NASA STS RECORDATION ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

MYRON "MIKE" A. PESSIN
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is June 30, 2010. This interview is being conducted with Myron (Mike) Pessin in Huntsville, Alabama, for the STS Recordation Oral History Project. The interviewer is Rebecca Wright.

PESSIN: After I retired, the NASA Administrator decided that he wanted to transfer all the Marshall [Space Flight Center, Huntsville, Alabama] Shuttle projects to USA [United Space Alliance]. USA was going to procure the tank, the motors, and the engines. After I retired, USA came to me and said, "We've got a three-month assignment to help write a transition plan." That lasted four years. But some Marshall personnel weren't interested in transitioning, so they fought it virtually every step of the way, and we ended up with a lot of sit-around time.

The Marshall External Tank (ET) Project people didn't really want us in their meetings. Lockheed Martin [Corporation] didn't want us involved. They wouldn't let me play solitaire on my computer so I started writing a history [*Lessons Learned from Space Shuttle External Tank Development: A Technical History of the External Tank*]. This was strictly on my own, not an assignment from USA. The only USA involvement was a couple of the secretaries would type stuff for me, unofficially. When USA finally decided NASA wasn't serious about transitioning the Shuttle elements they laid off Ken Jones, who had been the SRM [solid rocket motor] chief engineer, Dennis Godtsen who had been SSME [Space Shuttle main engine] deputy chief engineer, Jim Smith who had been SRB [solid rocket booster] chief engineer, and me. Since I was over 65, I draw retirement pay from USA—if you get laid off after 65 you draw some of the retirement pay.

I'd finished about three quarters of the ET History by this time, so I took what I had done, and gave it to the NASA ET project manager, who was a friend. He gave it to the next generation project people and they had me finish it. They put me on contract through Madison Research [Corporation]—Madison is a local firm—to finish it as a lessons learned document. Fortunately, Madison had an editor who corrected my poor grammar and a typist who did the typing for me. That's how it got published.

Then after the [Space Shuttle] *Columbia* accident [STS-107], the press found out about it and wanted copies. Of course, I wouldn't give them a copy, so they requested it under the Freedom of Information [FOI] provisions and got it put on the FOI website. I got calls from *Aviation Week* [magazine]; I got calls from *New York Times*, and the *Washington Post*. The *Orlando Sentinel* came by the house, ABC [American Broadcasting Company] called me—they wanted to do a special. By this time USA had brought me back as a consultant on the accident, and I didn't want to get involved in talking to the press because I knew the [NASA] Administrator didn't want anybody talking to the press except the members of the board. So I wouldn't talk to the press but they said, "We just have a few questions."

I said, "I wrote it as a lessons learned document for young engineers."

They said, "We're not engineers."

I was tempted to say, "Obviously," but I was too polite.

WRIGHT: You had all those years with the tank, but actually when you started out you were put with the engines. Give us a brief history about how you started.

PESSIN: When I got out of college in 1953, I went to work as a propulsion engineer for an aircraft company in Dallas [Texas] making Navy fighters and cruise missiles. However, my draft board decided I was more use to the defense effort as a clerk typist in the Army, so they sent me to a two-year vacation in New Jersey. At the end of that period I went back to the aircraft company in Dallas. In December of '58 this company lost two contracts which they had been counting on for the future so by March of 1960 the company had gone from 20,000 to 8,000. I was 8,001 so I was looking for a home.

When ABMA—the Army Ballistic Missile Agency—research and development organization split off from the Army and became Marshall [Space Flight Center], a lot of the people stayed with the Army so both groups had a lot of holes in their organizations. Chrysler Space Division was given the task by NASA of finding people for those holes. So I hired in with Chrysler Space Division, and came to Huntsville on July 1st of '60 when Marshall was established. I wasn't civil service; I was Chrysler.

Chrysler hired me as a propulsion designer. All my background had been in analysis and test, and I'm a lousy designer so after seven months I went to work for NASA. At that time Marshall had responsibility for all the light and medium launch vehicles, which were Scout, Agena, and Centaur. I ended up on the Centaur project. Somehow I ended up as a propulsion engineer, working in mission analysis, trajectory analysis and performance planning. I spent two years there. We flew the first Centaur, it blew up. [NASA] Headquarters [Washington, DC] transferred the program to Lewis Research Center in Cleveland [Ohio, currently Glenn Research Center]. Lewis offered me a job, but Cleveland in the wintertime was too cold for a good Southern boy and Lewis didn't really want us anyway; it's just that Headquarters was pushing it.

So I went down to New Orleans [Louisiana]. At that time they were staffing up the resident office in New Orleans—and I'm a native of New Orleans—for the Saturn program. The Michoud plant [currently Michoud Assembly Facility] was down there. It was left over from World War II, a 46-acre plant. Chrysler was given half of it and [The] Boeing [Company] was given half of it, and I moved into the office that oversaw Boeing.

Just from a brief historical point, the way NASA found that plant was interesting. We had a guy in our facilities office named [C. L.] Horton Webb whose job was to search out big facilities that might be useful for the Apollo program. He went down to this plant. During World War II, the plant had been active and then had been abandoned. Chrysler moved in during the Korean War and was making main battle tank engines. At the end of the war it was abandoned again.

There was one civil servant from Birmingham Ordnance District [Alabama] and a dozen contractors in this 46-acre plant. So Horton looked at the plant—40-foot-high ceilings, 125-foot column spacing, had its own barge dock, had its own runway. He said, "This is great. Can I have copies of the drawings?" They took him in a room and rolled up against the wall was every drawing that had ever been made of the plant. He just sat there and started unrolling drawings until he found what he wanted, went downtown to a blueprint shop, got a copy, brought the drawing back to Huntsville.

A couple weeks later, he was down there visiting. The phone rang, and it was [Wernher] von Braun's office. He wanted to have a meeting with a couple of senators and a couple of congressmen. Two days. So Horton asked the Birmingham Ordnance District [BOD] guys, he said, "Where's your conference room?" It hadn't been touched in eight years. Inches deep in dust, the halls were this deep in dust [demonstrates].

He asked, "Well, what about air conditioning?" The BOD people had a window unit in their office. They had no idea how to fire up the main air conditioning plant. He said, "What about food service?" They had an icebox with drinks in it. So he went out and he hired a janitorial service to clean up the place; he hired those people with portable air conditioning units that they use for weddings to stick an "elephant trunk" in the window; he hired a catering service for coffee and doughnuts; and he hired a limousine service to pick up the people at the airport.

He did this on his personal credit card. When he got back to Huntsville and turned in his travel voucher the Finance Office was disturbed. He said, "Call von Braun's office." Von Braun okayed it. Horton did the right thing. You don't tell a senator you can't meet with him because the place is dirty. To let a contract to get the building cleaned would have been a three-month job. The Birmingham Ordnance District guys, they didn't work for NASA, so he did exactly the right thing.

At that time Marshall was under von Braun and a very new agency, so most of the regulations hadn't been written yet. The philosophy was to get the job done, that was von Braun's approach. "We'll worry about some of the regulations, some of the purity afterwards."

I spent eight years there. I was badged to the local office, but I filled the role of what Marshall called propulsion and vehicle engineering. Propulsion and vehicle engineering is essentially the mechanical engineering lab in one of the MSFC technical laboratories. I filled the role of the mechanical engineering representative. I had been there for eight years, and by the end of the 1960s the Saturns had finished and all we were doing was storing them and preparing them for flight. Michoud had gone from a civil service workforce of 280 down to 35, and it was still shrinking so I figured I better find a bigger home.

I'd been working with the Shuttle Task Team as a representative from Michoud and there was a group which was doing systems integration. They had an opening for somebody in the mission analysis/trajectory planning world, which is what I had done on Centaur, and I had worked with those guys directly. They offered me a transfer, and I said yes. I put in the paperwork, got notified my transfer was approved, but I was going to work in the engine office. I said, "I didn't apply to the engine office." They said, "You're going to work in the engine office." The engine office had a representative who was the manager of the Space Shuttle engine in the task team. I went to work on his staff working engine vehicle integration.

At that time for the Space Shuttle main engine we had three contractors. These were Aerojet, Rocketdyne, and Pratt & Whitney. The contractors were finishing up their Phase B study reports. The RFP [request for proposal] was going out for the Phase C-D. Each contractor had some differences in their design. On the vehicle Phase B studies, the reports were open, and all the contractors got to sit in the other contractors' presentations. The engines were all secret from each other; the engine guys wanted everything proprietary.

When they established the Source Evaluation Board [SEB] for the Phase C-D, they took the three Phase B study managers, who were three of the most experienced people we had, moved them over to the Source Evaluation Board, and gave me all three study contracts to finish. At that time since everybody who knew one end of an engine from the other had gone over to the SEB. I was one of three people at Marshall who could talk to the contractors. I had all three contractors, and I had to keep everything secret from each other, and also make sure that I didn't tell them anything about what was going on in the SEB. I was so scrupulous there that if I'd see somebody from the SEB, I'd cross the street to avoid him. I made sure I knew absolutely nothing about what was going on.

WRIGHT: How long did that time period last where you had to be so guarded?

PESSIN: About three or four months. It was interesting because one of the contractors had electric valves, one of them had pneumatic valves, one of them had hydraulic valves. Pratt & Whitney had actually built a 250,000-pound-thrust high chamber pressure engine under an Air Force contract that they had fired a number of times. They had a lot of experience on what they call high PC [high chamber pressure]. Aerojet had some high PC experience on the M-1 [rocket] engine that they had worked on years before. Rocketdyne had no high PC experience so they made a little 1,000-pound-thrust chamber. And this 1,000-pound-thrust chamber was about this big [demonstrates], and operated at 3,000 psi [pounds per square inch], which is what SSME operates. All through their proposal they talked about their high PC experience, which I thought was stretching things a little bit. Rocketdyne won, but Pratt & Whitney protested and the program was delayed a year.

Then the orbiter and systems integration contract was awarded. Rockwell won that but the decision was made that the tank would be delayed a year to give the systems integration activities time to put the requirements together. During the time I was working engine [SSME], somebody got a very smart idea for the External Tank. They said, if we wait until we select a contractor and come up with his manufacturing approach and his facility requirements and write C of F [construction of facilities] budgets, it'll be three years before we can start doing any work. So let's come up with a Marshall in-house manufacturing plan, Marshall in-house facility requirements, and go in and start working C of F budgets before we pick a contractor, with the understanding with the congressional committees that when we pick a contractor if he wants to build it differently we will transfer the funds or reprogram them to the new approach. The congressional committees accepted this as being a smart way to go.

At that time [NASA] Johnson [Space Center, Houston, Texas] had given Marshall 12 tasks on Shuttle. They established task 13, and since I didn't duck fast enough, I ended up heading task 13, which was to develop this manufacturing plan. Not being totally stupid, I went over to Marshall's Manufacturing Engineering Lab and grabbed about four or five top-notch manufacturing engineers, went to the facilities office, got half a dozen facilities guys. We put together a manufacturing plan and C of F budgets for 1973 and '74. So when Martin [Marietta Corporation] came on board in September of '73, we had 14 million bucks [dollars] in the bank for C of F for them to get started.

