NASA JOHNSON SPACE CENTER ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

BRUCE G. JACKSON
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is March 23rd, 2009. This interview with Bruce Jackson is being conducted in Boerne, Texas for the Johnson Space Center Oral History Project. The interviewer is Sandra Johnson, assisted by Rebecca Wright. I want to thank you for allowing us to come to your home and to conduct this interview.

JACKSON: My pleasure.

JOHNSON: First of all I'd like to talk to you about your background a little bit. If you could share some of the details about your background and how you first became interested in aeronautical research.

JACKSON: I was a World War II baby, in that era. My father was a model builder of sorts. So I got building model airplanes rather young, and that was my introduction to aeronautics in essence. Then I went through high school in Miami, Florida. I went to the University of Miami [Coral Gables, Florida]. At the time I decided that I wanted to be an architect, because the girl I was dating, her father was an architect and he worked at home. Big influence. So I got to the university the first year, and took up drawing classes, going there all hours of the day and night for a measly one credit or two credits. I'm thinking I'm not sure I want to do this. That's when I transferred to aeronautics—to mechanical engineering in essence. They didn't have an aeronautical school at the time. But they did what they called an aero option, and you could take two aerodynamics classes in your senior year. That gave you the aero option in essence.

That was my background as far as education is concerned. I did take advanced classes when I was at [NASA] Langley [Research Center, Hampton,], Virginia through the University of Virginia extension. But that's the extent of my educational background.

JOHNSON: You received an internship to go to Langley. Is that correct?

JACKSON: Yes.

JOHNSON: How did that come about?

JACKSON: At the time of my senior year the interviewers from all businesses were at the university, one of which was Langley. I also interviewed with Westinghouse. The thought of designing refrigerator doors didn't appeal to me very much, so I decided I didn't want to do that. Another one I talked to was Boeing Aircraft. They were in the middle of building the Bomarc missile at the time. The Bomarc missile was fine. It tested out at Cape Canaveral [Florida]. But I thought here I am in Miami, and Seattle is one hell of a long way away from Miami. I wasn't sure I wanted sure I wanted to be that far away from home. The aerospace industry, at the time they weren't sure what was going to happen, what was going on.

Then the interviewer from Langley was there. He sat down and he talked about where we'd go to work and what the job was. He said, "You'll be assigned to an engineer, a senior engineer, and you'll work with him for a couple years to get your feet wet, see what's going on.

Then if you devise some research theme or idea you'd like to explore, you put together a proposal, you take it before management, management agrees with it, dislikes it, adds to it, complements it or whatever. If you get approval you go ahead. That authorizes money to be spent, work to be done in the Langley facilities, to put the package together."

But the interesting thing about it was you were the lead engineer. You were the whole kit and kaboodle. From the beginning, conceive the idea for the research you wanted to do, follow the design, which was done right there in the facility, follow the fabrication through the shops, handle the budgeting, all that sort of stuff. The complete gamut of project management. I said, "Boy, that's right up my alley." You did the testing of it at Wallops Island [Flight Facility, Virginia]. You wrote a report. That's the way you got promoted at Langley. If you didn't write a report you didn't get any promotions. That's just the way it was in those days. That was a measure of productivity and accomplishment, which is a damn good deal as far as I'm concerned.

I learned a hell of a lot. I loved the project management. So I worked in that from 1955 until 1958, whenever NASA was born. Then I worked in an organization called Pilotless Aircraft Research Division, PARD. That's where Max [Maxime A.] Faget was. I was not in his branch, but I was in another branch in the same building. [Robert R.] Gilruth took the Space Task Group [STG] cadre of people. I think there were 60 in the first group. I was in the second group that went. I decided I'd had enough research. Research was too slow-paced for me. I needed to get in something that was more fast-paced.

So I joined the group and worked in the aerodynamics section under Al [Alan B.] Kehlet. He was my mentor then from then on. He was head of the aerodynamics group. We went about the design of the Mercury configuration. I'd been doing a lot of research on ballistic missiles, ballistic warheads in essence. That's all this is, just a modified ballistic warhead configurationwise, that's all it is. So I was involved with the configuration development, testing in wind tunnels.

At the time, one of the reasons that I guess I moved the way I did is because at that time the aerospace industry was heavily involved in developing intercontinental ballistic missiles. The people that had been doing the research at Langley were in high regard throughout the industry. A lot of them left for high paid jobs and went to work for RCA [Corporation] and for Lockheed [Aircraft Corporation] and so forth. So there were openings. There were opportunities there as a result of those people leaving, which was a good deal. So I was at the right place at the right time.

JOHNSON: You mentioned that you could choose a research theme, something you wanted to work on when you were an intern. What did you work on in that first year or two?

JACKSON: Ballistic shapes, ballistic entry vehicle shapes. In fact I've got a toy downstairs. I can show you how we did this sort of thing. I also did aeronautic research on one of the McDonnell [Aircraft Corporation] airplanes. Had a model of it there at Langley, and I was the project engineer for that test for that project.

JOHNSON: Was that the F3-2N?

JACKSON: Yes. McDonnell F-3. The key there was the tail was configured so that it extended aft of the exit of the jet engines. There was a concern about the influence that the jet engines had on the aerodynamics of the flow over the tail and what happened when the engines went out as well as when they were operating, what kind of trim problems that gave it and so forth. So that was what that investigation was specifically targeted to. So yes, I think they had two configurations, and the whole concept was—I did not build that model. NASA did not build that model. McDonnell brought the model in and I was a project engineer for that particular one. I had to follow it all through the shops and the mating it to the launch rocket and that sort of thing, take it to Wallops Island, run the test, gather the data, come back home, do an analysis, write a report. That was the research activity.

The way you did the research is the airplane would be launched on a rocket up to supersonic speeds, and then it would be released. Then it would coast, and it would decelerate from Mach number say 2.5 down till it splashed in the ocean. But during that deceleration, what you would do was pulse the model with [small] rockets that were up in the nose that would [cause the model to] oscillate. [The oscillation would] damp out. Then it would slow down a little bit, and [another small rocket would create another oscillation at] a different Mach number. You analyzed [the motion to derive] the aerodynamic characteristics [at that Mach number]. That's what it was all about. We had a solid rocket that simulated the jet engines that would be pulsed on and off so that you could be able to get it upset. When the jet was on, it would trim differently than when it was off and you could pulse it and get the aerodynamics on it. It was a rather sophisticated testing technique for the time.

That group was the one that pioneered free flight testing. The organization I was in did all that pioneering work. Have you talked to [Joseph G. "Guy"] Thibodaux yet? Great guy. Memory like a hawk. That guy has got an incredible memory, a fabulous memory. Names and details and events and things that went on. Love to talk to him. He refreshes my brain. Anyway so that was the kind of research I was doing.

