NASA Johnson Space Center Oral History Project
Oral History Transcript

James Kirby Hinson
Interviewed by Kevin M. Rusnak
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RUSNAK: Today is May 2, 2000. This interview with James Kirby Hinson is being conducted in his home in Louisburg, North Carolina, for the Johnson Space Center Oral History Project. The interviewer is Kevin Rusnak, and is assisted by Rebecca Wright. I'd like to thank you for allowing us into your home today and for doing the interview.

HINSON: You’re most welcome.

RUSNAK: If we can just start, tell me a little bit about your background and any interests you might have had in aviation, engineering, the types of things that led you into the career you ended up in.

HINSON: My interest in aviation started during World War II when I was about five. I can’t explain why that happened, but growing up in North Carolina, a small town, my older brother and I, he was like seven, I was five, we found a vein of North Carolina red clay, and from that clay we molded little airplanes. I should say we sculpted little airplanes, because if you knew what a P-51 [Mustang] looked like, you could recognize these little four-inch airplanes as a P-51 or [P-47] Thunderbolt or a [A6M] Zero or a Messerschmidt. I don’t where we got our design knowledge. I guess it was from pictures in magazines and that kind of thing. But from that minute I never lost interest in aviation and airplanes.

On my sixth birthday, a young girl up the street gave me a model airplane kit. At that time you could buy these little rubber-powered scale airplanes for about a nickel. I'm sure no six-year-old ever completed one of those kits. I mean, they didn't have balsa, because balsa was all used in the war, I think for floatation devices and things like that mostly and maybe some for construction. At any rate, just the attempt to try to build one of those things was just grand. By the time I was ten or eleven, I was designing my own little free-flight airplanes with little motors, buying balsa for thirteen cents a sheet. You know, you worked a long time to get thirteen cents. I grew up with actual flying airplanes.

Somewhere in there I met the gentleman you mentioned earlier, John [W.] Kiker. He's from the same little town, Wadesboro [North Carolina], which is southeast of Charlotte about fifty miles. We started seriously flying airplanes. He was in college. I was about twelve. He worked at a hobby shop. He would bring me a kit when he came home on weekends, like a two-line glider or something like that. I would build it. Two weeks later, he would come home. We would take it out, and I'd lose it in a thermal. [Laughter] We carried that on for months and months and months. Occasionally I'd build one of the airplanes and he'd take back to school and promptly lose it in a thermal, bring me another kit. But I really developed modeling skills that way. He graduated from N.C. State [University] and went off to Wright-Patterson Air Force Base [Dayton, Ohio].

Eventually I went to N.C. State and got a degree in aeronautical engineering. While I was doing that, I worked in summers at Wright Field, kind of as a co-op [cooperative education student]. They really called it an engineering aide because I wasn't in a formal program, and I would basically quit at the end of the summer and rehire in at the end of the school term.

Thinking back about that, that was the most fantastic experience. I had a total of eleven months, and I did everything there was to do on a parachute system. I worked on design of the parachutes, materials, fabrication techniques, environmental and dynamic testing of hardware, testing of the parachutes themselves. It was strange how all that happened.

Basically one or the other of the engineers in the office would take me under his wing for one part of that. I think one reason I go to do so much outdoor testing is that they'd let me do design work on a Freiden calculator. Have you ever seen a Freiden calculator?

RUSNAK: I've been hearing a lot about them lately, though.

HINSON: It's a mechanical device, and it's fine until you divide 111 by 37. When you do that, you could go get a cup of coffee and come back and it's still sitting there going "clackety, clack, clack, clack." It's really loud. You just can't tone it down. I could drive one of those things crazy. One of the engineers gave me his cookbook for parachute design and said, "Design me an 18-foot-diameter parachute that will take these kind of loads and has this kind of porosity where you have a fabric section, then an open section," kind of like the Apollo and the Mercury parachutes in the top. I'd sit there on that Freiden and clackety clack, just design like crazy. Finally, one of the other engineers couldn't stand it any longer, and he'd say, "Kirby, could you go tomorrow over to Patterson Field and run some tests for me?" [Laughter] And so I'd go away for a day. Really had a good time with that and learned a lot, probably more about working with people than anything else.

I was given jobs like the pilot parachute for a personnel parachute system. When you pull the ripcord, you open flaps and the pilot parachute has a built-in spring contained in the fabric so it doesn't get tangled. It's a long spring that is compressed down to very thin, so it doesn't have very many coils. They told me to make that spring stronger. It had to fit inside a certain area and had to extend a certain amount with the spring. So I got me a spring design book, and I designed a couple of springs. Went down to the machine shop and got them to make them for me. They looked like slinkies. I mean, they had absolutely no force output whatsoever.

The head of the shop said, "What are you trying to do?"

I said, "I'm trying to design a spring."

He said, "No engineer ever designs a spring. Come down here and tell me what you want and go away, and I'll make you a spring." And that's what I did, and he made me a nice spring. It probably took him two or three tries, but he made a nice spring.

But I had a lot of episodes like that up there, so that I was comfortable in just about every aspect of parachute work. The parachute is just an incidental thing. Most of that applies to all engineering jobs.

When I walked in to the Langley [Research Center, Hampton, Virginia] Space Task Group after graduating, the first thing I did was bump into Phil [Philip M.] Deans halfway up the stairs in Building 58. He says, "Are you on board?"

I says, "I'm almost on board."

He says, "Where are you going to work?"

I says, "I don't know yet."

He says, "Tell them John [B.] Lee's section. We've got parachutes." He had known that we had a project in school in the senior class, which was to subject a Rhesus monkey, a fifteen-pound monkey, to fifteen minutes of weightlessness. We had to design a rocket system to do that, including a recovery system. I designed the recovery system, so everybody in school knew I had the parachute background.

That was one of the greatest things that ever happened, was bumping into Phil before I was locked into some other group. They had intended to send me to Thermal Technology Group, heat transfer of some kind. So when they kind of interviewed me to sign me in and everything, they talked about that and I said, "I'd really like to work in the parachute group."

They said, "You would *like* to work in the parachute group?" [Laughter] I think that cast you a little bit off to start with. But it was a fine, fine system to work on. I enjoyed that.

Went to work for a little group of people that came to be known as "John Lee and his boys." We had four graduate engineers, fresh graduate engineers, something called Mechanical Systems Section, which was part of an on-board systems branch. The other section was Electrical Systems. They had all the communications electronics. We had just about everything else on the spacecraft, except environmental control. We had all the little rocket motors that did this and that and the other. We had the reaction control systems, the parachute systems, and the escape system, the escape rocket. So all those solid propellant rockets.

We had David Winterhalter for that. You really need to talk to him, if you don't have him on your list. David did all the rockets. Phil Deans did the Mercury drogue parachute. I had the Mercury main parachute system. Walt [Witalij] Karakulko had the reaction control system. Walt was from Russia by way of World War II. He was a young kid over there and watched the Russians trying to capture part of Berlin and the Americans trying to capture part of Berlin, and he saw teenagers out there with bazookas on their shoulders shooting at tanks, just right out in the middle of the street. He saw the very end of the war in that regard.

Very bright young people and managers we had. I know you are aware of some of the facts like at that time the Space Task Group was made up of about 250 people, some forty-odd new graduates. About the same number of people came from AVRO in Canada when they lost two contracts and basically folded up. I was shocked to find out they had 14,000 people working there. At that time the population of Canada was 14 million, which meant one in 1,000 people in Canada was put out of work in one day. Those people came to a lot of fairly high-level positions with us, I'd say middle management on up.

We had a core of people from Langley Research Center that were the heart of the organization. That was the [Robert R.] Gilruths, and [Maxime A.] Fagets and those kind of people, and a few people from Lewis [Research Center, Cleveland, Ohio]. But only about 250 people, and we were three buildings. One of those housed the original seven astronauts, so we bumped into them all the time. Straight out of college.

Tried to beat the Russians into orbit, and that had a strange significance to us. The significance it had to the whole nation that we were in a national competition, and it was the heart of the Cold War then, so it was very serious, but it also had a little bit of this "my college football team" flavor to it. I mean to tell you, when Yuri Gagarin—I can [remember] that little girl over there and Russian national anthem playing in the background and their big celebration they had—Yuri Gagarin, I bet she said it fifty times, and that's the only thing I understood from the whole thing. It was sickening in the way that your football team lost a really important game. They made like three orbits, and it was going to take us three or four manned missions to do that. [John H.] Glenn's flight was the first one that went the three orbits. That was a really rough day. Somewhere along in there, I’m getting a little bit ahead of myself, because I haven’t talked about the kind of things I worked on yet.

RUSNAK: You felt the same sense of competition when Sputnik went up? You were still in college then.

HINSON: Yes, and it was awesome. I mean, before I got out of college, the favorite joke was—we were trying to get—I sat there like a lot of people and watched that Vanguard rocket on TV. I think that was a joint Navy-NASA project. I was still in college. That was ‘58 or so. That thing was so slender that it just looked like it wasn’t going to make it. They would light it off, and it would just fold up in a big fireball.

Then they tried with the Atlas four or five times. I think the Air Force tried to launch that thing, and they would get up about two or three hundred feet, and it’d start turning sideways, and then they’d have to blow them. That was really disheartening. See, I was an aeronautical engineer then, had no earthly idea that I would work on the space program, but it was interesting to me, and I was close to it. And it hurt. Here we got this little "beep, beep, beep" Sputnik going around all the time. There wasn’t much to it, basketball—sized thing.

