NASA STS RECORDATION ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

GERALD W. SMITH
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is May 12, 2011. This interview is being conducted with Gerald Smith in Huntsville, Alabama, for the STS Recordation Oral History Project. Interviewer is Rebecca Wright, assisted by Jennifer Ross-Nazzal.

Thanks again for taking time out of your day to visit with us on this project. We know you joined NASA originally in 1961 and then left for a few years and came back in '67, where you started working on the Orbital Workshop, and then after that you were assigned to the SRB [solid rocket booster] Project Office. Can you share with us that transition to your assignment for the SRBs and then how your involvement and that project evolved?

SMITH: I was working, as you indicated, in a group that was responsible for auxiliary propulsion systems, small thrusters to control attitude of the workshop. I'd been in that group for several years, and then the Shuttle Program was getting started. The SRB Project was just getting started out at Thiokol, and I volunteered to work on the program. Charlie Woods was the program manager, and I was assigned to Charlie. We were planning to build a separation system, which are small solid rocket motors that staged the solid rocket boosters away from the vehicle. It was a new program, and I asked to work on that. I thought this would be fun, because you had to go through a Source Evaluation Board, pick a contractor, and then be the project manager. I was called a subsystem manager, but I managed the project.

It started out as a rather traditional program where we planned to use state-of-the-art propellants for small motors. We were involved in many separation studies and analysis conducted by the [NASA] Johnson Space Center [Houston, Texas] looking at sizing the motors, making sure that even with the motor out we could still separate safely away from the Orbiter and the tank. Early on in that program, we ran tests up at Tullahoma [Tennessee] where we tested a motor against some Orbiter tiles. I think they were about 20 feet away, which was the approximate distance we thought that we would have the motors separate from Orbiter, and it destroyed the tiles. We thought, "This is a major problem," because this was a traditional propellant with a lot of aluminum additives. Aluminum is a material that causes a very bright light when you see a booster fire, and the solid rocket industry used the aluminum as the fuel. I don't remember the percentage, but a major amount.

This started a major investigation on what we can do to avoid both impinging on the Orbiter with the motor firing, and at the same time what kind of propellant could we use to mitigate the damage. I remember in a telecon [teleconference] with Max [Maxime A.] Faget from JSC discussing the issue; my thought at the time was, "Why don't we just do something about the tile?" I learned very quickly that we do not do anything to this tile. They'd spent years developing a material to withstand reentry heating. I was a young engineer in this telecon, and I said, "Why can't we change the tile?" Later it was explained to me that a lot of research had gone into developing the tile, and there's no way we're going to change it out.

So we had to fix this problem on the separation motor side. We started going around and working with the industry, the Navy and others that were looking at that time at what they call smokeless propellant, which is the same thing as reducing the amount of aluminum, so that both the rockets being fired from aircraft wouldn't have a bright signature that could be traced by radar. They were working extensively on reduced additive propellants.

We did a lot of research and traveling around to the Air Force and Navy installations and found out what they're doing, and we did settle on a propellant that had about a 2 percent aluminum additive, which was far below traditional propellants. It was really pushing the state of the art at the time, which necessitated a lot of testing because the aluminum is a stabilizer for combustion stability. We had to know whether or not we could produce this material and have the motor be stable, because if it goes unstable it can explode since you get pressure oscillations inside the chamber. It was critical to us to establish a stable propellant design, so we did a lot of testing.

We had planned to put the motors on the side of the forward skirt, and we realized we needed to move the motors more forward and try to terminate the burn so that we fired quickly and then brought the pressure down before the Orbiter would fly through the plume. That drove us to moving the motors forward, canting the nozzle away from the Orbiter, and then controlling the burn so that at about eight-tenths of a second the motor firing was terminated. You blink your eye, and you miss it. All those things drove the design of the motor: the placement on the vehicle, the canting of the nozzle, the enhanced propellant, looking at even the igniter. We couldn't have pieces of the igniter that lights the motor be expelled and damage the Orbiter, so we placed propellant inside a Velostat or plastic bag inside the igniter and let that burn away quickly. We were very conscious of any kind of debris that would come out of the motor that could damage the Orbiter. It took a major test program to verify that we had accomplished our objective.

There are four motors on the forward end of the vehicle and four on the aft end, on the aft skirt. We didn't have to do anything to the aft motors since the exhaust plume did not impact the Orbiter. On the forward end of the vehicle we were worried about the nozzles being canted upward. During ascent, the ascent heating could prematurely light the propellant so we had to put a cover over it. We looked at a lot of different cover designs, and everything we looked at might generate debris. When you ignited the motor and blew the cover off, it could hit the Orbiter. We went through an evaluation and even considered some rubberized kind of materials (Kevlar) that would open up when you ignited the motor. We would score the material, and it would open up like a petal. But we were still concerned that we might burn off one of those petals and hit the Orbiter, so that wouldn't work.

Finally, we decided on a metal cover design that would cover the forward motors that would swing open at motor ignition. It had a ratchet device that once it swung open it would keep the cover open and lock it in place, and that solved the problem. It had never been done before, a first in the business. We did a lot of testing, a lot of experimentation on different cover designs, and finally found one that would ratchet open and at the same time not come off and generate debris.

This was a major challenge for a small motor. It was a great project to work on because it was small, about $7 million, and with all the other problems that the program was encountering, we were kind of off having a great time, and I did. Chemical Systems Division was the contractor out in [San Jose] California, and so was just the kind of program to learn to be a project manager.

We had an extensive test program, which included testing the motors under the coldest conditions, 30 degrees [Fahrenheit], and the hottest conditions, 120 degrees [Fahrenheit]. We would condition the motors to those temperatures and then fire them. Then we had to worry about aging the motors. We were going to produce these motors and then it'd be years before they'd fly. You've got to worry about what happens to the propellant during this extended period of time. Does the chemistry change? So we did what they call accelerated aging, where you put it under specific temperature conditions for an extended period of time and then take it out and fire it. It simulates the conditions the motor would see over several years' storage time, hot to cold.

We conducted an extensive test program. I think we had 8 development motors and 21 qualification motors that we tested to the range of environmental conditions, temperatures, shock, vibration, all the conditions the motor would see on the Shuttle. A relatively easy development and qualification program became quite challenging with interesting problems to solve. All the motors have fired successfully for every Shuttle mission, and there's really never been a problem. A very successful program, that was my introduction to project management.

I was later asked to work on the Space Shuttle main engine [SSME]. I declined because I thought I really knew very little about the main engine, especially the turbo machinery, which was the position to be filled. I graciously declined, and then I was informed that the following Monday my desk would be moved to the SSME Program. In fact, many career decisions along the way have been urged and encouraged by management. I started working on the Shuttle main engine and really knew nothing about turbo machinery. I had worked in advanced propulsion before in 1967 where we were doing some of the early component work on high-pressure engines. That was with Pratt & Whitney. I'd worked on that a short time, but really had very little knowledge about the turbo machinery and all the challenges to be faced in developing it.

This was at a time when we were just starting to test, and what we got into was failure after failure. We would fire the motor maybe a second and a half, and it would explode. This was when we realized an earlier decision had created a major problem in the development program. In an attempt to save money an engine was cancelled from the budget, and that engine was to be very highly instrumented with pressures, temperatures, strain gages, all kinds of measurements so that when we fired, we would know the conditions inside the motor. The decision to not build the motor drove us to a flight configuration where there are almost no pressure taps, no temperature sensors, none of the information that you need to know what's happening inside the motor.

Incidentally, in comparison, when I was working at General Electric [Company] from '65 to '67, the first supersonic transport engine to be tested was covered with instrumentation. What we were doing was just the opposite of what the aviation industry had always done. A bad decision made for probably good reasons, but it was a bad decision. We were faced with a flight configuration motor with very little pressure and temperature capability. What we were faced with was determining the engine health with the little instrumentation coming off the engine, and the visual and the photographic images. We found we had to go to very high-speed cameras, because early in the testing you'd have a video of the engine, and the next thing, there's nothing but a huge cloud. You couldn't tell what happened to it. We went to very high-speed cameras and film so we could start to see as the engine ruptures or comes apart where the first indication of failure was occurring.

We tried to do internal instrumentation, and in one instance one of the internal sensors caused a fire and we blew the engine up. So there was trepidation against adding much instrumentation inside the engine, which we really needed. We did a lot of testing and failure investigation, failure analysis, which on one hand was bad because we lost a $20 or $30 million piece of equipment. On the other hand, it was exciting trying to understand what happened to the engine. Examining all the pressures, temperatures, and other data where did things look like they were starting to happen, and then try and identify, if it's happening there, what's failing? It was things like turbine blades and sheet metal failures, and all of these things one after the other we kept encountering as we learned to get enough testing and design changes to get a full-duration test. We kept learning from testing.