There was also another one where we'd take a configuration, a ballistic missile warhead configuration, and it would be made up in say three or four sections, it would be screwed together. There would be different shapes for the nose we attached to this piece. Then there'd be a conical section and a tail section. We would change the materials on those to change the weight balance so the center of gravity would be different locations, and it would fly differently depending on where it was. So then we would take it up to Wallops and shoot it out of a gun. We would photograph its projection down the range so we could see how it oscillated. We'd extract every datum from that. It was kind of crude, but it was acceptable. We could get a lot of information that way. That was the kind of research activity I did.

It was slow-paced because it took a long time to get things through and working. Spent more time reading and doing research in the library than you did actually physically doing research work. So that was not my bag. With the opportunity to get in the space program, I jumped at that. Kehlet wanted me to come over there with him, so I went and joined him. Whenever the organization became a branch I became a section head, fortunate enough to have done a good enough job, they liked what I did. I always felt that my forte was in management, not in the technical skills or research. It was a good fit for me. I loved doing it. Loved working with people. Loved the project because it was fast-paced. Had something going, had targets you had to meet, schedules.

My job was the aerodynamics on the Mercury configuration and the weight and balance. The whole concept of aerodynamics revolves around this rotation around the center of gravity. They go hand in hand. So I had to design the rig that they used to gather the moments of inertia and the weight balance of the capsule that we'd build in house with the help of the [NASA] Lewis Research Center [now Glenn Research Center, Cleveland, Ohio] people. Scott [H.] Simpkinson is one name that comes to mind. He's no longer here, as are a lot of these people not here anymore.

I got involved with it there, and I went down to Cape Canaveral with the first Mercury capsule that we built. Interesting story. Once the Atlas flies and you release the capsule, it reenters the atmosphere and we gather [aerodynamic stability data] only if it's in motion, if it [oscillates]. If it doesn't [oscillate], you don't get [stability data]. All you get is drag. We wanted stability information, so it had to oscillate to get stability information. We tried to get them to allow us to fire the control jets to disturb it so that it would oscillate [during reentry]. The wizards of NASA decided no, we don't want to do that, we don't want to take the chance, we want to make this as clean a flight as we could possibly do.

Well, it turns out that they had a problem on the first flight, the flight of that Atlas. There was no escape tower on it. It was without an escape tower. We launched it up, and the Marman clamp that holds the capsule to the adapter on the front of the Atlas fired, but the capsule didn't separate, because there was still [residual] burning on the Atlas rocket engines. They didn't cut off properly. So what it did, it held the two together, and it started to tumble. While it was tumbling, because it was not in the position it was supposed to be in, the [capsule] control jets fired. What that did is spend all their [control] fuel. When it finally got back into the atmosphere, it [separated] from the Atlas, and it went through its oscillation gyrations all the way down. We got a hell of a lot of good data that we weren't supposed to get, but that was an interesting event. Anyway it [confirmed] our theories about the aerodynamics on the capsule. Aside from that, at that time, the Atlas pads were located a third of a mile apart. We were at the next pad at 2:19 in the morning when we launched. It was spectacular, let me tell you. That really was some spectacular sight.

We watched it. Al Kehlet and I were both there and we watched this thing take off. We had a little cloud bank, something like similar to this. Well, it disappeared through the cloud bank. We said, "Let's go up to TEL 2." That's where the data was coming in. We went up there to a bunch of glum faces, because the [capsule] didn't separate like it was supposed to. "Oh, God, we lost it, we lost it, lost it." So we sat there and watched the events that I've talked about ensue. That was a great time. Great event.

Then I was involved with the design for the real Mercury capsule, the one we actually flew people in. I did a lot of travel to St. Louis, [Missouri, McDonnell Aircraft Company manufacturing site].

JOHNSON: Can you talk about that travel? Because you were still pretty young at that time, and getting to travel to these different places and following, as you said, you would follow these projects. Did you have full autonomy when you were there as far as making decisions?

JACKSON: No, not really. I didn't have the authority to change the contractor, but I would work with the contractor, and we would talk about things. But no, I didn't have the authority to direct them directly. No, that was still done through the program office. I was engineering, so I was called by the program office to go do things to observe what was going on, but I didn't have the authority to control. It's interesting you bring it up. As an aside, when I first got involved with the Space Task Group, they had what they called TR, travel requests. You had a book of them, and I put it in my back pocket. Literally had a book of these things in my back pocket. They came in packs of 20 I think, maybe 10. I could talk to my boss and say, "I need to go see General Electric because they're doing thus and so, and I need to understand it better. Or I need to go hither and yon, whatever." I would just go, get on an airplane, and go. I'd call and make arrangements. I'd get in there. [Security was all pre-planned]. You went there and you identified yourself and you handed them your authorization and you went in and you conducted your business, you left, went home, wrote a report, it goes in the file, all in the way of business. You think about doing that now, think about doing something like that now?

We'd go up to Wallops Island. Per diem was $12 a day. Since you were staying on a government facility it was only $6. I have another per diem story. During the Apollo program we were going out to Sunnyvale, [California]. There was a motel that we all tried to stay there. It was right near the North American plant. I forget what the per diem numbers were. Probably $12 at the time. The motel was $6 or something like that or $8. We got a raise in the per diem up to 18 bucks. We said, "Boy, we're going to go out and eat a decent meal instead of going to McDonald's." Then the motel raised their rates up to the amount we got per diem, so they got all the per diem increase we got. Anyway, good time.

It was a great time because you had the freedom to do things. You were authorized, and they trusted you, which is rare these days in the government for anybody to trust anybody. But that's a side story.

Anyway we went through the Mercury program. You understand from the engineering point of view, once the product is delivered to NASA, engineering takes a backseat. We're off now thinking about the next project. Working on the next design, whatever it is. A lot of us were working on Gemini. The design work and the development on Gemini was done off to the side by a guy that came down from Avro [Aircraft Limited (Canada)] [James A. Chamberlin].

He was working on the side to put the Gemini program together, so there was not much involvement for the aerodynamics people on Gemini because the configuration basically was Mercury, just bigger. Bigger scale didn't make that much difference. So aerodynamicswise, configurationwise, didn't get involved very much with Gemini. We did in some of the abort analysis that went on. We did that sort of thing. We did the escape rocket design for Mercury. We did that ourselves.

[Gemini's aerodynamics were different in that we offset the center of gravity so the capsule would develop a small amount of lift. That allowed for some maneuverability which would alleviate the entry gravity loads.]