Then we finally put that—I can’t remember what the first one was, Pioneer or something—[Wernher] von Braun put into orbit, with a bore and base and clustered a bunch of little solids that he spun for stability and put a little payload on the top of it in orbit. And that’s the way you do it. But at about that time, the favorite joke I mentioned a while ago was, we had gotten maybe fifty pounds into orbit, the Russians had put up a five-thousand-pound spacecraft with cows in it. "It's the herd shot around the world." [Laughter] We were laughing, but we really wanted to do better. We really did.

I went to work, and about three weeks later was on a plane going to California to set up the test program, straight out of college now. That is the kind of environment that we were working under. Money was not a problem. It wasn't that we had money to throw away, but we had a mandate to put a man in orbit. This may sound silly, but jet airliners had just come into being. We flew first-class jet everywhere we went. The first class, I don't know how we justified that, but it was our time was important, we had to be places and do things. That really was probably a minor cost in the big scheme of things.

I remember the first contract with McDonnell-Douglas was well under a billion dollars for the entire Mercury Program, some like six or seven. I think it ended up being eight or nine hundred million. So I'm really not talking big bucks. But we went places and did things. And you can imagine young college guys going out to St. Louis to be the NASA representatives on projects, design and test and installation, every part of it. When St. Louis was snowed in and we were told at the counter, "You're not going to make it. You've got a choice of either going to Chicago or New Orleans and then you can get there," it was New Orleans every time. [Laughter] I mean, you start pulling for snow. We had a lot of fun during those days. And we worked hard, a lot of overtime work.

A lot of things happened that I got involved in because of my parachute work. I got really involved in all the emergency operations, like the escape rocket and later on we had other avenues, like on Gemini, we had personnel ejection seats. Early in the program, the Russians—I can't remember if they had already put a man in orbit or not, but all this stuff was timed very closely, a very few months. I went to Wallops Island [Virginia] for an escape system test. What we did was we took a Little Joe booster, which is just a cluster of solid rockets, just to get a certain thrust capability, ran the spacecraft up to Mach 1 at about 40,000 feet. That was the critical escape job on the way up. There was a clamp that held the spacecraft down to the booster, to the adapter. That clamp was operated by explosive bolts that released it into segments and turned the spacecraft loose.

We had an indication from a previous failure that that clamp was opening up with the air loads very near the critical point and operating microswitches that said, "Hey, we're free. Fire the escape rocket." And yet we're not free. We thought that's what was going on. So on this particular test we put a capability to fire the posigrade rockets on the bottom of the heat shield to get the spacecraft away if the escape rocket had already fired so at least we wouldn't lose the spacecraft.

Everything happened just like we thought it would. Sure enough, the escape rocket fired with it still attached, and very soon thereafter all the events happened the way it should. The marmon band separated. We fired the posigrade rocket. Shouldn't have fired that early, but we fired it at Mach 1 at 40,000 feet, and it could not push the spacecraft off at that condition because it dragged, but it could rotate it sideways enough to make it fall off. When it fell off, it slung so hard that it slung both the main and reserve parachutes out of their compartment at 40,000 feet at Mach 1. [Laughter] And they held together. And there we are, with a little old spacecraft with two parachutes out at 40,000 feet. It must have taken it three days to get down. [Laughter]

Before it hit the water, we hear from Cape Kennedy [Florida], "Mercury Redstone 1," I think was the number. We lit the Redstone up, unmanned, of course. Lit the Redstone up. It moved about an inch and a half, pulled the positive plug and the negative plug in a sequence, umbilicals in a sequence, that told it it was through with the mission, turned the main engines off. It settled back down in some kind of physical condition that nobody knew exactly what. Turned out it was safe, but nobody knew. And then starts flying the mission. Escape rocket leaves, the parachutes deploy and drape down. [Laughter] I'm laughing at this, but all this happened in one day, now, and this was when we were in a race with the Russians. Really making progress. [Laughter]

It was fun to look at the data. I was sitting there looking at a telemetry track. It had about twenty channels on it, and I could see the escape rocket fire. I could see the marmon clamp separate later, the posigrade rockets fire. You had absolute proof of what happened right there on that one track. We got past all that stuff. Fortunately, we salvaged that Redstone and detanked it and put it in a better condition and fixed the umbilical sequence and fired it again, and started having successes. I started writing a report on Project Mercury.

I need to tell you about weight increase before that. One of the most important things I ever had to contend with was the weight, the recovered weight of the vehicle. I'm sure most systems were like this. They had a certain weight sensitivity. But the parachute system was just directly dependent on the weight you're trying to recover, first for the loads it introduces when you start trying to open everything up, and then for the rate of descent you hit with, with the spacecraft and all that.

The first weight that I ever worked on for Project Mercury was 1,960-pound landing weight. Before we really got going, it was 2,160, and it stayed 2,160 for months. And then it went to 2,340. You know, you never forget these numbers. We were looking at something around 2,500 pounds as we started to actually fly missions.

I'll never forget, Kenny [Kenneth S.] Kleinknecht was manager of Mercury at that time, mostly through this Capsule Coordination Office, which really controlled everything that happened, any design change, any test, any decision that we made. I'd traveled back to Langley to talk about recovered weight on the parachute, and we were to implement a weight increase program and do some tests with the Mercury system as it was, not make any design changes, just put a heavier weight on there and do a few drops and see what happened. I said, "What weight do you want us to use?"

He said, "2,775 pounds."

And I says, "And what's it going to be next year?"

And he said, "2,775 pounds." And I had been through this so many times, and he could see that on my face that I'd been through that so many times. And he said, "Look. Don't worry about your parachute system. If the weight exceeds 2775 pounds, the landing weight, we won't have 95 percent chance of putting it into orbit with the Atlas. We won't have enough retrograde propulsion to get us a good entry trajectory. We won't have enough RCS [reaction control system] fuel to control it for a fourteen-day mission." Or whatever the—what was the longest Mercury flight? Twenty-two orbits or something like that.

Anyway, he went through just about every system in the spacecraft, and I said, "Okay, we're going to make the landing system."

So we went out to California and we weighted our vehicle up to 3,000 pounds. [Laughter] And made our tests and we were okay. I'll tell you, let me be sure to tell you later, that got us into trouble on Apollo. Real trouble. Because you can never get enough weight margin up front. You just can't do it. And we put ourselves in a real slot where we had to do something at a bad time.

From there, as far as my stuff was concerned, I did one other interesting thing on the Mercury. McDonnell-Douglas had done an assessment of the reaction control system fuel effect on the parachute nylon. It was hydrogen peroxide, and I mean like 98 percent or something. They said it had no effect on the nylon. So this other lame friend and I went out and performed a test just to make sure. And hydrogen peroxide eats nylon. I mean, you can pour it on the fabric and it will go right through it. After we were on the main parachute—the reason this is important is we dumped the remaining hydrogen peroxide so it's not a hazard at landing in case the tanks broke. We came up with the fact that it causes problems, so we've got to protect it.

Someone came up with the concept of protecting the suspension lines by coating them with polyvinyl chloride. In the first place, it increased the weight by a bit and made it tougher to get it in the pack, tougher to pack to a high density. The process also elevated the temperature of the nylon to the point where we got an automatic 5 percent reduction in strength. And I was really proud of myself for having helped do that. [Laughter] But that's just so typical of the kind of systems engineering problems that come up, that my perception of it is not always all that it needs to be looked at. We still came out with a good system and one that did the job.

So the next thing I do is start writing a report on it. The report was just murder, because the weight was classified because of the Atlas. So anything I wrote about the Mercury parachute system that included the landing weight, which was anything of significance, I had to classify it. So here I am. My boss asked me to write a report on what I knew about the development of the Mercury parachute system. So I write this report, and he reviewed it. I gave him about thirty or forty pages, and he gave me about six pages of comments. So I do a rewrite, and I give it back to him with about thirty or forty pages. He gives me about nine pages of comments. We had a different perspective on what that report should include.

In the middle of this, McDonnell-Douglas published a report, which was not classified, which had the weight all over it, and was a good report. I mean, it was all the program needed. So I went through about two more iterations on this report. I eventually got almost as much written back to me as I wrote in the first place, and I'm not a bad writer, just a different perspective. I started trying to trying to go to work on my own. I went off and started working on an Apollo parachute system and put that report on the desk, and it's still there, as far as I know. [Laughter] It just really wasn't that important.

At the end of my first year of work, we had this orientation. That is where I met [wife] Shirley [Hunt Hinson]. She'll probably tell you about that. Had an orientation series both on Langley Research Center and on general engineering within the Space Task Group on our jobs and our vehicle. At the end of the year, we had to write a little paper and make a little presentation to get our first promotion. It was from a GS-7 to GS-9. My evaluators were Chris [Christopher C.] Kraft [Jr.], the deputy at that time of my side of the house, Flight Systems Division, which was Aleck [C.] Bond and Tom [J. Thomas] Markley, who was the administrative assistant in the group.

I made my little presentation on the Mercury parachute system and its loads and what we do with the testing and all that, a pretty good little paper. The only questions they asked me were, what are we going to do for Apollo? Already we were thinking 10,000-pound vehicle, 5,000-pound vehicle. And I described the Apollo system at the end of the presentation, I said, "When the mass gets over about 5,000 pounds, the parachute diameter for a 30-feet-per-second rate of descent is going to get over about 100 feet, becomes unwieldy to manufacture, to handle, to test, to package, everything. So we'll get a cluster and size it so if one parachute fails, the other two will give you an acceptable impact." Those were the kinds of things I learned up at Wright Field as a co-op.

One other thing happened in this early stage as we were winding up on Mercury. My work generally was done about the time we started flying manned missions, and I was off on something else. That's when Shirley really got active. We've never worked together on any project, not even one hour of our entire careers, we've never worked together. That was not by design; it was just we took different paths.