Martin was pretty smart in the facilities world themselves in that they basically hired the Boeing and Chrysler facilities engineering organizations. Their position was these people have been operating this plant for 12 years, why bring in a bunch of new people who have never been here, who don't know the New Orleans environment? Obviously with New Orleans, like the Houston environment, it's quite different than it was in Denver [Colorado]. They hired most of these people so their facility requirements pretty much hit the ground running.

When Jim [James B.] Odom was given the task of project manager for External Tank, Jim grabbed me to work the facility requirements and manufacturing requirements. I moved right into Jim's office and got out of the engine world, which was really good for me because within Marshall there were a lot of very bright young propulsion engineers who had extensive engine background. My background was more in vehicle propulsion systems, and really these guys were far better qualified than I was and this was better use of the people, so I moved into the vehicle side.

I started on working facility requirements, facility implementation, manufacturing methods, manufacturing systems. This is in the project engineering side. Then as various people moved on, I gradually inherited loads, inherited structures, inherited propulsion, inherited many of the subsystems, and inherited tooling for a while. Then later on when we recognized that going to—at that time we had backed off from producing sixty ETs a year to twenty-four a year. Jim Odom set up a production office that Jerry [W.] Smelser headed up to go to twenty-four a year and they took facilities back from me. They took tooling and they took over the production side, and I stayed in the engineering side as staff to [Gene] Porter Bridwell.

Marshall at that time had project engineering functions and a chief engineer. The chief engineer was in the Science and Engineering Directorate. The project engineering reported to the project manager. I was in project engineering and stayed in there for a number of years. Then eventually [Thomas] Jack Lee, who was Center Director at the time, decided to combine project engineering and the chief engineer's office. Jack Nichols, who was the chief engineer at the time, went over to the advanced solid rocket motor project as chief engineer. And they combined both offices; they moved me into the chief engineer's role but I still had the project engineering function. My staff in the project office still reported to me but now in the Science and Engineering Directorate.

Jack took most of his staff with him so I ended up with mostly my project engineering staff now in the chief engineer's role, but I had both functions. In project engineering you were more concerned with the programmatic aspects. As chief engineer you were more concerned with interfacing with the Science and Engineering Directorate. In project engineering I usually was the primary interface to JSC systems integration, and some of the JSC systems integration people I had the good fortune to work with were outstanding—Dick [Richard H.] Kohrs particularly. Dick was great. Some of them I was not so fortunate. I did the external interfaces heavily as opposed to the chief engineer who was more involved with the internal interfaces within Marshall. I dealt with NASA Headquarters quite a bit.

When Jim got me in his office, I helped prepare the ET RFP. For the year gap between the orbiter award and ET, we did something that I think was sort of unique. We went to the contractors and said, "On Saturn these were the high-cost items of building Saturns. We now have to build a program laid out by Headquarters of sixty external tanks." We were going to fly sixty a year. We were going to fly twenty from each of the launch pads at the Cape [Canaveral, Florida] and twenty from Vandenberg [Air Force Base, California]. The orbiter turnaround time would be a little over two weeks per orbiter from the time it landed until the time it flew. That was the program.

There were five contractors who had expressed interest in bidding: Boeing, Chrysler, McDonnell Douglas, Martin Marietta and General Dynamics. We went to them and said, "Okay, tell us how you would build cheap tanks. The only proviso is that nothing will be held proprietary. If you give us something good, we may write it into the RFP." Well, the contractors were pretty open on this, I think primarily because they felt if we put it in the RFP that would give them a leg up. So they came in on a regular basis and told us how they were going to build cheap tanks and all the features. Of course, this helped me when I was writing the producibility portion of the RFP, because I had the education they'd given me on producibility.

Martin was quite strong in the standpoint that they had built 300 Titan rockets. General Dynamics put together an outstanding manufacturing plan, but later they decided to bid as a subcontractor to Boeing so they were not one of the four bidders we had. My role primarily in preparing the RFP and the SEB was in the producibility world. When the award was made I stayed in the production side, and I gradually moved over to the engineering side as other people moved on.

Regarding "design to cost containment," our role really was not so much design-to-cost. The program Level I requirement was that we build tanks for $2.3 billion per tank. When we presented this to our Center Director, Dr. [Eberhard F. M.] Rees, he was furious. He said, "You're robbing the taxpayers; you ought to be able to build them for $1 million apiece." I think he was a little bit naive on what production costs were.

Von Braun always had a philosophy that he always wanted an in-house project, so that he had his people knowledgeable about the state-of-the-art in manufacturing. This was extremely valuable because his people, when dealing with the contractors, were dealing from a position of strength. Too often the government, and particularly the Air Force, have a project manager who's experienced and half-a-dozen ROTC [Reserve Officers' Training Corps] second lieutenants dealing from a position of—I won't say weakness—but a position of not a lot of experience. And the contractors can snow them.

At Marshall, we typically had guys who were as strong or stronger than the contractor. I know the contractors were quite shocked when we would get started. We would go into a meeting and they would start, let's say, in stress analysis. Marshall would show up with a dozen PhDs in stress analysis. I don't think Johnson [Space Center] had that kind of depth. Johnson had some very, very good people, but they were very limited in numbers. Marshall could really bring in depth. I'll explain one area later on where that got very valuable.

WRIGHT: There were a number of areas where Marshall had that in-house experience?

PESSIN: In the area of manufacturing, one of the areas that we ran into is you have a very large tank, and you have to put foam on it. There are a number of approaches you could use. Boeing on the Saturn rocket stage—the S-IC—did not have foam, it was just painted. The S-II stage started off with a filled honeycomb that had to be bonded in place. It was very time-consuming, labor-intensive. They went to a sprayed approach, but because of the aluminum material they used they had to leave the weld lands bare when they proof-tested the tank. They had to proof-test it cryogenically, so they tested the tanks at Stennis [Space Center, Mississippi]. They filled them with liquid hydrogen. Then they had to go in and inspect the weld lands, after which they had to manually spray the closeout on the weld lands. I was told by the Rockwell manager from Mississippi that he had to rent every bit of scaffolding in southern Mississippi.

One of the things Rockwell had started looking at was a "barber's pole" approach. You rotate the tank past a spray gun that moves up, and you just wrap the foam around. Marshall Manufacturing Engineering Lab jumped on that. There's an old southern phrase, "We jumped on it like a chicken on a June bug." They had a ten-foot tank, and they'd done a very large amount of development work on that. We brought the potential contractors in and demonstrated it to them. We said, "Okay, this is what we've done." Not telling them that this is what you had to do, but this is what we've done and it makes a lot of sense to us.

We demonstrated that—forming these parts when you have curvature in both directions, is called compound curvature. Forming these compound curvature parts, dome gores and ogive gores, is rather complex. Boeing, on the S-IC, had hydraulically bulge-formed them, that is by taking a flat plate and with hydraulic pressure force it into a female die. We looked at that and we looked at explosive forming, which Langley [Research Center, Hampton, Virginia] had done a lot of. You take the flat plate and using an explosive charge force the metal, deform the metal, into a female die. We looked at peen forming where you take the flat plate and hit it with steel shot and force it into the die.

We looked at stretch forming where you stretch it over a male die. Boeing had the biggest stretch press in the world. When Boeing did not win the contract, we asked if they'd take subcontracts and they were not interested. The stretch press was integral to the 747 [aircraft] manufacturing, and we were going to be pushing it right up to its limits. Boeing said if we broke the press they couldn't make 747s. The 747 was a much bigger profit center than any work they could get from us. I completely understood where they were coming from, so Boeing didn't bid to make these parts.

Martin won the award and went out for competition. A contractor we had never heard of called Aircraft Hydroforming in LA [Los Angeles, California] won the competition. They used a stretch press made by a company called Sheridan Gray, but they modified it to fit our parts. Our parts were bigger than had been done, and they did a fantastic job. They became Martin's best supplier.

This approach of getting the contractors in, having them tell us how they would build cheap parts, this gave us a good reference to start the program. Some of the contractors came in and they were going to spin a one-piece dome. Well, 27 and-a-half-foot means you have to have a very large plate. You couldn't buy plate stock that big so they were going to have to spin it across a weld. But in fusion welding, the strength of the weld is half the strength of the parent metal, and spinning deforms the metal. When you deform the metal and you have a seam down the middle that's only half the strength. It's going to yield more than the rest of the metal, and you're going to have some interesting phenomena. They had people who came in who had not done their metallurgical homework and were telling us how they were going to do things, and some of them were rather pleased to leave after we got through asking hard questions. This was, to me, a valuable tool to bring the contractors in in an open forum.

We didn't have the other contractors listening, but we had agreed that nothing would be proprietary. We had Marshall manufacturing experts, Marshall materials experts, Marshall structural analysis experts sitting in these meetings, and they were free meetings for the people to ask questions. It wasn't the kind of meeting where you sit back and you just listen. We encouraged our people to ask questions, and after the first couple of meetings the contractors knew what to expect. They would bring in their staffs with the right backup. It helped us to develop a manufacturing understanding.

The four bidders who actually bid were Boeing, Martin Marietta, Chrysler and McDonnell Douglas. Martin Marietta had to me a big advantage. They had built 300 Titans so they had extensive experience on building pretty good size hardware. The Titan was 10-foot in diameter, 80 feet long. Boeing had built 15 S-ICs. Chrysler had built 15 S-IBs, but for S-IB the tanks were made by LTV [Ling-Temco-Vought] in Dallas, and Chrysler assembled the stages. McDonnell Douglas had built a number of S-IVBs but they again were smaller, and they used a very labor-intensive technique for insulating.

When the contractors bid, Martin bid this barber's pole approach. Chrysler said, "We will put the tank on a horizontal spit like a barbecue grill and use 50 spray guns and rotate the tank past them. It is difficult enough to get one spray gun to start. Trying to start 50 simultaneously we felt was unrealistic.

McDonnell Douglas said, "We will build an inside-out S-IVB." On the S-IVB, the insulation was preformed tiles that were bonded in place on the inside with a neoprene liner so that if the tiles were to break loose they wouldn't get into the propellant outlet. They said, "We will make preformed tiles, bond them on the outside." When you bond something you have to have a clamping force to hold it until the adhesive is set. We use a vacuum bag. We put a plastic sheet over it, seal the edges, and then connect a vacuum pump to suck the air out, and we have one-atmosphere pressure providing your clamping force. We call it vacuum bagging. But with hundreds of preformed foam tiles, they were going to have to do vacuum bagging, and to try and do that for sixty flights a year, we felt that was unrealistic.

Boeing was going to spray the foam but they were going to spray it under a shoe, and the shoe was going to smooth it. They'd have a nice smooth coating, but the foam wasn't going to be allowed to free-rise so you were going to have more dense foam. One of the big advantages of the foam is it's only a two-pound density material, two pounds per cubic foot. So with the Boeing approach, we couldn't figure out how they were going to maintain free-rise foam.

The Martin approach—they saw what we had done and they fed it back to us. It's good business, you can say. You see what the customer wants and give it to them, but we felt that was a relatively superior approach.

When the RFP was released, the orbiter tiles had not been finalized. The initial ET design only had foam on the side walls and forward dome of the hydrogen tank, and an ablator that Martin had developed for the Mars Viking lander on the aft dome. This was the heat shield for the Mars Viking lander. Martin had developed this material called SLA-561, which stood for superlight ablator.