JOHNSON: Do you want to talk about that for a minute?

JACKSON: Nothing particular to talk about. It had a problem in that it had two stable points, one nose forward, and one heat shield forward. Once it aborted, you had to make sure that you turned the [capsule] around before you released it, so you get the capsule [oriented in] the right direction. It had a double stability point too, so what you had to do is turn it around. We designed rocket jets on the front of the nose cone of the escape tower to pulse it so that it would turn around in the opposite direction [to orient it with] the heat shield forward. Then you could jettison the tower, and you'd have the capsule coming in okay. That was the design that we worked on to resolve that issue.

Then about that time we did some work on the jettison of the astronauts out of the Gemini capsule, but I won't brag about that. Thank God we never had to use it, is all I can say. Anyway we did a lot of statistical analysis of failures and failure modes to show that the numbers were high enough that it was acceptable risk, like 99.96 I think was the number we targeted for. Anyway, then we got involved in the design of the Apollo configuration.

JOHNSON: Was that after you did some work on the lenticular or disk type spacecraft?

JACKSON: Well, that's a side story. We didn't do much of that. That was all done by [NASA] Ames Research Center [Moffett Field, California]. They were the ones. The Flight Research Center, which at the time was an arm of Ames Research Center. They were the ones that were the lifting body [advocates]. That was their concept. Chris [Christopher C.] Kraft was the one that was the promoter behind the lifting body [not lenticular] if you will. When it came to compete to see who would win the prize to be the Mercury configuration, Faget's configuration won out. As a result, Faget became head of engineering and Chris Kraft became head of operations. It may have been a totally different story had it been the other way around.

At the time we were investigating lenticular shapes. We were looking at all kinds of shapes. That was what our business was, that's really what our research was about too. We were looking at those sorts of things. We flew some lenticular shapes. I don't particularly care for that word. But the shape that Kraft was competing against Mercury was not lenticular. It was what they called a raked cone, or a half cone. Take a cone, you cut it in half. It has lifting properties so you can maneuver. Mercury couldn't maneuver at all.

This is not an aerodynamics issue, but I'll tell you a story anyway. Jack [C.] Heberlig, was given the job of—Faget said, "The problem with Mercury is that there's a high G [gravity force] load during entry." The lenticular shape or the lifting body shapes, I like to refer to them as, ameliorate that problem by flying. They have the ability to maneuver, so they don't come in [purely] ballistically where they have no control over where they fly. So the Mercury [Redstone reentry] gets up 12, 13 Gs on entry. It's a rough ride. It's a rough ride. On aborts, it gets up to 18. So it's a rough ride. So Max devised this idea, this concept of a contour couch. I don't know whether you've heard of that yet or not.

Jack Heberlig was the project manager on the design of the contour couch. He came up to Brooks [City-Base, Texas], and went through the centrifuge and tested various [body support concepts to distribute the astronauts' weight uniformly which allowed the astronauts to tolerate the high G loads]. That was the savior to Faget's configuration. That was the thing that allowed Mercury configuration as you know it to have been accepted. Had they not solved that problem of how they'd distribute the loads, Chris Kraft's raked cone [might] have won the competition. The big problem with the raked cone is that it requires controls that we weren't sure we could handle well enough. The ballistic thing is simple. You don't do anything, you just push the button, deorbit, and in it comes. You have no control over it, you don't have to have any. All you have to do is make sure it doesn't tumble, just stabilize it.

JOHNSON: Could the lifting body land on land?

JACKSON: No, that did not have enough lift to do that. All it would do is alleviate the high Gs during entry. That's all it would do. It had some maneuverability, so the ships probably would be in a better location to pick up the capsule. No, it still descended and landed on parachute. It did not land. In none of the stuff that we did at Langley did we come up with a configuration to land horizontally with a lifting body. Some was done by Langley, but most of it was done by Ames Research Center and Flight Research Center. That's where that activity was going on.

JOHNSON: You were at NACA [National Advisory Committee for Aeronautics] during the time of the first Sputnik, and then again when the Yuri Gagarin flight occurred, and when Russia was always that one step ahead. Maybe you can just talk about that for a second, the feeling at Langley and how the workforce reacted to Russia being ahead.

JACKSON: Catch up. Catch up. Do it. We were down because we didn't make it before they did. That didn't stop us. That didn't dishearten us. Full speed ahead. Go. Let's get there as fast as we can.

JOHNSON: You were ready to keep going.

JACKSON: Absolutely, absolutely.

JOHNSON: Let's talk about when President [John F.] Kennedy announced that the US would land a man on the Moon and return him safely to Earth before the decade was out. At that time I know Apollo was one of those things that people were thinking about. Did you think that was a realistic goal?

JACKSON: Sure. Sure.

JOHNSON: You felt your research at that point was ready?

JACKSON: From an aerodynamics standpoint—I can talk from a project standpoint a little bit, or an aerodynamics standpoint. Aerodynamics standpoint we had no problem with it. There'd been enough research work analytically and experimentally. Analytically from Ames, experimentally from Langley with some tests that were done with Scout [Rocket Program] configurations that went into orbit and reentered at those velocities, 36,000 feet per second.

We had no problem with the ability to do it. When they configured the system to go to the Moon, they made a big bet. The big bet was that the microelectronics would be developed in time to integrate into the Apollo, because the systems that were available at the time we committed Apollo to go to the Moon, to the design, they were not available. So there was research there that we were betting on to come that we'd meet. Otherwise we never would have got to the Moon. It's probably one of the things that slowed the Russians down, because they were behind us in the microelectronics. The booster was big enough as it was. It was humongous compared to anything we'd ever dealt with.

JOHNSON: Let's talk about the Space Task Group. You said that you were anxious to join because they were moving a little quicker, and there were those goals that were set, and you wanted to keep working that way. We've heard before that some of the older Langley engineers discouraged some of the younger people from moving on to STG. Did you find that to be true?

JACKSON: Because I was in the cadre of people that became the core. They were all gung ho.

JOHNSON: They were ready to go.

JACKSON: Right. All ready to go.

JOHNSON: Well, they decided to move to Houston, [Texas].

JACKSON: Well, first they were going to move up to Maryland. But it wasn't a big deal. I was going to go wherever it was going to go.

JOHNSON: You didn't care where they sent it.

JACKSON: I came down on a visit to Houston in October after [Hurricane] Carla had come through in September. I said, "Whoa!" We drove down NASA Road 1 past cow pastures on the left with barbed wire fence. I said, "Are they really serious about this?" I drove around Clear Lake, and it was just totally devastated by Carla, just really torn up badly. So I said, "What the hell? We're going down there. Let's go. It's an adventure. Let's go."