That's how we developed the engine, which is not, in retrospect, the most intelligent way to do it, but that's what we had to do. That's what we were faced with, a lot of testing, some spectacular failures, and then all the investigation that ensued. It's remarkable today, even to this day, we've never had an engine failure. I believe anyone close to the program early on, *Challenger* [STS 51-L accident] as an example, felt if we ever had a failure, it would be the engine. I think everybody believed that. Certainly I believed it. I was in Washington, DC at the time, and I was totally convinced we had an SSME problem before I learned it was the solid rocket motor.

That was my involvement in the SSME. I was on that program until we flew two or three times, and then I went from that to deputy associate director of Engineering. Going back, though, I recall this. One of the problems we had, in addition to turbine blades cracking, we had bearing problems. We would run a test and then take the pump apart, and we'd find pieces spalled off the bearings. In the engine environment, that's not good to have metal pieces running around. We tested exhaustively and could never quite figure out where the loads were coming from and when they were occurring that would cause the bearings to spall. We were really concerned. This was a major problem, and we were approaching the first flight of the Shuttle.

J.R. [James R.] Thompson [Jr.] was the program manager of the engine at the time. A decision was made by J.R.—and management supported it both at the Center level and at NASA Headquarters [Washington, DC]—to build a pump that had spalled bearings. We ran a test for 800-plus seconds, which was a return-to-launch-site or abort of the mission simulation. The SSME normally runs for a little over eight minutes. In this case it was over 14 minutes, and we ran it with the spalled bearings. The reason we ran the test was because we didn't know where in the engine firing time spalling was occurring. The logic then was that the worst possible situation is where you actually start out with them spalled. You run a full-duration test, and if that's successful, it would clear the engines to fly the first Shuttle mission. It was a gutsy test, and I thought J.R. was absolutely out of his mind. We sat and listened to that test in his office, and 828 seconds was about a year long in terms of just sweating this thing out, because we knew if we blew the engine, we would set the Shuttle Program back years since we didn't understand the problem at that time. We didn't know how it was occurring, or where the loads were coming from. So we ran the test, and that cleared the engines for flight.

Because of the various problems we had encountered during our testing, we had to pull the pump after each flight and examine them. Initially, the SSME was designed to fly 55 missions before replacement. We were having to pull the engines and the pumps after every flight and inspect them, and, in many cases, replace bearings and other parts. As a result, we never remotely achieved the life expectancy we had planned for the engines. As a matter of fact, when we finally built the first engines and we learned how hot the engines were running and the stresses and temperatures on the turbine blades, we decided to meet with the aircraft industry, Delta Air Lines specifically. They told us that with the materials we were using, they would never operate under those conditions. Well, we didn't have a choice. The engine was built, it had high temperatures and extremely high pressures.

So we changed to single-crystal materials for the blades, the best material we could find in the industry, and then we restricted the engine to 100 percent power level to reduce temperature and stress levels. It was originally designed for a full-power level of 113 percent. In fact we tested several times at that power level, but it really was creating a lot of damage to the engine. So we never allowed the full-power-level operation, and we stopped it at 100 percent just because of the temperatures and pressures.

It was a very successful program and an outstanding contractor in Rocketdyne. We had a very close working relationship, the engineering people at Marshall [Space Flight Center, Huntsville (MSFC), Alabama] and the engineering people at Rocketdyne. NASA did individual post-test analysis, and Rocketdyne did theirs. After every test we looked at all the data, we compared notes to assess any and all anomalies that might have occurred. We built a competency on the NASA side, where we were technically capable of challenging the contractor. People like Otto [K.] Goetz and even J.R. were very competent people and helped to ensure that we were doing the best job possible. There was excellent coordination and rapport with the Rocketdyne engineers and management.

It was a challenging and interesting program. It's been an outstanding success that I would have never guessed we would have achieved this many missions without an engine failure. Quite honestly, when I started working on the engine, I knew very little about it. By the time I left the program, I was very knowledgeable of the engine.

ROSS-NAZZAL: You mentioned cameras. Where you were taking photos so you could actually see what had happened. Where were those located on the test stands?

SMITH: They were on the stand close to the engine. We lost some cameras, too, when the engines blew. We had them located on the stand and in close proximity to the stand, anyplace we could put them where we could see what was happening. It provided invaluable data because we were filming up to 2,000 frames a second and greater, whatever camera speeds we could find with the technology we were using, so we could see exactly where something started to fail. A little puff of smoke coming out of the engine or something like that. It gave us a lot of information, along with the instrumentation, as we tried to improve our ability to monitor the engine tests. As I previously noted, the engine has very limited pressure and temperature measurements so we were constrained and relied heavily on the visual aspects of what was happening.

ROSS-NAZZAL: By the time you started working on the engine, had they closed the facility for testing out in [Santa Susana] California?

SMITH: I think they basically had. They tried to build a facility that would simulate the pumps, and that facility got so complex that they never really used it. It was very expensive to create the conditions in a high-pressure fuel pump or a LOX (liquid oxygen) pump. So, to my knowledge, they never used it.

When I was assigned to the program, we had moved the testing to Stennis [Space Center (SSC), Mississippi], and of course we did some testing at Marshall, but most of the testing was at

Stennis. There might have been a test or two at Santa Susana, but it was being phased out at the time that I got on the program. During the development program we spent a lot of time at Canoga Park [California]. We had NASA teams rotating to California working with the contractor. I was gone a lot, working very long hours, especially as we did the failure investigations and design reviews. Almost all the testing that I was involved in was at Stennis.

ROSS-NAZZAL: Would you tell us about some of the more memorable tests that you took part in out at Stennis Space Center?

SMITH: The most memorable one was the one with the spalled bearings. That was a gutsy call. Again, that's where a project manager earns his pay and J.R. earned his. A memorable one was a test at Stennis. We had the NASA Administrator [Robert A. Frosch] there witnessing an SSME firing. When you fire the engine the combustion process produces water, which in this instance caused a cloud behind the engine as we were testing. The cloud drifted over the building where we were standing on the roof with the administrator, and it started to rain on us. We're all scrambling to get umbrellas on a perfectly clear day, and it's raining on the administrator. Pretty embarrassing when you're not ready for something like that. That was kind of an interesting phenomenon.

What other tests were memorable? They were all memorable in terms of the problems we encountered, and what's going to happen next. Like I say, some of the failures were spectacular, and trying to understand what caused those was always a very interesting exercise.

WRIGHT: Were the investigations done with the team that you had in place, or did you bring in outside people to investigate them?

SMITH: We had outside people that were supporting us and doing related research. We had one professor at Texas A&M [University, College Station] helping us with research, trying to understand the environments affecting the turbopump parts. Dr. Gene [Eugene E.] Covert from MIT [Massachusetts Institute of Technology, Cambridge] was a consultant. He was examining the same kinds of performance parameters that we were to try to help us determine what we might do differently to correct the problems. We drew on any resource we could find, including consulting with some of the aircraft manufacturers, Boeing and certainly Delta, to see what we could learn about their best practices that we could use in terms of materials, inspections, operating restrictions, etc. So we did use experts that we thought could help us, because it was a major engineering challenge.

You've got to understand that the high-pressure fuel pump, in terms of power density (size and the power that's generated) generated 75,000 horsepower and would almost fit under the hood of your car. We were harnessing an enormous amount of power in something not much longer than this table here, not even that wide. It was a huge engineering accomplishment to do that. We faced challenges every day in every test and learning as we went along.

Again, a very successful program, but not meeting the reusability requirements that we really wanted to achieve made the program quite expensive. The Shuttle is an expensive vehicle to operate, but there's never been a vehicle like it.

ROSS-NAZZAL: How did you solve some of the problems that you faced commonly, like subsynchronous whirl or problems with the turbo blades? Can you talk about how some of those issues eventually got resolved?

SMITH: Subsynchronous whirl was a concern for us at one time. We ran tests where the vibration levels got very high and we got into subsynchronous whirl, which could lead to total destruction of the turbopump. We did studies to learn how we could decouple some of the frequencies by making a stiffer shaft, and that's what I recall we ultimately decided to do, to change the frequency of the operation of the pump and eliminate conditions that would tend to drive these frequencies that could damage the pump. That was one of the areas that we really worked with A&M on, the subsynchronous whirl. Dr. Dara W. Childs did a lot of research in frequency response and how to mitigate it.

That was just one of the many problems that we had: bearings, turbine blades, sheet metal. We had sheet metal failures, and we investigated how we were exciting the sheet metal, which was failing under high-cycle vibration. So learning everything we could about the frequency response of the entire system and how they feed on each other was essential. One of the things you've got to make sure is we don't generate some kind of a frequency that affects the vehicle and hence the Orbiter. All of that required major studies, analysis, and tests.

ROSS-NAZZAL: Would you talk to us about the ISTB [Integrated Subsystem Test Bed] and the testing out at Stennis? Can you tell us about why it was conceived at that point and how it was successful?

SMITH: You know, I can't. I'm vague on that, I can't give you anything definitive. I don't remember exactly when we did the ISTB. Maybe some of the other guys could help you, but I can't. It's has been a long time, since I worked on the project and the things I remember were the mistakes I made that we had to fix. Those are vivid. The other things aren't that memorable.