We had a certain freedom back then that we probably don't have now because of a lot of the mentality of doing projects with groups. "Group" is not the right word. Team. We ended up doing exactly the same thing but it wasn't formalized like that.

David Winterhalter, the rocket specialist in my little section, and I decided that we would build a gliding parachute rocket system to land the Mercury spacecraft with. We'd lay out this little project, and we'd talk with people about it. David called around. He called down to the Army Ballistic Missile Agency, Redstone Arsenal, which is where von Braun came from, and eventually became Marshall [Space Flight Center, Huntsville, Alabama]. They had some little rocket motors about the diameter of that glass and about twice as long, that had a really good thrust history for what we wanted to do. They would burn for about three tenths of a second at about a 600-pound thrust level. They had a bunch of those things that were surplused because they were too old. So we said, "Send them up." And they sent them up.

We took the nozzles off, designed a central manifold that let us screw twelve of these little rocket motors into it. Then it had a central nozzle that fired downward. So we had a twelve-cylinder rocket cluster. It was about that thick, and it would go between the Mercury heat shield and the pressure vessel. We actually did a program of static firing of the rocket motors and get the thrust right. Then we'd hang a Mercury spacecraft up with a crane to the point where we could drop it and it would be doing thirty feet per second about six feet off the ground, and then fire the rocket. It would bring it essentially to zero.

We did that before the Russians did it. That is their landing system and always has been after that initial little series where they just put them in with a parachute.

RUSNAK: So was there serious consideration of actually using this?

HINSON: There was. There wasn't on Mercury because it was too late. But I was going to say, what would happen is, we had a management team that just understood what you have to do to make progress in systems work, what you have to do to make progress with your systems engineers, and keep the public interested. They were delighted when we went down to Texas.

We made a drop three weeks after we got down there, from a C-119 out of Ellington [Field, Texas] into Galveston Bay. Unfortunately, this was part of that program, and we made a test just to test our ability to drop it out of a C-119. We made a rig that theoretically that vehicle would just slide out the open clamshell doors backwards. I was the project engineer on that job, so I got to ride with the chase plane which was a little Meyers four-place, low-wing, single-engine airplane out of LaPorte Airport, flown by Cliff Hyde. I was flying jump seat, and when that dummy spacecraft came out of that C-119, I knew we were going to do one of these numbers and keep try to keep up with it, which is not possible, but we tried anyway.

As we were on the final countdown, I look out to my right and here comes Channel 2 in their own chase airplane, not in communication with us. [Laughter] It absolutely blew my mind. It turned out they didn't do anything bad. They stayed away when the thing came out and we started down. We didn't go down very far, because we could see it when it came out, it broke the static line, and I knew, no parachute all the way down. So here goes this Mercury boilerplate spacecraft into Galveston Bay.

This is where I learned something about the press. We all came back to Ellington. I very quickly ran around and talked to all the guys that would have mattered, and gals, and got how long it would take to it again. Three weeks. We could do it again correctly in three weeks. So the guy from Channel 2 or somewhere comes over from somewhere, and he says to me, "How long do you think it'll be before you can do this again?"

I said, "I don't know. We have about a month, I guess."

The next morning, "Kirby Hinson said the next test will be in three weeks." [Laughter] So, I mean, they listen.

RUSNAK: The next test was successful, though.

HINSON: We did it successfully in three weeks, and we had a lot of good success with that program down there. Actually, the stuff that we did with the Mercury vehicle was kind of to wrap up that effort and get something going down in Houston. The technical shops and people like that, it gave them something to do, to know how much space they needed.

We were in fourteen buildings all over Houston. I was in the Rich Building on Telephone Road. Then before we moved down to the center, I spent about a year, a year and a half at Langley in some of the temporary barracks down there.

From that program we initiated a similar one for the Gemini Project. At the time Gemini was still trying to go with the paraglider. We were very ineffective in—when I say "we," I'm talking about my little technical side of the house. We really tried to steer them away from a commitment to the paraglider so early in the program. They needed a little bit more background work first. What they were trying to do was just such a leap in technology from what had been done, that it carried risk. It carried schedule and program risk of not being able to do it the way they wanted to do it. And they weren't able to do it the way to do it.

We came along with a system with lesser performance, the parasail, like you see towed up behind boats all the time. We developed one of those with about an eighty-five foot diameter and actually tested it on a full-scale Gemini vehicle with the landing gear that was developed for the paraglider and with the landing rocket, and made some successful all-up systems tests. We spent about the last year of that program working directly with McDonnell-Douglas, actually integrating that system into the spacecraft. At the end of that time, it was considered a qualifiable system that could be used on Gemini.

We would have had to do all the full quality-control systems demonstrations that give you the reliability numbers. That's all that was left, and that would have been a significant effort. It would have been about a year's effort. We almost flew it. They canceled a couple of Gemini missions, and if they had not done that, we probably would have flown that system into orbit. Of course, it did have the ejection seats to back it up. So you would go with two systems.

RUSNAK: I want to ask you a couple of questions about that, actually. Both the paraglider and the parasail are predicated on the fact that they're going to land Gemini on dry ground versus the water.

HINSON: Right. We took that as a real objective. In fact, the Gemini Program took it as one of their major objectives. They were to do rendezvous and docking, EVA [extravehicular activity], land landing. I think there was another one. But the proponents of the Gemini Program at the highest levels considered those necessary steps before we went on to Apollo and the Moon. Of course, the land landing is kind of a side issue.

RUSNAK: Do you know why that was given such a priority?

HINSON: It's a more dignified way to land. It's almost that simple. There was a school of thought that said—in Mercury now, we had Navy forces on alert around the world. Well, the Navy just loves to go on alert for training exercises that mean something. So you can argue—also on Mercury we wanted worldwide coverage. We didn't want any quiet zones with no radio communications, so we put ships out there. I forgot to tell you that. I was in training as a flight controller on the side for a remote station, and I need to tell you about that because it was very important to my comfort with the vehicle and that kind of stuff. I knew every system in that spacecraft backwards and forwards, and all the numbers and quantities, and all the people that were in that program did. I went through much of the same education on paper as the astronauts did to support that activity.

I'm not sure I answered your question about water landing. Let me go back to that. Mercury had to have worldwide coverage in case we had to enter for whatever reason at any time. We really wanted a ship as close as possible, which in some cases meant a long way away. But we did have a lot of Navy involvement.

As we got more mature with our ability to put something into orbit and get it back where we thought we could, within a few hundred miles, the requirement for the Navy involvement shrunk from this absolute experiment of "We've got to cover everything, so cover all the water," be able to recover them from anywhere, we started shrinking that spot.

Some people wanted to shrink it enough that we come back on land, have the TV cameras out there, and all that. I never really appreciated the fact that we sent people to the Moon and then came back and dumped them into the worst vehicle for seasickness you ever saw, with a possibility that it might be hours before they were picked up. We certainly gave them that capability. I mean, those spacecraft would float that long. But it was not a very good boat. It really had two positions it likely floated in. One was upside down. [Laughter] We put floatation bags on the top to turn it over. That's all they were there for. And that stuff gets integrated into the recovery system. Anything that you don't know where it came from or why it's there and there's no special place for it, it goes in the recovery compartment, and the parachutes get smaller.

The people that did the parachute work were real strong advocates for land landing, because we knew they were stuck out there just over the horizon in terms of technology.

They could barely do that job. They could get that spacecraft back on land with acceptable G levels. The problem we always had was being able to avoid local obstacles and turn the parachute into the wind with some velocity so that you reduce the horizontal speed at contact. That's the approach we took.

Langley took the approach that we've got this wonderful wing that can be deployed, it's like an airplane, it's got an L-over-D [lift over drag] that will let it fly down there, look around for a landing spot, land like an airplane, slide out. Those two schools really never converged. They may on the X-38. They got a long way to go, too, a long way to go.

RUSNAK: As you pointed out, the Soviets used a similar system where they have the retro rockets.

HINSON: The thing they had that we didn't was they had that tundra, miles and miles and miles of tundra, so they didn't have to come back to a spot anywhere. They've got this kind of relatively flat terrain. And they had some rough landings, too. They hurt some people. I don't know if they killed anybody or not, but they hurt some people.

RUSNAK: I believe there was the one mission where the parachute failed. The lines had become tangled.

HINSON: Yes, they did. They did. I don't really what happened on that, but that was considered a parachute failure. I was thinking of the ones where the parachute worked okay, but they had some problem in landing, tumbling and that kind of thing. That's the other thing they did; they didn't worry about tumbling. Maybe they worried about it, but they didn't do anything about it. They basically had a round vehicle. Fire a rocket, touch down as gently as you can, and then drag it 200 yards, you know. [Laughter]

The land landing of spacecraft was a really strong technology driver for an awful lot of parachute work, particularly the lifting gliding parachute type. A lot of the rectangular parachutes you see out here now, some of that effort was driven by a desire to come up with something that could land a spacecraft, by people like Dr. John Nicolides [phonetic] at the University of Notre Dame. He was determined to land a spacecraft with one of those things. I don't know where he really got that determination, but he did a lot of early work. He didn't invent that parachute—a guy named Jarlburg [phonetic] down in Florida invented it—but he developed it, did a lot of testing, probably advanced the state of the art as much as anybody did.

RUSNAK: Where did NASA get the idea to look into the parasail?

HINSON: My little section talked with Pioneer Parachute Company and they had just bought the patent from a guy named Pierre LeMoigne in France who developed it as a towing, ascending parachute, the same way that we use it behind a speedboat these days. My section worked with Pioneer Parachute Company to start developing a larger size and started developing deployment techniques where we could package it and throw it out on an airplane.