Interesting thing. I drove down from Virginia—the family flew down and I drove down. I drove down, and I arrived in Houston on the Gulf Freeway [Interstate Highway 45], and it was under construction. When I left Houston ten years ago, still under construction.

JOHNSON: And it's still under construction now. Just to let you know. Nothing's changed. Were you married at that time? Did you have a family that you had to move?

JACKSON: Yes. Yes. Three children and a wife.

JOHNSON: How did they react to the idea of moving to Houston?

JACKSON: They were too young to appreciate it. Five, three, and one.

JOHNSON: What about your wife?

JACKSON: She was gung ho. She was ready to go. She was a New England gal that never left New England. In fact my family, all my roots are in New England. I was born in Salem, Massachusetts. My father is the first of the clan to leave New England. [In 1938 our family left Bangor, Maine, and moved to Miami, Florida.] Everyone else is [still] up there. There was a family history put together by one of my uncles, second line of the family. It was so easy for him, because everybody was there. They were all right there within 100 miles. Nobody ever left. They didn't have big families, most of them. My family was two, my father was two, grandfather was two, that sort of thing. So it was not a lot of people to communicate with.

Then we moved to Houston, and my wife [developed] a brain tumor [in 1973]. So that upset the apple cart. Anyway she's since passed on. [My three children are all Texans, happily married, and each has two children.]

JOHNSON: Where did you live when you first moved to Houston?

JACKSON: First house we bought was up on Anacortes [Street], which is in the development just toward town from Hobby Airport. It was a new development, new house. We lived in a rental house down off Edgebrook [Drive] to start with. Lived there for four to six months, something like that, before we bought the house.

JOHNSON: Let's talk about those first days. Where were your offices?

JACKSON: The Rich Building. I guess it was fans they built, yes. You could see [the outside through] holes in the wall, cracks. It was quite a place. So that's where I spent all the time up there. You traveled around to all the facilities, because you had meetings to go to and so forth. That's where I was based, out of there. Then we moved to Ellington [Air Force Base]. Our aerodynamics group was at Ellington for a few years in one of the barracks they converted. Then we moved down to the site. We moved around a bunch of places in the site. I don't remember all the facilities we were in, we moved around a bunch of them.

JOHNSON: Do you want to talk about what you were working on during that time, and your positions when you first moved to Houston, and some of the projects? I know you were working on Apollo, but some of the specific things that you were working on for Apollo?

JACKSON: The escape system was always a challenge, that configuration. That's where we devised the canards, we called them, that deployed out from the nose cone. In testing that configuration, we had the model builders at the shop build us small configurations. Can you take time out a minute? [Pause in audio to retrieve model] Okay. We decided we were going to see how these fins worked to turn the capsule around. They were deployed from the front of the nose cone of the rocket to escape the capsule from the booster. It's supposed to be balanced. It's not balanced right now. What we did is put different materials in here so we could change the balance, like I described in other research we were doing. We actually built a wind tunnel, a subsonic wind tunnel, in the stairwell of a building, Building 16 at NASA.

JOHNSON: The wind tunnel was in the stairwell?

JACKSON: Yes, because it was only about this big around, with a fan at one end. You just blow air through it. So we could test how this thing—whether it was stable or not, and what it would take in terms of the size of these [canards] to be able to turn it around and stabilize it. This [model] is out of balance. You should be able to position it anywhere, and it would stay there. So we had different materials and different locations where the balance point was, to effectively show the center of gravity change shift. Anyway, that's an example of the stuff that we were doing. Just playing around. It was good. It was effective for the subsonic speeds it did good. It [gave us] a good idea [of] what [its] stability was like, how big these things needed to be to turn it around, make sure it'd be stable, turn around so it was [oriented] the right [direction]. It launched this way. Then it was stable, because this is like a big skirt in the back of it to keep it pointed, like the feathers on an arrow, keeps it pointed. So we had to do something to upset it. That's why we deployed these canards they're called. They were deployed. That upset the aerodynamics. Now there was more aerodynamic force up here, so that would turn it around to the wind and it would be stable [the heat shields forward]. That's the way we wanted to go. Flying this way. Wind vector is this way. So now we could safely jettison the tower, in essence, and the capsule would go about its business and deploy its parachutes and land.

The whole idea was what did it take aerodynamically in terms of the size of these canards to be able to affect that amount of change. That was a big component in the solution, that simple little stupid model I [provided]. Anyway that's an example of the kind of stuff we dabbled around with. We had the liberty to do that kind of stuff. We could do that kind of experiment.

JOHNSON: I've heard other people talk about walking down the hall and seeing charts just pinned up on the wall where people were working and doing things. That was the atmosphere then. People were working all the time.

JACKSON: All the time, yes. There was no bureaucratic hindrance to be perfectly honest. A lot of things you try to do now, you can't do without getting approval from God knows who. Just simple things, like going to the shop, you just write a work order.

JOHNSON: As you mentioned, you enjoyed the project management side of it. In 1963 you were Chief of the Aerodynamics Branch. You were talking about the atmosphere, and how people are allowed to work, and people were allowed to do more. As Chief of that branch, how did you encourage people to do that?

JACKSON: I tried to develop my engineers. That was my chief goal. I gave them a lot of responsibility. We had a procedure where once a week, we had what we called a show-and-tell. People that were working on projects would come forth and make presentation on the status of what they were doing. If a presentation needed to be made higher up in management, they came through and were critiqued and helped to get it in form. But they took the project forward. That was my style, my philosophy. I was always there in the meeting to help if I needed to. I did a lot of [personnel] development. I tried to develop people. In fact, if you're in an organization, you got a lot of people you just can't develop, but you've got to find a place for them. They've got to be productive. That was my biggest asset I guess was being able to find a job and make everybody in my organization productive. That was the big thing. Like you say, working on the walls, whatever they're doing, that was encouraged. Whatever they could do to help improve the job, get the job done, was encouraged. That was a big thing.

JOHNSON: What were your hours like? What type of hours were you working at that point?

JACKSON: I had a family. I was not one of these people that would work 18 hours a day. I did not do that. I got things going. That's the reason. I'd pass responsibility off for certain things to people and let them go do it. If they wanted to work those hours, or they had to work those hours, that was their problem. But I always made sure that whatever they did was quality work, it was always supervised by somebody. As part of their development, it was supervised by someone who could steer them and guide them.

JOHNSON: That mentorship was encouraged.

JACKSON: Yes ma'am, absolutely. That was a big thing.

