NASA STS Recordation Oral History Project
Edited Oral History Transcript

Royce Mitchell
Interviewed by Jennifer Ross-Nazzal
Huntsville, Alabama – 30 June 2010

ROSS-NAZZAL: Today is June 30th, 2010. This interview is being conducted with Royce Mitchell in Huntsville, Alabama, as part of the STS Recordation Oral History Project. The interviewer is Jennifer Ross-Nazzal. Thanks again for talking with me this morning. I certainly appreciate it.

MITCHELL: I'm glad to take part in this.

ROSS-NAZZAL: And I'm glad you agreed. I thought we could start off by having you describe your career with NASA.

MITCHELL: I started off as just what was called at the time an aerospace technologist. The word is engineer. I came to NASA in 1963, put in my dues with the laboratories for about eight years or so, and wound up in an organization that Dr. Wernher von Braun established called Central Systems Engineering. Part of the group I was in eventually moved over and became part of the Space Shuttle Task Team.

The Space Shuttle Task Team was charged with defining an operational Space Transportation System. Many many trade studies, many many configurations were looked at, and part of the problem was that they knew there was going to be a limit on development cost. Operationally the best solution was a liquid fuel booster carrying a liquid fuel orbiter. The orbiter had its own internal tanks, like any aircraft, but the resulting vehicle was monstrous. The booster, which was a flyback and land like an airplane type booster, was immense. It makes the Airbus 380 [aircraft] look like a popgun. It was a large booster. The orbiter was extremely large and did not have the payload capability that the Shuttle eventually wound up with.

It became obvious pretty soon that that was not going to be a viable option. The [Richard M.] Nixon administration, by edict, limited the amount of development money, knowing full well that limiting the development investment would add to the operational cost. It was a trade in favor of early spending limits to avoid having that problem, but it cost a lot of operational money downstream. That was understood and was walked into with open eyes.

We eventually settled on the current [configuration], which at the time was called rocket-assisted takeoff Shuttle (RATO). You've never seen that in print probably but when you think about it, that's what it is. Then of course the term external tank is a carryover from the studies that debated whether the tank should go to orbit and come back and be reused over and over or not. A lot of people have always thought it quaint that the tank is called the external tank, but it's from those days of program definition.

When it became obvious we were going to have solid rocket boosters because they were cheap, the technology was felt to be established, the risk low, the question became do we reuse the solid rocket boosters? Many trade studies were run there. Due to the fact that the orbiter hangs off the side of the external tank, it presented some control problems for certain conditions, certain winds, certain payloads, certain engine-out cases. It was determined that the boosters had to have a thrust vector control system. It had to have steering in it, and that made the cost such that it was economically advantageous to recover and reuse the boosters.

With the recovery system somehow I wound up being the recovery subsystem guy. I didn't know the first thing about parachutes; I certainly [had] never jumped, but it's amazing how quickly you can come up on any subject if you make the right calls and talk to the right people. The first thing I did was call the Army, see what they used to drop those tanks—the [M551] Sheridan vehicle. Called the Army people and they finally put me in touch with the Sandia [National Laboratories, New Mexico] people who made very heavy-duty parachutes to lay down nuclear weapons. They were some help as a consultant, but we still tripled the amount of weight ever recovered, and we did it with an edict that we could only have six drop tests. I talked to John [W.] Kiker at Houston [JSC] who had done the Apollo parachutes. He had hundreds of drop tests. I had six. We could not carry the weight of an empty booster into the air to drop it, so we had to jimmy the test. That's how boosters became recoverable, and how I wound up as the recovery subsystem manager.

After that system was developed, Bob Marshall, who eventually became the Shuttle Manager, moved me off of the booster and put me [on] a solar electric propulsion system. I was chief engineer of a system that would use huge solar arrays and an ion drive engine developed up at the Glenn [Research] Center [Cleveland, Ohio]. We were going to fly out and intercept comets, fly out and land on asteroids, and do all these wonderful things with exotic ion drive with unbelievable performance. Even though wide open the engines produced only about a half an ounce of thrust apiece. [The US] Congress looked at this for a while and said, "Ah, we don't think so."

Then I was moved to the Space Telescope that became Hubble, and I was head of the systems office trying to integrate the telescope into the spacecraft, integrate all the scientific instruments which were under Goddard's purview [Goddard Space Flight Center, Greenbelt, Maryland], and make all that come together, test it and get it ready for flight.

I was also responsible for planning for on-orbit maintenance and refurbishment, which turned out to be a very fortunate capability and was getting ready to be named head of Hubble because Jim [James B.] Odom was moving on to another job. Just at that critical moment in my career path, we had the *Challenger* accident [STS 51-L]. And I was very upset when I was told I would be going over to join the solid rocket motor project as manager of solid rocket motor. I really was upset about that. After I moved over, and eventually when Hubble flew and they found the problem with the optics on Hubble, I said, "Well, the good Lord might have been looking after me after all."

The redesigned solid rocket motor project was extremely exhausting, very challenging, but was also very rewarding. A lot of good people worked very, very hard to make that project work. I later became project manager for an advanced solid rocket motor, which had some support in Congress but eventually, with the continued good success of the redesigned solid rocket motor, the advanced solid rocket motor was canceled. About that time I left NASA and [re-]joined private industry. I worked on the Space Shuttle main engine out at Rocketdyne. Brought along the so-called Block I improved Shuttle main engine.

From there I moved over and took over the power system for the International Space Station with all the solar arrays, trusses, power conditioning, batteries, and assorted subsystems on International Space Station. I must have messed up, because they decided I ought to be moved down to Houston to be the deputy on the entire program for Space Station and especially be in charge of the technical direction. I integrated the international partners, enjoyed [ISS] very much, and now I'm fully retired.

ROSS-NAZZAL: You've had a wide and varied career.

MITCHELL: Sometimes I pinch myself over my career.

ROSS-NAZZAL: It's amazing all the things that you've worked on for so many years. I thought today we could focus on your efforts on the solid rocket motor after *Challenger*. You were displeased that you were assigned to become the project manager for the SRM [solid rocket motor] effort after the accident. How or why were you assigned to this task?

MITCHELL: There was a wholesale turnover everywhere from the associate administrator down to center directors down to program managers. Bob Marshall was put in charge of the Shuttle Program, replacing Stan [Stanley R.] Reinartz. Bob Marshall had been my boss on the SEPS [Solar Electric Propulsion Stage] Program and on other interactions with him. I think it was good, as far as being selected to go to RSRM [reusable solid rocket motor], to have been away from the Shuttle for a while.

J. R. [James R.] Thompson had developed the Space Shuttle main engine, which is a remarkable machine, unbelievable machine. J. R. had left Marshall [Space Flight Center, Huntsville] and left the agency and he was brought back as Center Director. I think the fact that he had been away for a while put a little cleanliness, if you will, on the situation. I think the same is true of me, that I had Shuttle experience but I had been away from the project, and importantly was not on the project at the time of the accident. I think that's one reason, maybe *the* reason, I was picked. It was a daunting task.

I met with J. R. and he said, "We've got a heck of a hill to climb here, and I don't think I've ever seen a bigger problem." I'm thinking, "And you put me here?" Once I became resigned to having to give up what I thought was going to be a lot of fun on Hubble, I plunged in and worked hard. I was very upset about leaving Hubble, but as it turned out the motor was very rewarding.

ROSS-NAZZAL: In Allan [J.] McDonald's book [*Truth, Lies, and O-Rings: Inside the Space Shuttle Challenger Disaster*] he talks about you and John [W.] Thomas relocating to [Promontory] Utah [headquarters of Morton Thiokol Inc., contractor for the solid rocket motors] from Alabama. Would you tell us about that decision?

MITCHELL: I think there was no question that it was good to have NASA present [on-site]. Of course we'd always had a resident office there, but not for the kind of frenetic activity that was going to be going on for the next year or two. Because there was going to be a lot of paperwork, a lot of reviews, a lot of coordination, we just felt it healthy from NASA's viewpoint and from Thiokol's viewpoint that we be located on site there to look those people in the eye, to turn around requests, to turn around studies, to turn around approvals, and do it quickly and on site.