They very quickly recognized that it a superb personnel parachute for sky divers, and that just opened up the sky diving market. They had a glide ratio of about one. So if you paid attention to where the wind was coming from, you could pretty much get down to a point, turn into the wind, and land standing up, with very little horizontal velocity. Now the parachutes they're flying now can just fly circles around that parasail, go anywhere they want to go.

In the middle of developing that parasail, we came up with the philosophy that if you're going to work on these parachutes and ask other people to ride them, you have to make at least one jump. So one day after we had thrown the little Gemini spacecraft third scale out of a helicopter, with a parasail on it, a couple of us got in a helicopter. Near—where is that place? On 146. Shore Acres, there was a huge open field down there, so a couple of us fell out of a perfectly good helicopter, having called back to the Army base, they were out of Fort Hood, Texas. We called the commanding officer at Fort Hood, Texas, and he said, "Sure, if y'all are through with your work, go ahead." [Laughter]

So the next day the word comes down the chain of command from Chris Kraft that he really appreciated our getting permission from the base commander to do that, but he really didn't want anybody else doing it. [Laughter] It's a great experience. I recommend it.

RUSNAK: Well, the paraglider was run out of the Gemini Program office, and the parasail was with your section.

HINSON: The parasail was fully a technology effort up until it began to appear to everyone that the paraglider would not be ready before the Gemini was finished and that the parasail landing rocket might offer the opportunity to fulfill that really important mission objective of landing on land. So, as I said, for about the last year of our effort, it was integrated. We put somebody in the Gemini Program Office to have that direct contact with McDonnell-Douglas. But up until that time, everything I did was technology, and the Gemini Program had the paraglider on it.

RUSNAK: Was there competition or support between the program office and the efforts going there and in your own?

HINSON: It would be hard to say there was not, because we were trying to accomplish the same job with two different paths. But our office was also dedicated to supporting the Gemini office on—they had a conventional parachute system and the ejection seat system. We supported them on both of those activities, and these people are the same people, many of the same people. We did not really work on the paraglider system that much except for an occasional review.

Now, after all of that, to show you where things were going politically, if you will, toward the end of the Gemini Program, Langley Research Center came up with what they called a flexible paraglider, which was basically a triangular parachute with a central keel of suspension lines that held it in that paraglider shape. It had a glide ratio a bit better than the parasail, a bit better than our parachute.

So we started also trying to develop parachutes that would glide a bit better than the parasail did. So we had an effort using the Apollo vehicle as our test vehicle with about three different kinds of parachutes, including the Langley what we called the limp paraglider. Those efforts didn't really come to fruition because we had so much trouble with the Apollo system and had to put our energies there. It had no land-landing requirement per se in the program, so we had to concentrate on getting a parachute system that would do the Apollo job. That became difficult, among a lot of other things, because of weight increase in the spacecraft.

I'll tell you about one other technical issue that was a pure parachute issue that surprised us, and that was cluster interference between the parachutes and the cluster as they were inflating. Like the lead parachute would begin to inflate faster than the trailing ones. It sometimes just swoggled from the side, of course. The result of that was the lead parachute would take an excessive amount of loads, and we couldn't design a single parachute to take all those loads. It would have been too heavy, entirely too heavy.

While we were struggling with that problem, just over a pure parachute problem of our own making, the weight of the spacecraft kept going up. This was in what we called Block I. The [Apollo 1] fire was in January of '67. In May of '67, Shirley and I came to North Carolina and got married. Between those two dates, I had my guys look into things we could do to really accommodate a much heavier spacecraft, and I mean much, much heavier.

They originally sized the Apollo vehicle for a landing weight of 9,500 pounds, and we had very coyly got them to accept 10,500 as our test weight. What that 10,500 did was that it allowed us to make that system work and test it and prove it would work up to 11,750 pounds. That was the design weight at the time of the fire. The fire caused everybody to go in. Every system on board that spacecraft looked at itself from top to bottom, and you know nobody came out with a lighter system. We could see 2,000 pounds coming, and it was necessary. You can't start with something that you want to handle 9,500 pounds and handle 13,500. You just can't do it.

So between January and May, when I went off to get married, like a good boy, I had my guys look at two or three things. One was a larger drogue parachute to get the initial condition for the main parachutes down a little bit. The other was two-stage reefing on the main parachutes. Are you familiar with those terms? Reefing is just restricting the mouth of the parachute for a certain time with a cord and then cut that cord and let it open up. We put a secondary stage in there. So I had my real good analyst, Bill [Charles W.] Norris, running numbers on those two things.

When I got back from my honeymoon, Frank Borman was already out at [North American] Rockwell implementing everything I could think of. I mean, he led the effort to turn the Block II spacecraft into a safe and effective vehicle. All this, of course, was based on the problems with the fire. So he led the effort to basically review every system in there and do what needed to be done to make it a better spacecraft. Of course, Block II was better than Block I anyway, but he made it better. And it really did end up—I think 13,700 pounds was the heaviest we ever flew it.

Boy, when I got back from the honeymoon, we were sitting there ready to go with the numbers having been on two-stage reefing and all that, and when I got back, we had two-stage reefing and larger drogue in the program. In many ways, that was starting over for us.

The worst part about it is when you say two-stage reefing, somebody says, "Well, what if a reefing line breaks?" We'd say, "Oh, we'll put two in." When you do that, in order not to reduce your reliability of cutting the line, you have to look at maybe putting more cutters on each line, and it just got fatter and fatter and fatter. We packed that parachute to a density with a—I'm serious, with a press that would put 10,000 pounds of force on foot that was resting on the fabric, just to get the air out of it. We'd leave that pressure on, like overnight, you know, pack it in in stages and leave that pressure on for hours. Some monumental undertaking to get those three main parachutes packed and the system installed on the upper deck, but that all worked out, too.

But then that became troublesome during the program as we packed some of them up and left them for like a year to see if there was a problem with age on the pressure packing, because nobody had pressure packed that tightly before for a manned system. We thought we were really smart by doing that a year. Well, the first one we had down there stayed on the pad for longer than that, I think, a long interval between packing the system and installing it on the spacecraft and actually launching. So we ended up dropping one that had been stored for like two years, three years before the program was over. It wasn't a problem. Never found an age problem.

RUSNAK: So by the time Apollo came around, what were your specific responsibilities with the parachute system?

HINSON: Starting on the Mercury Project, I had a function that would have been called subsystems manager for the main parachutes. By the time Apollo came around, I had managed to acquire a section-head position, and I had two or three subsystems managers under me for various parts of the system, like one for the parachutes, one for the impact system and that kind of thing. So I was throughout the system by that time.

From the engineering side of the house, supporting the program office and managing the contractors' efforts, that's what we did, in every aspect of it, technical. We would do our own analysis and check Rockwell's. We would support the testing, review the test plans, go out and participate in flight readiness reviews, that type of thing, just basically participated as eyes and ears for the program office with a technical flavor on all parts of the program.

At the same time, we had this marvelous technology effort go in. I mentioned the cluster problems on the parachutes. There are a lot of ways out of that, and one is to modify the parachute itself. One problem with the Apollo parachute was that it would fill through the slots down in the lower part of the skirt. As it grew, it would enjoy filling more through those slots, not just through the mouth of it. So the Apollo parachute would grow to a big bowl if you just let it keep coming down, even with a small mouth. That's the kind of problem you got into where if one grew faster, the others would go alongside it like that and then the mouth would not be at a good angle to get air. So we lifted closing-up slots, redistributing the slots in the canopy, and actually built some parachutes in those configurations and initiated a wind tunnel program at Ames in their full-scale tunnel where we actually put a full-scale reefed parachute in there and got in there with a tuft on the end of a stick—the velocity's fairly low—and just look at where the air goes, just move the stick around and watch what's going in and what's coming out. So we had those kind of technology efforts going, and those directly supported the program. The contractor participated in those, too, so that if we found anything that was worthwhile, they could implement it immediately.

We had other more high-technology kind of efforts going, like a single large parachute. I mentioned that if you get over 100 feet in diameter, it gets unwieldy. Well, nothing was more unwieldy than having one parachute inflate, take all the loads, and blow. The next one inflates, takes all the load and blows, and the third one does the same thing. [Laughter] So we had contractor build a 167-foot ringsail parachute that would have done the Apollo job. Of course, we'd have to put another one in there. That's where it got really sticky, was trying to get the volume down to where we could get two of those monstrous things on that upper deck.

We made successful tests with those systems, but they were never to the point of being implemented. Actually, we put enough effort into the other problems that we got those at least tamed down enough we could fly with them.

We ran into another reaction control system problem on Apollo 15. We always seemed to outsmart ourselves on these things. We tested nitrogen tetroxide and monomethyl hydrazine. The hydrazine was no problem for the fabric, but the nitrogen tetroxide would go right through it, burn little holes in it. So after we deployed the main parachutes, when we dumped the propellants, the way we did it on Apollo was we dumped the propellants through the nozzles, we made sure that we loaded up fuel rich so that we'd not be dumping raw—they would burn when you first turned them on, and when you run out of nitrogen tetroxide, you just start dumping fuel out.