ROSS-NAZZAL: Did you ever feel pressure from Johnson or from Headquarters because the engines were such a pacing item for the Shuttle Program? Was there ever any insistence that you've got to hurry it up or costing too much money?

SMITH: J.R. was the one that felt the pressure, as the program manager, although he dealt with that better than anybody I've ever seen. Yes, we had pressure from our own management that kept looking at failure after failure in this marching schedule to get to flight, and I think that was felt by everybody on the project. I felt it because the major problem on the engine was the thing that I was the deputy chief engineer on (the turbopumps). So at the time we've got all these problems, I'm learning about what the thing looks like and how it operates.

Yes, I felt an enormous amount of pressure personally, simply because it was my project, and we'll talk about that later on the SRB. I think there was a sense throughout the project by all the people that we really have got to learn as much as we can, as quick as we can, so that we can be successful on this program. I think that just permeated the entire organization.

Of course, the Orbiter was off having its own problems, too. We were not worried about the Orbiter, but we knew that we had to be successful in getting the engine to run full-duration missions so we could fly. I think we all felt the pressure of the schedule and certainly the pressure of the technical problems, and that was coming from Headquarters to our management, to the project manager, to all of us to try to solve these problems.

There's a lot of stress that people not having been there will never understand. There really is. It's very rewarding in retrospect as you look back and think, "We did it," but, yes, certainly I felt pressure and I think everyone else did too to varying degrees, depending on the individual. I've seen people that don't appear to be feeling the pressure, but really do, some of them much more obvious than others, no question about it. J.R., as the project manager where it's your project, your responsibility, had to feel that pressure, but you would hardly ever have known it, being around him.

ROSS-NAZZAL: What was the media interest like in the SSME as you were working on the project?

SMITH: Nothing like it was on the SRB later after the *Challenger* accident. The media, I'm sure, followed it. In my role, I had very little interaction with the media. I'm sure getting ready to fly the Shuttle there was media attention being placed on it, but nothing like it was later on the solid rocket booster. I think the media was following the progress of the program, but not that close. They got much closer later.

The other interesting thing, too, was the transition from the Apollo Program to the Shuttle Program, which offered different challenges for project managers versus the Apollo days. Apollo had unlimited funding and a very tight schedule. The staffers in the Congress knew very little about the space program or the vehicle, and so you were able to really do all the things you wanted to do. You had the money, and you could be very successful. Throwing enough money at it, you could really make it happen.

Shuttle comes along, and now staffers think they really know everything there is to know about the program. Therefore, they start setting budget limits, and you're finding all of a sudden you've got a very constrained budget, a very tight schedule, and it became much, much harder to manage. This is my view of management in Apollo versus Shuttle. A very different environment where you didn't have unlimited money and couldn't do all the things you'd like to do. As you were developing the system, the requirements would change, and you had to work smarter. Again, the money wasn't there, so you're really trying to manage a very tight schedule and very tight budgets. It was just an entirely different management challenge and certainly as technically difficult as Apollo.

The Shuttle is just an amazing vehicle and very complex. So you had all the technical challenge that I'm sure the Apollo people had under a much more constrained environment. It made managing much harder with much more accountability. If you overran, everybody was in your knickers, so to speak. Media interest back then I'm sure was there, but not to the extent of *Challenger*. My involvement was less so I really don't know much about how extensively they followed it.

ROSS-NAZZAL: Were there any changes that were made after the first flight of STS-1 to the main engine itself?

SMITH: Yes. After many missions, we went to a different contractor, Pratt & Whitney, to build the turbopumps. A lot of criticism from the Congress and others that the rising costs of this program was because Rocketdyne was not doing a good job of building the hardware and improving it. A decision was made, by NASA Headquarters, to go to a different contractor for the pumps, so we did change contractors to Pratt & Whitney.

We were always changing and improving the engine in subtle ways as we learned from flight and all the testing we were doing. One of the things that J.R. put in place was a fleet leader concept. We had to test an engine on the ground so that it had at least twice the test exposure of any experience we would have on a flight motor. We ran an extensive test program at Stennis, always trying to extend the life of the hardware, so that we never flew something that we had not tested at least twice the duration on the ground. It was a very extensive test program that ran throughout the entire Shuttle Program. Lead the fleet was probably one of the major reasons we were successful on the engine and never had a problem.

We did have a failure prior to a launch. We had a shutdown on the launchpad where a valve did not open and had a shutdown on the pad. A major investigation ensued that was pretty exciting. I was involved in that investigation. We thought the engine controller had caused the valve to malfunction and did extensive testing of the valves and the controller over several months.

I was the deputy program manager at the time, and Bill [William E.] Taylor was the program manager. We tested and tested and couldn't specifically identify the failure. We had to brief the administrator before the next mission and were supposed to explain to him what happened. As it turns out, for reasons not clear in my mind, I had to brief the administrator. The night before, I briefed senior MSFC management and they didn't like my charts. So I was up all night long with Rocketdyne on the phone, redoing all the charts.

The next morning, I got on a plane in Huntsville to fly to Washington. We had to pick up Dr. [William R.] Lucas, the Center Director, who was in Virginia. Dr. Lucas was a taskmaster, technically very competent and very demanding of other people as far as technical competency. We picked him up and I thought, "I'm going to get killed right here," because he got on the plane and I had to brief him. I had to tell him we didn't know what caused the problem. We had a most-probable explanation, which was contamination in an actuator, but we had contaminated actuators and tried to simulate what we thought could have happened and could never get the actuator to fail. So now we're going to Washington to tell the administrator we don't know what happened, and we're getting ready to fly two weeks later.

When we got there, after I had two hours sleep, there were other meetings that lasted all day. About six o'clock in the evening, we were in the administrator's conference room with all the deputy administrators there. It was an audience that I had never previously been exposed to and, needless to say, I was very nervous. The air conditioning at Headquarters had been turned off about five o'clock, and it was getting very warm. There were two instances that we had to present to the administrator; there had been a fire on the pad that Bob [Robert B.] Sieck from KSC was going to have to explain, and I'm to explain what had happened with the SSME.

We start the briefing, and the administrator says, "Tell me what happened."

It was [James M.] Beggs. I said, "Mr. Beggs, we don't know what happened, but we have a most-probable cause and, if I could, I'd like to take you through this presentation."

I could tell he was really irritated. It had been a long day; it's hot. He said, "Okay, go ahead."

I gave the briefing, which was probably an hour and a half long. When finished, I said, "This is the most-probable cause, and this is what we think."

I never will forget this. He looked at his staff, all the deputy administrators, and he said, "Does anybody here have any other ideas?" He looked back in the room, there were a few people in the back, "Any of you have any ideas?" He looked up the ceiling and he says, "Do *you* have any ideas?" I felt a tremendous sense of relief, and his comments broke the tension in the meeting. Because of the heat and the stress of the briefing I was soaking wet.

He accepted the briefing, and we flew a successful mission a couple of weeks later. But talk about stress. I'd flunked the internal review and I had to change the charts, in fact in ways I didn't even like. So I'm briefing charts I really don't care for, but it went fine. That was the first time that I'd ever been exposed to that level of management, and it was a tough time to be there. That was memorable for me.

ROSS-NAZZAL: How did things change once the Shuttle became operational?

SMITH: It didn't change. Everybody said we were we're operational, and it was never that. People kept declaring we're operational, but we were doing the same flight readiness reviews, all the analysis, and continuing to test at Stennis. Really nothing changed. We didn't reduce manpower because we recognized this vehicle was so complex, we have got to stay on top of it. So we never changed that as long as I was on the program. It was just extremely challenging, and we recognized that. There was no difference operationally that I ever recall. We looked at ways to improve processing at the Cape [Canaveral, Florida] reducing the timelines, and as I recall there really wasn't a lot we were able to do.

ROSS-NAZZAL: Did you provide any assistance at the Cape for launch? Was that one of your responsibilities?

SMITH: On the SSME, I was always in the Huntsville Operations Support Center and not at the Cape. We were monitoring all the information from consoles and the chief engineer was at the Cape. On the SRB, I was monitoring the consoles in the Launch Control Center (LCC).

ROSS-NAZZAL: We haven't heard about this Huntsville Operations Support Center before. Can you tell us about that?

SMITH: We had all the consoles set up, monitoring all the systems: the tank, the engine, the boosters. We had a large support group in this Huntsville Operations Support Center that was monitoring what was happening at the Cape. We'd have a planeload of people fly to the Cape and be there at the consoles, but we had the support group in Huntsville. Contractors were tied in during the missions and doing their own monitoring. We had a very elaborate system monitoring the vehicle during the launch.

ROSS-NAZZAL: Did you ever do any sort of sims [simulations] like they might do at Mission Control?