A little background about John Thomas—after the accident in January [1986], Marshall Space Flight Center immediately began to put together a team to see what could be done about this. He, in effect, became my chief engineer because he already had a head start on the redesign possibilities, possible routes to take to get the thing flying again. Fortunately, it made his job easier and I guess my job easier. Thiokol had already started looking at a redesigned case to limit the joint deflection [long before the *Challenger* accident. In fact, there was a special version of the SRM, using a carbon fiber wound case. This lightweight gained about 5,000 pounds of payload and was to be used for DoD (Department of Defense) payloads out of Vandenburg Air Force Base in California.

These special cases used metal rings at the ends of the segments to join the segments together. The important "capture feature," which kept the joint parts from tending to separate, the filament wounds cases. There were already such flight SRMs stacked on the launch pad at VAFB at the time of the *Challenger*.]

I guess I really need to take time to describe the joint and what the redesign did. The joint is called a tang and clevis. You can think of it as tongue and groove. If you're familiar with flooring, you have a board with a groove and then a board with a tongue that sticks out and the tongue goes into that groove. That's the way the motor was put together. There was a clevis, which is the groove part of things, and there's a tang, which is the tongue part of things. The tang fits down into the clevis, and then you pin the joint together with 177 one-inch steel pins. You've got this U-shaped clevis in cross section. Keep in mind that the tang and clevis are all part of the piece of motor case, because it's all one continuous piece of metal. There's no welding allowed on the motor case.

When the motor pressurizes there's something like, depending on which joint you're talking about, anywhere from 13 million to 17 million pounds trying to pull those case segments apart. That's a hell of a force for those pins and the tang and clevis to try to react against. That's [mainly] what caused the clevis to move away from the tang a little bit, a few thousandths [of an inch], but a very critical few thousandths. The thing that Morton Thiokol had started down the pipeline or hoped to get permission to do even before *Challenger* was to add another lip on the tang side to hold that leg of the clevis in place.

What we eventually wound up with the redesigned joint—it was like putting two clevises together where the upper one gripped the lower one and the lower one of course gripped the upper one. That so-called capture feature, which was like another leg of a clevis on the tang side, gripped the inboard side of the clevis and kept it from moving. That was the secret to the redesign, and as I said something that Thiokol had started some time [before *Challenger*].

I think it's very important that people understand that the "O-rings" did not fail. The leak happened because the joint went through structural deflection as the motor came up to pressure at ignition. The motor didn't take long to pressurize, less than a second. This flexure of the joints created gaps here and there, and [sealing] the O-rings couldn't keep up, but it really wasn't the O-rings' fault. The O-rings were asked to do things that they shouldn't have been asked to do with this joint deflection business. Thiokol, even before *Challenger*, had taken some pretty strong company steps to try to limit the deflection of the joints at motor ignition. They had gone to the case suppliers [and put a hold on the next cases to be delivered while they tried to get NASA's okay for the steel case capture feature].

It's important to understand that the rocket case [segments] are single pieces of metal. Each segment is a single piece of metal that Ladish Company [Inc.] punched a hole in a billet of metal, D6AC steel, and then shear-formed—I'm sure wearing very large earplugs, because it's got to be a noisy operation—shear-formed these billets into cylindrical shapes. Ladish was very advanced in that. They were very secretive and protective of their processes. They would do this job, and then after they were formed to close to what the final configuration would be, they were shipped to machinists, Rohr [Industries] and other companies that would machine the [tangs, clevises and] grooves for the O-rings.

At their risk—they didn't have NASA approval—Thiokol told [the machinists], "Do not finish conforming these cases. We want to add a capture feature, an extra little cleat that would keep the pieces of the joint from moving so much." That was already in work. It had been designed; the metal was standing by. That helped things tremendously. As I said, the redesign team's job and my job [was made] much easier to have that head start. Otherwise it would have taken a lot longer because getting pieces of the case from an ingot at the steel mill to a finished product at the Thiokol factory was a long process. But we had the advantage, due to their foresight, in that part of it.

ROSS-NAZZAL: As I understand it, there were two groups working on redesign. There was one headed by John Thomas and one headed by Thiokol. Would you tell us about those two groups?

MITCHELL: Yes. It was good to have—I don't want to say competition—but have fresh eyeballs looking at different aspects of the redesign. There were certain very rigid constraints placed on the project in the form of recommendations from the so-called [William P.] Rogers Commission [also known as the Presidential Commission on the Space Shuttle Challenger Accident], and they had laid down some very specific guidelines on what the overall performance of the redesigned motor should be. Within that constraint, the two teams were trying different options. It was just a way to have fresh ideas, fresh [insights]. Fortunately we didn't have to do a lot of dictation. There was one or two dictations that I let the technical guys talk me into that I wish I hadn't. I'll talk about those later, but that was in effect what was going on and it was a friendly thing.

ROSS-NAZZAL: Were there ever any ideas tossed out that you thought well, that's just too crazy?

MITCHELL: Yes. There were ideas of having a totally different joint. Rather than have what amounted [to] and eventually became a modified version of the *Challenger* design, there were those who wanted to just have a completely different approach to bolting segments of the motor case together using oil field and boiler plate technology, and metal seals instead of flexible seals, and any number of approaches. Some of those, while they were well-meaningly proposed, were just obviously not to be given serious consideration.

People would write in from the general public with ideas about how they thought things ought to go and how crazy those NASA people were. There were proposals to use copper seals. We had one proposal that said don't worry about [any escaping] hot gas, just contain it with a baggie system. At 6,000 degrees, this gas is not to be fooled with. But it was all in a spirit of let's get the Shuttle flying again, let's get this country back in space, and everybody trying to do the right thing.

There were quite a number of designs proposed. We went through a triage process. Some were rejected immediately, some were given reasonable consideration, and even some prototypes were built. There was a company called Vetco Gray who did a metal seal proposal that got serious attention. Roger [M.] Boisjoly—whom many people may have heard of, who opposed the launch and took a very personal beating emotionally and otherwise about the *Challenger* accident—thought the Vetco Gray idea was probably a very good one. He was pretty dismissive of our redesign that we eventually settled on, but it was all carefully considered and a lot of serious thought given to it.

Our new associate administrator Richard [H.] Truly had put an additional [requirement] to the Rogers Commission [list]. He had put the constraint on us to make maximum use of the hardware in the inventory. That's a good idea. With the caveat of course, that it [must have] nothing to do with compromising safety just because something is in inventory and can be used. If there's a better way, do it the better way. But keep in mind to try to maximize use of the inventory. That became one of the considerations, and a fairly strong one, as long as safety was not compromised.

ROSS-NAZZAL: You had mentioned that the Rogers Commission drove the idea of performance. What were some of the requirements that you were designing toward?

MITCHELL: I want to take time to read a couple of them, I have them here. They have a section called Section 1, which is addressing the motor immediately. They introduce it by saying the faulty motor joint and seal must be changed. Okay, that's basic principle. "No design option should be prematurely precluded because of schedule, cost or reliance on existing hardware," which is something we have to balance against Mr. Truly's consideration. But—and I quote—"All solid rocket motor joints should satisfy the following requirements. One, the joints should be fully understood, tested and verified. Two, the integrity of the structure and of the seals of all joints should not be less than that of the case walls throughout the design envelope."

The more you think about that, the more you realize that is a challenge, that the joints would have to be as good as a plain piece of metal covered with insulation. That's quite a challenge. Returning to the commission, three, "the integrity of the joints should be insensitive to dimensional tolerances, transportation and handling, assembly procedures, inspection and test procedures, environmental effects, internal case operating pressure, recovery and reuse efforts, flight and water impact loads." In other words, you got to make sure you take all those into consideration.

[The commission] went on to talk about tests. "The certification of the new design should include tests which duplicate the actual launch configuration as closely as possible, tests over the full range of operating conditions including temperature." Then another major bullet: "Full consideration to conducting static firings"—that's the ground test firings—"of the exact flight configuration in a vertical attitude." That gave us a lot of food for thought, to build a new test stand to fire these motors in the upright condition.