JOHNSON: Well, the Little Joe 2 program, Milt [Milton A.] Silveira, we've talked to him.

JACKSON: He was my deputy.

JOHNSON: He was your deputy during that time. Do you want to talk about that project or that program for a minute?

JACKSON: I can't do anything more than Milt. That's one of those cases. Perfect example of what I was saying.

JOHNSON: You gave it to him.

JACKSON: I gave it to him. We used to have meetings regularly on the status of what was going on. If he had any problems the door was always open, come in. But I was always there backing him up. Milt was a good guy.

JOHNSON: I enjoyed talking to him.

JACKSON: He went on to bigger and better things. We had families the same age, lived in the same neighborhood too.

JOHNSON: Oh really?

JACKSON: Yes, so it was more than just a work relationship, it was a family relationship.

JOHNSON: We've heard that from a lot of people, that the people you worked with were the people you socialized with.

JACKSON: That's right. In fact the neighborhood I was in—a lot of people lived in apartments in various and sundry places, but there must have been six or seven families of the NASA people that lived in the neighborhood that we lived in. We had a regular gathering get-together. Interesting too at the time, there were contractors. We could socialize with contractors and not be held in bad stead. It was great. We had gatherings and we played golf together, go to golf tournaments and things like that. There was no favoritism shown selecting those contractors. [I believe it could have been] blatant favoritism [that led to] the selection of Rockwell to build the Apollo spacecraft. Those guys didn't know crap about ballistic vehicles. They were great airplane builders. The only reason they won that contract is because the astronauts went to Gilruth and demanded, because they built good airplanes. That's the only defense they had, they build good airplanes, they build good airplanes.

I really got in trouble with Gilruth one time. I'll get to that later. That's one of the projects.

JOHNSON: Do you want to go ahead and talk about some of the projects during that time?

JACKSON: I was in the Experimental Program Office with Bob [Robert O.] Piland. Great guy. That's where I [developed] a lot of my management style. He was that way. He was a great guy. While I was in the Experimental Project Office I had two projects that I worked on, I was the manager of. In the event Surveyor didn't take pictures of the landing sites on the Moon, there was an alternative being worked on, which was to put a high-resolution camera and a mapping camera in the vacant bay of the service module of Apollo. What we would do was fly, circumnavigate the Moon, stay in orbit, fly over the sites, and take pictures.

My job was to not build that equipment, but to bring the equipment out from the black world. They were secret projects back in the ’60s, big stuff. Interesting side effect. You take two cameras, you take what they call a mapping camera, which is a large field of view camera, so you can see where you are. Then you have a high-resolution camera that takes very fine pictures. If all you had is that you wouldn’t know what you took a picture of, but that has to be coordinated and boresighted with the regular camera so that you know what exactly you’re taking a picture of. Well, at the time the Earth had been mapped so well that the spy satellites that were flying did not take mapping cameras with them, they did not need them anymore. We’re talking about 1962.

Itek was the builder of the mapping camera, and Kodak was the builder of the lens for the high-resolution camera. We went up there to Rochester [New York] to look at the equipment as it was being developed. In fact, I had a liaison with the military who helped bring it out of the black world, so that was how I got to go around with it. But they brought a picture taken from space of Rice Stadium [Rice University, Houston, Texas] when Gilruth was there watching a football game. You could see Gilruth in the crowd. It was awesome.

JOHNSON: Thinking about that time period, you don’t think that they would have had that then. But that’s amazing.

JACKSON: Really is amazing. So my job as project manager—this is way away from aerodynamics, this was strictly a management job, I guess because I managed well before they gave it to me. So I brought that. I had to work with Rockwell because they had to integrate it into the payload bay. I had to go meet with the manager [at] NASA Headquarters. His idea—he's ex-Rockwell, took a job as NASA deputy administrator for space up in Washington. His idea was, "I want to make this as simple an integration as possible. I want two wires, one to turn it on, one to turn it off." So anyway Rockwell of course didn't want to do that, because the more wires there were, the more they'd get to charge, they'd get the fee for integration. He knew what was going on. Interesting experience.

Once a month I had to go out to Rockwell to see how the integration activity was going. Then I had to go back to Washington to report to the [NASA] Administrator. Anyway, it turns out Surveyor worked fine, so they never needed it. Then they thought about trying to put those two pieces of equipment in the payload bay while doing Earth surface surveys.

That was the other program called [Apollo Experiment] Pallet Program. The project at the time, there were four competing companies. Martin [Marietta Corporation], Lockheed, Northrop [Corporation], and McDonnell Douglas.

Once a month we had to take a week trip. We'd go fly out Sunday night, meet with [McDonnell Monday, fly to Denver, Colorado, and meet with Martin on [Tuesday], have a meeting all day. Get on the airplane [Tuesday night and] fly to Sunnyvale, meet with Lockheed all day [Wednesday]. Get on the airplane, fly to southern California, meet with Northrop [on Thursday]. Then Friday we'd meet with Rockwell, because they were the integrators. So we did this every month for six months.

It's the only time that we had the program defined well enough and scoped well enough that we [could negotiate] a fixed price contract for the fabrication of that package. It [also] included the integration [of instruments from] a selected list of experiments. Fixed price contract. We prided ourselves on being able to define everything well enough that we could do that comfortably. So that was an interesting adventure. Everybody was proud of that work. Unfortunately it never flew either. They did fly experiments in the payload bay around Earth orbit, which is what they were going to do with this thing, but not like it—they didn't use the pallet. That program never went through. So about that time is when I went back to the aerodynamics branch.

Interesting. Back to Mercury. We did some flying of the Mercury abort configuration out of Wallops Island. We did Ham, the [chimpanzee], was on one of the flights. Only thing we noticed about it really was testing the contour seat we used on Mercury. I told you that allowed our configuration to go. [As I remember being told], they took a pig, and put him on his back, and built a contour seat for him. They were going to test it, going to test this with a pig. A pig's anatomy is a lot like human's, more so than any other animal they could pick. So got everything up there at the pad, and going through the countdown, and all of a sudden the pig died. They went to the farmer where they got the pig. He said, "What happened? What'd you do? Dumb ass, everybody knows if you put a pig on his back he's going to die, suffocate." Of course here are the engineers, didn't know a damn thing about it. Just hushed up and kept on—you may not find many people tell you that story.

JOHNSON: You mentioned working with the black world and DoD [Department of Defense]. What was that experience like?