SMITH: Yes, we did sims. The simulations were challenging. There would be one failure after another to challenge the whole team. The sims were very important as to how the teams responded, how they dealt with problems, and I was always astounded at how well each of the subsystems managed the crisis they were faced with. I think the simulations exercised the people and their knowledge of the systems extremely well, so we did those throughout, and that was quite helpful.

It was just a very exciting program. Unfortunately, the young people today at NASA can't ever get a program going and continue it through design, development, and testing. They miss the opportunity to learn so much and you do learn in that environment.

During my tenure on the SSME, the head of Science and Engineering Jim [James E.] Kingsbury decided I needed management experience. I'd not had people working for me that I'd supervised. I was working for the chief engineer on turbo machinery at the time. So Jim decided that I needed management experience and named me as branch chief in the Propulsion Division over combustion devices and turbo machinery. Now all of a sudden I've got a branch of people in propulsion, the same people who had been doing all this heavy involvement in development of the engine and doing all of the data analysis. I was having a great time there, loved it, because it was where most of the action was on the engine.

So I'm having a great time in Science and Engineering, and I get a call from the head of Personnel and he said, "Bob [Robert E.] Lindstrom, the MSFC Shuttle Program manager, would like for you to become the deputy SSME project manager."

I explained to Don Bean, who was head of Human Resources, "I like where I'm at, and I'm not interested."

Three months later he called and said, "Lindstrom would really like for you to come over and be the deputy engine program manager." Again, I told him that I wasn't interested. This went on for several months.

Then I went to the MSFC first-level supervisory training, a management development program at a state park in Guntersville [Lake Guntersville State Park, Alabama]. It was my first exposure to the entire Center organization and what everyone does. I'd been working in Science and Engineering all these years, and I just really didn't have much knowledge of the way programs were run, what programs the Center was involved in, and how administration worked. I was very naïve, even though I'd been at NASA for many years. It was quite an eye-opener for me. We had all the department heads and project heads giving briefings, and I never will forget Bob Lindstrom's presentation. I didn't know Bob Lindstrom, other than who he was.

It reminds me of something on the SSME, regarding Lindstrom. There was a problem during the development program where occasionally we would rotate the pumps to check the torque. The torque had to be low so that during the start transient the pump wouldn't seize up. What could have happened if the fuel pump doesn't come to speed quickly and the oxidizer flows in, and there isn't enough fuel, it can cause a fire. The fuel pump's got to start up at the same time as the oxidizer pump, so we would do a torque test. A mechanic would take a wrench and rotate the fuel pump to check against established limits that said if the torque is greater than some value, we'd pull the pump and check to see what's causing the high torque. It was something we had learned during the test program that could affect the way the engine starts and possibly cause a failure.

We were getting ready to run a test and checked the torque. The fuel pump was a little high, but in my opinion, it was fine. We had enough information for me to believe it's not going to be a problem. But the chief engineer I worked for wanted to pull the pump and inspect it.

There was a meeting with the program office, J.R., and Lindstrom, to review the situation, and my boss, the chief engineer, couldn't go. He sent me to the meeting, and the discussion came around to this particular instance, and Lindstrom asked me what to do. I said, "The S&E [Science and Engineering] position is we should pull the pump."

He said, "What's your opinion?"

I said, "Bob, the Science and Engineering position is we should pull the pump."

He said, "I want to know what you think."

I said, "It's fine. It's okay."

So we tested, and it was okay. But my boss was very upset with me. Even though I explained, "I told him what the S&E position was, what the recommendation was, but he kept asking me mine. I had to tell him, 'This is what I think.'"

There are instances along the way where you get kind of in a bind with your boss and somebody higher up wanting to know what is your opinion. I think Lindstrom respected my opinion because I had worked so long on the project, even though the chief engineer was outstanding too in his own way. Some of the situations you get into, you've got to call it the way you see it, which was something that I had learned along the way, always be honest with people. In my career I had people that misled me, did not tell the truth, and I promised myself I would never do that with people. I've tried never to do that.

Lindstrom gave this briefing at Guntersville. The thing that kept impressing me as people gave presentations—that "I must always know what you're doing, because I've got to talk to the boss, so I've got to know everything," and my chief engineer was like that. Lindstrom gets up and explains what he as the program manager for Shuttle does, with his program managers, J.R. Thompson and George [B.] Hardy (SRB) and the external tank manager Jim [James B.] Odom. "My responsibility is to keep the Center Director informed, but that can be either through me or through my project managers. As long as he's being informed, it's fine. It doesn't all have to come from me."

I thought, "Well, that's refreshing," because he's flowing down responsibility to the project managers. And I thought, I really like that idea. That had impressed me.

Later I'm at the Cape at a Manned Flight Awareness Honoree Award watching a launch, and Lindstrom corners me and says, "I want you to come work for me on the SSME Program."

So I did although at the time I didn't know if I wanted to be in the project office because I liked engineering challenges. I initially worked for Jud [Judson A.] Lovingood, who was the SSME Project manager and really had a great time and enjoyed it. Jud gave me a lot of latitude to follow the testing. Jud had been a laboratory director and was wanting to learn more about running the project, the budgets, and the schedules. He said, "You take care of the technical," which I did, and enjoyed the assignment.

Then Judd moved on to deputy Shuttle Program manager, and at that point in time they separated the SSME Project into two pieces. One was a flight project, the other a development project. Joe [Joseph A.] Lombardo was named to head up the development program. Bill [William F.] Taylor had the flight program, and I became Bill Taylor's deputy. So I worked for Bill for a year or two. Bill was a different kind of manager and wanted to be the face of the project to everyone. Basically I just supported Bill, and somewhere along the way I realized I can't do this. All my responsibilities had been taken away. I was really responsible for nothing as a deputy and decided that being a deputy sucks. It really did.

Finally, Bill and I had a meeting of the minds. I said, "Bill, you're a good guy. Your management style requires certain things from a deputy that I'm not, and I can't do this."

So Lindstrom moved me, and at that point in time I was named deputy associate director of Engineering, my first SES [senior executive service] position. That was a great job because it overviewed all projects at Marshall, and I was learning more about all these other little projects and big projects. I was familiar with Shuttle, but the Hubble Space Telescope and other projects the Center was doing were just exciting. All the chief engineers reported into that office. [J.] Wayne Littles was my boss at the time, and I was having a great time.

Approximately six months later, I was named to lead a Source Evaluation Board for TDRSS (Tracking and Data Relay Satellite System). Now I'm off on an SEB, cloistered in a building out in test area for months. We got through most of the SEB activity, and at the time there were overtures coming from Headquarters for me to go to Washington and be the project manager for the SSME in Washington Headquarters. I was not interested since I had been to Washington many times, and I had no desire to go. I kept being quizzed by Center management and Jim Kingsbury. They'd keep asking, "What are you telling them?"

I said, "Jim, I don't want to go. It's nothing I'm doing."

But they kept requesting the Center assign me to Headquarters, and I kept being told, "You're not going," which was fine with me.

As I'm working on this Source Evaluation Board, I get a call to Dr. Lucas' office. He'd had a bad situation occurring in a meeting with the administrator about a problem we had on the engine. There was a lot of misinformation going around, and he either got embarrassed or he was upset that nobody in Washington seemed to know what was going on. He said, "I want you to go up there and head up that program," which I did.

I was in Washington as director of the SSME Project in the Office of Space Flight. It was a good experience because I got to see kind of the problems that Headquarters deals with and had a greater appreciation for what they did. It was an excellent experience for me to get to work in Washington.

I was there at the time of the *Challenger* accident. The day it happened, I was in the conference room watching the launch, and as soon as I saw the explosion, I said, "We've lost it." I watched it for no more than four or five minutes, walked out of there into my office, turned off the lights. I was convinced the engine had caused the failure, and I was asking myself all day long if maybe something I had done, some decision I had made, caused the accident, and it gave me a major problem for a long time.

I sat there all day and went home. There was a blackout of Marshall Space Flight Center to the media and Center officials would not talk to them or me. Even though I was a Marshall employee, they wouldn't even talk to me. I called Lindstrom and said, "Bob, we're getting all kinds of media requests, and I don't know what's happened. I don't know what's going on. Can you tell me something?" He was under blackout orders like everybody else and couldn't talk to me.

So my boss, the division chief, says, "You've got to go talk to the press."

I said, "I don't know anything."

He said, "Well, you have to talk to them anyway."

So I'm talking to the *New York Times*, people like this, with no knowledge of what happened. They had followed the progression of the SSME development, so they're quizzing me about turbine blades, bearings, etc. I explained, "I have no idea what caused the failure, and I don't know if the engine was involved. Anything I say is just historical information, and it has no bearing on what happened, because I don't know." That was very hard talking to the media, and they asked question after question. When I had no knowledge what had happened. I was upset at the Center that I was put in this position of talking to the media.