Worse possible thing we could have done, because on Apollo 15, I'm sitting at home watching TV with Shirley for that particular mission, and I see this spacecraft come down, and I see three good main parachutes, and I relax. I'd been thinking about this for the entire lunar mission. Like a week I've been thinking about "Let's have some good main parachutes." It goes behind a little cloud deck, and when it comes out, I've got one parachute doing this [gestures]. Two good ones and one parachute doing this. I said, "Shirley, there's no loads on that system that would make that happen." So for the next 10,000 feet, it descends, I wait for the next one to come off. That was burning through the reaction control systems, thrusters, fuel rich, and what we didn't know was that when we run out of oxidizer, the fuel going through those hot nozzles can act as a blowtorch, and we basically burned that parachute off. Lucky we didn't burn two off. Just pure geometry. So you can look too hard for problems and create them.

RUSNAK: Fortunately, that's what you have redundant parachutes there for.

HINSON: That's right. That was the only landing we made with just two, and it was okay.

That's another reason for going into the water. If your objective isn't to prove you can land on land, water is a really forgiving substance. I say that, but we fought the water impact. I didn't work directly on this as much, but the Apollo vehicle, if you put it in the water too flat, that heat shield would give and make itself even flatter, and you could develop some extreme pressures, more than the heat shield would stand. You could bust it by hitting at thirty feet per second just at the wrong angle, like on a wave slope. So we did a lot of technology efforts to try to solve that problem. And that's one reason we looked to landing on land for Apollo. We had some neat systems for landing on land for Apollo, like the rocking rocket. Have you heard of that?

RUSNAK: No, I'm not familiar with that one.

HINSON: It would come dragging in like this and touch down on the back of the heat shield. Then it would rock over, and this gets to be a really dynamic event. After this contact, we would fire a small rocket up here and arrest the rotation rate. That worked good.

RUSNAK: Then just land with a regular parachute then?

HINSON: Just drifting with a regular parachute.

RUSNAK: At what point did they give up trying to put Apollo down on the ground?

HINSON: We never really had that as a strong objective. It was a technology driver more than anything else. I would say we probably started really giving up on those kind of efforts when it became clear we were build a Shuttle, which was fairly early for us. Usually people like me were kind of loose from a program a year or two before we stopped flying those missions and already working on the next one.

So the first thing we worked on was the Space Station before Apollo was over. The Agency kind of made a choice that the Shuttle—that the more convenient access to Earth orbit was the most important part of that future overall plan. So we went in the Shuttle direction. Suddenly I'm without parachutes, except the first mission had ejection seats.

RUSNAK: So as someone who had been doing parachutes all along, how did that—

HINSON: I started looking around for something to do and for something for my guys to do by that time, because I did have a section. There was a lot to do, but, honestly, I had trouble finding my niche in what to do in that lot. We were in the area of flight mechanical systems, so things like wheels, tires, brakes, landing gear deployment systems, payload bay door mechanisms and latches, aerothermal seals and the elevons and all the moving surfaces, attachment of thermal tiles and things like that, a lot of that stuff falls naturally into mechanical systems area because we have experience with certain materials and bonding techniques and that kind of thing. It was pretty clear that there was going to be plenty to do, but I had trouble finding what I and my section was going to do.

One thing I did was I got into analysis of performance of the braking system. I had never used a computer to this point. One of the early things that I heard was that Rockwell said if you have a deceleration parachute on the Orbiter, it takes something like 40 percent of the landing energy has to go into the brakes. If you don't have a deceleration parachute, then 70 percent has to go into the brakes. I thought, "Man, what an analysis. Is that the way we decide how to get our Orbiter down the runway?" [Laughter]

So I took a Hewlett-Packard 35. The 45 wasn't out yet. The 45 was the first one that had any kind of programming capability. I took a Hewlett-Packard 35 and stepped an Orbiter down the runway with little intervals of constant aerodynamic coefficients and constant braking coefficients, and I did a braking analysis. I mean, I did a braking analysis. I did parachute effects, different-size parachutes. I even did a couple of cases where I did the parachute inflation transient to see if that made any difference. I opened the speed brakes different amounts. I went through a transition at the speed brake inflation rate. I changed elevon positions which changed the aerodynamic force down on the ground. I'm sure you've heard about short-nosed gears so the Orbiter runs down the runway like this. That's really great except for main tire loads.

Orbiter is the only plane landing that I know of that lands at the highest loads at the highest speed. When it touches down, when the nose gear touches down, not when you first contact on the mains, because you've got the lift that way, but when you touch down that way, you've got a lift load that's almost the same as the Orbiter load down on those main tires. If you ever get a chance to see a main tire test on a dynamometer, it sounds like a grinding wheel. You deflect that tire—30 percent is normal maximum on that tire. You mash that tire maybe 60 percent. It looks flat. It looks like that you got rubber on rubber. [Demonstrates sound of tire.] It makes noise like a grinding wheel, just the tire now. We had a lot of fun with those Orbiter tires, wheels, and brakes. We used to pick up pieces off the runway after every flight. Do you remember that? Of the brakes.

The Orbiter brake was a first of a kind and the only of the kind. It was a carbon-lined beryllium heat-sink brake, and nobody else flies that. I'm not sure exactly how we got off in that direction except that it's got incredible heat capacity and we really were going to work it hard. But in development we had failure of every piece in it, every piece in it, from clips that hold carbon faces on the beryllium rotors and stators, basically every piece in it. We got through all of that. Remind me to tell you something about program cost, because that's when I shifted gears on that issue, and a lot of people did.

Anyway, I did my little analysis, and my little analysis said, "Gee, if you have a 15,000-foot runway, you don't need a parachute." Another one of these worst things you ever did. [Laughter] Well, it was the truth. We were planning to have a 15,000-foot runway at the Cape and weren't really planning to land anywhere else. We had, of course, the lake bed [at Edwards Air Force Base, California].

I wrote that all up, and we started sharing results with Rockwell and B.F. Goodrich, the brake people and all that. Shirley was so impressed with my work on that HP 35, that meanwhile the 45 came out and she bought me one for Christmas, for $400. A hand-held calculator. [Laughter] I can get that much capability right now for $10.

The next step was we got a Wang computer, and I actually programmed it, in a fashion. From that I was able to represent that carbon-lined beryllium brake and its performance down the runway as it heated up and, depending on the rate of power input to them, analyze the brake performance so well that later B.F. Goodrich was doing some off-limits tests and they asked me which test would be more severe on the brake, like a cycling test at a certain level or just constant at this level, and my little program would spit that out. So that was very enjoyable, very enjoyable, and I steered a lot of my section activities in the direction of analysis and analysis of tests, you know, that really meant something.

About this same time, the program really got into money troubles. Aaron Cohen was the manager of the Orbiter at that time. I found out months after this period–I'm talking about right now–that we would send directives out to Rockwell, that Cohen would approve, there simply wasn't enough money to cover everything. So Rockwell had to make some choices, and I'm sure with Aaron Cohen's consent, these are the most important right now so we'll do these. You can't order somebody to do something if you don't pay them for it.

What I found out was we had maybe twenty brake dynamometer tests, and they said, "We can't afford twenty. You're going to have to get by with fifteen." Then the fifteen became twelve, became ten. You know there's got to be a stopping point somewhere. But way before we reached that point of no return where we didn't have enough information, I suddenly appreciated the challenge of trying to get more information out of every test, and we started working in that direction. That's one reason I developed that simulation program as far as I did, was to try to get more information out of every test. I would take a test and get 1,000 data points out of it and massage them every way it was possible to massage them, looking for anything that was unusual or abnormal. I love that kind of systems analysis.

Boy, the tools you have to do that with now. My group had those kind of tools before I left there, but by that time they were doing docking analyses and everything that really mattered. But it was really fun to try to do. That's the first time we had ever really had to marry at the lowest possible level the cost of the program with the technical output and the schedule and everything else. I mean, it would cost dearly if you weren't ready on time. You could not fail to be ready on time. You think you're going to have twenty tests and you get twelve? You've got to do something. This is not better, smarter, cheaper, faster that I'm talking about. It's definitely not that. It's applying a little more concern to your choice of test conditions and making sure that everything is the right configuration and getting as much instrumentation as possible that doesn't influence the test, and then doing something with that data. I enjoyed that.

RUSNAK: We'll take a short break now to change out our tape.

HINSON: A lot of the guys had rooms in the bachelor officers' quarters at Ellington. I don't know if I should tell this or not. I stayed there for probably six weeks, without a room. Like some many, I was always on travel. I mean, we really did a lot of traveling back then. So somebody was on travel, and there was always a room to stay in. I came down and, like I say, stayed about a week and went back to Virginia. Then I came back and went to California. Then I came back and went to Dayton, Ohio. I had about a half a dozen travel checks uncashed, and they started begging me to cash them. [Laughter] But, boy, I got ahead on personal resources when I moved down here, and I never got behind again. A lot from that aspect of it was I was traveling so much when I got down here, I didn't have to have a place to say. Basically I had no expenses except food at home. That helped.

Do you remember where we were?

RUSNAK: We were talking about Shuttle and your program to analyze the braking and that type of thing.

HINSON: Yes. I really believe that that activity was the first time that I had a tool that I could do systems analysis with, and it was the first time that I had a system that the flight crew had an interaction with other than to flip the switch and deploy something. The manner in which they used the brakes, they could destroy the brakes simply by pressing on the pedal, and that just made no sense to me whatsoever. I'm serious.

When we first started with that braking system, they had a 3,000 psi system. I looked at the dynamometer tests, which were done using an auxiliary pressure source and the controlled torque of the brake. They measured the torque and adjusted the pressure accordingly. The designed torque level—some of these numbers are the same, but it's just coincidence—1,500 psi of pressure would generate 1,500-foot pounds of torque, which was the designed limit. And that's the way they ran the test. We could not operate in orbit or brake that way. There was no information the pilot had that would allow him to operate that brake at constant torque, particularly on this brake, since its torque generated with a given pressure varied during the run.