Then they go on to talk about having an oversight committee, which eventually was turned over to the National Research Council under Dr. [H.] Guyford Stever, who was presidential science adviser to Nixon and a very eminent technical guy, and a very reasonable guy. So we had the NRC oversight committee, which the Rogers Commission handed off to do the nitty-gritty day-to-day interface with us and oversight with us. They were a bunch of really great guys in the National Research Council oversight group. Like other panels such as the Aerospace Safety Advisory Panel, which was formed after the Apollo [1] fire many years ago, these men were smart and experienced, and they understood the day-to-day nitty-gritty problems that poor old engineers were faced with, and they were tolerant of the limitations. They were not dictatorial; they were extremely cooperative, but rigorous in their oversight. It was just a pleasure to work with [them].

They go on in their requirements to talk about management structure, including putting astronauts in management, and to have a safety organization set to one side of the project management organization all the way up to the associate administrator. George [A.] Rodney, as I recall, was the first guy in the associate administrator role [for the Office of Safety and

Mission Quality]. They wanted to make sure that there was an independent voice that could holler "Stop" and make sure safety was being adhered to. And especially to have astronauts in management, which they noted at one time was very active and somehow had [been] gotten away from, and to get the astronauts more involved, which of course we did. There were other considerations in the organization, but they put some pretty strong technical limitations on us, and things that we had to do. We worked hard to hold to that course.

ROSS-NAZZAL: Tell us about the eventual design that you did come up with and how you built the consensus with the NASA folks and the Thiokol people and then also involving all of these other groups and convincing them that this is the right design.

MITCHELL: The design evolved with a lot of characterization tests. There was a guy at Thiokol named Dr. Joseph [E.] Pelham. He's deceased now. Dr. Pelham invented two things that were absolutely remarkable in the return to flight activity. [First] he invented a system of testing these joints full-scale but with limited duration. The problem with *Challenger* and with any joint sealing technique is that you have a transient period while the motor is coming up to pressure. Things are changing, there's a dynamic effect, and a lot of things are happening as this motor flexes and finally stabilizes when it gets to full operating pressure. He invented a way to at least look at this transient start of the motor.

We built full-size case segments and put them together to get a couple of joints tested. Not the whole length of the motor, but they were full-size as far as diameter, using real pieces of flight hardware. But with limited duration, which means he took slabs of propellant and carefully sized them and located them to generate the heat flux, to generate the pressure flux, and pressurize the case without going into a full-scale full-duration firing. That was a brilliant move and helped us characterize the joints, characterize the performance, and help convince people that our design was on the right track.

ROSS-NAZZAL: Was that the joint environment simulator?

MITCHELL: It's the joint environment simulator. He [also] designed one for the case-to-nozzle joint, which was called the nozzle joint environment simulator. There was a similar test setup at Marshall Space Flight Center called the transient pressure test article. It combined both field joints and the case-to-nozzle joint and was used in a similar fashion. It had a little more capability to react to [external] loads. It had a mass simulator on top of its case and side loads simulating being attached to the external tank with those struts. Those loads, due to interaction between the motor and the tank, could be inputted to the transient pressure test article as well. That was a big help in driving this design, and testing always was the ultimate referee of how good our design was. There were disparate people, and that's good, that's healthy to have challenges to how design is proceeding.

[One of the first tests we ran in the field joint environmental simulator used the *Challenger*-type joint. We put zinc chromate putty in the joint, with a deliberate blow-hole in the putty. We then cooled the joint down to the calculated temperature of *Challenger* and fired the test. The result was emotional eerie: there was this jet of black smoke that emerged from the field joint and up the side of the case—looking exactly like the deadly puff of black smoke recorded by the launch pad cameras for *Challenger* just after ignition. We had duplicated the failure. People looked at that test film and were moved.

The mystery of *Challenger* was that the leak re-sealed and stayed sealed for about 57 seconds, then reopened. The theory is that aluminum oxide, a motor combustion by-product called slag, sealed the leak temporarily. Then as the vehicle passed through the flight environment of maximum dynamic pressure, the resultant buffeting and structural flexing caused the slag to break open. There is no competing theory which matches the conditions of motor operation and vehicle dynamics as well. We did not reconstruct or duplicate this part of the *Challenger* failure, but there was little doubt as to the validity of the theory.]

"Why don't you change O-rings?" It's very curious but appropriate that the O-rings that were in *Challenger* remained the same O-rings for the reflight, because the O-ring material was good. It was compatible with the best lubricants to accommodate O-rings moving and taking their seated place. They were tough, they were resistant to all kinds of effects, and reasonably resilient, especially when properly temperature condition[ed]. Even though we considered and tested and looked at just about virtually every material known to man as a new O-ring material, we wound up right back [at the start], which gives some credence to the original selection in the first place. The design progressed [by] a lot of analysis, a lot of characterization, a lot of subscale tests that could produce pressure fluxes, thermal fluxes, and conditions for challenging competing designs. As different designs were proposed, it was always test[ing] that was the ultimate referee for choosing the evolution of this redesign.

ROSS-NAZZAL: Would you describe for me the difference between the redesigned SRM and what was flown prior to the *Challenger* accident?

MITCHELL: The difference between *Challenger's* motor and the reflight motor was that virtually every element of the solid rocket motor saw some changes. If you start at the front end where the igniter is, we beefed up the igniter flanges. We learned to clamp down the igniter bolts to a higher torque because every one of these features has seals, and they have to accommodate pressurization transients. The igniter is actually a small rocket motor inside the large motor which throws a flame down the entire length of the solid rocket motor instantly and starts the ignition and burning process, so the igniter has a nozzle.

On paper, [the igniter cases] had a slight negative margin of safety. They had a factor of safety, but it did not meet the 1.4 [f.s.] design requirement that was on every element of the Shuttle. Even though this igniter had never had any problem with performance or otherwise, because on paper we didn't meet the 1.4 at the nozzle of that little motor, it was changed.

The case joints became different, which means because these case segments—as I discussed previously—are a single piece of metal, we had to go all the way back to the plant and get new pieces of motor case. [As discussed] the joints were different. [Besides the capture feature] we added a lot of good features to the joints, including heaters, environmental protection, a lot of protection for water impact.

Adding the heaters, the so-called capture feature that gripped the joint to keep it from flexing as much—the biggest thing we did—and this is another tribute to the late Dr. Joe Pelham—he designed the so-called J-seal, which was a curved flap [of internal insulation]. That's why it was called J, because it had a hook on the end that was part of the case wall insulation that hung down. As you brought segments together, that [circumferential] flap interfaced with the next segment and in effect sealed the joint. The joint never would see the flame front if that J-seal, the sealed insulation that Dr. Pelham designed, worked properly. And it did work properly and was an amazing addition.

I want to take time from describing all the differences between the redesigned motor and the original motor to talk about a very controversial subject. This was a controversy we had with some members of the National Research Council oversight group. While the motor is igniting and the flame front is coming to the joint, should you try to keep the flame front away from that joint, away from the O-rings? That may sound like an awful simple question, "Of course you're going to keep the flame [away]." Well, it wasn't that simple.

The O-rings sealed by being pushed into the gap between the two elements that we were trying to seal. That required that it happen all the way around circumferentially at the same time. What happened on *Challenger* was that there was putty in the place of what eventually became the J-seal and the sealed insulation. As the two segments that were being mated were brought together, a layer of putty was placed on the surfaces between the segments and then was compressed when the segments came together.

Unfortunately it was impossible to avoid trapping air between the joints as you brought those two segments together, so there was air trapped in among and behind the putty. Over time this air would work its way to the surface and leave what was called a blowhole. In a way, when *Challenger* was doomed, you could almost say that the putty killed the crew because there was a blowhole in the putty which let the flames impinge on one part of the O-ring.

Not only that, when it was through—when the flame had pushed its way through that wormhole, through the airhole, through the putty—as the motor continued to supply pressure, hot gas started filling up the annulus, the circular tunnel in that joint, which means that that jet of hot gas that was hitting the O-ring was sustained. It didn't stop [for some time]. When the motor finally pressurized and equalized, most of the problem was over because the hot gas was stagnated, the pressure was stable, and transient was over. But you had a hot gas jet, and as the annulus filled around the whole circumference of the motor it sustained that jet as more and more gas tried to fill the circular tunnel, and that led to burnthrough of the O-rings.