Then we learned it was the booster. I had spent a year in Washington, and the associate administrator for Space Flight, Jesse [W.] Moore, had asked the Center to extend me at least six months maybe longer to go through another budget-cycle review with the administrator. I was on that extension when the *Challenger* occurred. But the Center wanted me to come back to work on the redesign of the booster, so I came back to the Center. Jim Kingsbury, the director of Science and Engineering, was leading the redesign team at Marshall. We had a redesign team at Thiokol and a redesign team at Marshall, working pretty much independently to come up with ideas about what we should do to the motor. I worked for Jim Kingsbury for quite a while. Then later they named John [W.] Thomas to head the redesign team, and about that time I was asked to take over the SRB Project from Larry [Lawrence] Mulloy, because Larry was going to be leaving the agency.

I might add there was no way I wanted to be the SRB manager. I thought, "I do *not* need this." I had been led to believe that I was going to be offered a position in Science and Engineering. When I came back though, after the accident, I spoke to Dr. Lucas and said, "You know, I feel terrible about what's happened." Morale at the Center and at Thiokol was awful. "If there is anything I can do to help, I'll do it." Well, that was a mistake. I was called into Dr. Lucas' office, and he asked me to head up the SRB Project. Again, he asked in a way that you don't say no, and I thought, "I don't need this."

So I took over the job. Larry and I overlapped for about a month, and then he left so now I'm managing the SRB Project. Other than the small project on the booster separation motor, I had never really managed a project, certainly not of this magnitude. I felt that MSFC was making a mistake by naming me, since I felt NASA should have the best manager the agency had to manage that project because of the criticality of the project to the Shuttle Program.

About that time, J.R. was named Center Director and Dr. Lucas retired. I'd worked with J.R. for years, and his new assignment was the best thing that ever happened to the Center and to me personally. I had great admiration for him since I'd worked for him on Skylab. I wanted him as Center Director since I now had to lead the SRB Project and the SRB redesign. In fact, I bugged him constantly to leave Princeton [University, Princeton, New Jersey] and come to Marshall. He once called and told me, "Gerald, don't you have anything to do but call me and ask me to take the job?"

The media was contacting me all the time. I never will forget Kathy Sawyer, a *Washington Post* writer, a very knowledgeable lady and one very critical of NASA. Somewhere in the redesign effort Kathy called and she was following up a lead for a story which was totally wrong. I don't remember what the subject was, but she had bad information. I said, "Kathy, that's not right. Here's what I know, and I'm reasonably certain that I'm correct." I told her what the real story was, and I said, "Kathy, let me tell you something, here's my home number and my cell phone. You call me anytime day or night, and I will give you the correct information." She seemed startled with that. After that she never called, and I never had a problem and really no negative articles. I just said, "Look, anything you hear, call me." That was the rapport I tried to build with the local newspapers and media outlets.

Interestingly enough, I had an assistant who came out of Public Affairs, and up until J.R. came, you could not meet with the media unless someone from the Center staff was there. You didn't talk to the media at all without them present. J.R. came along and changed the policy to allow me and others to talk to the media which I thought was reflective of a Lindstrom philosophy, which puts responsibility on the project manager, and you didn't have Big Brother looking over your head.

I would meet with the media and my assistant would say, "You talk too much, you just answer the questions. Don't talk so much." She'd debrief me after every one of these. Same information each time, "You talk too much, and I'm going to send you to charm school (a media relations course)." They scheduled me time and again to go and learn how to interact with the media, and I never had time to do it so I never got training. I'm sure that most of what I did was wrong, but I thought, "This is crazy. Why can't you be honest and open with people?" Even though sometimes an article would come out and it would totally be in error in terms of what I said versus the way the person either heard it or the emphasis they wanted to place on something. I struggled with that, but that's what you get dealing with the media.

WRIGHT: Were you able to provide real information for them, or were you constantly doing damage control?

SMITH: It was both. A lot of times it would be information, because I built a pretty good rapport and they would talk to me. Then a lot of times it was damage control, because there were just a lot of rumors flying around. We had hired people, job shoppers that at least in one instance started making fallacious claims about things that Thiokol was doing and covering up. We had all of that. In fact, we even had an FBI [Federal Bureau of Investigation] investigation in a situation where we had some damaged O-rings that had gotten packaged, and we were trying to find the source. We were even being criticized that we were not cooperating with the FBI, and I had one of my people assigned to the FBI working with them constantly. I went to the FBI, and I said, "Look, tell the media that we're cooperating." They refused and wouldn't talk to the media. We went through many investigations from the Inspector General's office and the GAO [Government Accountability Office].

On the SRM Project, we were doing things in parallel. We were building development motors and flight motors at the same time. Normally it's sequential. You design the hardware, you build the development hardware, you test it, you qualify it, then you build the flight motors. Well, we were doing both concurrently, taking a huge risk that the design wouldn't change to the point that we would have to scrap all these motors we're building for flight.

Again, it was a very tight schedule. We worked a three-shift-a-day, seven-day-a-week schedule for two years at Thiokol. We were building test stands at Thiokol and test facilities at MSFC to test joints and nozzles. These were major facilities, all being built concurrently with doing the redesign.

I had a project team at Thiokol led by Royce [E.] Mitchell. John Thomas was at Thiokol assigned by the Center Director's office. We also had the National Research Council [NRC], Dr. [H. Guyford] Stever, providing oversight. So I had a lot of help managing my project. Every morning on Monday, I met with J.R. at six-thirty in the morning in his office, and they were very interesting sessions where we would meet for about an hour and he wanted to know what's going on and how I was going to solve the myriad of problems I was facing. But part of it was a learning process, I think he was teaching in a lot of ways. The other was he wanted to know, "Do you need any help from anybody?" And I'd tell him.

[Robert J.] Schwinghamer is a classic example of providing help to my project. Schwinghamer is another character, brilliant guy, very opinionated. I had great respect for him, but he didn't want any one else managing his lab. Not that I wanted to manage the lab, but I'd have a situation where I might ask for some materials person to be sent out to Thiokol, and I'd ask by name. Well, he didn't like that. He said, "You just tell me your requirement. I'll figure out who to do it." I'd had a few meetings with him, and in one meeting with J.R. I told him, "I need this from Schwinghamer." I leave, and I'm waiting, sitting at my phone, knowing the phone's going to ring. Schwinghamer called and was very upset, because he's having to send somebody out there and it wasn't his decision. J.R. had told him. It was one of the many ways J.R. helped me.

Then there was the NRC Stever Committee that provided oversight and reported to the NASA Administrator. I was always amazed that Guy could take these guys, all very opinionated, very bright, from throughout industry and manage them in such a way that they could come up with a recommendation of the majority of the members. They were a great help because they did provide a lot of oversight, really challenged us, and made us work very hard to answer their questions. They also imposed requirements above and beyond things we had planned, which gave me budget problems. Each year when I established my budget, I never knew what they would ask for, maybe additional testing or analysis, so I was always fighting a budget problem managing that project, driven by so many people providing oversight. It was quite an experience, and I had great respect for that committee and their contribution to the success of the motor redesign.

We had an extensive test program, eight full-scale motors, which was more than the original Solid Rocket Motor Project. We built a full scale and subscale test facilities to assess joint operation and conducted extensive materials testing of O-ring materials and insulation. In the latter part of the test program certain engineering managers wanted to test a full scale motor with flaws since we had redundancy throughout the motor, and they wanted to go in and flaw the motor in ways beyond anything we would ever build.

I disagreed since we would be creating flaws that would be easily detected during the manufacturing and development process and significantly risk a motor failure. Therefore I was adamantly opposed to the idea. "I don't want to do this," but S&E was pushing me to do it. I thought this was overkill. If we blow this motor up because of flawing it, first of all, I'll be fired—I'm the project manager—even though I didn't want to do it. So my first thought, "Well, my career will end here," and secondly a failure will set the Shuttle Program back again years. We just couldn't afford to do that over what I was felt was totally unrealistic tests.

I lost the argument. We did it, and the result is that you have ultimate confidence in this motor, because we have tested something far worse than anything we will ever build and would not find in the assembly and inspection process. It gave me great confidence, "the motor's fine," although I didn't need that. I was already convinced that we had fixed the motor problem with the redesign.

We built a building to go over the motor so we could condition it to the worst case hot and cold conditions at KSC. This had never been done in the original certification. We had certified it by analysis. We were wrong because the joint opened rather than closed and allowed the O-rings to leak, and that's what caused the accident. We tested at 120 degrees and to do that you've got to condition the motor for about 30 days because there are tons of propellant in the motor. It takes a long time for the temperature of the propellant to reach that desired temperature of 120 degrees. The same was also true at the minimum temperature conditions.

As a part of the redesign effort, we reviewed every requirement that existed for the motor and how we verified the requirement. We had to show that every one of these requirements was traceable to either tests or analysis to make sure that we had verified the requirement. This was a major activity that disclosed the many areas in the original certification we had not done. Testing hot and cold were examples.

The redesign effort included a failure modes and effects analysis, which assessed how can this part or subsystem system fail? The analysis identified all the ways the part/subsystem might fail and what can we do to mitigate it. Solutions might include providing redundancy, changing the design, or doing a more thorough analysis and making sure that we've identified all Crit-1 [Criticality-1] failure modes, which says it could be catastrophic. It was essential we protected against all the failure modes. In addition, we performed hazards analyses, which was top-down, looking at what hazards the design might generate and how to mitigate them. It was an enormous task for both the contractors and MSFC.