That's all the insulation which was applied up to that point of the program. We had some ablator patches up on the intertank and the front of the hydrogen tank where the shock impingement came off of the SRB nose and the nose of the orbiter hit the tank. That was all we had. We had to eliminate ice. When the tiles were designed, they reported that an ice cube dropped four inches would crack a tile. We did not necessarily eliminate foam from coming off but needed to eliminate ice. That was the big driver. So we covered the LOX [liquid oxygen] tank with foam and we covered the intertank with foam. Then the program said, "On all of your protuberances, all the pieces that stick out that have thermal shorts where they could get cold enough to freeze moisture out of the air, we want those insulated also. But you've got 500 pounds of weight that you can use. Start at the front end. Work your way as far back as you can; go for 500 pounds." That was a rather strong challenge.

Some of the hardware was fairly straightforward, like the hardware at the back end where the orbiter attaches, the big thrust struts, the vertical struts. The forward ET/SRB attach fitting, those were fairly straightforward. The ones that were not—you have to remember that when the vehicle is sitting on the pad before you start to load, everything is at room temperature. The orbiter stays basically at room temperature. The tank initially shrinks to -423 degrees [Fahrenheit]. It shrinks several inches. It's shrinking with relationship to the orbiter so the forward attach, which is a bipod spindle, has to move. Then as you fly and you start putting warm pressurization gas in the front end of the hydrogen tank, it begins to grow. The bipod has to move back in the other direction. That says you just can't cover everything with foam. You've got to have the capability built in for this to swivel.

Foaming the side walls was straightforward; well, semi-straightforward. We started off with BX-250, the same foam S-II had used, but it has fairly poor high-temperature characteristics so we were going to have to underlay it with ablator. You put the ablator underneath because if you put it on top, the foam would sublime out from underneath the ablator and the ablator would then fly off. The ablator is a 17-pound density and it would definitely do damage to the orbiter.

We had to underlay a lot of the foam with ablator. Marshall's Materials Laboratory found a new supplier called CPR. I believe this stood for Chemical Products Research. They had a product, CPR-421,with much better high-temperature properties. We started switching over to it, but then a Professor [Irving N.] Einhorn from the University of Utah [Salt Lake City] pyrolized a whole roomful. When you pyrolize, you burn it in the absence of oxygen. He caught the effluent gas in a cryogenic trap and injected a rat, and it killed the rat. He published this report. CPR came to us and said, "Okay, we are shutting down."

We said, "What do you mean you're shutting down?"

They said, "We're selling this stuff to insulate steam pipes in office buildings. If there's ever a fire and anybody within a half mile could conceivably have breathed that smoke, we're going to get sued once this report is in the literature. So we are shutting down."

We went in, did some chemical analysis, and decided that the toxic product is a material called TMPP [trimethylolpropane phosphate]. We said, "Can you reformulate it without the phosphorus?" They did, and that became CPR-488. We had Southern Research [Institute] down in Birmingham [Alabama] do an extensive test program of trying to kill rats with it, and it was proven safe and it met the environmental restrictions, so we switched over to it.

Because of the environmental requirements for spraying BX250, you had to be below about 60 percent relative humidity in a moderate temperature band. CPR was totally different. You had to be below 20 percent relative humidity. The substrate had to be at about 145 degrees, the foam had to be at about 150 degrees. The air environment in the cell where you spray had to be—they actually wanted it hotter than 105, but because people had to go in there, safety officials wouldn't let us go above 105, so we had to greatly modify the facility capability of spraying it. We actually had to pass hot gas through the tank to heat the tank wall. Turned out that when we were doing the first tank, which was the main propulsion test tank, the facility wasn't available. So we sprayed it with the BX, which was much easier to spray, and that was the MPTA [main propulsion test article] which went over to Stennis. It wasn't going to see the high heating anyway.

Then we had to develop the capability to spray this other foam. We had a number of issues on spraying foam and on the foam manufacture. For nonmetallics particularly, you have suppliers and the supplier buys stuff from somebody, and he buys stuff from somebody, and he buys stuff from somebody. And it gets down to what we used to call bucket chemistry, two guys in their garage pouring stuff from one bucket into another. These subsuppliers have no idea where their product is going to be used. They have no idea what the effects of a change will be.

The prime supplier, he's selling us a product but he's not really knowledgeable of what the product's needs are. You run into great difficulty in the world of nonmetallics. I'll give you a couple of examples. CPR got bought out by a major chemical company, and this chemical company delivered a lot of material, and when Martin sprayed it, it was the wrong color. So we called the chemical company. They brought in the vice president of engineering, vice president of research, vice president of sales, whole bunch of senior people. We had a big meeting at Marshall. They said, "Well, we used to allow up to five parts per million of iron. Now we have limited it to two parts per million because most of our isocyanate is used in picnic coolers, and the lighter color, if it bleeds through the liner of a picnic cooler doesn't hurt. But the darker material, if it bleeds through the liner people complain."

NASA was 5 percent of their output, and the other 95 percent was other sources. So, they tightened down. We said, "Well, we don't know what the material that we qualified was since their specification was less than 5 ppm, not a discrete number. We don't know really how critical it is so we're going to have to go back and test it." The test series meant going into the wind tunnels at Arnold Engineering Development Center [Tennessee] and running a $1 million test series. The wind tunnels are quite expensive to operate.

They were very sorry about that, but they didn't offer to pay the $1 million. Really legally, they weren't liable. At the end of the meeting I asked them one question, and I got the only answer I'd believe. I said, "Are you going to make any more changes?"

They said, "You're a 5 percent customer. We are not going to let a 5 percent customer drive our market penetration for the other 95 percent of our capacity. We do promise to tell you in advance if we make a change, but we're not going to let you drive us." That's the world that you live in in nonmetallics.

The second problem that we ran into—the primer manufacturer, the guy who actually made the primers for the S-II, had been making primers for some time. Martin placed an order and the shipping paper was fine, cans were labeled properly, the very very preliminary test that's done at receiving said the material was fine, and it was released to the floor. The technician sprayed the 1,000-square-foot dome and said, "This isn't the same stuff I've been spraying." I think he got a bonus for reporting it, hopefully he did.

When he looked at that, they found the supplier had put the wrong solvent reducer in the can. How do you catch that? In Marshall's Materials and Processes Lab, the lab lead was a fellow named Bob Lynn, an excellent chemist. That was my world in the ET Project Office, so Bob and I got together. We said, "Is there some way we can sample this material?" Well, in the days I grew up, organic chemistry or wet chemistry was titration columns and very very slow, very detailed, very agonizing work.

Right in this timeframe, industry was developing a bunch of computer-controlled chemical analysis tools. Bob got with Martin and developed a plan and we bought Martin several million dollars' worth of equipment, each of which would sample for one characteristic and would give you a spectrum. We called that spectrum the fingerprint. The family of them, we called a signature, and we fed this into a computer. For each lot of new material that came in, we compared that to the previous lots, which would tell us if there was anything different.

Martin hired a very very bright young PhD who'd just gotten out of the University of New Orleans. She had been developing sampling techniques for the chemical industry. Located between New Orleans and Baton Rouge [Louisiana] is just one massive chemical industry, and she developed the sampling techniques for the chemical industry, so we set her up in a fingerprinting lab. Dr. Laurie Rando is her name, and she did a fantastic job. She's still running the lab, and prior to the ramp-down she had in the lab employees with two PhDs, half a dozen masters, and nobody with less than a bachelor's degree in chemistry. A group of tremendously qualified people.

NASA Headquarters Safety Office was so impressed, they gave the ET project $1 million to have Martin write a fingerprinting manual, which they, NASA Safety, gave to every NASA contractor. ATK [Aerospace Systems] uses it extensively. I think USA at the Cape does some of that work. For nonmetallic, we virtually have to do that because there are so many tiers of suppliers, and the guy three and four tiers down just doesn't know what his product is being used for. He's selling a product to a spec [specification] which calls out certain characteristics.

One other material we were using, one of the things we got involved with when we sprayed foam—the intertank is corrugated, and when you spray foam on it you end up with big blobs of foam on the top of the corrugations when you are spiral-wrapping it. So Martin came up with a plan. They said, “If we manually spray BX-250 between the corrugations and smooth it, and then put an adhesion enhancer”—foam won’t stick to itself once it’s cured—“we put an adhesion enhancer on”—and that was a material called Isochem—“and then we can spiral-wrap a smooth tank.” Worked very well for half-a-dozen tanks.

Then suddenly we flew one and we had massive chunks of foam coming off the intertank. Intertank is normally very benign environment. It’s not cryogenic, it has a heated purge so it’s roughly 40, 50 degrees Fahrenheit, and it doesn’t see high aeroheating. It’s a very easy environment for materials. Suddenly, we were losing massive chunks of foam, big chunks. The umbilical-well camera pictures from the orbiter looked like the craters of the Moon. What we found is the Isochem had changed one of their subcomponents. When they put the material on, it wasn’t completely curing, and then when the outer layer was sprayed over it—when you spray foam it’s an exothermic reaction. It generates heat as the foam cures.

That heat caused the Isochem to cure, which gave off gas. So you had gas pockets between the two layers of foam. What we ended up with was we found that if the pockets were less than five inches across they would stay on. If they were greater than five inches they’d come off. We went in and drilled holes through the outer layer into the pockets. We didn’t know where they were. We drilled holes on five-inch centers to vent the pockets so that that way we could keep on flying.

In this case the vendor made a change—this is again before we started fingerprinting—we didn’t know the change had been made, and he didn’t know what the effects of the change would be. In the nonmetallic, that’s the world that you have to fight. In metals, there’s a little bit better control.

WRIGHT: With the fingerprinting process, did you find areas as you went along that it proved its worth?

PESSIN: I’m not sure what the results were. I know that Laurie ended up building a much better capability than most of her suppliers. She actually does the work for the suppliers. They will send in stuff to her to be tested because she can do better and cheaper with the computerized equipment. She can run much cheaper tests than most of them could if they don’t have the computerized equipment. This precluded the suppliers shipping defective products.

It’s the real world in that nonmetallics are very very—I won’t say unreliable. But the suppliers, because they have so many tiers of subcontractors, they don’t really know what they’ve got. Most of the suppliers just don’t have the kind of money to completely sample all of their incoming products.

One other point. When we went to the CPR-488, which was the later version of CPR-421, we applied it to the side walls. It worked great. One of the tests we ran for the aft dome, which sees extremely high radiant heating from the solid rocket motor exhaust and the very high acoustics, was what we called a combined environments test. This was done at Wyle Labs [Laboratories] just up the highway here in Huntsville.

They put foam on basically a flat plate with liquid helium on the back side. Using liquid hydrogen gets a little dangerous to get the cryo [cryogenic] backface. Then you subject it to extreme radiant heating with heat lamps. You blast it with a horn at about 165 dB [decibels]. You mechanically strain the metal and then you see whether it comes off. Well, previously when we’d done it on CPR-421, everything was fine. When we went to CPR-488, the foam ignited and burned down to the substrate, and that caused significant concerns.