So the question became then why not let the flames go all around and push the O-rings in place, stagnate almost instantly. No chance for a jet, no chance for a sustained hot spot that went on and on and on to burn through. That was a big question. Do you want to protect the O-rings from the flame front or do you want to just let the flame front come in, activate the O-rings into position, stop the flow, and stagnate, and everything would be okay?

The thing that led us to try to keep the flame front away from the O-rings was that we could not be sure that the motor would not develop circumferential flow due to several factors which could produce that. If you get hot gas swirling around inside the large solid rocket motor, and it's traveling circumferentially over the O-rings, it might cause devastation. It's a very difficult problem to model, to understand what's going in. No instruments can stand up to being fired inside a motor to measure if there's circumferential flow. There's variation from motor to motor whether you'd have had circumferential flow or not. Because we could not put that problem to bed, we could not characterize the possibility of circumferential flow. If there were circumferential flow swirling gases around and around, we did not know what Mach number, what speed it was, and so we said, "We don't have any choice but to try to protect the O-rings." Thus the sealed insulation.

The J-seal was a brilliant design. [The second major contribution of Joe Pelham.] Dr. Pelham should have been hoisted on shoulders and carried around all over the agency for the work he did in that regard. I understand there's been one or two instances of hot gas getting past the J-seal, but [withut] causing any problem. In my tenure, after we implemented that we never had a problem. It was a wonderful solution. All the banjo work we put in—the new insulation, which had to be contoured to accommodate the J-seal, the heaters, the environmental protection and the weather protection, all that at the field joints for the case. [We also added a "volume filler" in the annulus above the inner clevis leg. This would limit the amount of hot gas in the joint should the J-seal and the capture feature O-ring ever leak.]

Moving on aft, we come to the case-to-nozzle joint. The case-to-nozzle joint had exhibited more postflight damage than the field joints prior to *Challenger*. There were a number of instances of O-ring damage both in the field joints and the case-to-nozzle joint, but the case-to-nozzle joint had more. The reason it was less of a concern was that the case-to-nozzle joint had two types of seals using O-rings. One, the primary seal O-ring for the case-to-nozzle joint worked like the case field joint O-rings. That is, it was a bore seal. A bore seal seals between two cylinders. It's like you slide one pipe inside another pipe, and you seal between those two pipes by having an O-ring in the space between them.

The secondary seal for the case-to-nozzle joint was a face seal. There was a metal lip as part of the nozzle metal structure that could mount flush with the end of the aft dome, and so you had two flat surfaces coming together and you put the O-ring there. It's like a rubber ring that you put in a jar of canned goods. You're sealing a flat face, and that was extremely reliable. If we could have had face seals everywhere—and believe me they were proposed and we had to consider that and argue the merits and demerits of them. A face seal is a wonderful thing, if you can accommodate it reasonably well.

In order to put suspenders with the belt, we added a bunch of bolts to the case-to-nozzle joint to keep it from trying to open so that the bore seal between the cylindrical part of the nozzle and the cylindrical part of the motor case—that primary seal we protected by adding a couple hundred bolts all the way around the case-to-nozzle joint. Of course each bolt had to be sealed too. So there was a face seal under each bolt head to try to keep that joint from opening at all. Never mind that we had a very good face seal downstream that had produced very good results even though that joint had seen a lot of damage pre-*Challenger*.

The nozzle itself, because of its complex shape and because it has to move, had five different joints. Prior to *Challenger*, each one of those joints had had a single O-ring as a seal. We redesigned the parts to have dual O-rings at each nozzle joint. The nozzle had also experienced pocketing erosion even prior to *Challenger*. It was a pretty big concern to a lot of people so a number of fixes were put in place. Curiously, all you had to do was change the ply angle of the carbon-phenolic cloth, which made up the thermal liner of the nozzle. It took a terrific thermal load because every bit of energy that's in that big huge solid rocket motor comes through the nozzle, and the nozzle has to take it and stand up to it. The solution to the uncharacteristic erosion was to change the ply angle of this carbon-phenolic cloth, and it changes how that cloth is [thermally] stressed and how well it can hold together. [The redesign] changed a lot of things on the nozzle.

There was a design [change] made to the nozzle that I wish I'd never allowed to happen but I let myself get talked into it. There was an outer boot ring which became famous in the redesign effort. The nozzle has a very expensive bearing that allows the nozzle to move. It's a series of kind of cupped metal plates that nest inside each other with this elastic rubber bonding each one to the next one.

You've got a stack of metal dishes with a big hole in the middle for the eventual flame front to get out. That thing is very expensive and has to be protected because it can't resist the motor burning so there was a flexible phenolic curtain put around it [called the "boot"]. [It was] expendable; it burned out each flight, but protected the bearing. You attached one end of it good and solid to the [aft dome], but the other end of this curtain had to be attached to the phenolic material of the nozzle [to a "boot ring"]. Phenolic materials, while they can resist heat, they're not really good structural components.

We found after flight, before *Challenger*, that sometimes that outer boot ring made of phenolic that held one end of this protective curtain would be broken. It was fairly well known that the break of that outer boot ring happened after motor burnout. The boot had vent holes in it because obviously when you fire up 1,000 pounds of pressure per square inch of the motor, that curtain is not going to hold together so it had to be vented to allow hot gas to go through it and equalize on the other side so that you didn't get a big pressure buildup.

[As] the motor burned, those vent holes filled up with slag. One of the by-products of the combustion is aluminum oxide, which is generally called slag. Slag would get in those vent holes. [But after] burn out, you still have 1,000 psi behind those vent holes on the bearing side of the boot, so it popped the boot out and broke the thing. Who cares? It's after burnout. Well, a lot of people cared because you couldn't really prove it. That was the theory. We said, "Okay, we'll make the outer boot ring stronger."

They went to a very expensive, time-consuming, hand lay-up of what was called an "involute" design where the layers of carbon-phenolic material were like a rosette. If you've ever seen a sanding machine that has a bunch of flapping pieces of sandpaper that you use to take paint off of something. The lay-up of the [involute] is like that. You put a bunch of layers of this material fanned out radially and then you lay them all over and try to glue them down, bond them down, and it was thought that the fact that there was a lot of overlap between each one of those layers and the next one to it and then the next one would make the outer boot ring very strong and the problem solved.

Well, it didn't work. And we had already built flight motors with the involute design, and we sent them to the Cape [Canaveral, Florida] before we had that test to prove that the involute didn't work. As a contingency, we had [also] built the old style, and we were able to swap out the motors without any program impact. But I wish we'd never tried the involute system, and I'm still mad at some people.

ROSS-NAZZAL: As you were talking about this, I'm curious. You're doing all this testing, you're basically redesigning what sounds like every part of the motor.

MITCHELL: Every part.

ROSS-NAZZAL: How many people were working on this effort?

MITCHELL: Hundreds. I doubt if I [accurately] knew at the time how many people were supporting my project [day-to-day]. Because of the pressure to turn this around, the Shuttle Program was spending over $10 million a day, $4 billion a year. While we had to try to not just go wild with cost, we were given a lot of rein, a lot of slack, to load up. Thiokol hired many, many job shoppers and independent people because they couldn't staff up permanent staff that fast and then be faced with the post-return-to-flight downsizing again. They hired a lot of job shoppers, some of whom turned out to be real kooks, but we weeded those guys out. Many, many people. I'm sure it was over 1,000 people. If you count NASA, the technical people in the labs, the team I had out at Thiokol, all the contractors, independent study people like Vetco Gray that had the metal seal design, just on and on and on. There were many many hundreds of people in this redesign. As you said, we changed virtually everything in the motor.

We stuck with a lot of original materials, [for example] the insulation. Obviously we didn't do anything to the propellant. Here and there if we found through lab tests [say] a better adhesive, we would use the adhesive. It was a big effort to characterize, to fully understand. Sometimes it was amazing we would come up with. We didn't realize all [the prior] time what had been going on here at all, but we solved that with a very rigorous test program.