JACKSON: Very interesting. Lockheed was [also] the contractor for the pallet. Lockheed was the contractor for the system, to integrate the system for us, in essence. The people that they had at Lockheed working on the military side of this hardware, compared to the Lockheed people they had working on the pallet program, were like night and day different. Like night and day different, I'm not kidding you. It was incredible the difference in dealing with those engineers. They put the [less capable and experienced engineers] on the pallet program, and they put the experts and their super engineers on [the military] program, because they're the ones that were working on the military side. They just came over from there. So that was an interesting experience. It's an enlightening experience going through a project like that. Unfortunate thing, none of them came to fruition, so I never got to find out whether everything worked right or worked like it's supposed to.

When we went through the evaluation of the contract to see who won, McDonnell Douglas was real low. When I went to present the findings to the Source Selection Board chaired by Gilruth, McDonnell Douglas did not win. That really upset Dr. Gilruth. He got all over my ass, I'll tell you. "I don't care what your results say, they build good spacecraft!" It was wild. So I said, "Dr. Gilruth, the law says I've got to go by the paper they gave me. I know they can do better. I've been there while they were doing better. All I'm telling you is, I've got to go by the paper they gave me. All right? They gave me crap. I'm sorry." He never forgave me, never forgave me for that.

JOHNSON: That's an interesting aside.

JACKSON: I've got lots of asides by the way.

JOHNSON: That's good, those are the kind of stories only you have. So those are the ones we want to hear because it's your experiences.

During that time the Apollo 1 fire happened. Do you want to talk about that time period for a moment and just reflect on some of your memories of that time?

JACKSON: Again, it's another one of these cases where the engineering [design] work was done by that time. I was in the engineering organization. Once the vehicle is designed and built, there's just a small cadre of engineers that go over [to Mission Control to] support the launches. Or they're on call for whatever event occurs. Some engineering calamity occurs or something, a problem occurs. That was at the time that engineering was basically out of the Apollo program. I was not there. I had emotions involved with it, yes, but no physical work or anything like that. Because it wasn't an aerodynamics problem, it wasn't unstable, it didn't crash, it wasn't an aerodynamics problem. What more can I say?

JOHNSON: You didn't have anything to do with any of the reviews?

JACKSON: No, nothing at all, no, I was not involved in any of that. After the fact, all the analyses that were done were done to system—all the modifications were done to systems, not to configuration, which is what my bag was. My bag was the configuration, how it flew, that sort of thing. That was not an issue that came on my plate.

JOHNSON: Let's talk about [your position as] the Chief of Engineering Analysis Division in '70, in the early Shuttle [timeframe].

JACKSON: In the early Shuttle days we did all the wind tunnel testing that was done. Interesting aside. I'll tell it to you now before we get going on this thing. There were 47,000 hours of wind tunnel time done on the Shuttle configuration, 47,000 wind tunnel hours. All over the country. And another interesting aside, the Shuttle is the only configuration that has ever flown, airplane ever flown that had a complete set of wind tunnel data on the configuration before it flew. The airplanes you see flying? They'll go through a major wind tunnel experiment test, but if they make a change they'll do an analytical adjustment to the aerodynamics. They don't go back and rerun, unless it's a serious major modification, major change. Most of the major changes come out in the development phase.

One of the reasons that we did that is Shuttle is the only airplane that's ever flown through the wide angle of attack range and the wide Mach number range. It reenters the atmosphere at a high angle of attack, makes a transition at supersonic speeds to a low angle of attack, and then must land horizontally at subsonic speeds. The control of the airplane aerodynamically through those flight regimes requires as good an aerodynamic definition as you can get, because the airplane can fly with a certain center of gravity range and still be able to trim it out. In other words, by putting the control surface to the point where it'll still fly trimmed statically.

When you run a test in a wind tunnel, there's an uncertainty in the data. When you try to scale it up to full-scale airplane, there're uncertainties. So if the CG [center of gravity] range is say this long, and you have uncertainties in your aerodynamic data, they cut down [the angle of attack] where you can safely fly. You can safely fly within [a given] range, if you knew the aerodynamics perfectly. If you don't know them, then [any] aerodynamic error encroaches on the CG range [that you] can be in and fly safely.

We hashed all that out and got that all done. The other unknown in reentering the airplane was the atmospheric properties. The density of the air. Because you have to traverse the [atmosphere over a long range], fly it, maneuver it, and so forth, [knowing the atomosphere is critical]. That was another [uncertainty] that encroached on the ability where we can safely fly, so we had a very small [margin] where the center of gravity was allowed to be. We had to ballast it to make sure it was there, so we could fly it through the full aerodynamic range, safely be within the margins that we felt were comfortable and safe for flight. The first flight we flew the Shuttle, we analyzed the data after [the flight].

Faget was an avid sailor. He was out in the bay all the time sailing. Doug [Douglas R.] Cooke was a sailor too, avid sailor. They met there while Doug was still going to [Texas] A&M [University, College Station, Texas]. So Faget said, "You got to hire him, got to hire him, got to hire him." I said, "Glad to, glad to, glad to." So he came to work for us, and he was a sharp kid, and he got into this idea of analyzing the data and extracting out the aerodynamic properties for the full-scale vehicle and comparing those to wind tunnel data.

He was responsible for analyzing the data after the flight. We flew the Shuttle, he analyzed the data, and he said, "Hey, the aerodynamics are pretty good. The atmosphere is wrong." "What do you mean, the atmosphere is wrong?" "Well, it's got to be, because blah blah blah if that would happen, this would happen," and so forth. He presented his story, and said, "The atmosphere is wrong."

So we went to the [National] Weather Bureau and said, "The atmosphere is wrong."

"No it isn't. We've been doing this for years, and we know what we're doing." The problem is they interpolate a lot from [widely spaced atmospheric soundings]. They said, "It doesn't make a difference, it doesn't change up there that much."

We said, "All right." We went back. Doug came back home from that meeting and said, "Well, we'll have to do it again." So it flew again.

Aerodynamics are right on. The aerodynamic [uncertainty] is diminishing. Still had a hiccup with the atmosphere. So finally after about three or four flights, we made a full-scale presentation to the Weather Department. They admitted it. They had to back down and admit the fact that, "The atmospheric information we're giving you for all your flight planning is not the best because of the interpolation, things going on up there we obviously don't know about." So I don't know whether they went then and did redefining how they're gathering atmosphere, whatever happened. But they finally had to relent to the fact that no, they weren't as accurate as they were saying. We got through that okay.

JOHNSON: Let's talk about the design of the Shuttle and the first time you were made aware of that design. Do you remember that and what your impressions were?