As you first get on it, it's a real high power level. The rate of energy going into the brakes is real high. Then the brake heats up, so the surface friction characteristics of carbon on carbon changed, the way the beryllium is absorbing the heat changes, and we're sitting there with a 3,000 psi brake and it only takes 1,500 to generate design torque. So you really could destroy it simply by pressing on the brake.

The one thing we got implemented very early was to reduce that pressure to 1,500, the pressure that the brakes could actually see, and that basically made them more difficult to destroy during one operation. But we still had the issue of how does the pilot apply the pedal and then what feedback does he have? What measurements or what field, or what does he have to tell him he's doing it right?

We set up some tests out at Ames [Research Center, Moffett Field, California] in a vertical motion simulator. That's one of the most interesting things I've ever done. It's a simulator set up for helicopters. It has a plus or minus about thirty-foot stroke to the side and then that's a platform. That whole platform is mounted to give about a plus or minus thirty-foot stroke vertically. Then it's got some limited angular motion. Not only did it have thirty feet of stroke to the side, but it had a full 1G acceleration. It would move that spacecraft around.

So we mounted a cabin in the middle there and put my brake models in there to generate the right brake torque, put different runway friction coefficients between the tire and the surface and let Brewster [H.] Shaw [Jr.] and Mike [Michael L.] Coates do some landing. Brewster Shaw we named "Twinkle Toes" because with no knowledge whatsoever and limited physical feedback of the simulator, he was able to do the most precise beautiful braking you ever saw. You'd tell him what you wanted him to do and he would do it. He looked like he was maybe two years out of high school. To hear Brewster and Mike Coates talk about, "Well, I was instructing at [Naval] Test Pilot School at Pax [Patuxent] River

[Maryland]," and they looked like they were two years out of high school. [Laughter] They just got incredible training and skill development and all that.

Anyway, from that simulator we were able to decide a few things about how to operate the brakes. But things like the simulator fed our knowledge base to do a systems engineering evaluation of the system, including pilot interaction. I enjoyed that, too. First time I'd really ever had the pilot interaction I could work on.

I believe that our analysis that supported that systems engineering, I believe it affected the design of the brakes, the way they were used, the way they were tested, and management had confidence in them and flight crew confidence in them, which was refreshing, to be able to go through all of that on a system.

The tires were designed for five landings. Can you imagine putting a tire on your automobile that's designed for—this is serious now—you're going to land on a 15,000-foot runway. Say it takes all of it, three miles, times five, that's fifteen miles those tires are designed for. Then you've got to worry about getting the Orbiter back to the hangar from the middle of the runway. In general, that was a tow operation, but even towing, you got tires that are designed for fifteen miles, you're going to tow them another fifteen miles? You've got to think about that. I think they're probably doing much better than that now.

The brakes were also designed for five uses. They were designed for one emergency stop. The five uses were at a level that the military considers an emergency stop. That would be an abort at maximum velocity. Like at maximum weight and you start down the runway, you get almost to flying speed and decide you can't make it, got to stop, that's the level that we stopped that Orbiter every time. Now, since they put the drag chute back on, it's much better than that now. The drag chute won't stop it, but it'll come close. You've got to tweak the brakes a bit to keep from visiting the alligators at the end of the runway.

RUSNAK: I did want to ask you about that. I may be jumping ahead, but they decided to add the drag chute back in.

HINSON: The drag chute was added back in because—well, there was one other thing that made that something that was looking at again. When we started out, we had a really tail-heavy Orbiter, and getting that parachute out of the tail was the real advantage. I think that situation changed dramatically. I think if anything, it went the other direction so that it was not such a bad idea. That drag chute probably weighs 250 pounds. I don't remember exactly, because I didn't really work on it. I would guess it probably weighs a couple hundred, 250 pounds, which is not something you want to take to orbit if you don't need it. I think initially we were trying for something like thirty dollars a pound to orbit, and I don't think we ever achieved that.

At any rate, looked at it again. Early in the program, as I said, we were spreading brake pieces all up and down the runway. So they did several things. One was look real hard at how they were using them, look at the drag chute, look at a different brake, and they do have a different brake now. I didn't work on that either. I was off doing Space Station then. But the new brake, I don't even know what it's composed of. It's not carbon-lined beryllium. It may be all structural carbon. I'm not sure. It's got more energy capacity. I'm sure the tires are better. The whole system's better. The re-implementation of the drag chute was part of that process of trying to stop. It's embarrassing when you land on land on a runway and you have the TV cameras there and you stop the Orbiter fine, and then you have these people out there all over picking up brake pieces off the runway. You just don't want to do that. [Laughter]

RUSNAK: Were these issues coming out during the Approach and Landing Tests [ALT]?

HINSON: They did not there quite so much. We had other problems there, which shows false economy in the program. I'll talk about that in a minute. We landed on the lake bed, so we didn't have a stop-on-the-runway issue and we didn't have so much of a cross-wind issue. We made one landing on the runway itself, and that was okay, barely okay. Has anybody told you about the first landing on the runway?

RUSNAK: Is that the one where they bounced?

HINSON: It's the one where they got into pilot-induced oscillation. For people that really like airplanes, it really looked bad. It was one of these wallow-in kind of things. It probably was not as bad as it looked. A lot of that is that Orbiter control system. You have to anticipate. You don't sit there and correct for gust and fight it and try to fly it right down the pipe. If you go over to the Space Center Houston and fly the simulator, it's like that. You can't get yourself out of trouble if you let yourself get into trouble. You just have to stay ahead of it all the way and don't make any control inputs that you don't absolutely need, but the ones you need, you've got to make them quickly.

Anyway, what happened on ALT that was interesting to me in that I really got involved in was the first time we landed on the lake bed and we'd told them to put on brakes gently, just use them as much as you need to to stop, but do feel them out a little bit. They just about shook the airplane apart. Something like 5 hertz of shake that was like coming on and turning off kind of thing. It was a real concern. The magnitude of the oscillations that got fed into the landing gears were frightening.

We cured that. We really scrambled digging through all the information we had, and we had one series of tests run at Bendix that had an all-up Orbiter landing gear with the real brake system and the real anti-skid system on it, as opposed to just some source of pressure to do whatever job we were trying to do. That was the only place we have actually implemented the anti-skid system with the brake system on the dynamometer. I looked at that, and there was a 5-hertz oscillation in there.

What came out of all of that eventually was, there was a test they could do that there's like a little capacitor or something in the anti-skid system that changes what they call lead lag on the pressure. They adjusted that, and we didn't shake anymore. That test cost something like $250, and we had been trying to get Rockwell to do that for months, and it was not part of the program.

Sometimes you can leave out the wrong things, save the wrong money. But that one went away. But it was a frightening problem there for a while because we didn't know whether we were picking up some kind of a lake bed-tire interaction effect or it was a system problem or it was a landing-gear frequency problem.

Another thing that made life interesting in those days, things that are surprising to people that don't do that—and I had never done a landing gear before—the landing gear was a forging. The main part of the landing gear was a forging, a large, really precise steel forging. The lead time on getting one of those forgings was something like three years. So you don't just arbitrarily change your landing gear. [Laughter] You don't necessarily have one for your test program that's three years ahead of flight. Those things are interesting to me. Surprises. I guess that's what makes these things take ten years.

Somewhere about the time we started flying, really flying Orbiters, Shuttles, I got back on Space Station again. The mechanism for getting on Space Station was, we had some re-orgs [reorganizations] that kind of left me loose in a systems branch head position, and the jobs in the branch were pretty much covered except for some of the advanced work. I'd see some of the configurations for Station that are coming out, and I'd think about an Orbiter trying to dock with that station. You had a couple of long bodies that were going to touch on the ends, like—well, like trying to catch a baseball bat. If somebody throws a baseball bat at you with a little bit of angular rotation on it, it's not easy.

So I got involved in setting up some docking simulations. My purpose always was to try to come up with requirements on the docking hardware for attenuation and compliance and that kind of thing and for location in the Orbiter. So I spent my last, really, engineering efforts in that area.

From that I got into a group that supported the Space Station Program per se, as opposed to the engineering side of it, so I was going back to those simulation efforts from that perspective rather than others. That's where I was when we stopped, when I turned fifty-five.

I enjoyed every minute that I ever worked at that place. I loved the missions. I loved the people. I loved the kinds of things we were working on. I retired when I was able to, and I have not looked back one minute. I have enjoyed every minute that I've been away from it.

RUSNAK: So why did you choose to retire at that time? Was it just the age?

HINSON: We had thoughts of moving up here and building this house and doing things like that. The work environment had changed. I am glad I'm not there right now. I really am, and that isn't from the old person's perspective of "It was really great when we were there, but you guys don't how to do it." I don't really mean that. It's different now. I didn't have to learn an entire new way of working and new sets of tools to work with and all that. If I were there now, I would have had to go through that learning process and learning to deal with new and different people. That's not bad. It's just something I didn't do, didn't have to do. But every time there's a launch, well, I get pitter patter.

I had three friends on *Challenger*. For months after *Challenger*, all they had to do was list the names on some TV report or some story about it, and I could my heart rate go to 120. I didn't even have to count it; I knew it was going 120.

Mike [Michael J.] Smith was a North Carolina boy. We visited here shortly after the *Challenger* for some kind of reunion down on the coast, and he was from, I think, Wilmington. We were just walking down the street in a park, and we heard some people talking about, "They killed those fellows." People were really upset about that Challenger. I was upset about the Challenger.