To me, the most important part of the redesign effort was the many, many tests that we ran. There was even a circumferential flow test device where we simulated a field joint and then shot a little rocket motor across it lengthwise—or what would be circumferentially in the big motor. We shot thermal fluxes across the scale size joint to test that. We had what was called a 32-kilogram charge motor, which we could fire on a full-scale joint but not full-diameter. In other words it wasn't 12 feet in diameter but it was full-scale as far as the local joint was concerned and would hold the firing for about 80 seconds, which is two thirds of a regular [motor]. Just on and on, all these test rigs. Many, many test rigs to get pressures, transients, deflections, thermal fluxes, and all the action of firing a motor without having to do a full-scale motor. Of course the heart of verification was a series of full-scale motor tests. We did some great jobs in testing the motors. We had to call one a partial failure when the [involute] outer boot ring failed.

As in another part of the test program, we introduced deliberate flaws into a full-scale motor, something that had never been done in the solid rocket motor world. There was a movement—I don't know where it started but J. R. Thompson, our Center Director, picked up on it and ran. Some people say some of the NRC [National Research Council] guys proposed it. It bubbled up from probably various sources but J. R. became the focal point that we would challenge this design by putting deliberate flaws and making sure that our system worked.

We had a designed a system that was not supposed to let the flame front get to the O-ring, but we were going to carve channels into that J-seal, that sealed insulation, to create a hot gas jet that was anathema to everything we had done, and now we're going to deliberately do it to show that our system downstream worked. We put damage in every joint in the motor, every joint. Here, there, different sizes, different impingements. Then the question became, "Okay, you're going to fire this motor with these deliberate defects in it. What's the pass/fail criteria?" Well, here we go again. Big debate.

J. R. said we ought to meet full spec [specification] requirement. "Well, come on, J. R." "Yes, we need to," he said. "I've got NRC backing on this." He had some members of the NRC guys that were on his side. We went round and round and round. We talked to Dr. [James C.] Fletcher about this upcoming deliberately flawed motor test. He said, "My suggestion is that if it holds together and doesn't come apart on the test stand, don't open it. Go ahead and fly. Don't take it apart because we'll be debating the results for months if not years." Dr. Fletcher, our new [NASA] Administrator, that was his take.

We finally got to where we thought we had a reasonable answer, and the motor worked perfectly well. The flame did get to the O-ring, did not cause damage, and did show that we maintained the seal. It was a wonderful test. It's probably the one thing that let me go to sleep at night, once that test was successful. We were right on the threshold of flying when we ran that test. When we flew, I had absolutely no concerns about that solid rocket motor.

That was a wonderful job a lot of people did. A lot of people didn't get the credit they deserved. I told [an audience] at a post briefing here at Marshall—it was informal out here at the Space & Rocket Center—there were three things that kept me awake at night on that project. One was J. R.'s comment that this was the biggest hill NASA had ever had to climb, and I was part of it. Two, I had lost faith in NASA management because they had put me on the program. And therefore they've got to be crazy, if I'm doing this job. Three, I was a pseudo bachelor in Brigham City, Utah. My wife was a reference librarian at the public library. They were moving into a new facility, and she wouldn't have seen me much if she'd come to Utah anyway, so she stayed in Huntsville. Those kept me awake. What let me sleep at night was having good management support, having good domestic report, and successful test of that flawed motor.

ROSS-NAZZAL: How long were you in Utah working on this effort?

MITCHELL: I was playing catch-up. I went out there [immediately]; he came out later. I think I came home in March of '88, we flew in September of '88, so 18 to 20 months. It was a very hectic time. Trips to Washington [DC, NASA Headquarters, and all the centers]. Many a morning I woke up and wasn't sure where I was, whether I was in Huntsville, Washington, Utah, the Cape, or Houston.

The associate administrator, Dick Truly, had what he called the management [council] which was all the project managers from all the centers: Houston, Cape, Marshall and on and on. I remember spending eight hours on my feet with a [wrenched] back, with a snowstorm outside, for eight hours in front of Truly and that management group here at Marshall talking about the outer boot ring, why it was failing, what we were doing about it. I can't remember a more painful day in my life. It was [all] part of it.

ROSS-NAZZAL: When you were working this project was there ever any thought on your part that you would succeed? Were you convinced that you were going to come up with a design and you were going to fly again?

MITCHELL: Once we laid out the many test programs, so many tests, such good anchoring of all the analyses, I said, "I don't know how long it's going to take us, but when we finish we're going to have a hell of a motor." And that's the way it was. I didn't have time to get concerned. I didn't envision anything but success and the good people, all the good support. And that's the way it turned out. We implemented a lot of QA [Quality Assurance] items that helped give you the good feeling. You have to understand the big drawback of a solid rocket motor is you can't test it. You can test [the] test motors, but the motor you build that's going to fly, you have to depend on procedures and inspections and quality control. That is a frightening thought. You cannot test it.

The Space Shuttle main engine, which is probably the technological marvel of the century—J. R. was instrumental in developing that thing, and it's incredible. It's a machine that just boggles the mind. The more you learn about it, the more you're convinced it'll never work, but it can be tested. Once you build one, you can run it up, you can overpower it, you can put it through its paces, and you know what you got.

With a solid rocket motor you are depending on the excellence of procedures, the excellence of workmanship, and you're depending on all that to put a crew on. Quality control, product assurance becomes extremely important. That was one nerve-racking part of the whole [project]. In order to make sure we had the right squeeze of the O-rings—you can't squeeze an O-ring too hard, or you can't let it be too relaxed—we developed what was called a profile measuring device, which was a swing arm that went round and round inside the motor case on the areas where the O-rings would be with a very tiny, sensitive transducer on the end to measure within 1/1,000 or 2/1,000 of an inch. Here you're talking about something 37 feet in circumference, and you're swinging this arm around, and you're measuring down to less than the thickness of a sheet of paper.

[An SRM has lots of bonding, and surface cleanliness is essential.] We had Mr. [Robert J.] Schwinghamer out at Marshall who has done some very good work over the years. His laboratory developed what was called "optical stimulation of electron emission" [OSEE]. That relates back to Dr. [Albert] Einstein's Nobel Prize about how incident radiation can cause electrons to be emitted from metal. When you do that, if you take say in this case an ultraviolet light highly calibrated and with the standoff distance highly controlled—by shining that light and then having a system that will detect and measure the amount of electrons that get stimulated out of the metal, it can give you an indication of cleanliness. If there's a greasy surface or something that can't be detected on the surface of that metal, this will tell you there's something there because we're not getting the electrons to bounce off this metal.

When you're putting together a motor [by] bonding, where you're depending on bonds to do the job of holding the insulation in place, of holding the systems tunneled on the outside of the case, a lot of places where bonding becomes extremely important—having a device like this optical stimulator is a breakthrough. That's a good thing too.

We were able to do leak checks in a much more controlled and systematic way. Once the O-rings were put in place and the joint made, you had leak check ports where you measured by pressurizing the joint. We could measure down to just almost unimaginable small volumes if you were getting a leak, which brings up another subject.

Once you did a leak check port, you got to plug the hole. Well, the [specs] said you have to have redundant [seals]. So here you have a leak check port plug, and it's got redundant seals. How do you check the check plug? You drill a hole in it and test it. Well, then you got another hole. You find you're marching to infinity by putting a series of plugs to check plugs to check plugs. We bit off after checking the [primary] plug. We sealed it off with a small well-designed military plug with an O-ring on it. But we tested it six ways to Sunday in the laboratories and every way we could possibly think of to say, "Okay here is one place where we'll have one O-ring, but it's such a small and well-controlled plug that we'll stop there."

Also you turn to ultrasonics to measure case insulation bonds once they were done, and to measure thicknesses and that sort of thing used a lot of ultrasonics too. To say again, once this thing is built it can't be tested, and you're about to put a manned crew on it. It's a very sobering consideration.

ROSS-NAZZAL: Would you talk about the vertical testing? As I understood from the book that McDonald wrote, that was challenging because you normally test in the horizontal position.

MITCHELL: As I read from the NRC requirements, they said, "You should give consideration to testing in the vertical," which kicked off a very large debate. One of the deciding factors was the simple fact that when you put a motor in a horizontal position, which was the way we had been testing them historically, it sags. It sags [in the middle] anywhere from three and a half to four inches. That's about the same as the motor sees in the vertical position on the pad when they fire the main engines. When they fire the main engines, either as a flight readiness firing out on the pad or as we're getting ready to launch, the whole stack bends over. The motors are supporting the whole stack; the motors are the backbone of support to hold everything on the pad.