The design process included multiple reviews including requirements, preliminary design, critical design, and then a certification review at the end for all the different systems. The reviews consisted of engineers reading a document or examining a drawing and if they saw a discrepancy, it is written up as a review item discrepancy [RID]. The RID has to be got to be cleared or closed before we can complete the review. It's in essence a concern that somebody has raised, either the contractor or the engineers at MSFC.

Needless to say, the engineering community post-*Challenger* was very risk-averse. If it was something that they had any responsibility for, they would write it up. As a consequence there were thousands of RIDs generated in these reviews. So we're months away from flying, and we have had six to seven thousand review item discrepancies written, some of them multiple pages. I had a room set aside to house the books of all of these RIDs that had to be closed. The contractor had to respond to the RID and try to close it, and sometimes it cycled three times through to try to get a resolution to that. When we couldn't get resolutions, the chief engineer and I would sit in my office and using our best judgment would disposition the RID because I couldn't get Engineering to sign off.

In one of the Monday meetings with J.R. he asked, "What are you doing about those 6,000 RIDs?"

I said, "I'm working on it."

He said, "Well, what are you doing?"

I said, "Let me explain something. Those six thousand RIDs were written by Science and Engineering, and the last time I looked they worked for you. I'm not generating these, but I've got the task of closing them. I'm trying to, but I wish you would lean on Engineering and say quit writing these things, because it's killing us." It was a huge task, and we were trying to get ready for the flight certification review at KSC. We successfully closed the RIDs and then we got ready to fly. We had gone through this extensive testing and evaluation to get there.

SRB side—when I talk about SRB, I'm talking about the aft skirt, the separation system, the nose frustum, and the recovery parachute system. In a similar way, we had to do the design changes, testing, and evaluation on the motor project. Thiokol was the motor contractor, and USBI [United Space Boosters, Inc.] was the contractor for the SRB. USBI was located both at Huntsville and at KSC. We had to do the same thing on the booster side, relative to the review of documentation. Interestingly enough, the SRB had been an in-house MSFC design.

We learned a great lesson from this; NASA is not a good designer. We did the design and handed it over to McDonnell Douglas to produce on a fixed-price contract. NASA Headquarters had decided that "We're going to quit doing cost-plus contracts because it cost us a lot of money. We're going to fix-price this project." So we went into a fixed-price contract, and they ate us alive with changes, because, first of all, we had not done a good job at documenting the design. As a consequence we had major cost overruns on the booster.

Then once we were getting the hardware manufactured, USBI won a contract for the booster. They were a part of United Technologies. We went through all the same kind of paperwork verification, testing, all the same things that we did on the motor for the booster. There were many design changes, I can't even think of all of them. We did major redesign and testing of the separation system, the separation bolts, igniter systems, and then we had to do design and develop a flight instrumentation package. For the first three or four flights we had extensive instrumentation on the booster to better define the environment and to better understand its operation, and we had not done that originally in the Shuttle Program. So we were faced with installing a lot of instrumentation to define the environments for the boosters which included the separation sequence and the extensive use of cameras.

We were always dealing with a debris problem that ultimately caused the second failure on *Columbia* [STS-107 accident] where the insulation came off the external tank. The program had always been getting tile damage. Based on complex flow analyses the ET [external tank] Project, had rationalized that nothing big enough to really cause a problem will come off. And there was always finger pointing, "It's coming off the booster; it's coming off the external tank." We were convinced it wasn't coming off the booster, and the tank people felt that "It's not our insulation." We were always going through that and continued to assess ways we could mitigate any debris coming off the motor. It was a major focus area.

When we got ready to fly, we had been identified as the tall pole in the program, at least we thought. In fact the Orbiter was not ready to fly before we were, but they were under our umbrella. Had we been able to fly two months earlier they couldn't have, so we got there about the same time. For a project manager, this entire effort was very rewarding, and I'm glad I did it, but would never want to do it again. I was working probably between 70 and 80 hours a week for two years until we flew. It took a toll on the family and on me physically.

There was a question asked about crew involvement. We had extensive astronaut involvement during the redesign. They attended all of our design reviews and were in my office quite a bit. They monitored the design effort, knew how it worked, and knew how we got to a final design both motor and boosters. They were very inquisitive, including the flight crew. Rick [Frederick H.] Hauck reminded me, "We're depending on you," and I told him I understood that and my people understood it. When I went to work on the SRB I had a picture of the flight crew on *Challenger*, "Lest We Forget" under it, and that's what would motivate me on weekends and at night. That would keep me going, and we all felt that way. I kept that as a reminder when I'd get tired. We can't let it happen again. By the time we flew three or four times, I was burned out. Fortunately for me, although I didn't want to do it, I was transferred to Stennis as the deputy director.

WRIGHT: What was the biggest challenge that you had on the redesigned SRM [solid rocket motor] and then redesigning the SRB? Is there one challenge you can point to?

SMITH: Initially the morale was terrible at MSFC and at Thiokol. They were both being blamed for the accident so the morale was really, really bad. The way I dealt with my own team and with Thiokol is I tried to build one team, because there was a lot of finger-pointing. NASA was blaming Thiokol; Thiokol was blaming NASA for putting pressure on them to fly.

We had new faces, a new motor project manager, Royce Mitchell, with no propulsion background. We had new people at Thiokol leading the Thiokol effort, even though several of them had solid rocket motor experience. So basically you had a Marshall team with very little solid rocket motor experience. I had the most, coming from the booster separation world, but essentially we had no propulsion experience. Trying to get the people to believe in themselves again and stop the "blame game" was difficult. I constantly focused on our challenge to build the best solid rocket motor that's ever been built and getting them to believe that was a major challenge. It took a long time.

The other thing that I would do is at every meeting with my staff and the people that worked for me, when they would start being critical of Thiokol I'd say, "Wait a minute, let me explain something, we're one team. This is not us and them, it's us, all of us, Thiokol and the project office." I had to really deal with that and keep reminding people, "This is one team, it's the only way we'll be successful." That was another major challenge.

After the *Challenger* accident, a deputy administrator for Safety and Mission Assurance was established. The big push then was the independence of the Quality people from the project, which I had a problem with. Not the independence, but I had Quality people sitting down in another building critiquing what we're doing, and all I got was notes, "This is a problem; this is a problem."

I said, "Wait a minute. I want you helping me, I want you to identify the problem, and I want you to help me solve it." I lobbied for a long time and finally got the head of Quality on the SRB Project as a part of my team co-located in the project office because I wanted him there. I wanted him to know what we were doing. I didn't want to put any pressure on him, but I wanted him to be a part of not only identifying problems but helping me solve them. So we were able to change the organization, get him on my team and it worked great, improved the communication flow. That was a major change and a significant help to me in trying to run the program.

The magnitude of the task was a major challenge—seven days a week, three shifts a day—it was just an incredible pace. Every time I'd show J.R. a schedule, he'd say, "You've got a little contingency here, take it out."

I said, "I've got to have some contingency, J.R., or I will never be able to do the schedule."

He said, "I know it, but we're going to do seven days a week, three shifts a day because we'll get there faster that way. If I give you contingency you'll use it all up." That was his philosophy.

As a project manager, you normally measure yourself against, "How am I doing on schedule?" I had an impossible schedule, and we couldn't do it. Somewhere through all this I'm in his office, and I said, "You know, J.R., I don't know whether I should get a grade of C, F, or A. I have no idea what my performance is as a project manager, because I'm overrun on my budget because I've got requirements proliferation and behind schedule with no contingency. Things I hadn't planned for were flowing in from the NRC and JSC, Level Two, driving a lot of requirements." It's hard to understand, are you doing good, bad, or otherwise? In retrospect, I never did know.

Since he didn't fire me, I assumed I was doing okay. You never get a feedback from him, and he'd never say you're doing a good job or whatever. It's just "Go do your damn job." I'm in there complaining; he's telling me, "Go do your job." That was difficult, to be working that hard and having no idea how you should grade your report card. I always felt that probably a better program manager might have managed better and done it better, but who knows? That was a challenge.

We struggled with our contractor management. John [D.] Thirkill was managing the program at Thiokol and his attitude was, "We're the experts. Just send us the money and we'll take care of everything." I had trouble explaining to John, "It doesn't work that way. We're going to be in your knickers making sure that we agree with what's going on, and that we feel the money's being properly spent." We had a rather strained relationship throughout much of the time.

There were many issues that we had to deal with, and pressure on the people out there working day and night made it difficult. When somebody would get fired at Thiokol for whatever the reason, a lot of times we'd get feedback that we had asked the contractor to fire them, which was never true. We had those kind of relationship problems, and it took a long time to build a good working relationship and get past the morale problem that we had.