Ellison [S.] Onizuka, I was on umpteen little teams with him to do this or that or the other. Like before we ever flew the Orbiter, we were afraid if we took a meteoric hit or maybe even a bird hit it on the way out, we'd damage to the tile to the point we didn't need enter with that. So we came up with little repair techniques that they go EVA and do. Fortunately, we never had to use that kind of thing. But I got to know him very well.

They would run an astronaut—I'm sure they still do—they would assign an astronaut to wheel, tires, and brakes. So it would constantly be Mike Coates or somebody like that would be over to talk about them. He became their expert in all those things. We got to know a lot of them that way.

I mentioned—well, I need to keep going if you want to keep going.

RUSNAK: Go ahead.

HINSON: I was going to go back and pick up that remote flight controller stuff.

RUSNAK: Yes, I do want to ask you about that.

HINSON: Shortly after I went to work, I can't remember exact numbers, but somewhere around twenty to twenty-five of us in the flight systems or engineering side of the house were assigned as capsule communications—no, not capsule communicators. At every remote station, of which there were about seventeen around the world, they had a capsule communicator and a doctor and an engineering specialist. There were about twenty to twenty-five of us in engineering that were assigned that engineering specialist task. So the first thing we did was learn Morse code. I thought I would never learn Morse code. But I learned Morse code all right.

We were being prepared to go to these seventeen stations around the world. I mean places like Woomera, Australia, and there was an Hawaiian station, and Canary Islands on the other side of the Atlantic, and a couple of other places intermediate. And then there was an Indian Ocean ship and maybe a mid-Atlantic ship. I can't remember. There was another ship somewhere. I went through that whole program. When you're talking to Shirley, I'll go get you a booklet that's the Mercury spacecraft—it's the configuration document that tells you everything that there is to know about everything in there. It's the pilot's manual for like operating this car. So it's got all the systems described. I worked through that just like they did, and learned all those systems, including the systems that I wasn't working on. So it was a real pleasure for me to know that much about the spacecraft.

It was difficult later on not to know that much about things like Apollo, when they just got so complex you couldn't. If you paid attention to other people's systems, you couldn't do your own very well. But this was part of my training and part of my familiarization as a engineer there, too.

I took one trip, and that was the very first practice run where they had an all-up sim. I went to a ship in Jacksonville Harbor. It was a destroyer. I was amazed at how small a destroyer is. I can't imagine being out in the middle of the ocean on a destroyer. I mean, it's about this wide. But we went through a mission sim there. I enjoyed that, all the participation in that activity.

But when I came back, I got word somehow there was a rumor that us guys from engineering that were in that program were going to be transferred over to Operations because we were about to start flying missions and they needed more controllers. So I went to my boss, who was Max Faget at the time, and I talked with him about that. I said, "I don't want to make that transfer. I haven't learned what I want to learn in engineering first. Maybe later I'll do that, but right now I want to work on engineering problems."

He said, "Well, I haven't really heard much about that." But he let me go over and talk with the assistant division chief in operations, who was Chris Kraft, and he let me go talk to the division chief in operations, who was Chuck [Charles W.] Mathews.

Chuck Mathews was the one I had to say, "I really don't want to transfer over to your group right now. I have other things I want to do."

He says, "I don't think you have anything to worry about."

The very next day, a memo came out transferring everybody but me to Operations. I bumped into Chuck Mathews between the two buildings, and he grinned and says, "I told you, you didn't have anything to worry about." [Laughter]

RUSNAK: Good timing on your part anyway.

HINSON: Yes. Later when we needed a representative in the Gemini Program Office to interface with McDonnell-Douglas on that lifting parachute landing rocket system, Chuck Mathews was at that time the manager of the Gemini Program Office, and I had to tell him that I'd really rather somebody else do that job. [Laughter] I was already working on some other things on Apollo and wanted to stay there. So twice I've turned that man down. He's a wonderful human being. I mean, we had some managers that were just first-class human beings. I could name fifty, but I don't think I'd better.

RUSNAK: Overall, did you get a good sense of the relationship between the operations people and the engineering people?

HINSON: My perception of that was that it improved a lot as we went on. My problem early on was I never got to work with them that much. The main parachute system was basically automatic, and all they could was back it up with a manual switch.

The first simulation I did as an engineering remote site station operator, they failed my main parachute, and then they failed my reserve parachute. [Laughter] They says, "What's going on in there?"

And I said, "Well..."

"We just don't do those kind of things."

My own relationship with operations improved tremendously as I got into a system that required some operation. In the middle of some of these braking problems, I mean, we were running tests at B.F. Goodrich to find out whether it was safe to fly the next mission in the condition we were going to fly it or not. I was called over to a little conference that Gene [Eugene F.] Kranz was chairing, because he was flight director at that time. They were in the middle of a sim that involved the Cape and Goddard and Rockwell. Everybody was on the net, and they were flying a mission. He calls me over and he says, "We found out that—we think the tire pressure is not as high as it should be. We need two things. We need to know how low could it be if we had this worst-case starting point and what to do about it."

We sit there and we talk about it. I tell him everything I think about how to find out what the pressure is and what the lowest pressure could be. I said, "Then we really need a dynamometer test at that pressure."

You would enjoy this. He says, "Okay, implement the dynamometer test. I'll get B.F. Goodrich involved. Can we get the hardware?" He's asking around.

"Yeah, we can get a tire out there," and all that stuff.

I had to ask him, "We are just doing a simulation, aren't we?" [Laughter] I really did. I had to ask him did he really want me to do that test. And, of course, he didn't.

Obviously that wasn't the first time he had ever had that response, because he gave me an honest, straightforward answer, "No, no, no. Don't do it." That's the only time I ever worked with Gene Kranz.

Got involved with operations a bit in that we had—I can't even remember the acronyms now. There was an engineering control center off to the side. You know what I'm talking about?

RUSNAK: Mission Evaluation [Room] [MER]?

HINSON: Yes. I worked with them some. So I had some operations interfaces, but they were always down stream of what Shirley's activity had been, so I still never had any. Let me tell one thing about Shirley, about working at Johnson Space Center. We would go to work, have a really pressurized day, both of us from different directions, with either schedule problems or engineering decision problems or whatever, come home and watch recorded "Days of Our Lives," complain about the acting, the directing, every part of it, that even the background music is bad. I mean, we would vent for a whole hour of "Days of Our Lives" and wonder why we kept watching it. And that would get the icky part of the day out of our systems, and then we'd get to do whatever it was we were doing.

Many, many, many a night we would start talking about work, some aspect of work, during the evening news, like after ten. At two, three, four in the morning, we were still talking about work. I'm not kidding you. I bet you we did that 100 times where we'd talk past two o'clock in the morning. It might technical, it might be organizational, it might be mission objectives, and we enjoyed that, really enjoyed it. That's the only time we ever worked together. Very casual.

RUSNAK: Sounds like it. We were just talking about your interaction with operations. How about the astronauts? You mentioned on Shuttle that they would assign an astronaut. Was that the same in Mercury?

HINSON: Yes, it was the same during all of it.

RUSNAK: Do you remember the contribution, who the specific astronauts were, a few of them?

HINSON: In terms of my own systems work activity, there wasn't that kind of interaction because they didn't have that much to do. All they had to do was learn how the systems functioned. But in Mercury it was all of them. I don't know one any more than the other except maybe [Alan B.] Shepard [Jr.]. I do remember he was talking with my division chief, and I waited outside the room until he'd left. This was after work. Waited outside the room until he left. I have a picture of a parachute system with the Mercury vehicle descending. When he came out, I told him I would sign that for him if he would sign one of his for me. And he said, "Come on over to the office, boy." [Laughter] I think that's the cockiest human being that's ever been on the face of the earth. He really is. Was. He really was. I mean, he strutted. I mean, that rascal, he could strut sitting down. But really a wonderful human being. Nice guy. All of them were, but he was the cockiest of the lot, no doubt about it. I enjoyed him.

After the missions, they would come back and get on the side exit there to Building 58, and the astronaut would thank us and tell us about the flight and all that. It was real homey. And that added to this "Our team is better than your team" kind of stuff.

I really wish we were not involved with the Russians on the Space Station. That's not politically correct, but that's okay. Part of that is the Russian mentality, I mean our feelings about the Russians from past years. Part of it is that. Part of it is they have a different way of doing business. They make different choices in their systems. The whole time I was at work, the Russians always had bigger lift capability. Therefore, they did not have to miniaturize as much, and they ended up with different spacecraft, in many ways cruder than what we did. I don't say worse, but they were cruder than what we did. And now we're marrying all that. It's not just the hardware either; it's the way of dealing with people.

If Shirley came in and told me she had to go over to Russia for two months for a NASA job, I would say, "Nuh-uh." I don't want to go over to Russia particularly. I know this is not really the kind of stuff you want to hear, I believe, but I really wish we were not involved with them.

RUSNAK: Had you had any experience working with them on the Apollo-Soyuz Test Project?

HINSON: I met a few of them. I shook hands with one of the Russian flight crewmen down at the twentieth anniversary celebration for the lunar landing, the one that had [Walter] Cronkite there and it has the Apollo 11 flight crew down at the Sheraton Hotel. First time I've had a dinner jacket on since high school prom, and we had a great time, just a great time. We spent the night there at the hotel so we wouldn't have to drive home and all that.

But, anyway, I shook hands with one of the Russians that was on the Apollo-Soyuz thing, and that was the roughest, largest, strongest hand I have ever had my hand on. It was like a mitt. I mean, it was like a dirt farmer that didn't have any tools. It could rip a furrow in the ground bare-handed. [Laughter] That was my impression of him.