So that flex is an important consideration. The fact that in the horizontal position it flexes was a strong consideration in not going to the vertical. There were other considerations. One of the main things you want to test when you fire a motor is measure the thrust. Measuring the thrust in a horizontal motor is considerably more accommodated than trying to measure thrust in the vertical. It's really, really difficult to measure thrust when you're firing in the vertical position.

Also one of the things that the NRC considered was you ought to introduce those strut loads, the attachment to the ET [external tank] that introduces loads during flight, because of those struts. You can simulate those loads with hydraulic actuators to push and tug on a solid rocket motor. That is easier to do in the horizontal position. While the vertical got a lot of consideration we stayed with the horizontal tests, and that turned out to be all right. There was additional pressure put on NASA because the Air Force, they were building those huge Titans. They had built a new test stand out at Edwards [Air Force Base, California] to fire in the vertical. "Well, if the Air Force can do it, NASA can do it." But that went away after a [lot of study].

ROSS-NAZZAL: One other thing that I thought was interesting that McDonald had mentioned is that the rocket underwent six times more testing than the original certification program. How were you able to test so quickly but launch in a period of less than three years?

MITCHELL: I don't know what drove the original test program. We did a lot of network analysis trying to identify what drives test turnaround time, the time between tests. There were even things like concrete aprons. We had to pour new concrete at the test stand because the plume would burn it away so we found ways to improve that situation.

Of course a lot of the schedule compression had to do with risk. "We assume this test is going to be right, and we're proceeding on getting ready for the next test with the same hardware." It bit us with the outer boot ring redesign that never should have happened. We had to back up and backtrack. Most of the time it became a matter of we're willing to take the risk to go ahead and commit to the next test article, to the flight article, assuming that this test is going to be successful. Most of the time we were successful so we were able to turn around and bring on the new hardware and test it.

But if we'd failed then, we would have been in a cascade problem of recovering from those failures because we had already committed hardware for the next step in the program. That was a lot of it. The fact that we also hired or brought in a lot of new manpower to turn it around. I think in the paper I wrote I said we did five times the testing. If McDonald says six, okay.

ROSS-NAZZAL: Still that's impressive.

MITCHELL: There were a lot of [reflections]—it's always bothered me that the two simplest elements of the Shuttle program killed the two crews. We had the solid rocket motor. They were chosen for their established technology. Comparing the motor to the Space Shuttle main engine, everybody said, "If we ever lose a Shuttle it'll be because of the main engines. They'll shed a turbine blade at 28,000 rpm [revolutions per minute], or some preburner explosion. It'll be a main engine, because it's so complicated, so high-performance, so close to the edge of theoretical limits. It'll be a main engine." And here we've got the solid rocket motor, this giant firecracker, and we killed the crew. Then on the *Columbia* [STS-107 accident]—to take a virtually inert tank with maybe one or two valves but virtually no moving parts and you kill the crew. Somehow that's not the way it should have been. [There] should have been more attention to what was going on.

[Speaking of "what was going on," I learned of a troublesome action taken by a senior member of Thiokol for each static test motor in the original development and qualification of the SRM. Without any documentation or Quality Assurance notation or buy-off, he entered each assembled test motor on the test stand and, using a Teflon-tipped rod or dowel, tamped down any sign of blow holes in the zinc chromate putty in the joints. His alleged excuse, I am told, was that the sagging distortion of motors in the horizontal orientation made them highly susceptible to blow holes.

So the test motors were subjected to different, undocumented procedures than the flight motors. Mainly, it meant that the putty—and potential blow holes—were never fairly evaluated or characterized in hot firings of the original design. And to my knowledge, no vertically-assembled flight motor was ever inspected or evaluated for blow holes in the original program. I recall commenting to Dr. Jud Lovingood—he had been Marshall's deputy Shuttle manager at the time of the accident—to me, that seemed almost criminal. Jud was more philosophical, giving him the benefit of a doubt as to well-meant intent, even if it were misguided.]

[Sometimes] you wondered if we were snakebit. When we were shipping the first return to flight segments to the Cape we had a train wreck. Some people were killed in an automobile. That train—why that train would be involved in a wreck! Fortunately nothing really bad came out of all that [for the motor segments]. [Then] Pacific Engineering [& Production Company], who supplied the oxidizer that goes into the solid rocket motor, had a huge explosion out at Henderson, Nevada. Pacific Engineering ammonium perchlorate plant blew up. Killed the president of the company and one other guy. The explosion measured 3.5 on the Richter scale some distance away. It was a huge explosion. Fortunately we had a second source of [ammonium perchlorate], Kerr-McGee [Corporation]. That happened in May of ’88, just when we were cranking up to start producing motors again. “What is happening to us?”

After the redesign team disbanded I was still the project manager. I got a call one morning when I was about to head out to the [KSC] control room to sit in my mission management seat. I got a call saying, “We’ve had a joint heater fail. It shorted out. What do we do? Can we fly or not?” So I spent the morning putting together a package. Fortunately—back to the test program—Thiokol had run some tests where they had taken a heater cable and mashed it into the side of the case and deliberately shorted it. Then they measured the pitting that resulted. It was acceptable, and I was able to draw on that.

We went through other considerations. “Did this current surge affect nearby cables? Critical cables?” No, we were able to test all the way to the igniter filament itself and it’s in good shape. “Did this do other things to other [components]?” Then I had to brief [Robert L. “Bob”] Crippen and company on the net, and we were able to proceed with that [launch]. We knew someday that [sort of failure] might happen and sure enough it did.

ROSS-NAZZAL: You mentioned Bob Crippen. There were some astronauts who were assigned to follow the redesign effort.

MITCHELL: Right. Rick [Frederick H.] Hauck, Dick [Richard O.] Covey, Mike [John M.] Lounge, [David C.] Hilmers, and Pinky [George D.] Nelson—the reflight crew—we didn't see much of them. I know they kept up with what was going on. But as far as sitting in on our meetings, coming out to static tests, it just didn't happen.

We were assigned some really great guys. Steve [Stephen S.] Oswald and Dan [Daniel C.] Brandenstein. They both stayed very close. There was another astronaut—I [recall] it was Mark [N.] Brown. There was astronaut presence, and quite a bit of it, but not the flight crew. I'm sure the flight crew kept up, but we didn't see that. It was actually good [to have astronaut involvement]. And those guys were fair, they were attentive, they took what we were doing very seriously. They presented Silver Snoopys [award presented by astronauts] to me [and] some of the other people, [including Thiokol]. It was well appreciated. They did a good job.

ROSS-NAZZAL: There was also a lot of congressional interest in what was happening out in Utah. Would you tell me about that? Did you have to testify in front of Congress?

MITCHELL: Yes. We had one [particular] status briefing to Al [Albert A.] Gore's committee. John Thomas made that presentation. Dick Truly was there, I was there. Al Gore personally reacted enthusiastically about the J-seal. He thought the J-seal was wonderful. I regret not standing up and saying, "Dr. Joe Pelham did that." There was a lot of interest.

Then there was a parade of congressional staffers and congressmen who would come out to the Thiokol plant. I recall one whose name shall remain silent who said, "Fellows, you have to understand. We congressmen come out to a place like this to posture. We want to be seen, we want to appear to be on top of things." We had quite a number. Congressman [C. William] Nelson from Florida. Of course the Utah people would come by fairly often. Senator [Ernest F.] Hollings of South Carolina tried to interject himself into the proceedings some now and then. He didn't come to the plant. Some congressional people had agendas I'm sure from constituents regarding such things as the advanced solid rocket motor and other considerations that were politically motivated. But they didn't get in our way, not [seriously].

ROSS-NAZZAL: Would you tell me about the media interest in the redesign and recertification?

MITCHELL: Yes, there was quite a bit. Surprisingly I got a lot of interviews with Howard Berkes of NPR [National Public Radio]. He's stationed at Salt Lake City [Utah] and covers a huge part of the West. Nice guy, very fair. The local television stations—of course they'd show up for a JES (joint environment simulator) test and film it. Sometimes it was picked up by the national networks, sometimes they weren't.