We didn't have another material so we came up with a temporary fix that said the critical condition—the dome is 1,000 square feet, but the outer portion is curved around the dome where it sees fairly high airflow velocities so it is only in the center that you have this burning condition where the airflow is stagnant. For the 800 square feet in the center, when we would build the tank, we would spray ablator on that 800 square feet and then spray foam over it. If the foam ignited, it would burn down, but the ablator would protect the tank from the heat.

The ablator is an interesting material. I mentioned it was developed by Martin for the Mars Viking lander. It uses heptane as a solvent. Heptane is white gasoline. Spraying white gasoline out of an atomizing spray gun is a challenging job. OSHA [Occupational Safety and Health Administration] Standards say you have to have ventilation that stays below 25 percent of the lower explosive limit. Martin puts a factor of two on that and gets it down to 12.5 percent of the lower explosive limit.

In this particular application we had to build a new building so we could spray this. At a pass we had to spray it 4/10 of an inch thick at 50/1,000. We had guys out there with spray guns spraying 50/1,000; then going back with another layer, another layer, building up 4/10. Everything had to be Class I Division 1 explosion proof. The guys had to be in full breathing gear. We had enormous ventilation system in there to stay below the lower explosive limit of the material.

We later found a new manufacturer who could make us a slightly denser—three-pound density versus two-pound density—material on the aft dome. This company was called NCFI, North Carolina Foam, Incorporated. Their primary product was sofa cushions for the North Carolina furniture industry, but they became one of our major suppliers, and they are really an outstanding supplier. They are small enough that they are interested in ET business. The big guys, really they were treating us as a cross they had to bear. For a large manufacturer, the size of our orders when we went down to four a year, they just weren't interested. When we started out at sixty a year, the whole world was interested.

When we started off on the intertank, Avco [Corporation] in Nashville [Tennessee] was making it. They have two big thrust panels on the side that are machined flat, and then have to be formed, then six panels that are skin/stringer panels. Avco was doing well on the skin/stringer panels but on the big thrust panels, they broke the first six they tried forming. So Martin pulled the work back to Denver and modified the forming technique and was making them at Denver.

Later we moved those. For the next buy, Martin moved them to LTV in Dallas. LTV made them for a while. When Martin went out for another buy, LTV said, "Your production rate has just gotten down so low we're not really interested. If you can find somebody else, we'll work with them to develop the process. If you can't find anybody else, we'll continue to make it." Which I felt was a fair attitude.

We went to Learjet, and Learjet made them for a while. Then on the next buy Learjet no-bid, so Martin pulled it back in-house. The point is that when you start off with sixty a year the whole world is your buddy. When you go to these low production rates, finding the suppliers, particularly from within the big aerospace suppliers, it's not economical for them to take these small jobs. They have to clear an area, maintain an area, maintain the tooling, and the skilled workforce.

The other point is if they do it, and you want to buy enough for twenty flights, they want to build the twenty flight sets and ship them; then shut down and go off and do something else. If you're going to build four a year, they don't want to deliver one ship set every three months;

they want to deliver the whole set. At Michoud we were fairly able to support this because Michoud is a big plant and we had a lot of storage space, so we did that as much as we could.

One other thing—when we started the program the aluminum industry was saturated. They were selling all the aluminum they could make, so they had a rule of thumb that said they would only deliver what you had bought the previous year. Of course when we came in, we hadn't bought anything the previous year. Fortunately, Martin had a little bit of clout and was able to get the aluminum industry to deliver materials for us. But 2219, the alloy we were using, is 6.4 percent copper, which from an aluminum refining process poisons their furnaces. So they would only run 2219 once a year. They would collect all the 2219 buys, deliver them once a year, and then shut down, clean the furnaces, and go back to making other aluminums.

We did run into one other interesting problem with Reynolds [Metals Company]. When you make the aluminum plate that we use, which is age-hardened and work-hardened—after the plate had been rolled, it is solution-heat-treated—that is you put it in an oven at 985 Fahrenheit to drive the alloying elements into a solid solution. Then it comes out of this 985F oven and has to be quenched in ten seconds. It goes through a water spray bath with massive water sprays top and bottom.

Reynolds ran into a problem where the lower spray nozzles were clogged so they were getting inadequate quenching. The way this was found—General Dynamics was building F-16s jets at the time. They were selling them to Belgium, and as part of the offset Belgium was going to manufacture some of the pieces. Well, they sent the material to the Belgians, and the guys manufacturing it, who were pretty knowledgeable—I don't know whether they were using manual machinery as opposed to automated machinery or what, but—they said, "Something's wrong with this material, it's too soft." So they went back and checked. Reynolds had delivered probably hundreds of tons of soft material to the whole industry. There was this massive exercise looking for soft aluminum by DoD [Department of Defense], commercial airlines, and NASA.

We were lucky. When we would get our plates, the plate would come out flat, and the machining subcontractor would machine the upper surface to get it absolutely smooth. Then they would turn it over and they would then machine the Ts into the plate. Because there was no need to rotate the plates, for us the soft part was on the bottom so when they turned it over, they were machining away the soft metal. So we lucked out.

A number of the other customers had major major problems. We had a team out chasing soft aluminum for six months. Marshall had an aluminum metallurgist, fantastic lady, named Hap Brennecke. Her name was Margaret, but she went by Hap. She had been a process metallurgist at Alcoa [Inc.] on 2219. Alcoa developed it for ABMA. ABMA had hired her, and Hap became Marshall's senior aluminum metallurgist. Hap had a unique personality. If you were humbly ignorant, she would spend days teaching you aluminum metallurgy, more time than you could possibly tolerate. If you were arrogantly ignorant, she had a tongue like a rapier and she would carve you to pieces. We had contractors come in who were arrogantly ignorant, and they were not pleased dealing with Hap. She spent six months chasing soft aluminum.

When the ET RFP was released, the orbiter tiles had not been finalized. In addition to the bipod, when you start to load liquid oxygen, the feed line immediately gets cold. It goes down to -300F. It shrinks with relation to the hydrogen tank, which is warm. Then as the hydrogen tank gets cold, it's 125 degrees colder, so it shrinks with relation with the feed line. As it warms up, it grows. All these brackets have to provide movement capability so you can't just encapsulate them in foam. The cable tray stays at room temperature but the hydrogen tank shrinks. All these pieces have to be able to move to provide some sliding capability or some movement capability.

We came up with designs on those. The designs, if done properly, would hold. Foam wouldn't come off. But one thing that we, Marshall engineering, Martin engineering, didn't do was a technique called process deviation analysis. You look at a manufacturing process and say, "Okay, if we deviate from this process, this step-by-step operation, if we deviate, what is the implication?" Then, "How do you detect that?"

NDE [non-destructive evaluation] on foam was virtually impossible because X-ray goes right through it. The technique North American used on the SII stage was one quality supervisor that used a half dollar coin; he'd go around tapping with a half dollar. We tried to automate that system. We actually had a thumper that would go in, and we would get the spectrum of the sound response and analyze it, and we tried using thumpers. Now they have developed this technique using the X-ray that they use at airports. But even that, that's still a raster system. It's very very slow. So really NDE for foam is very very difficult.

What we didn't do—and I say we, Marshall engineering, we, Martin engineering—we didn't really understand the process failure modes. We, Marshall engineering, Martin engineering, and JSC didn't understand the failure effects of foam coming off. Because as you remember after the *Columbia* accident, the program manager at Johnson says, "Oh that little piece of foam couldn't possibly have damaged the wing." You may have seen that on television.

All of us didn't understand the risk, but as far as the process—we certified spray technicians. We certified them to spray, and we gave them the design and told them to go spray it, but we didn't look at where the process could fail the design. The classic example is, if you left a void in the foam, that void trapped gas at one atmosphere, 14.7psi. As the vehicle would climb out, the pressure in the void would act on the projected surface area. That pressure-area term gave you a certain force. If that force were greater than the shear strength of the wall around it, it would pop a chunk loose. If it were less than shear strength, you'd fly and never know it.

We didn't go into the intensive analysis of the process and then the training of the technician to ensure they wouldn't leave the voids. After *Columbia* they did of course, but this basically was the cause of the *Columbia* failure—and I'm not faulting the technicians. I'm saying the technicians had not been trained, had not been sensitized to the risk in their process. It was engineering's weakness. Really Marshall engineering, Martin engineering, and to a degree Johnson, because when we dealt with the Johnson guys foam coming off was always a refurbish issue for the orbiter. It was never flight safety.

Two flights before *Columbia*, we lost a big chunk of foam. The ET project manager sent his deputy to the Program Requirements Change Board, Level II—Neil [E.] Otte was his deputy—to open an IFA, in-flight anomaly. The program said, "We don't need an in-flight anomaly, we know about foam coming off." The program manager turned to the ET project manager and said, "Do a study to see what you can do to improve it."

The project manager went one step beyond that. He directed Martin to submit an engineering change to fix it. Martin's chief engineer had just retired because, when Martin merged with Lockheed, the retirement system was such that a whole bunch of their people had retired in December, right before the *Columbia* accident. The chief engineer had just retired and he was brought back as a consultant to head up this study of redesigning the bipod ice protection system. But because we didn't realize the risk, and because Johnson apparently didn't realize the risk, we treated it as a routine engineering change, not as a "stop the world we got to get off," not as a "ground the fleet" type of change. And that change was in work at the time of *Columbia*.

In the area of feed line brackets—cable tray brackets, the bipod spindle—all those were difficult because they had to move. When we started, the program we had a choice. We could have gone to an active control system—that is, heaters—or a passive system. The active system would have meant that we would have had to power high current flow coming across from the stand the ground, through the intertank into these heaters. According to the Cape, we would have had to add wiring in the tower to get this high current flow, because at the ET umbilical we had no high-power circuits. We would have had to add instrumentation. The instrumentation would have been—in those days had to be hardwired all the way back to the Launch Control Center. It would have been tied into the launch processing computers, because laptops and PCs [personal computers] didn't exist.

Or we could go to a passive system, cover it with foam. We put a calrod heater in the spindle itself, but that was it. That didn't have any instrumentation attached to it, it was just a heater. We chose the passive system rather than the active because of the problems with an active system. Now they've converted and they've gone to an active system, of course.

The decisions were made, and the reasons were that the passive system was much easier, not just for ET but for the whole stack, the whole program. The issue of foam on the protuberances has always been a challenge. We felt that there were certain areas where you just couldn't perfectly seal it, and we would get some small ice issues. The program basically was accepting those. Since then they've come back, done redesigns, and fixed those. But the whole issue of foam and orbiter tiles has always been a challenge.

WRIGHT: Would you say the other challenge would be the weight component? That every time you designed it affected the weight of the tank?

PESSIN: Yes. Every pound of weight on the tank is a pound of orbiter payload. I'll go into some of the weight reduction programs we went through and discuss the different manufacturing processes and challenges related to them.

Delay of aluminum. I mentioned that the aluminum industry would only let you buy the previous year's supply. One thing Martin has done that to me was very smart, they would go in with a yearly buy of the aluminum for all of the aluminum used at all locations, and then have the aluminum mills drop-ship it to the various suppliers who are going to be machining it, rather than for each guy go in for his small buy and the mills having to run off a batch for this guy, batch for that guy. Martin goes in there, they identify all the different forms and shapes—whether it's a plate or sheet, or casting or forging—they buy it under one buy.