A lot of my friends and associates worked a lot closer with them than I did because they were involved in docking systems earlier. I was not really involved in the Apollo docking system, except that my parachutes had to go around it. People in my office were very much involved in it, but I just didn't happen to work on those systems. The parachute system for Apollo-Soyuz was the same. So I didn't get to bump into too many Russians. That program went very smoothly. I didn't really have a bad feeling at all for that program. I thought that was an extremely worthwhile thing to do.

But on Space Station we're talking a commitment for many, many, many years of operation and hardware that is in a really tough environment, particularly long term. If we had it all ourselves—I don't object to the International Station concept, I just wish we weren't so locked in to the Russians doing about half the job. And I hope we get that Shuttle flight off in about a month or so.

RUSNAK: I wanted to ask you also, looking on your career as a whole, what do you think the biggest challenge was for you, either personally or technically?

HINSON: You know, it really changed. Early on the challenge was technical. We were constantly trying to get a 1.1 performance out of a 1.0 system and then put that in a 0.9 container. Those kind of challenges were dominant.

Then toward the middle of it, the biggest challenge was getting enough resources. I mean, we really felt that at my level in the early part of the Shuttle Program. In the mid-Shuttle and current Shuttle-type activity, I don't know how it is, because I was off on Space Station by then.

Space Station, the absolute biggest challenge in the world was, there was not a clear objective, set of objectives for the Space Station to perform to. That is one of the worst designed problems in the world, is where you don't know what this thing is supposed to do. There are myriads of opinions trying to converge on that. When we first got into looking seriously at Space Station configurations, we were trying to an $8 billion Space Station. We had these costing models that it seemed no matter what we designed, it would come out 8.1, 8.2, 8.3 billion. Obviously, we weren't looking at something that we should have been looking at.

A little after that, when we got the skunkworks going over in the Beta Building or whatever it was off site, and we got Marshall people with us, a lot of Marshall people with us, and some contractor input, we started seeing really diverging opinions of what the configuration should be. It was again more how you want to do the job than it was what the job was.

Marshall got it in their mind they wanted a certain kind of station. A group of JSC instructors got it in mind they wanted something called a delta. The delta was a beautiful structure. Have you seen the delta? It's the mats of trusswork put together in that configuration. So you've got people that say, "This configuration works better if it's sun-pointing, and this configuration works better if it's Earth-pointing." And way off here in the side, you've got somebody that says, "Well, I want to look at the Earth all the time. That's one reason we want to go there, is for Earth observations."

Then we'd go next door and we'd talk about trajectory. We're going to put this thing in a 27.5-degree orbit because we're launching from the Cape and we can get the most payload up at that inclination. If you click it between plus and minus 27.5 degrees on the Earth, we barely touch the United States. We touch none of Europe. I mean, why would anybody want to look at the earth plus or minus 27.5 degrees from the equator?

That's one thing the involvement with the Russians has helped, is we're talking higher incline in orbit so it's going to go over some worthwhile farm land and stuff like that. But that's the problem we had with configuration studies at my level where we were trying to make some inputs on docking and stuff like that.

And then what the Station's going to do—with Mercury and Gemini and Apollo and Shuttle to a large extent, the what we wanted to do was really well defined. It was just a question of getting some hardware and some ways of working with it that would make that happen.

Space Station can be a transportation node to Mars. Can be. It can be an Earth-orbiting laboratory, Earth-observation laboratory. It can be a real environmental laboratory, but there you've really got to start worrying about acceleration levels. I'll never forget, we were looking at acceleration levels of 10^{-6}G or something like that, and that is a sneeze. I mean, nobody can sneeze. Nobody can bump into a wall. Nobody can do anything. You can't walk around on the station and bump into things and not create little G spikes higher than that. It's not anything like that. [bumps table] I move that water. That was probably 10^{-2}. Those things couldn't really drive the Station with all this other junk going on.

Russia has got to have some help, or Russia's going under. So are we going to just give them some foreign aid or are we going to join up with them on some programs like Space Station where we could swap resources and effort around? In that environment it's hard to make 10^{-5}G seem really significant, or whether it's 10^{-5} or 10^{-6}.

In the middle of all that, I told one of my bosses, I said, "What I think we should do," this is when I got transferred under a new boss, and I says, "I don't understand why we don't just build a pressure vessel that will hold a couple of people and do some useful work and put a small solar array on it and a small heat rejection capability and a little bit of attitude control and some communications and maybe reboost." You might not even do that. You might just bring a Shuttle and reboost it like we're going to do next month, I hope, which they said we couldn't do that ten years ago. You do what you have to do. And I said, "And put one of them in a high inclination orbit, and put one of them in equatorial orbit with more weight capability or something."

He said, "That's sounds like Mir."

I said, "Have you looked up there lately? They're up there." [Laughter]

He says, "We can't do Mir."

But that probably was the biggest challenge. It wasn't a challenge to me as much as a challenge to the organization, but that's the challenge that I felt probably most recently, was to try to help identify some requirements for the Station that would drive its early development. That was a toughie.

RUSNAK: Likewise, what do you think your greatest accomplishment was?

HINSON: Absolutely without question, the part the I played and the people that worked for me in totally redesigning and requalifying the Apollo Earth-landing system for not 11,700 pounds, but 13,700 pounds in eighteen months. We basically redid that entire job in eighteen months, and successfully. I think the whole agency can be proud of that, contractors, too. It was just incredible. And that is was successful from that point on in the sense that we did land on the Moon a few times and we didn't kill anybody else, although we came awfully close on [Apollo] 13. That was a tough three days, too.

RUSNAK: Were there any concerns about your systems from that mission?

HINSON: Not really my systems, except would they ever get a chance to function. We did some things on that mission we had never done before. For instance, just before we entered, we separated the LM [lunar module] from the command module. We'd never done that before down here. The main concern I had was the same concern all of us had: was there enough oxygen and all the other life support systems that needed to get back? I guess I felt pretty good about the propulsion and all that. My system, if it ever got a chance, there was nothing that showed it was in any kind of jeopardy at all.

RUSNAK: I wanted to give Rebecca a chance to ask any questions if she had some.

WRIGHT: I just have one, and you talked off and on about it. Can I get you to bring it all together? It's about communication. You mentioned earlier that the management team that you had during the very early days kind of gave you the freedom to make those decisions that needed to be made. Did you see that change as the spacecraft changed and technology changed and organizations changed? And how did that communication level change to allow you to do what you needed to do to get these systems done?

HINSON: It did change dramatically. The largest changes that I saw were in the last very few years, so I'm not sure that I really tested their effect on my ability to get things done, but I saw around me a change in people's ability to get things done, or at least a change in the approach they had to take to sell a project or whatever. The main reason that changed during my—let's say up to early Shuttle days, was how much resources did the agency have. On Apollo, I'm not kidding you, on Apollo, if we had concern about a piece of hardware or even a system like a main parachute, we would initiate a program to develop an alternate, and there was absolutely no restraint of that kind of activity. If it was a legitimate concern, management was quite receptive to your going out and spending a few hundred thousand dollars to develop an alternate early. I don't you'll see much of that these days.

I did mention the team approach. I never really worked that much in a—the last few years I was at work, there was program after program after program of things like TQM [Total Quality Management], and I think I can put that into words for you. I don't know if it makes as much sense to you as it does to me. But in the last few years of my work, and it was not only NASA, I saw that everywhere, the country, maybe the world, there is an awful lot more concern now about procedures than there is about products.

I saw something the other day that really drove me up the wall. I don't even remember where I saw it. Somebody sent an e-mail to somebody, and they sent it to me. It's just kind of incidental in that was a suggestion that we need to put more effort into design knowledge capture now that we have computer capability to do it. They even mentioned that we need to know how you do a job. We need to capture that knowledge of how you do a job. It strikes me as one more use of a tool that you do because the tool is there and you can do it. They can capture in a computer now everything there is to know about everything.

I think they're driving toward a cookbook for how you do a job. I have never seen that be successful. You can have a cookbook of mistakes people have made, or, say, limits of the capability of steel and other materials. You need those kinds of things, but do you need the things that say you need to do a job in a certain way? Like, develop this plan, go to this level of management for approval, develop a little more detailed plan, go over it for resources, those kinds of things. That doesn't do much for me. I never faced that at work. I mean, I never saw anything like that because we didn't have the capability.

Shirley mentioned memos. I loved having a piece of paper like that signed by you that says, "This is what I think my system will do. You guys need to learn how to use it." I love stuff like that. You can sink your teeth into that. All this e-mail stuff, before I left, there were people that generated e-mail and there were people who didn’t generate e-mail. The e-mail was so excessive, unsigned. When you go to the web today, how do you know that information is right? It’s that kind of thing. How do you know it’s right?

WRIGHT: From what you’re saying, it sounds like the human factor helped develop so much of the success because it was the human elements that interacted with each other, that helped provide you with results.

HINSON: You said that very well, very well. I felt that very strongly throughout my career to the last few years. In the last few years, some of the problems just got to be a bit too big for us. I’m talking about Space Station requirements. One of the Space Station requirements is bailing Russia out of their financial problems. I’m not saying that’s not important for the country; it’s just different. It’s not the way I did systems engineering.

WRIGHT: It sounds to us that everything that you learned you were able to build on. We certainly have enjoyed listening how you made changes and made those results that you were looking for.

HINSON: Thank you.

RUSNAK: Those were all the questions I had as well, so unless there’s anything else you’d like to say, I'd like to give you the opportunity.

HINSON: I don’t think so. I might really like to sit down when I see the result and add a few things.

RUSNAK: Well, you’ll certainly have that opportunity. I'd like to thank you for joining us today.

HINSON: Thank you for coming. I enjoyed it.

[End of interview]