JACKSON: Cussed a lot. That was an Air Force requirement. They never used the requirement. Their requirement was to have a certain [high] cross range, and they never used it. Their requirements, the military's requirements is what dictated the Shuttle configuration, not NASA's requirements, not the requirements to go to space and deliver payloads and set up Space Stations and all that stuff. Configuration looks as it does simply because of the long cross range requirement the Air Force placed on the configuration. You couldn't do it with like another model downstairs—the straight wing Shuttle. That configuration was driven totally by the military requirements, which they never exercised, never used. That was a big expense, big cost.

JOHNSON: As it was approaching, and as it was going through those different designs and testing, is there anything about that time period that you want to talk about as far as the Shuttle design and testing that we haven't talked about?

JACKSON: I don't think so. I think I've said everything I can recall. As I say, it was the only airplane that's ever flown that had a complete set of aerodynamic tests on a wind tunnel, on the design that actually flew. That's the most noteworthy thing I think [from my perspective].

Another aside going back to Apollo. We always sent an engineer to all the tests that were done in wind tunnels around the country, wherever they were. We always had an engineer on site for those tests. We went up to Ames to a test for the Apollo configuration, and the Rockwell engineers that were there had no idea what the aerodynamics of a blunt body was, no idea at all, had done nothing, no research, all they ever did was airplanes. Here's this blunt body they're putting in a wind tunnel. In fact, they called it the flying doorknob.

But thank God, our engineers were there and watching what was going on, because there's a sting [model support] that has a strain gauge in it that measures forces as you move this model through the air at different angles of attack; what it does is measure the force in X, Y, and Z, measure the moments about the center of where it's attached. The guys are over there watching the data as it comes out, and all of a sudden the data goes steady. Rockwell didn't have any idea what the hell was going on. Our guy said, "Hey, that's not right. That model is binding on the sting." They called the test off, went in there, sure enough it was. They fixed that problem. The Rockwell people had no idea what they were doing, no idea at all. They were all airplane people. Anyway that's another aside. [We did educate them]

JOHNSON: As far as the management is concerned, and the way things were done in Shuttle versus the way they were done in the earlier programs, did things change a lot once the Shuttle program started? You were mentioning in the early programs the engineers were encouraged to follow and to be there during the testing. You were talking about how you were encouraged to go with it, just work with it. Was it the same way during Shuttle?

JACKSON: Yes. We were there for all 47,000 hours of testing. We were there, absolutely. We were on top of everything we did. We didn't let anything get by. That's absolutely right.

JOHNSON: [Did you see any changes at NASA between the beginning of the Shuttle Program and when you decided to retire?]

JACKSON: The change I saw was—I was in systems engineering. That was my organization. Systems engineering fundamentally is responsible for the requirements for a program, defining the requirements. Well, NASA opened up its guts. There was a fear that there were too many "antis," and if they didn't get their way, [it] would result in the program being canceled. So they opened up their guts and let everybody put in requirements. The requirements got unmanageable. You had no control over them. Systems engineering usually has control over the process of defining the requirements [at this state of] a project. I had none. [The program office controlled the process.] It was way above my project office, and blah blah over here and hither and yon doing things making decisions.

As I told you, when I first got there working for NACA or NASA when they first started the space program, I had a book of TRs in my pocket, go anywhere I wanted. When I was first [level] supervisor, I'd write up a memo for a guy's promotion, and it would go in the files and be approved up the line based on what was going on and get approved. You couldn't do anything when I was there the last five years, and having grown up in that other environment, it just got to the point where I just couldn't take it. I had to get out. I was ineffective. I couldn't do anything. My hands were tied. There were so damn many other people and pieces of paper you got to sign, and this you got to do, and that you got to do. I couldn't do a job the way I grew up and learned how to do it. That's when I decided it's time for me to get out.

JOHNSON: The relationship with the contractors at that time, had that changed also? You mentioned early on it was almost badgeless. Had it started to change by that time too?

JACKSON: Oh yes. It had changed completely. Just totally changed. That was a totally different world then.

JOHNSON: Going back to Shuttle, you worked on the abort systems in those earlier [spacecraft] designs. The ejection seats that were there for those first flights—they didn't have those anymore once it was flying. What were your thoughts on flying without that capability?

JACKSON: I was gung ho. We'd been so successful in the other adventures we had flightwise. I just felt that we think we know enough about space and reentry and the ascent phase that it's a good bet to do that.

JOHNSON: You decided to retire in 1985. What did you plan to do when you retired, and what did you do once you retired?

JACKSON: When I first retired, I wound up being a representative for RCA. I'd go to all the meetings over there for the Space Station, because they had a piece of equipment that they were trying to sell as part of the Space Station program. There was a big problem with it, because they had been so independent as a commercial operation only, no government work. They sold their product to the government, but they didn't take a government contract to build something for them. So I kept trying to convince those guys, "Hey, your ass belongs to the government." As soon as you buy that contract your free will is gone. You can't go do what you just want to do. I worked for those guys I guess two or three years. They never did accept that reality, but that's exactly what it is.

Just like now what's going on. All this financial crap that's going on with the government. All the strings that are attached if you take government money. That's exactly the way it is with contracts. You abide by the government's wish if you're going to take government money. You don't have any—no singularities now.

JOHNSON: When we first came in downstairs, we were talking about the brakes. Do you want to just talk about that for a second, the problem with the braking?

JACKSON: You know how the brakes are designed? Those pads [are attached to] metal plates. The metal plates compress just like disk brakes in a car. When you push the brakes, they compress, and slow [the vehicle]. There's so much energy involved, [the discs have to be] made out of carbon, they get extremely hot. As they go down the runway and apply the brakes, these things would come apart basically. But they wouldn't come apart in the sense where they're going to result in a vehicle failure.

All it is, when they stop and they took the brakes off, pieces fell out on the ground. Not very nice to look at, but as a result of all the analysis and studies we did, they would stay together. The money wasn't available at that time to fix them. They did later go back and redesign and fix them, but that was kind of embarrassing. One of my organizations was the mechanical design branch in my division. It was responsible for monitoring the brake development activity. That's where the brake work was done. It was not an aerodynamic thing, but that's one of the things I inherited as a systems engineering division.

JOHNSON: That's another thing. You mentioned earlier that when you went into that Experiments Program, and you did it because you were a good manager and you'd proven yourself that way. Did you find any difference as a manager and as a chief over the different areas that you worked, as far as managing an area that you weren't as familiar with and it wasn't your background?

JACKSON: You just have to trust your people. I've got a management sense I guess. I can get a feel for what's going on. I can't sit down and [design a brake system], I can't do that. I could do some aerodynamic stuff at one time when I was in aerodynamics. The reason that I got the responsibility, I'm sure, is just because of my management ability to manage people and projects and things like that, which I had demonstrated.