Of course that means Martin procurement people have to keep track of all of this. They have to know what the suppliers need, when the suppliers need it, and get it into their order. And it's a yearly order to be drop-shipped to the suppliers. It did work out well in that we had a pretty good response out of the aluminum industry. When you buy from jobbers (supply houses), traceability can be lost.

Regarding various tests and their uniqueness, we had two classes of tests. One was the overall system test. The other was a component test. From a systems test standpoint, there were two classes. There was the one that was ET-unique and the one where ET was just part of a system. The ET-unique were the structural tests. We tested the liquid oxygen tank with barium sulfate. Barium sulfate is drilling mud, which I'm sure you have heard of. When you have liquid oxygen at a 70-pounds-per-cubic-foot density at three Gs [gravity], it gives you the equivalent of 210-pounds-per-cubic-foot material pressing on the back of the tank. To do this we used barium sulfate, dense material.

We tested the LOX tank at Marshall in Building 4619. During the room temperature test, it tested fine. We tested the intertank at Marshall in Building 4619. We had big hydraulic jacks. We could load it, putting loads into all the different interface points. We tested the hydrogen tanks under the legs of the old S-IC test stand. We actually used the legs of the test stand as the load frame to react to the loads we'd be putting into the tank. We took the tank to 140 percent of design load for three different load cases filled with 53,000 cubic feet of liquid hydrogen and went to 140 percent or zero design margin, three times. You sit well back, because it was a challenging test. The tank passed fine.

We learned one thing, and this was later something that gave us a problem at the Cape. When we have the tank sitting between the two solids and we load hydrogen, the tank shrinks radially. So that aft dome is tending to shrink. Because the SRBs are anchored at the top and at the bottom they're stiff, this causes a tension load to go into this aft dome as it shrinks, causes the dome to go ellipsoidal. This ellipsoidal dome tends to deform and pop foam off.

We said, "Okay, where do we go from here?" We've got the hardware built for the first four, five vehicles. For the first six flights down at the Cape when we would stack the SRBs, we put the tank in place, attach it at the front end, and then using giant hydraulic jacks pull the SRBs apart, physically bend the SRBs—because they were fixed at the top and fixed at the bottom. We would bend the SRBs, put the tank in place, snug up the struts, and then release the load, which would put the tank in a precompression state so that when it went into tension from the shrinkage it would have to take up those compressive loads before it went into tension. We flew the first six vehicles that way.

None of us were overjoyed about bending SRBs. Fortunately we had stopped doing this before the [Space Shuttle] *Challenger* [STS 51-L accident]. If we had been bending SRBs, [Morton] Thiokol [Inc.] would have never accepted the fact that that was not the cause of the *Challenger* accident. But we had stopped it well before the accident—we went in and we added 800 pounds of aluminum to the ET aft LH2 [liquid hydrogen] dome to stiffen it up. That was the structural test program.

On the main propulsion test, that was at Stennis. An awful lot was learned there, but I think if you talk to the propulsion guys they can probably give you a better handle there. I talked to Len [Leonard] Worlund recently. Len was chief of propulsion analysis for years and years and years in the lab, and then later he was chief engineer for SSME. He took over after Otto [K.] Goetz, an extremely competent individual, retired. Former college professor and on the MPTA he's extremely knowledgeable. One other person you may want to talk to on MPTA here in town is a guy named Jim Bruce. Jim was one of Rockwell's senior managers at Stennis on MPTA. He's up here now but he was a Rockwell employee. Very knowledgeable on what was done on MPTA. I think those guys can talk MPTA better than I can.

The last test we ran was called the Mated Ground Vibration Test. This is where we stack the whole vehicle and shake it. We stacked it with one set of loaded SRBs. I say loaded—they were inert grain—and one set of empty SRBs. The loaded ones were to simulate the launch condition; the empty ones are to simulate the end of flight condition. What you're looking at is bending frequencies, bending nodal crossing points. What you're trying to do is verify your analysis. Your analysis predicts how the tank is going to flex; this test verifies it.

There were two interesting points to me on that one. Point one is we used the *Enterprise*. They flew it into Redstone [Army Airfield, Alabama], which is a very short runway, on top of a 747. We all got out there and watched. It was interesting. Before we handled *Enterprise* we wanted to make sure we could handle it safely; that when we took it off the airplane, put it on the road, our transporters, that when we lifted it into the stand—so we made a simulator which was just angle irons and steel. It matched the *Enterprise* overall dimensions and the weight and the cg [center of gravity], to make sure our handling techniques were safe.

After it finished its role, we put it in the boneyard. A few years later the Japanese came in. They were going to have a space fair, and they wanted *Enterprise*. Well, the Smithsonian [Air and Space Museum] had *Enterprise* up in Washington [DC]. They weren't about to turn it loose. [The Japanese] wanted a flight orbiter. Well, your friends at Johnson weren't about to turn them loose so they said, "Well, what about the simulator you had?"

We said, "Doesn't look anything like an orbiter."

They said, "Make it look like an orbiter."

We said, "Well, you can do that in Japan for a tenth of the cost."

They said, "No, we want something that was used in the program." So they hired [Teledyne] Brown Engineering [Inc.], a local firm in Huntsville, to put a fairing over this whole thing, make it look just like an orbiter, shipped it to Japan for their space fair, shipped it back. When you look out in back of this building in the U.S. Space & Rocket Center that is the orbiter on display—that's where that orbiter came from. The other thing we learned on MGVT [mated ground vibration test] —as part of the test, we loaded the LOX tank with water. LOX is 70 pounds a cubic foot, water is 62.4. It's pretty close. It's close enough to give you a good test. You can use what they call a Prandtl number to scale it.

When we loaded it, we had the vent valve open. We are just filling it, pouring water in a bucket. The ogive portion of the LO2 tank buckled, and we pulled a buckle about six feet long. So we said, "What's going on?" We went back and looked at Martin's model. The model said it should be stable under this condition, the weight of the water.

We sent a half dozen PhDs down to Martin, and they dug into the details of the model, and discovered the model had been hardcoded at 1.7 psi. The model knew that there was 1.7 psi pressure in the tank, even though when they ran it they input zero pressure. We went back and corrected the model, and the model said it is supposed to buckle. This is what was happening—when we had performed the qualification test of the LOX tank, we had tested it on the floor on a solid support. But when it was sitting on top of the intertank, the intertank has a big crossbeam. There are two hard points on the intertank. The rest of it is relatively soft. The weight of the water caused the parts that were relatively soft to push down, causing a tension load on this ogive, causing the ogive to buckle.

We put a requirement on the Cape that said, "Until we can fix this, always keep a minimum 1.7 psi in the tank any time there is any liquid on board." The Cape was able to do that, but one of the things you like to do when you've got propellants loaded is open the vent valve and let the propellant boil. As it boils it carries off heat and gets denser, and then you continue to replenish the material that is lost. It's called densification of the propellant. You do this on both the LOX and the hydrogen side. Well, we couldn't densify the LOX because we couldn't leave the vent valve open.

For the first six flights we did not densify, but Martin on the lightweight tank redesign went ahead and put 200 pounds of aluminum in the front end so that we could take the loads. This let us go back to densifying the propellant. Even now between 2 percent and 98 percent we have to keep pressure in the tank, because to stiffen up the whole tank would have cost 800 pounds. Working with the Johnson Systems Integration Team, we felt that the problem for the Cape of keeping 1.7 psi in—which is fairly straightforward for them—between 2 percent and 98 percent, was an acceptable issue to gain 800 pounds. We went in on the lightweight tank and actually added 1,000 pounds of aluminum at the ogive and at the back end to fix problems, and still took 10,000 pounds out of the ET, which is 10,000 pounds of payload. But we had to add 1,000 pounds of aluminum back there because of these things that we learned.

The component tests, like the big structure that is at the back where the orbiter attaches to the ET, all that was tested at Michoud. We set up all sorts of text fixtures at Michoud. I don't know of anything we really learned uniquely on that. One thing we did learn—we had excess margins in many cases. We were more than the 1.4 factor of safety. When we went to the lightweight tank [LWT] one of the things we did, we went back and scrubbed this excess margin out because the program requirement is a 1.4 factor of safety.

One other thing we did on the LWT that was unique, we said, "Factor of safety is there for four reasons." One reason is because the uncertainty in material properties, but we were using MIL-HDBK [military handbook]-5 "A" values. "A" values is where you have 99 percent probability, 95 percent confidence that the material is at least this strong—so we did not have to maintain a big margin for uncertainty in material properties. Next one is uncertainty in the ability to analyze the material, the structure. Well, we had a very very complex structural analysis and test program, so we said, "We don't have to maintain this high a factor of safety." The third one is variations in the manufacturing capability. All the tanks were proof-tested, and I'll go into that later on.

The fourth is uncertainty in induced environments—airloads, winds, that type of thing. So what we said on the lightweight tank, we would maintain a 1.4 factor of safety on the less well behaved loads but use a 1.25 factor of safety where we had well defined loads, which are thrust loads, pressure loads, inertia loads. Where we had less well defined loads like aerodynamic loads, wind loads, gusts, shears, we would maintain the 1.4. That was a break in the industry, but it let us take some weight out.

Another weight reduction we had—the pressurization system, which is controlled on the orbiter, was such that a single failure would get us outside of our control band. Dick Kohrs had the MPS [main propulsion system] group go in and redesign the pressurization system so that it would take two failures to get outside the control band. This let us proof-test the tank to the top of the control band rather than the top of the relief band, which is 3 psi higher. On a tank this big, 3 psi is a lot of weight so we were able to take some of that weight out. There were some new materials that had come along, new titanium alloys and new aluminum alloys for the secondary structure. Where we had demonstrated excess margin, we went in and took the margin out.

There was a fellow named Frank [S.] Boardman, who was Marshall's structural analysis chief for ET. Frank and I put in a requirement that wherever we ran a test we would test to failure rather than just test to 1.4, or test to failure or the limit of the test equipment because that told you what your margins were. On the Shuttle program, the loads seemed to change every other week. I talked to our loads chief at Marshall who had grown up on the Saturn program. He said if we had deliberately set out to pick a configuration as hard to calculate loads as it could be, we could not have done a better job than we did on Shuttle, particularly with the orbiter wing and tail being so sensitive. Johnson fine-tuned everything around the orbiter wing and tail.

Whatever else fell out we had to live with, so ET was getting loads changes on a weekly basis. By testing to failure, we knew what our margins were, and that helped significantly.

We later were able to come up with load indicators which Johnson loaded into their prelaunch computer, where we were able to tell them exactly what the tank capability was. If the upper atmosphere winds were so high that the load indicators were exceeded, we could not fly that day. We had several hundred load indicators loaded into the Johnson day-of-launch calculations. As you know they launch balloons and they calculate loads based on those balloons. With these load indicators, we could tell very clearly whether the tank could stand it or not. Prior to that, we had to have a team of stress analysts and loads experts standing by at Martin for every launch. They would sit out there every night, and when the balloon data came in, they would have to analyze it. That was a waste of money.