I recall one broadcast that Dan Rather [CBS news anchor] talked about the results of a joint environment simulator test. He said, "It shows the redesign is not working." I don't know where he got that. I spent a lot of time preparing a rebuttal for whoever might call me. Fortunately nobody paid any attention to it. There was not an overburgeoning interest in the national media, [but] there was quite a bit of media attention when anything notable would happen it would get picked up. Static firings especially, full motor firings.

ROSS-NAZZAL: Was that pretty much at the DM [development motor?]-8, DM-9 test?

MITCHELL: Qual [qualification] motors as well. Before we flew there were five major tests. Demonstration Motor-8, which put in a J-seal, but it wasn't quite what we eventually wound up flying. We had most of the redesign in DM-8, [around 90%]. Then we had Demonstration Motor-9, which had the refined J-seal contour. It was in the flight configuration. That was the motor we had the outer boot ring failure on when we had tried to go fancy with the involute winding of that boot.

Then we had two formal qual motors, QM-6 and QM-7. (The numbers picked up as a continuation of the original program.) QM-6 was the sixth qual motor but it was the first of the redesigned motor, and then QM-7 was the first time we put in side loads to simulate the attachment to the external tank. That was something we introduced on the test stand to be able to do during the redesign program. [We had one solid rocket motor, full-scale motor, and it was one where we chilled the motor down really cold and then put in some worst case loads, but that was after we flew.]

All these things were excellent except for the outer boot ring thing that never should have been. Then we had what was called a Production Verification Motor-1 [PVM-1], just before we flew. It was tested in August, and we flew in September [1988]. It was the one that we flawed all over the place. Those five motors, DM-8, DM-9, QM-6, QM-7 and Production Verification Motor-1, were all really well done.

We had to abort one test one time because a fire hose came loose and sprayed water all over everything; [we] had to recycle and fire later. We had one time when the safe and arm device up front didn't rotate. It had been taken off the shelf after being in storage for like five years. Nobody bothered to set it on the bench and rotate it to see if it would rotate. Then we found out the really, really long cable all the way out to the test stand, there was a voltage drop in that cable. But overall, it was a tremendous test program, just a really enjoyable test program.

Plus one test I haven't mentioned so far. Here at Marshall there was a structural test article. We did that before the first flight of any Shuttle here at Marshall. We brought motor cases here to tug and bend and push and pull and show that the structure was good.

ROSS-NAZZAL: I wanted to ask you. When was the motor finally certified for flight?

MITCHELL: [Very close to flight. PVM-1.] We go through a very formal design certification review. That consists of taking all the requirements so you'll have something to judge against, taking all the analyses, all the tests, and of course the design itself—and you fan it out to the technical community at Marshall, [KSC, Headquarters] and at Johnson. You have review teams. A propulsion review team, a materials review team. They review all the design data, all the test results that prove the design, all the test results that anchor the analyses, just on and on and on. You've got guys with tweezers and microscopes who really, really dig into [it], and that's called the design certification review.

That process consists of each reviewer writing up, on a formal form, [any] review item discrepancy. It may be as simple as asking for clarification of something he's read or seen, or it may be a very critical serious review item discrepancy that has to be dispositioned. You generate hundreds and hundreds of these review item discrepancies, affectionately known as RIDs. You have to deal with every one of them. Some of them can be delayed because they're going to be covered through some subsequent test or something, but every RID has to be dispositioned as required to be cleared before flight. Again a kind of triage.

That process was finished up around July. We're going to be flying, but contingent [on] this flawed test, which everybody got very excited about, emotional about, and very interested in—that test did not happen till August. Once that test was done everybody said, "Okay. We're ready." We'd had this formal design certification review in the July timeframe. Then we had the flawed motor test, which was the icing, the pudding proof. So it went right up to flight time.

Of course there was still some open certification to be done even after first flight because we had a motor we flew in balmy weather but we had a test motor that was going to be chilled down to the minimums, and that was not fired till after we had flown. The certification process is not so crisp as okay, chop, here we are. It's an ongoing thing, but we did reach a milestone of saying, "We're certified for first flight."

ROSS-NAZZAL: Where were you when the return to flight actually happened?

MITCHELL: I was sitting in the firing room, and I was excited. When the vehicle took off, the pressure transducers data line went off for a while, and it was like the motors had no pressure. We could look out the firing room window and see the rocket climbing. Dick [Richard] Davis, Thiokol vice president, was yelling, "Does anybody have motor pressure?" Well, we could see the motor flying, Dick. It was exciting.

ROSS-NAZZAL: How did the motors perform on that flight?

MITCHELL: The motors performed wonderfully. There were two minor discrepancies. One, when we took the joints apart, this new so-called capture feature really had [a tight fit and had] rubbed against the clevis that it was holding in place. This capture feature—where it had rubbed the clevis—there were little tiny scratches, not very deep, not very long, couple of tenths of an inch long and maybe anywhere from 1/1,000 to 13/1,000 deep. You could see little bitty particles of metal so we had to consider is this significant or not. Finally it was [determined that] it was in a highly unloaded area and there was no threat to seal integrity because all those little particles of metal were still upstream of the [capture feature] third O-ring.

The other thing about the postflight—look at our first flight motors: we had added heaters, we had put a weather seal over the heaters, and then we had added cork on top of that. A little piece of cork, maybe a foot long and a couple inches wide and a quarter inch thick, had come off during flight. What was significant about that was that there was something that had impacted the orbiter and damaged a tile. There was some pretty serious erosion. As a matter of fact it could have turned out to be very serious if the flight had lasted [much] longer.

The damage to that tile was blamed [by some] on that piece of cork, which Thiokol refuted, which we looked at and didn't believe, and which other elements like the E tank [external tank] and the booster—booster elements are different than motor elements—[could have contributed]. The [booster] nose cone and the parachute compartment and all that is up ahead of us. That was a potential source for the impacts, [plus] the external tank. They had always shed some insulation and had caused little nicks in the orbiter so it was a big debate whether our piece of cork had caused the problem or not.

For the next flight we took steps to bond that cork better. It didn't matter whether our cork had caused it or not in the ongoing debate because we had fixed the bonding problem and the cork would stay on better. As later flights would show, it turned out it was the nose cap of the solid rocket booster forward compartment. The nose cap part of the nose cone had shed some ablative material that had impacted the orbiter and caused damage on another flight too. So we were absolved we felt in the solid rocket motor world.

I don't want to [tarnish] the beautiful results of that first flight. We took those joints apart, they were gorgeous. There was no damage. Nothing had gotten past the J-seal, the sealed insulation. Every joint carefully looked at, every joint just wonderful. And continued to be that way for many, many flights.

ROSS-NAZZAL: You served in this position until 1989, is that correct?

MITCHELL: Until 1990. I was sent to take over a new project that was in its fledgling state called the Advanced Solid Rocket Motor. It was to be built fairly close to the Marshall Space Flight Center across the state line into Mississippi. Quaint little town called Iuka, Mississippi.

Near Iuka there was an old nuclear reactor site ["Yellow Creek"] that the government owned and they were going to give it to NASA and we were going to build rocket motors on that site. The advanced solid rocket motor was to have features to make it superior to the redesigned solid rocket motor. I got a lot of kidding from Arnie [Arnold D.] Aldrich the AA [associate administrator] about here I had told him for years about how wonderful the redesigned motor was, and now I was coming in telling that I had something better. He was of course ribbing me about that.

The advanced solid rocket motor was going to be slightly larger in diameter, three, four inches. You can get a lot of propellant in that so it was going to have more performance. The performance was not only for payload, but it would close the gap on some of the vulnerable times during ascent when astronauts would have to do a so-called return to the launch site [RTLS] safety maneuver. In other words a main engine goes out at a bad time, you don't have enough energy to reach orbit, [or to] you cross the Atlantic [Ocean] and land in Spain or Morocco or some of the other transatlantic [TAL] emergency sites.