Budget was a major problem that I had to contend with. We were always in an overrun situation trying to anticipate what are the right set of requirements that we should budget for. Arnie [Arnold D.] Aldrich and I had a lot of discussions about my budget problems.

WRIGHT: You mentioned you had a six-thirty a.m. Monday morning meeting with J.R. Thompson, and you just mentioned Arnie Aldrich. Did you have specific times that you had to report to all of those people?

SMITH: Dick [Richard H.] Kohrs was in charge of JSC Shuttle engineering, so Dick and I gave a lot of briefings and presentations to Level Two and to Arnie at NASA Headquarters. I'm probably not one of Arnie's favorite people, because he always had this budget problem that I introduced early in the program as a result of underestimating the extent of the design and documentation requirements. I used up most of his reserve just on my program, and he wanted it for other places, so Arnie Aldrich would probably not grade my report card high as a project manager.

As a project manager the one thing I did not do well, along with others, was a closer communication with Level Two at JSC. I liked the people, it wasn't that, it's just that I had a job to do and I had my head down trying to get the job done. When I'd need to ask for money for some new requirement out of Level Two, I would hit him cold with it, and that didn't go down well. As I looked back and observed other people like the JSC projects, they were always talking to Arnie and Dick. They were always in communication, and they would get funded and I wouldn't. I was perplexed by that because I thought, "I have a real need for this money."

I think that I could have done a far better job keeping that communication loop open with Dick and with Arnie. I was communicating with the Center and even Headquarters, but I wasn't doing nearly the job with JSC that I should have. That's just a side comment that as I look back over my career, that's one of the things I could have done better. I'm really not a great networker. Give me a job and let me go do it, and I'll do it the best I can. One of the things I tell my sons is that networking is really important.

WRIGHT: I guess your report card finally came with the launch of STS-26. Will you tell us about preparing for that flight and then being at the Cape and witnessing the launch?

SMITH: As you know we redesigned the motor segment joints, and we put heaters around the SRB to keep them warm so that we wouldn't get into a cold resiliency problem with the O-rings. We made sure that the temperature was controlled. It was either STS-26 or a subsequent launch that we had a short in one of the wires on the heater. I'd gone to bed about midnight because I was working an SRB problem at the Cape with the USBI people. When we'd gotten that resolved, I went to bed. I got a call from Bob Sieck, the launch director, indicating we've got a problem. The problem was a heater short, and the concern was it might have pitted the motor case where the short occurred. We tried to assess the problem knowing we were getting ready to launch. All night long we had the engineers at Marshall and Thiokol trying to do analysis to see whether or not there was enough energy to pit the motor case. Number one, could that pit be such that once you pressurize the motor, it would fail? The analysis and debate went back and forth all night.

Sieck walks in and says to me, "Gerald, we're about to wake the crew, and I don't want to wake them if we're not going to be able to fly today. What do you think?"

I said, "Bob, I believe when all the dust settles we're going to solve this problem, so go ahead and wake the crew," which I was really taking a risk because I wasn't sure we could solve the problem. We did clear the problem before the launch and had a successful launch.

I don't recall whether that was STS-26 or a later flight, but that was pretty nerve-wracking. I had total confidence in the motor working, but the thing that kept me awake at night was both motors had to light simultaneously because if one lights and one doesn't, it's a bad day. We had no way for the crew to escape, and you're just going to tumble the vehicle. My thought going to sleep at night was, "Both motors have to light."

In an earlier test as we were doing our development test program, we commanded ignition and the motor didn't fire, so we were standing there stunned. We learned later that design changes to the test stand caused a voltage drop such that there wasn't enough energy to charge the motor igniter and fire. We made sure that a similar condition couldn't exist on the flight motor. Having had that experience and knowing that if both motors don't light at the same time it is a very bad day, and I'd go to bed at night praying that both of them would fire at the same time.

On STS-26 I'm sitting at the console in the LCC, and the contractor program manager is sitting beside me. As we get motor ignition I'm looking out and I see the motor start to light, but there's no pressure on the gauges we're looking at. We're sitting there and it's two seconds, three seconds and I'm watching and it looks like it's fired, but there's no pressure reading on the console. The contractor program manager grabbed my leg and said, "Gerald, we didn't get ignition" even though I was looking at it. That was pretty nerve-wracking.

Later we learned that there was a time delay of about four seconds in the update of our screen and what was actually happening on the motor, but that was a pretty tense time. You literally hold your breath for about two minutes until separation, and I just about did. Because of the *Challenger* and the vivid memories of the accident, you just want to get through SRB separation, which we did, and the flight was successful.

I remember I got a call, while still sitting at the console, from a local reporter from the *Huntsville Times*. He was a young reporter that I had never built a rapport with. He always thought we were not totally telling the truth, no matter what I told him. He was just one of those investigative reporters that thinks, "Somewhere, someplace there's a story here, and I'm not getting it." So, he calls me and says, "I think I saw a puff of smoke or something coming out of the booster. What about it?"

Of course, I had not seen anything other than the launch and the console data. It really annoyed me. I chewed him out and told him, "Can't we once just sit back and enjoy the fact that we had a successful flight for a few minutes before we start delving into what could have gone wrong, what might have gone wrong?" I was pretty upset with him.

It was a great day, because I knew Rick and the entire crew. They'd been out several times and I'd met with them. In fact, later I got a Silver Snoopy from them. I valued very highly the recognition coming from the crew. Rick and his crew were gutsy people to climb aboard *Discovery* after the *Challenger* accident. The astronauts were my heroes. During my career I've worked for [Richard H.] Truly at Georgia Tech Research Institute [Atlanta, Georgia] and later for Crip [Robert L.] Crippen at Thiokol, and of course, I've known many of the astronaut corps members through our working relationship and monitoring the SRB Project.

Every launch you kind of hold your breath. It never gets old, ever. I was at many launches before I finally went to Stennis, and then I followed the launches at Stennis. The director at SSC, Roy [S.] Estess, and I would alternate going to all the launches and sitting in the flight readiness reviews, and it was always exciting.

ROSS-NAZZAL: For people who aren't from Marshall, if you could differentiate between Science and Engineering, that directorate, and then the difference between the program offices, the relationships between those two organizations.

SMITH: Basically you have a matrix structure where there is a project/program office, but you're being supported by Engineering, Safety/Quality, and other organizations matrixed in to support the program office. As opposed to a program office that's has all the Engineering and support personnel reporting to the project manager, these people reported in to different organizations but worked for the program manager.

My chief engineer and all the engineers down in the laboratories—Propulsion, Structural, Electrical—all worked for the director of Science and Engineering, but they were matrixed to my program. You'd never know the difference. They worked for me just as if they reported to me. It's a matrixed structure that worked very well.

One reason it worked very well was Bob Lindstrom was the MSFC Shuttle Program manager. Jim Kingsbury, Director of S&E, and Lindstrom had been at Redstone Arsenal [Huntsville, Alabama] as enlisted men in the Army early in their careers and had worked together and were good friends. So you've got an excellent relationship at the top, which is important. They knew their responsibilities and they supported each other as well.

At one time during the development of the SSME we were having so many failures that J.R. moved his office over into our building where propulsion engineering was located. He co-located with us to further improve the communications, which I thought was a great move. I always felt that it was the better management configuration, where you're co-located together so that that there's no reason to say, "You're not communicating," versus being located in different buildings. It worked very well for us to have J.R. co-located with us

ROSS-NAZZAL: You've mentioned many of the people that you worked with—Bob Lindstrom, J.R., Kingsbury. What impact did their leadership have on the Space Shuttle Program and its successes?

SMITH: In my opinion, Lindstrom, with his project team of Jim Odom, George Hardy, and J.R., had the finest project team that Marshall's ever had. I don't think any of us measured up to those guys. That's not to take anything away from other people, but Lindstrom and those three guys were outstanding in terms of their ability to deal with the myriad of problems in developing the systems, the tank, the SRB, and the engines.

Lindstrom was a manager's manager. That was one thing I learned when I was being asked by Lindstrom to take over as deputy manager for the SSME. I called J.R., Jim Odom, and George Hardy, just to get their views of really whether I should take the job. One of the constant feedbacks that I got from those guys was Lindstrom is really a project manager's manager. He lets project managers do their job. He's not totally hands-off, I don't want to imply that, but he really delegates and lets people do their job. He had three very, very strong managers, and I think they were the key to the success of the program, no question. There was very good engineering support, but I think their management skills were just outstanding. They set the example for me and my peers.

At one time I tried to emulate J.R. as a manager, and I gave up. I thought, there's no way, because in some ways we're the same, but we're very different. You try to pick the best attributes of each manager since they were all different, but all very good at what they did. I think the Shuttle Program was blessed to have those four people at one time during the development of this program.

ROSS-NAZZAL: You talked a little bit about your relationship with Arnie Aldrich. Could you give us a sense of the relationship between Marshall and JSC during the Shuttle Program, from your perspective?