Of course the tank was involved with range safety. The Air Force range safety people are chartered by the President. Their goal is to make sure nobody on the ground is injured. When John [W.] Young flew Shuttle [STS]-1, he and [Robert L.] Crippen had ejection seats. John has said many times that the agreement he had with the range is when we took the ejection seats out, they were going to take the range safety system off the tank. The range did not do that. They reneged, or Young claims they reneged. I think they claimed they never had that agreement.

It was probably fifty or sixty flights later before we took off the range safety system of the ET. Range safety is propellant dispersion; it cuts the two tanks open and dumps the propellant. Since the tank has no propulsion system of its own and it just goes along with the orbiter, Young was quite adamant that the tank did not need a propellant dispersion system. The solids do, of course, but the tank didn't. Young was adamant about that and he fought that for years and years and years and finally won.

From a tank standpoint, the only thing we furnished on the range safety system was the linear shaped charges. The command receiver/decoder, the batteries, the antenna, the confined detonating fuse—all that was GFE [government furnished equipment] to us from the SRB project. We installed it, but as far as saving money on the tank, it was not part of the tank.

Regarding what was planned to determine the extent of the debris, I wasn't involved in that on the first flights, but when we went to lightweight tank, we had to go back and redo it. It was interesting. I was Marshall ET representative to the Range Safety Panel. The way the program works, Program Level II calculates the ascent trajectory and the ascent heating. They turn that over to ET. ET then calculates the reentry trajectory and they calculate the break-up altitude. I think Martin had a good background on this from their Titan ballistic missile reentry. They then come up with a debris catalog which shows how the tank breaks up and gives you a random scatter of the size and shape of the pieces, of the ballistic coefficients, of the lift-to-drag ratio, of the velocities, and the direction.

On the super light tank we said, "Okay, let's do a worst-on-worst analysis. Take the worst-case velocity, worst-case lift-to-drag ratio, worst-case ballistic coefficient, go in one direction. And then the least, go in the other direction, and let's see what the footprint is." The intact impact point, the point where it would have reentered intact, was south of Hawaii. So with the intact impact point south of Hawaii, the nose of the footprint was south of Lake Charles, Louisiana, and the heel was south of Borneo. It flew clear across Mexico, clear across Texas. That wasn't what we wanted.

We said, "Let's do a probabilistic assessment," and went back and did a Monte Carlo type probabilistic assessment, and ended up with a footprint about 1,600 miles long and about sixty or eighty miles wide. Range has to approve the analysis technique, and then Shuttle has to do an analysis before each flight and present it to the range, and the range has to approve that before they can fly. There's an international treaty that says you can't dump anything within 200 miles of a foreign landmass or 50 miles of a domestic landmass in the event of a no-failure. In the event of a failure if it hits in the surf, it is okay, but if it hits on the beach you violated an international treaty.

As far as what actually hits the ground, the extent of debris—during the second mission, the second Shuttle flight, that's when they were coming down in the Indian Ocean. There was a tracking ship in the Indian Ocean to look for pieces, but it was a Navy ship and they were charging by the day. The program kept slipping so Level II canceled the tracking ship. We have never had any hard data as to what actually hits the water, unless there is a Russian submarine or Russian trawler out there and they haven't told us. But we have had data from a Navy Cast Glance airplane watching the tank reenter; we've had Air Force tracking aircraft.

We have NORAD [North American Aerospace Defense Command] data that tells us the altitudes at which it breaks up. It breaks up in three phases. We have two reports from a South African airline pilot flying from South Africa to Australia who had been vectored off of the flight path by the notice to airmen and mariners. He saw the sky filled with glowing green pieces so he circled the airplane so all of his passengers could see it. This happened on two flights. He wrote us a very nice letter. But as far as what actually hits the water we have never had—as far as debris, we don't know.

There are seven titanium castings, which I'm almost certain survive. The big aluminum forgings where the SRB attaches at the front end, they survive in some form. Some of the other big aluminum structure where we tie the orbiter, I'm almost certain that will survive in some form. Some of the propulsion parts which are stainless or 21-6-9 steels, they I'm sure will survive in some form, but as far as having any hard evidence that's all conjecture.

WRIGHT: The tank was always envisioned as a throwaway item?

PESSIN: Yes, yes. To try and recover the tank would be extremely expensive. It is a thin-wall aluminum, you have to get it down to extremely low velocities to keep it from breaking up when it hits the water. In that case you would then still have an enormous refurbishment cost after it has been dumped in the ocean.

SRBs are a different animal. Remember, the tank operates at about 35 pounds per square inch internal pressure, so its wall is roughly 100/1,000 of an inch. SRBs operate at 1,000 psi. They are D6AC steel and the walls are quite thick, so they can take the water impact loads with parachutes. The tank cannot take the water. You have to get it down to virtually zero velocity, and that means in addition to parachutes you probably have to have a retrorocket pack, you'd have to have a sensor system, you'd have to have some sort of a guidance system to make sure the rockets were pointing down. It gets expensive.

When I was chief engineer, Ken Jones was the chief engineer for the motor and Jim Smith was chief engineer for the booster. We used to have discussions as to, is it worthwhile trying to recover the motors? Jim's comment was if we had started from scratch not trying to recover them, we wouldn't have had to spend the money to develop parachutes, wouldn't have to buy the recovery ships, wouldn't have to refurbish the motors and spend all the refurbish expense. Jim said it'd be a push. His judgment is we would not really be saving anything by recovering. This was a political ploy.

Ken is different. His philosophy is that by having the hardware to look at he feels that we may have detected some incipient problems that could have caused issues later. He likes the idea of looking at the used hardware. On ET, we would have loved to look at the hardware. Martin put in a proposal that instead of immediately staging a tank off to take the tank to orbit—and it's 50 feet per second short of orbital velocity at staging which would be a very small payload hit—and have astronauts go EVA [extra vehicular activity] and look at the tank insulation.

Eagle Engineering [Eagle Aerospace, Inc.] from Houston did a study for us. This was Owen [G.] Morris and some of his guys. It was doable but the Shuttle Program Office was not interested and that raises an interesting point. President [Ronald W.] Reagan issued an executive order that said that NASA would take the tanks to orbit and turn them over to industry if industry would come up with a good plan. We put an ad [advertisement] in *Commerce Business Daily* and had ten responses. The responses were everything from a consortium of 57 universities to a junior high school science class.

The consortium of 57 universities called UCAR, University Consortium for Atmospheric Research, is headed by the University of Colorado [Boulder]. It includes MIT [Massachusetts Institute of Technology, Cambridge], Caltech [California Institute of Technology, Pasadena], and all the biggies. Then there was an outfit called Global Outpost [Inc.]. These were the two that went forward.

The requirements were they had to pay $50,000 up front. They had to pay all of NASA's expenses. Both of them let contracts with Martin to look at taking the tanks to orbit. When the time came for them to put up cash, both of them backed down. That executive order is still out there I guess. The junior high school science class response graded 6th or 7th, and theirs was spelled correctly.

One thing that you'll find interesting, if you remember, Skylab started as a wet workshop where Marshall was managing it because we were going to take an S-IVB to orbit and outfit it in orbit. Then they came up with a plan that said we would outfit it on the ground. Well, Johnson took the position that this should be their job because it is a spacecraft. Because Marshall was using an S-IVB, Marshall stayed on as manager of Skylab. And Johnson has never forgiven us; any time we come up with an approach of taking a tank to orbit, Johnson sees Skylab, or "The Skylab Syndrome."

There is a very bright young professor from Smithsonian Astronomical [Observatory, Cambridge, Massachusetts] who wanted to take a tank to orbit and convert it into a gamma-ray telescope. Since it was being done in Alabama he was calling it GRITS. Marshall's advanced studies guys worked with him on it quite a bit. Again, he didn't have the money but that was completely doable.

When the Shuttle became operational—that was the sixth flight—Johnson recognized they needed more performance so they gave ET an action to reduce the weight of the ET by 6,000 pounds. orbiter was given a task to reduce the weight by 4,000. I think on [OV]-103 [*Discovery*] they reduced it by 2,800 and maybe on 104 [*Atlantis*] and 105 [*Endeavour*] they got to 4,000. Of course on 102 [*Columbia*], they didn't go back and attempt to rework and 099 [*Challenger*].

As I mentioned, we developed the lightweight tank. I mentioned what we did was to take the weight out of the ET. This was a very effective program. I was happy with it because I negotiated the nonrecurring cost in the contract at $45 million; it came in at $43 million. We delivered a 10,000 pounds lighter tank as opposed to a 6,000 pounds lighter tank requirement, and we delivered it one day ahead of schedule. Everybody said, "ET is no problem, don't worry about it, that's nothing."

Some other challenges we encountered—the stiction. We have ullage pressure transducers which are pot wiper transducers, there's a potentiometer with a wiper over it. The early ones would stick. We went into a slow ramp rate test at acceptance and solved the stiction problem. Not solved it, just threw out those that were sticky. Stiction stands for sticky friction.

Transducer output dropout—when the ET was sitting on the pad and just holding pressure in the tank, the ullage pressure transducers (the pot wiper ones) would tend to drop out, but as soon as you came off of that pressure they'd go back to working normally. Only one flight did we ever have a flight issue. There are four transducers with only three active during flight. The orbiter computer would check them very late in the flow, and if one of them was bad, it would switch in the spare. Only one flight did we ever have to switch in the spare, and that was interesting.

On the FRF [flight readiness firing] for 104, during the FRF we had three transducers acting up. Since it was an FRF we could pull them out and send them back to the lab. Tested them every which way you can imagine, examined them—they were from different lots; they weren't all the same lot. We could find nothing common about them. We put new transducers in that orbiter, and on that flight one of the transducers dropped out, and the orbiter computer switched in the spare. Never had it happen before or since.

WRIGHT: No, but it worked the way it was supposed to.

PESSIN: We got little sense hoses, different length sense hoses. We looked. "Could you have swapped the sense hoses? Their orifices? These dampened any oscillations in the pressure. Could the orifices have been swapped?" They looked at everything. Since we had the hardware we could examine it in depth. We did not find anything wrong, but it was only on that first flight of orbiter vehicle-104.

WRIGHT: Oddity.

PESSIN: "Changes to the lightweight tank." The paint was on there—there was some concern that ultraviolet radiation would damage the foam, so we painted the first tanks with a white UV [ultra-violet]-protective paint. We put a bunch of panels up on the roof of the factory at Michoud, let them sit there for three or four years, and also had some panels at the Cape exposed to the Cape environment.

After we'd had a chance to analyze those we found that the damage was a few angstroms deep. There's 7,000 angstroms to an inch. So the damage was insignificant. By taking the paint off we were able to save about 400, 500 pounds and a lot of labor, because you don't paint something that big casually. So we took the paint off. The Public Affairs Office was unhappy; they preferred the pretty white ET but it wasn't worth 500 pound as payload.

"*Challenger* accident." From an ET standpoint, we were relatively unhit. One of the things I did on lightweight tank that Mr. Boardman insisted I buy for us was a stress analysis report. On lightweight tank we had to redesign basically every part because we took the margins down so Martin was going to have to redo their stress analysis. The initial stress analysis, they had the job shoppers come in from all over the world, and each one documented the stress analysis they had done on their part in their own format and had it in notebooks, loose-leaf binders, every way you can imagine. When we went to the stress analysis report we put everything in a common format and had it all in a 17-volume document.