This extra performance was going to give the Shuttle enough energy to skip some of those times when the orbiter would have to come back to the launch site. [RTLS] is a very, very tricky job. I'm sure every astronaut who ever practiced that in simulators sweats it. [You must] wait till the boosters burn out, continue to let the main engine run to burn off X amount of energy, try to slowly turn the orbiter around, and get it flying back towards the United States, eventually separate the E tank, and then you glide down and land again at the Cape. It was just nightmarish what all had to happen during that. This motor was to decrease the chance of that ever having to happen.

In addition it was going to be a three-segment booster instead of a four-segment booster, and thereby you eliminated one joint. Not earthshaking, but an improvement. Thirdly, the joints were going to be of a different type, and the bolts that were going to hold these joints together—not pins put in from the side but bolts that were laid in longitudinally—there was going to be a dip in the joint so that these bolts would be on a circumference that was inside the main circumference of the motor. You've got a pinched-in part of the motor where the joint is going to be.

The reason for that pinching in, with the bolts inside the skin of the motor—that giant force trying to pull those segments apart happens when the motor is fired, that force is outside the bolts, and the geometry is such that the loads of firing the motor actually close the joint.

Actually close the joint. That was going to be the big break. We didn't have to worry about tracking deflections and O-ring resiliency because the joint was going to close. The astronauts that saw that said, "Hey, as long as the joint has sealed insulation we don't care what you do with bolts or pins." That was a [somewhat] demoralizing comment, but that was the way they saw it. Again a tribute to Dr. Pelham and his sealed insulation.

A manufacturing innovation for the advanced solid rocket motor was going to be a continuous casting process where the propellant was mixed real close to the casting pit and then piped to the motor, and you don't do it a batch at a time the way the redesigned motor was done where you mix up 600 gallons at a time, cart it over, pick it up, pour it down. Once you start the process it's a continuous process. You get good consistency throughout the whole segment. It was theoretically going to be a very efficient way to cast motors with a continuous mix process.

This advanced motor had been proposed before *Challenger*. As a matter of fact, there were some people who thought that Thiokol was feeling such pressure to be in competition with this advanced motor on the horizon, that that was one thing that drove their management to agree to launch *Challenger*, because they didn't want to get in bad with a customer that was playing around with a competing motor. That was some interesting politics and maneuvering that I didn't get involved with, didn't want to get involved with, because I never heard of the advanced motor. At the time I was still on the Hubble Space Telescope.

All this machination came to light later, which I found very interesting. I'm sure some congressmen had been for this motor due to constituents they might have had. Interestingly, the site for this motor was as I said in Iuka, Mississippi, which was in the district of Representative Jamie [L.] Whitten. Jamie Whitten was head of the congressional appropriations. Very powerful man in Congress. This was going to be built in his district, so I'm sure that had nothing to do with the location or anything.

About the time this program was up and really cranking and serious money was being spent, Mr. Jamie Whitten had a very disastrous stroke and had to step down. And [with] the continued good flights of the redesigned motor, finally the advanced motor was quietly abandoned. The people of Iuka, Mississippi who were dreaming of large influx of economic growth [were] disappointed again. That poor town. When there was a canal built from the Tennessee River all the way to the Gulf [of Mexico], which used the Tombigbee River as part of it, that had always been touted as going to bring great prosperity to the area. The Yellow Creek site for a nuclear power plant was going to bring great prosperity to the area, and then it went away. Then we were going to build rockets there and they went away. They've been through a lot.

ROSS-NAZZAL: You had mentioned that you were the deputy director for a while for the Space Shuttle main engines when they made the change to the Block I engine and the new designed turbopumps. Can you share with us some of those changes?

MITCHELL: Having worked on the solid rocket motors, I couldn't go to Thiokol and work for Thiokol. Dr. Joe [Joseph A.] Lombardo, who had taken over the main engines at Marshall after J. R. left, couldn't go to work at Rocketdyne. He went to work at Thiokol, so I went to work out at Rocketdyne and got interested in the main engine and became deputy. The program manager of the main engine at that time, very brilliant man, looked like he might be moving on further and I would take over the main engine so I took the job of deputyship.

One of my main jobs was to bring the Block I engine along with improved turbopumps. These turbopumps were built by Pratt & Whitney. The contract was given to Pratt & Whitney mainly to keep competition up in the rocket engine world. Pratt was given the luxury of being able to add a couple hundred pounds to the pumps, to use castings instead of weldments. There were many, many welds in the main engine. That was a concern to some people, although we seemed to have the processes and any crack propagation under control. The main engine was well built, had done a superlative job. But they said, "Okay, but we can eliminate X number of welds by these cast turbopump housings and turbopumps." They were rugged.

If we, Rocketdyne, had been given the luxury of adding a couple hundred pounds to the pumps, we could have beefed up the pumps too, but Pratt got the job. They did a good job. The pumps they built integrated well right into the engine, and I got a feather in my cap for getting that done ahead of schedule and ahead of budget, and they improved the engine.

The Block II Space Shuttle main engine was a significant step in safety and reliability of the main engine. It used what was called the large throat. One of the things that made the Space Shuttle main engine so edgy was it operated at such a tremendous chamber pressure. It operated right on the theoretical limit of how much juice, how much energy you could get out of the hydrogen and oxygen propellants due to this high chamber compression. When you're pumping [thousands] and thousands of gallons of propellant into a chamber at that pressure you have to have a lot of energy. So the main engine used preburners to generate energy to turn those turbopumps, and they ejected the exhaust from the turbopumps, which was hydrogen-rich steam, pumped that into the main chamber, added liquid oxygen, and you got further combustion. All that together produced [an exhaust of] a lot of high-pressure steam.

The Block II engine used this "large throat," which meant if you think of a nozzle, [it] characteristically starts out fat, pinches down, and then flares out again. That pinched area, the throat, was enlarged, which reduced the pressure in the [combustion] chamber, which meant all the pumps and all the rotating machinery and all the flow upstream of the reduced chamber could be relaxed, could be lower-pressure, lower-rpm, and the safety of the main engine took a quantum jump when the Block II came along. I'm sorry to say I did not get [deeply] involved with the Block II engine [because of moving over to the International Space Station], but it was a wonderful thing to have happen for the industry as well as the Shuttle.

What is still amazing to me is it was done without sacrificing performance. The energy and efficiency of that engine was just as high with the large throat as it was with the higher pressure. I think that's some kind of black magic that those geniuses that designed those things really did a heck of a job to do that.

ROSS-NAZZAL: As we close down today, is there anything else that you would like to talk about? Or anything I might have overlooked when talking about the redesigned solid rocket motor?

MITCHELL: From a personal standpoint, I was very proud. Proud of the team, proud of the organization, proud of the integrity, proud of the working relationships we were able to develop with the NRC oversight people, with the Aerospace Safety Advisory Panel, with the so-called Norton Committee—he headed up the NASA overview team—and all this parade of astronaut committees and astronauts themselves. We were able to work in a friendly, positive, cooperative, common goal way without hard feelings, without recriminations. When I moved to Utah, the Thiokol morale was devastated. The people were terrified, they already knew that

NASA was looking at an alternate solid rocket motor, they just knew—they were convinced that, "Well, this will put us out of business, it's just a matter of when." They were frightened, they were angry. Every negative emotion you could name was present.

That was another reason for moving the NASA team to the plant. We went out with a positive attitude. We didn't point fingers. We didn't ignore the white elephant in the room. We said, "Hey we've [all] dug ourselves a hole. But the only thing to do is get out of it in a positive way." So we did, and we worked. The cooperation and the get-it-done attitude and the work and the many hours and the sacrifices and all the things it took to turn the vehicle around and get it flying again was just the most satisfying thing you could imagine.

ROSS-NAZZAL: Could you say it was probably your most significant accomplishment at NASA?

MITCHELL: I recall commenting to Gerald [W.] Smith, who was the booster manager, as we left the firing room after STS-26 after the return to flight. The data stream had looked good, we left the firing room, and we were on our way back to the rental car. I said, "Well, Gerald, how does it feel to have reached the peak of your life?" Nothing will ever beat it. That's pretty much true, pretty much true.

ROSS-NAZZAL: Well, I thank you very much for your time today. It was very interesting.

MITCHELL: Thank you very much.

[End of interview]