JOHNSON: Working with RCA afterwards with the Space Station, did you work on Space Station before you retired?

JACKSON: There's no aerodynamics in Space Station and no brakes either.

JOHNSON: So you didn't support that program planning in any way or design.

JACKSON: No. We had a little bit of work done, in terms of are there any aerodynamic effects up in orbit. There are a few molecules floating around up there that bang into the Shuttle. But no, the aerodynamic effect is insignificant. I had responsibility, as I say, because I was systems engineering division, and one systems engineering function is to gather, collect, review, assess, and everything else, and massage the requirements. The requirements logically come under systems engineering as a function, but not in that project. That was one of the reasons why I cut and left. I just couldn't see a place for me. My interest and area of expertise, if I had any, was aerodynamics. There was nothing coming up that was going to be aerodynamic.

The requirements that were a part of systems engineering, I could have managed had I been given the authority to do so. But it got to the point where you would present a presentation to the program office, and if the program office didn't like it or said something derogatory about it, that guy would get on the phone and call the Administrator. All these guys knew the Administrator. So it would rain back down. It was totally out of control. It was out of control.

JOHNSON: You worked for a lot of different directors and managers throughout your career, beginning with NACA and on through NASA. You mentioned a few, but which ones do you think really influenced you the most as far as your management style?

JACKSON: Al Kehlet is number one. Bob Piland is number two. Faget in a sense. Faget was not a good manager. [He had a bunch of good managers working for him.] He was a hell of a designer, hell of an [idea man and the best creative engineer I ever knew]. He was smart as a whip on ideas and concepts and that sort of thing.

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

WRIGHT: I just have one. The years that you were there, technology changed. How did that impact your job as an engineer?

JACKSON: Aerodynamics didn't change. Wind tunnels were the same ones. In fact, they're still there right now. They're dilapidated and going into disrepair. Well, I've already alluded to it but I'll go back to it again. That is that when NASA was first formed, it was not bureaucratic in the least sense. Everything is so bureaucratic in NASA now that [it's difficult to do anything creative].

WRIGHT: What time period do you feel is the one that you really enjoyed the most?

JACKSON: I loved working on Apollo. Loved working on Apollo. That was futuristic. A real challenge. Nobody'd ever done that before. People built airplanes, people built rockets, people built warheads. Nobody ever went to the Moon. That was exciting. It was exciting.

JOHNSON: You mentioned Apollo was your favorite time. Is there anything that you can think of as the most challenging part of your career? Or anything you're most proud of for accomplishing?

JACKSON: Going to the Moon. Well, the Shuttle has got to be an aerodynamic achievement. I can't discount that at all, that was a great achievement. Building an airplane that flies through that flight range and that angle of attack range, successful in that sense. Great design. I can't claim fame to the design. That's Faget's. [But helping put meat on the bones of Faget's creations, I will always cherish.]

JOHNSON: What are your memories of Apollo 11 when they actually landed?

JACKSON: Euphoria. Glee. Hollering.

JOHNSON: Were you at the Center?

JACKSON: No, I was home, with the family. We were all hollering, yes.

JOHNSON: Was there anything we haven't talked about that you'd like to mention?

JACKSON: When I left NASA, a couple years after I left, I guess, Ivy and I and two other people got together and started a company. We eventually had three parts to it. We decided that the requirements process is so vital to be done properly to get a program off and running properly, it's one of the things that NASA used to do extremely well. NASA couldn't—NASA I say. Not NACA, NASA didn't have the facilities to build anything. So what we had to do was define what we wanted in a way that we could make sure we got what we needed, which is, in essence, requirements. What are the requirements for whatever you want to buy? What are you going to spend government money on to buy?

So we're looking around. There was no project out there, no program out there, that could manage the requirement process. We had a guy that came to work as a part of the company. He thought he could write a program to do that. He sat down and did that. We were consulting at the time with people, writing requirements documents for projects and whatever. That was one side of the company.

We had another bright Air Force kid that worked in NASA while he was on his tour of duty. When Shuttle came along he said, "All this crap, I have to go over there to Mission Control [Center] to get data." He said, "I can do that on my PC." So he designed a program called PCDecom [Pulse Code Decommuntation]. What it does is take the PC stream that comes down from Shuttle and decoms it. You know what I'm talking about when I say decom? It's commutated, so it strips out the data and displays it on a PC. Sold that to NASA, and they took that up, and they paid him, I don't know, $50,000. Then they went and spent millions to install a wire in the Shuttle to one of the remote consoles in the back off the center displays to somebody riding back seat to plug in a computer, PC computer, he can decom the whole data stream. You can do more than the damn Shuttle can do. So anyway, he's still dabbling around. That was the second part of the company.

The third part of the company was subcontracting back to IBM. People that had left IBM and retired, and they were under contract to NASA, so it was a sidewise thing. So we had those three components of it. We got to the point in '89 where we decided we wanted to split the company up. We broke off PCDecom, and Tom took that part and went his way, and he's still working with NASA on that. That's not a part of the company now.

The other part we sold off to a guy who has since run it into the ground, and we never got any money out of it. Third part is the one that's requirements, that I told you the guy built a program to do the requirements. We're not smart enough in business acumen to be able to go out and market it properly and get the financial backing and everything else to sell it, do what we need to do. Other tools that come on the market, they're terrible tools that attempt to do the job. They have slick salesmen that went out there and sold it. As a result, our program never got effectively implemented. It's still there. We've still got it. It's still the best on the market. It needs to be updated in technology because it hasn't been employed now for seven, eight years, ten years.

But as a part of that, we got into the process of training people to write requirements. So we got into basically teaching. That's all Ivy does. That's what she does. But that's how it all got started. And Dave [David Hottman], the guy that wrote the program, is still a company member. He's the president of the company right now. He's up in Denver, and he's doing systems engineering work for a military contract that we hold up there at one of the installations around Denver. He doesn't do any training. Ivy does all the training; Ivy and other employees. So we've had various and sundry people training around the country. It's gotten away from just training on engineering now. It's training on other disciplines as well. That's what's left of the company.

JOHNSON: What are you doing now?

JACKSON: Playing golf, enjoying retirement. Walking the dog every day. Working in the garden and taking care of the house and yard and all that sort of stuff. I just piddle around.

WRIGHT: A full-time job.

JACKSON: Yes, I make it a full-time job. Enjoying every minute of it too. I love it. I love being up here, love the place we're at, love our lifestyle, it's great, just love it.

JOHNSON: That's wonderful. That's what we should all strive for in retirement.

JACKSON: I agree.

JOHNSON: I appreciate you sharing this time with us today and sharing your story with us.

[End of interview]