Because Martin had to redo all their stress analysis, I was able to buy this for $50,000, which was a tremendous saving. After *Challenger* Mr. Boardman took the 17 volumes and got two of his stress analysts in the lab assigned to each volume and sat them down and said, "Is there anything in there that could conceivably have caused the accident?" They scrubbed it, they finished that in a week. The programs which didn't have stress analysis reports I'm sure took months, but we were able to get that done immediately.

Also, in manufacturing you write rejection tags. When a rejection tag is dispositioned on the factory floor, it can be dispositioned to scrap up to a certain dollar value or it can be dispositioned return to drawing. But if you want to do anything else you go to a Material Review Board, which is a board chaired by the NASA quality engineering, includes the contractor engineering, contractor quality, and in the case of ET we had a NASA engineering rep if it was a fracture-critical part, which is over and above the minimum program requirement but we felt was necessary.

This is documented on an MRB [material review board]. MRB can buy basically an alternate design solution. If it's a weld defect they can buy a weld repair technique. If a hole is drilled oversize they can buy a larger fastener. If parts don't fit they can cause the parts to be reworked. But essentially what they're doing is buying an alternate design solution, and this is the reason on ET we insisted on having a government engineering rep, because we did not want quality to be buying alternate designs. We did that on Saturn, which I was never comfortable with.

We brought up from MAF every MRB. There were boxes and boxes and boxes of them. Had them in the HOSC at Marshall, Huntsville Operations Support Center, and sat down and had a team of Martin engineering, Martin quality, NASA engineering, NASA quality go over every MRB to see whether something in that MRB could have been a contributor to the *Challenger* accident. It was apparent that it wasn't. The foam on the tank is good for short periods of time, maybe 15 Btu [British thermal unit] per square foot per second. That's called Qdot. The Qdot of the gas coming off of the SRB was on the order of 700. It just cut through the tank. There's no way the tank could have begun to survive that. Now if it had been a little bit further around the SRM, where it wouldn't have hit the tank, we could have probably completed the mission before the SRB burned out. But as it burned it widened the gap. When that gap hit the tank, the Qdot was just—plus in addition to heating rate, it also was a highly aluminized propellant. An aluminized propellant acts as a grit agent so there's no way the tank could survive.

"The impact of the *Challenger* accident." Basically the one change we made was—in the fracture toughness world we proof-test every tank, we take it to 105 percent of design limit load. The theory is that if we have the largest crack we can't reliably find, at this proof-test level the crack will burst. If it doesn't fail, then it says we're good for eight flights. But during proof test there are certain areas which we can't fully get up to the required proof stress. So for those areas, we do a postproof NDE, non-destructive evaluation. We do a penetrant inspection. Say if you get 80 percent, that stress level is enough to open up the crack so that the penetrant solution will show it up. We had the philosophy we'd only reinspect those areas that didn't get fully proofed.

We pulled together a team of fracture experts, and made sure that it was headed by somebody outside of NASA, a guy from Northrop [Corporation] headed it up. We had Marshall representation, we had Johnson representation and Langley and Lewis and people from industry. Two changes they made. One is they recommended that all repairs be reinspected, regardless of whether they were fully proofed or not. Which is I think a good conservative position because most of your cracks are going to be in repairs. We've never found cracks in parent welds that did not have a repair.

The other thing is that Martin had put out a crack allowance handbook where if you had cracks up to a certain size they were insignificant and therefore they were allowing quality supervisors to buy them on the floor. This board recommended that you not do that. They recommended that you have an engineering review on all cracks, and that would be the MRB. They said all cracks had to go to MRB. We made those changes. It wasn't a major impact, but it was changes in the right direction I believe. That was basically the output from ET on *Challenger*. ET was a victim on *Challenger*, not the aggressor.

WRIGHT: If I can ask, because it was a couple years later, the super lightweight tank came on board. Would you like to talk about that?

PESSIN: As I mentioned, prior to the time I became chief engineer, Marshall had project engineering within the project, and we had a senior person, and I had half a dozen engineers. Some of the other projects had 15 and 20, for the SSME particularly. I had by far the smallest staff. I had some very very good people, some real self-starters.

At that time the chief engineers were in Science and Engineering, and they reported to the deputy director of Science and Engineering, and they were the interface with the lab experts. They had half a dozen people on their staff who were specialists in the different fields. When

Jack Lee made the decision in '88 to combine them, as I mentioned, Jack Nichols, who was the chief engineer, moved over to ASRM [advanced solid rocket motor], and I took over both roles on ET, that is chief of Project Engineering and chief engineer ET within S&E. My role within the project didn't change but I added the chief engineer role, and had to deal with Mr. [Robert J.] Schwinghamer.

Mr. Schwinghamer is a most unique individual. He is a man of strong beliefs. He's a Purdue [University, West Lafayette, Indiana] graduate. Excellent. He was director of Materials and Processes for years, and then deputy director of Science and Engineering. One other person that you may want to talk to is Paul [M.] Munafo, Dr. Paul Munafo. He was with Boeing and Chrysler, and he is a fracture mechanics expert. Within Marshall, the Structural Dynamics Lab did fracture mechanics. Mr. Schwinghammer, when he had Materials Lab, decided he wanted his own fracture mechanics expert so he hired Paul and brought Paul in. Paul eventually was director of Materials and Processes and got his doctorate on super light tank.

Interesting story. Paul's undergraduate degree was MIT. When he worked for Boeing he got his master's at Tulane [University, New Orleans]. Did his coursework for his doctorate, but Boeing would not pay for him to go spend a year on campus, and he couldn't afford it otherwise. So when Marshall hired him he went through Auburn [University, Auburn, Alabama] and got his doctorate, had to retake all the courses because he lost everything that he had taken at Tulane. When he was head of Materials Lab, because Auburn has a strong materials program, a lot of his underlings were Auburn graduates.

Super lightweight tank. The ET was chugging along pretty quietly. At one time Martin had owned an aluminum company. They'd sold off the company but kept the labs. Let me step back a few years—in the late '50s Alcoa had developed an aluminum-lithium alloy. This had been used on an aircraft called the A5J, which is an attack airplane for the Navy. It was a Navy strategic bomber when the Navy had a strategic role to deliver nuclear weapons. When the Navy's strategic role was taken away they converted these to reconnaissance airplanes and they flew extensively during the Vietnam era. They were Mach 2 airplanes. They were very good airplanes but they had a lot of materials problems.

The material gave them a lot of problems so the industry pretty much backed away from aluminum-lithium. The Russians had been using it extensively but the US industry backed away from it. Then when the composite world started coming along, the aluminum industry perked up and said, "Hey we've got to compete with the composites." Aluminum-lithium is a lighter, stronger alloy so they started developing aluminum-lithium. Alcoa developed one that is used on the [Boeing] C-17 [aircraft]. But when we looked at it, their alloy was not particularly weldable, and it tended to be laminar—that is if you made it in thick sections it would be in layers. The strength through the thickness, which is called the short transverse strength, was very poor so we couldn't use their alloy.

Martin in their lab developed what they call a family of Weldalites, which is lightweight weldable aluminums. They came up with an alloy that looked promising. They came to us and said, "Hey, we have this alloy. We would like to deliver you an 8,000 pounds lighter tank." We went to Johnson, and Johnson says, "We don't need performance. We can launch now on a due east mission with more weight than we can safely land on an RTLS [return to launch site]." So Johnson said, "Don't worry about it."

At the end of the year we had $1 million or so left over. We said, "Can we go buy some material and just get started? It could be very useful for the next-generation program." Johnson says, "We can't spend Shuttle money on next-generation." So we didn't do it. Then our friends on the International Space Station decided they wanted to have the capability for the Russians to fly directly to Station so they changed the inclination from 28.5 degrees to 51.6. That cost Shuttle 13,500 pounds of payload.

You have to remember the history of Station. Station went through about five design iterations, pretty much at congressional direction. The last one passed by one vote. If Station had come back and said, "We have to redesign again to a lighter configuration," I don't think Congress would have bought it. We were already off building the modules. Boeing was building the modules up here at Marshall. The modules were already being built to the previous weight. If we had had to go back and redesign it again, I don't think it would have survived.

Martin had been bidding against Boeing for the Space Station. Martin was going to build them in New Orleans and Boeing was going to build them somewhere else. We felt that that would open grounds for protest. So our local office New Orleans people went in and figured out what area of the factory could be cleared out, and we took Boeing down there and said, "Hey, we can give you this space." I was part of the team who went down when we took Boeing to the selected area. Of course Martin was very unhappy with that.

At every aisle Martin had people standing there to make sure the Boeing guys didn't depart from the team. We had NASA people escorting Boeing. We took them, showed them the area, and walked back. One of the Boeing people had been one of the managers on S-IC. His ex-secretary was now the chief of the staff for the vice president of manufacturing at Martin. They saw each other, went over, hugging and kissing. Martin guy says, "I'm not supposed to let them talk—what about hugging and kissing?"

WRIGHT: That wasn't in the rules. That's funny.

PESSIN: The NASA folks were just laughing their heads off. But Boeing chose to use some space here at Marshall. We had a Building 4755 that had been built for Saturn and was in essence empty, and we turned that over to Boeing and Boeing built the Space Station modules here on NASA property, and that way we didn't have to pay Boeing for space.

The Station was in a mode where if they went back to redesign there was a good chance that Station would be killed so we had to come up with performance. The Shuttle Program came back to ET and asked about the 8000 pounds of additional payload we had offered. They convened what's called a Nonadvocate Review Board. It was chaired by Bob [Robert D.] White from Johnson who was deputy chief of the Systems Integration Office. It included John [A.] Wagner from Langley who's Langley's aluminum-lithium expert, Tim [Timothy P.] Vaughn here at Marshall who's Marshall's aluminum-lithium expert, I think Neil Otte out of Marshall's structural analysis, and I was the chief engineer's rep on the board. Martin Marietta presented where they were. The aluminum-lithium experts said, "Yes, we're ready to come out of the lab."

This was the material that Martin had in their lab, and they had sold the production rights to Reynolds, but no production material had ever been made. So they said, "It's ready to come out of the lab. What's the schedule?" When Martin came into an empty factory with no workforce and no tools, they delivered the MPTA four years after they were turned on. So I said, "Four years is probably a reasonable time." That's what Martin had proposed. Johnson waited four months to turn us on, gave us 44 months to do the job. Since the agency had to have it ET felt that was an acceptable challenge, so we got started.

Since Reynolds had the production rights, we went to Reynolds to cast the first material. I was up there when they cast it, not that I know anything about casting aluminum. In fact Mike

[Michael E.] Lopez-Alegria was with us. One of the technicians there was Hispanic and Mike went over and started talking Spanish to him, and the guy had a grin wrapped around his head three times. Overjoyed that an astronaut was talking to him—Mike was in his blue suit of course. The first materials that Reynolds cast didn't have the same properties as the Martin material. So we said, "You need to do a Taguchi design of experiments to see what's wrong."

They said, "What's that?" Martin had a young lady here in Huntsville who, when we had to get rid of the Freon blowing agent to go to another blowing agent, she had headed up the design of experiments activities in the foam formulation lab here. She went up and taught Reynolds how to do design of experiments. Their material never was really consistent so every plate had to be tested. Every heat from the oven—the size of our plate is 20 feet long, 12.5 feet wide and roughly two inches thick—every heat was one plate.