SMITH: Early on, Bob [Robert F.] Thompson was head of the Shuttle Program at JSC. I think management got along well. I think there's always been—how do I say this—a culture at Marshall that felt that we were more technically competent than the people at JSC. We were more hands-on because of the nature of the hardware development programs. I don't think that feeling persisted at the working level. The people at JSC that were doing our separation analysis did a great job, and we had a great rapport. I think there was some friction at the senior level, maybe even between the Center Directors between JSC and Marshall, but I think at the working level we didn't have a problem.

The friction that Arnie and I had was strictly early on. I presented a huge budget problem to him that persisted for a long time, and so I think he was frustrated with that. But otherwise Arnie, Dick Kohrs, and the other JSC people I interfaced with were excellent. I don't know if there was some jealousy between the two Centers, but if there was it was more at the senior level. I did detect a certain degree of arrogance on the part of Marshall when I was in Washington. Certain senior management had the attitude, "Headquarters, send me the money, and we'll take care of it." They did not communicate well with Headquarters, and I suspect that was true with JSC.

This might be explained by the early competition between Centers as they were evolving. We had [Wernher] von Braun and JSC had their leaders, KSC theirs. It's normal competition between very strong leaders during the Apollo Program.

ROSS-NAZZAL: You mentioned working at NASA Headquarters for a while representing the SSME. What were your duties up there and how did they differ from working at Marshall?

SMITH: As the Headquarters SSME Project manager I tried to stay abreast of what was happening with the test program because I would have to brief the associate administrator, Jesse Moore, and the NASA administrator. So a primary responsibility was the communication and staying on top of the program. I had good relationships with the Marshall people since I had worked with them on the engine program. Since I was the engine expert at Headquarters, if there was a question they would come to me. Also if somebody had to brief the media about the SSME I was assigned that task.

Another responsibility was presenting the budget in reviews with the administrator. I recall the chief financial officer and I got into a debate in front of Beggs (the administrator) about the SSME budget. Knowing the program really well enabled me to defend the budget, and Beggs enjoyed the interchange between me and his chief financial officer.

Jesse Moore, the associate administrator, had asked that I extend several months beyond my one year assignment to go through another budget cycle. By virtue of my knowledge of the program, I could represent to the people at Headquarters the SSME Program and why it was important, why we needed the money. That was my principal responsibility, to be the resident expert on the engine, so if there was any question they could come and talk to me.

An example of an issue I had to deal with was the Stennis Space Center. MSFC had always used Stennis as the test site for the SSME, but kept the money and we gave them money and a budget. MSFC gave SSC the money to support the facilities and do the testing. Stennis wanted to be more independent and get that part of the SSME and then manage it, and Marshall did *not* want to give it to them. I happened to be at Headquarters at the time, and I remember Jess Moore called me in and wanted to know what I thought about it. I said, "I think we should give them the money, because they should have the responsibility for how they manage their facilities and their contractors that do the testing," and he did.

Dr. Lucas was very upset with me about that, and some of the people in the engine program office were very unhappy. They thought, "We just lost a part of the budget," but it really belonged at Stennis. So that's how the associate administrator would use me as a sounding board, "What do you think?" That was my function at Headquarters, and that's the reason they transferred me there. They needed somebody that knew the engine program well enough to defend it.

It was an excellent learning experience to deal with congressional staffers in a way that I had not previously done. It took me a long time to accept that certain decisions are made politically. We were trying to get an engine test bed at Marshall for testing technology and to modify a stand to test the SSME. It was in the MSFC capital budget, and Senator [John C.] Stennis stopped it because he wanted all the SSME testing be done at Stennis. I met with his staffer and explained that this doesn't make sense. Marshall, with all their expertise, should be responsible for propulsion technology. Stennis should be responsible for the development testing.

He said, "You don't understand. You've got to put together a story to show how Stennis benefits from Marshall having this test bed."

So I had to put together a different presentation entirely and explain to the staffer that as MSFC develops new propulsion technology, new propulsion systems, Stennis would be the beneficiary because the development and flight testing would be done there. They agreed, and we were able to release the money. These were the kind of issues I dealt with at Headquarters. I had to learn that in some instances there had to be some political advantage to another state or another constituency. As an engineer I was taught to use logic whereas they weren't interested, they were interested in "How does it benefit my state?" It was quite a learning experience.

After the *Challenger* accident, J.R. was asked to lead the failure investigation at KSC and to take a leave of absence from Princeton. Dick Truly had convinced J.R. to take the job, and then J.R. asked for me to go with him and assist in the investigation. As we were doing the early failure investigation we were starting to brief the Rogers Commission [Presidential Commission on the Space Shuttle *Challenger* Accident] about the investigation.

Another little insight into J.R. He'd want to get up at four o'clock in the morning and do wheels-up at four, and then we'd ride all over the Cocoa Beach area trying to find a restaurant that served breakfast. So I had this brilliant idea, why don't we get up at six o'clock because the restaurants will be open and then we can eat breakfast and can go to work. Just one of the things that was constantly playing out between me and J.R.

I subsequently went back to Headquarters, and that's when MSFC asked that I be assigned back to the Center. I've always felt that one reason, and probably the only reason, that they picked me as the SRB Project manager was not because of my wealth of experience or demonstrated capabilities, but many of the candidates at Marshall had been implicated by their participation in the telecon the night before the *Challenger* launch. That cast a cloud over a lot of very capable people that thought they were making the right decision.

So you rule that group out, and you had to have somebody that was acceptable to Headquarters. This was no small assignment, putting a project manager over the SRB redesign; this was a major assignment, and they had to have somebody acceptable to Headquarters. Well I worked there, knew the associate administrator, and had worked for him. So when you stack all these cards, I think Marshall didn't have a lot of options for picking somebody. It's my own opinion that I got the job—not by default, but I think all of those things interplay into what happens to you in your career. I just happened to be at Headquarters at the time of the accident, happened to know the people at Headquarters, was probably acceptable to them, and had a strong a propulsion background. All of these things played together in an interesting way that as you look back, if it hadn't been for that it wouldn't have happened.

Luck is key, in my opinion, to the success of many people, because there are so many hardworking, very bright people out there. I keep telling my sons, "You are not going to be the brightest person in your field, so you've got to work hard to achieve your goals." That's what I did, and being in the right place at the right time made the difference. I had a great career.

WRIGHT: Can you just share with us for a few minutes about your transition to ATK [Alliant Techsystems, Inc.] and Thiokol?

SMITH: After five years at Stennis I decided to retire, and I called J.R., Jim Odom, and Bob Lindstrom about what I should do. They said, "If you're going to retire from NASA and continue to work, retire at 55, because then you're more marketable." So I retired without a job.

A few companies were talking to me, but I got a call from Admiral Truly to come work for him at GTRI (Georgia Tech Research Institute). I accepted and I worked there for about two and a half years. Richard (Dick) was a great guy and an excellent manager, and GTRI was a fascinating place to work because of the diversity of the research and I thoroughly enjoyed it. Dick left to become Director at the National Renewable Energy Lab in Boulder, Colorado.

Shortly afterwards I got a call from Bob Crippen, who had been named the president of Thiokol. He talked to me about replacing Joe Lombardo, who was his vice president of Space Operations. Joe was approaching a mandatory retirement age of 65. I wasn't really interested because I was enjoying GTRI and was acting director of all of their labs at the time.

I met with Crippen at dinner, and he made the offer. At the time this was occurring, my daughter lived in Logan, Utah. Her husband was the basketball coach at Utah State University, [Logan], and they had two children. At night I'd get a call from this little voice saying, "Granddaddy, are you going to come out here and live with us?"

I'd tell my daughter, "This is undue pressure that I don't need." This went on for two or three months, so I had my daughter and grandkids really wanting us to move out. With that and after the meeting with Crippen, I took the job and went to ATK as the vice president for Space Operations.

During the next few years we were got bought a couple of times, the first time by Alcoa. They bought us because there was a Fastener Division in Condant, our parent company. Alcoa decided they didn't need a rocket company and put us up for sale, and we were bought by ATK. Crippen had a buy-out clause and retired and ATK named me president of Thiokol.

I agreed to stay two years, and we merged Thiokol with Alliant into one company that I had to manage. This was another interesting challenge because the two biggest competitors in the solid rocket business were now merged. We kept the Thiokol name because of the strength of the brand, even though Alliant personnel felt they bought us, and therefore should keep the Alliant name. This was a cause of significant resentment on the part of Alliant. I had to deal with this and other issues while trying to build a rapport with a group that didn't want to be a part of us. Even up to my retirement this was a major challenge.

All this time, I moved my wife around these places, Mississippi, Washington DC, and Utah. None of which she wanted to go to, but she supported my career. She was a real trooper. It was an amalgam of lots of opportunities that I would never have expected. I have been very fortunate and truly blessed.

WRIGHT: Thank you. Anything else you have?

ROSS-NAZZAL: No, thank you very much for your time. We certainly appreciate it.

SMITH: You're quite welcome, I've enjoyed it.

[End of interview]