that in everybody. You just get a feel for it.

Then we went to eat at the big executive dining room, and everybody sat very prim and proper and no one picked up a fork till Von Braun came in and started to eat, you see. So I learned a little bit about the German hierarchy there.

Then later on I was having lunch with Walter Dornberger up at Bell, and Dornberger had been von Braun's boss over at Peenemunde. I didn't know von Braun was up there that day, but he showed up in the dining room, and as soon as the two of them's eyes met, all Walter did was go [demonstrates] and von Braun comes over and talks to him in German, and clicks his heels, bows, says hello or something in German.

I've seen other cases of that happening. I remember when Wernher was talking up in Washington at one of the big hotels there at a conference and they had a guy that was jackhammering outside, hammering away. His deputy was Eberhard Rees, and he was disturbed by it. Von Braun looks down, he says, "Eberhard, go stop that!" [Laughter] So Eberhard goes out and he somehow or another talked the guy into quit using the jackhammer until von Braun finished, anyhow. It was kind of interesting to see that.

Later on I got to know all these guys, and they're real fine people. I mean, they're real nice. They're just like us and they have the same—I got to know a lot of the Japanese the same way, you know, even though I fought against the Japanese. They were all victims of whatever the political system is, like we were. They're not bad people at all.

I had to live with these guys for almost a year on all these source boards. When you get cloistered in a room for hours on end with people, you kind of get to know a little bit about them. I got to know a lot about Willi. Willi was probably the best engineer that they had. Willi was Bosnian, in fact. He was from Bosnia. He wasn't a German. The guy who they put up in front was Hermann [K.] Weidner. I think Hermann spoke better English than Willie did. Willie never did speak super English. He spoke good enough English to understand, but he always had a kind of accent. So he kind of ran the engineering and Weidner was kind of the head of the operation, I guess. Weidner was very suave and he was, I guess, an old Prussian.

Years later, Willi had a stroke. I went to see him after he had his stroke, and he was telling me, he said, "When I woke up, I couldn't speak English anymore." I said, "Well, what's new?" [Laughter]

They were all very nice people. I got along with them fine. There was always friction between JSC and Marshall. At my level I never had any of that. It was a very, very nice relationship we had with all of them. Some of the lower levels, I had trouble with some of the lower levels, but none of the management guys. They were very easy to talk to and get along.

The other thing, I guess, Dryden sent us down later on to talk with General [Donald N.] Yates, who was running the Cape, to talk about launch sites and things like that. I went down with Joe Shortal and Ray Hooker, who had been chief of our construction organization there. We went down to the Cape and spent a little time down there, kind of casing that joint, too, and taking a look at the operations down there, get some feel for that.

I don't remember any other things I got involved in there. Those were very interesting times, by the way.

Dryden was the other guy who was a nice person, too. He was a super, super guy, actually. If you read any of the interviews of people who were even much closer to him than I ever got, they'll all tell you he was a really tremendous individual.

Out of all the administrators, Webb's the only one who wins any prize. [T. Keith] Glennan is passable, but Webb's the only one who anybody would really have an awful lot of nice to say about. I thought he was a real special individual.

RUSNAK: I'd like to close today with a question about the future of manned space. Where do you see manned spaceflight headed now, and what do you think the biggest technical challenges they face are?

THIBODAUX: Well, I'm sure we'll be doing something. I'm not a big fan of the Space Station. Gee, I've been retired almost twenty years now, and on my watch we did Mercury, Gemini, Apollo, Apollo-Soyuz, Skylab, and the Space Shuttle. In nineteen years since then, we launched one little piece of a Space Station. I mean, you know, I don't know what that tells you. It tells you there's something wrong somewhere as far as a manned operation goes. We've been flying the Shuttle. We've been doing some things, sure, but, hell, that's what we designed the thing to do. It's only doing what we designed it to do. It's not doing anything different. I mean, those are all the goals and all the missions we had envisioned for the thing, you see. That's what it's doing.

We finally got a little piece of Space Station up here and we have trouble getting all of it up. I don't know what that tells anybody, but it tells me there's something drastically wrong somewhere. You know, how many billions of dollars we spent and I don't know what we have to show for it.

We have a few things going, you know, like Hubble [Space Telescope] and some of the other interplanetary things. Those are all done pretty much by JPL ever since, you see, and JPL's a different outfit from—see, it's what you call an FFRDC, federally funded research and development center. It's got to get along with Washington and it's got to convince someone to give them some money and contracts, but after they get the money and contracts, they don't have to listen to anything the rest of the people do. They've got a board of directors that reads like *Who's Who in Industrial America*, you see, fundamentally, a board of regents. They don't have to pay much attention to technical direction from anybody else, you see.

Most of the stuff I see that been done since I retired, not only that, most of those projects were started probably about the time I retired or before, when you really get down to it, too. It's taken that long for them to come into being. Part of it is public support, administration. It's gone from a bottoms-up organization to a top-down organization, where the people up above have all the smarts and tell you what you're supposed to do and how you're supposed to do it. That just doesn't work. It may work in the business world, but it doesn't work in the type of world we live in. I'm not too sure it even works in the business world. I think we're getting fooled a lot, you see. I'd like for Harvard Business School to take a look at the NACA and see what they think about how that operation went, for example, in comparison with what they teach people to do these days.

RUSNAK: It would be interesting, indeed.

THIBODAUX: Yes, well, maybe we can get something done on that, I don't know. Maybe Ken and I and Robbie can get something done on that.

RUSNAK: All right. I'd like to thank you for sharing all your memories with us today.

THIBODAUX: Okay. Good enough.

[End of interview]