For every plate they would crop something off the end. They would go in and they would EDM—Electrodeposition machining, which gives you very, very fine notch—and they would pull it to failure. Then they would take some more material, EDM a notch, and then stress it to the level that it would be stressed in proof test, stress it eight times at the level it would see during loading, stress it to the level it'd see in flight, and do that four times. On the last one we'd break it, and it had to break within a certain percentage of the original notch. If it didn't, that whole plate went back in the oven and got remelted. We called it simulated service test. We had to do a sim service test on every plate.

The first challenge is Johnson cut four months off the time. The second challenge, we couldn't make the material. When Martin took their lab material out to Aircraft Hydroforming and when Aircraft Hydroforming had tried to stretch it, the material was so stiff it broke the jaws on the stretch press. The energy released look like an earthquake, and it almost took the roof off the building. We had less time than we needed, we couldn't make it, and we couldn't form it. We tried to weld it. It didn't weld like anything we'd ever had. Had to have a back-side purge. Since most of our weld tools cast the material into a chill bar there wasn't room for a backside purge. We had to redesign all of our weld tools. Normally we would weld at four inches a minute. That was too much heat for this material, it welded ten inches a minute. So we couldn't weld it.

Martin had welded up three of these gores, and one of the problems you always have when you make a repair, the repair tends to shrink which tends to give you a flat spot. When you pressurize a compound curvature dome with a flat spot, the flat spot pops out and it pops the foam off. So Martin says, "Well, let's practice our contour repair techniques." There are two or three different techniques that you can use. They deliberately made a flat spot where they made multiple repairs on the same spot. Lo and behold, it cracked. Cracks in multiple repairs in aluminum is not unusual. But this cracked so wide open, you could see light through it, read a newspaper through it. So we couldn't weld it, couldn't form it, we couldn't repair it, we couldn't make it, we had less time.

When we tried to figure out how to repair it, we brought in the Edison Welding Institute, top welding people in the world. They looked at what we had and said, "You're never going to learn to repair this material."

We said, "Would change of weld wires help?"

They said, "No." So Fred [P.] Bickley [III], who was working on his doctorate on aluminum-lithium, was put in my office as czar of weld repair. Fred still has their letter in his file. We had to learn how to repair it. That gave us some interesting challenges, and this is something.

One of our bright young stress analysts, guy named Pat [Patrick] Rogers. Pat was a 4.0 graduate at Tulane. I'm a Tulane graduate, but I was not a 4.0 graduate. When you make a repair and then you develop the strength of the repair, you'll make a weld, and then you'll make a repair, and you'll cut a dog bone out of that repair, and then you'll pull it. You do a number of these to give you a statistical database. That's what you use as your allowable for the repair in your structural analysis. Pat says, "We're cheating ourselves, because in the real world, when you load that weld with a repair in it, the loads are going to redistribute. The repair is going to stretch, and the loads will redistribute, and the virgin weld will carry more of the load."

So we said, "That sounds reasonable." Pat had a finite element model that would show in color the weld distribution. We had a photo stress program that a visiting professor from the University of Alabama [Tuscaloosa] had developed. A coating that you put on and look at under UV. We put that on and we ran some 17-inch-wide panels. Started off on 2219, which was the old alloy that we thought we understood, and it did exactly what Pat was predicting. We got much higher strengths than we were taking credit for. Then we said, "Let's try that on aluminum-lithium."

We were shooting for about 32 ksi [kilo-pound-force per square inch]. It failed at about 17 or 18. What was happening, when you make the repair you get a certain amount of shrinkage, and the shrinkage was subtracting from the capability. The material was so stiff, the loads didn't redistribute. So they said, "What can we do?"

Well, let's get rid of the shrinkage. Before you make the repair, you have to scribe two lines of it, measure the amount of shrinkage, and then go and planish. Planishing is where you use a rivet gun with a mushroom head, and a guy on the other side of the bucking bar, and force the weld bead back into the metal, and spread the metal back out to get rid of the shrinkage.

Well, if you've got a 27.5-foot tank and you've got one guy on the outside with the rivet gun and a guy on the inside with the bucking bar and they can't see each other, and they're communicating through a headset, and if they get out of line, you'll be putting a moment into the weld. It's challenging.

One of the requirements we put in is that before you make any unique kind of repair you have to verify it on two 17-inch-wide panels. At the first R17 repair where we had made 17 repairs at one spot, to make the first test panel took 10,000 hours. The material is very unforgiving. It is not a user-friendly material. With friction stir welding it's a different animal. Friction is beautiful, but this was fusion welding.

But we were able to develop it, were able to deliver the 7,500 pounds lighter tank. The initial tank weighed 78,000 pounds. The tank that's flying today is 58,000. So that's 20,000 pounds of payload. The payload to Station is about 35,000; 20,000 of that came from weight reduction on the tank. The rest of the program, orbiter did some, but not a whole lot. The orbiter is so complex that they just weren't able to do much.

When you get into the *Columbia* accident—I had retired five years before *Columbia*. I really don't think I should be talking about the *Columbia* accident. The people you can talk to is Steve [Steven G.] Holmes—if you're looking at NASA people. John Chapman was project manager in the return to flight. John has now retired. Steve Holmes is currently NASA, and he was the materials lead within the project office. Scotty [J. Scott] Sparks was our materials guy in the lab, and then he moved over to the project. He's deputy chief engineer. Ken Welzyn's is chief engineer. These guys can talk in depth what went on in the *Columbia* investigation and return to flight, what they've had to do. They have done a fantastic job, but as I say, since I had retired five years before—USA brought me back as a consultant during the investigation, but as a consultant you're on the outside. I was not in a lot of the NASA-Boeing-Martin meetings because as a USA consultant I wasn't invited. I didn't feel that I should just go charging into meetings.

WRIGHT: That makes sense. You covered the *Challenger* very well. Is there anything else that you can think of adding right now about your experiences with the tank that we didn't cover? We talked about digital X-rays—did you introduce those at some point?

PESSIN: Yes, typically we previously had used film X-rays. That gets expensive, because the film is quite expensive. We did go into digital X-ray in some applications where you could put the digital receiver in. Of course this lets you expand and do a lot of work with the pictures. For the foam they went in and started doing a lot of work trying to analyze the foam capability.

One of the techniques that Scotty or Welzyn can talk about—you fire an X-ray beam in, but it hits the aluminum surface and reflects back. You're looking at the attenuation in the beam as it reflects back. This was to try and find voids in the foam. If you fire something energetic enough through the aluminum substrate, it just washes out, you don't see anything in the foam. There were two X-ray techniques that they have tried to detect foam. One is not exactly an X-ray, it's an ultrahigh-frequency.

Foam is such a good energy absorber that it's very difficult to get any meaningful data, but they do have those techniques. As I say, last time I was familiar with them—Scotty can tell you more—it was a rastering process. That means it's very very slow. When we're looking to do this on a production basis, you have to have the part in a location where you can do it, and you tie up—again, when you're trying to produce parts, you can't park something for two weeks while you scan it. You got parts that have to flow. I won't say you can't—you could, but it's not conducive to production flow so that's a challenge.

Really it's also workstations. You've got a limited number of workstations, and if you tie up a workstation for long periods of time, then everything else backs up. I know we were moving along pretty good—the initial cost of an ET was about 400,000 man-hours, about 200,000 of that on TPS [thermal protection system] and 200,000 in metal parts. At the time of *Columbia* I think we were down to 200,000 with 100,000 and 100,000. The man-hours had come way down. Now after *Columbia* the TPS cost just soared astronomically.

WRIGHT: I'm curious. I know there was an expectation to do 60 a year. How many at one time did you have?

PESSIN: At the time of *Challenger* we were building at about 17 I believe.

WRIGHT: A year?

PESSIN: We had backed off from the 60 to 24. We had done what we called a 60 minus 36. We had facilitized the plant for 60—whether we would have ever been able to get there is another question—but we put in tooling for 24. We left the open workstations so that if we needed to go up to more we had space to install more tools, but the tooling was supposed to support 24. As I say, at the time of *Challenger* we had ramped up to about I think 18. Again we didn't have the extreme TPS activities that we went into after *Columbia*, so we probably would have been able to meet 24.

WRIGHT: I'm curious how many of them are left, how many tanks?

PESSIN: Three. There are two of them set for the next two flights, and there's one that was in stacking position in the Vertical Assembly Building at Michoud during Hurricane Katrina, and some of the roof fell in and some of the concrete fell and hit the tank. They have gone back and repaired it since then and I believe it's been recertified for flight. But the way the program is set up, when we fly we've got to have a rescue vehicle on the pad, and that tank is the tank for the rescue vehicle. Now whether the program is willing to fly it without a rescue vehicle—they initially said, "We're flying to Station, we can park at Station." Then they backed off on that. You've got to have a rescue vehicle in case there's a problem before you get to Station. Of course you had to have that when you went to the Hubble [Space Telescope] because you couldn't really park at Hubble.

WRIGHT: No room at that inn, is there? Based on all the improvements and enhancements up until now, how long would it take from beginning to end to put a tank in place?

PESSIN: I can't say. I know in the welding area there's one tool that has been removed—we've been looking at this under the heavy-lift vehicle. If we go to a 27.5-foot heavy-lift, could we build a 27.5-foot heavy-lift?

Martin has done some studies on that, Marshall manufacturing engineering guys have done some studies, and the ET project. Basically from the standpoint of building the tankage, there's one tool that was removed to clear out areas for the Ares I upper stage. We think there's a work-around on that tool. There's a robotic friction stir weld tool that National Council for Advanced Manufacturing has down in New Orleans. We have been partnering with NCAM developing new manufacturing techniques. The state of Louisiana in essence built a tool—the state of Louisiana is partnering with them also. For the heavy-lift we have to build a thrust structure and other stuff, but as far as building the tanks, the tooling is basically there now as far as Martin is concerned. NASA put out a request for information, and Martin came back with a study. If you want to talk to Martin, the local Martin manager is a fellow name of Ron [Ronald W.] Wetmore.

Ron is an unusual individual. He was in the Naval Academy, and at the end of his junior year Admiral [Hyman G.] Rickover came to him and said, "If you get your grades up you can get into nuclear school." Ron said, "I don't want to go to nuclear school, I want to fly F-14s." Well, when he graduated, he got his grades close to that level, and he got assigned to nuclear school. I've later learned that there was one or two years there where Rickover didn't get as many candidates as he wanted, so he was drafting academy graduates to nuclear school. Ron was chief engineer on a nuclear ballistic sub and on an attack sub, was in line to be a captain of a nuclear sub.

He had a family and kids, so he got out of the Navy and Martin hired him as their chief engineer down at the Cape for a number of years. Then they brought him back to Michoud, and he's now heading up the Martin office here that has been bidding on stuff and he has two master's degrees. He's just an outstanding individual, with an unusual background. Very very nice person. Ron is an extremely competent individual.

WRIGHT: Well, thank you so much for being so kind with us today and offering so much of your time. I really appreciate all the information. It's been very valuable.

[End of interview]