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ROSS-NAZZAL: Today is January 31st, 2006. This oral history with Norm Chaffee is being conducted for the Johnson Space Center Oral History Project in Houston, Texas. Jennifer Ross-Nazzal is the interviewer, and she is assisted by Sandra Johnson.

Thanks again for joining us for a third session.

CHAFFEE: Well, it's a pleasure to be here. It's been a lot of fun.

ROSS-NAZZAL: Good. I'm glad you're enjoying it. We're enjoying it, too.

Can you talk to us about your involvement in the Skylab Project?

CHAFFEE: Yeah. I wasn't really involved too much on the Skylab Project. The center of gravity of that was the laboratory, and the Marshall Space Flight Center [Huntsville, Alabama], essentially did that, and they were using residual Apollo hardware to [build] the laboratory. The laboratory, as you know, was a refurbished S-IVB third-stage from the Saturn launch vehicle stack where they basically used the external structure, took the tank and converted it to a laboratory that the people could live in. A lot of the people at JSC did work on the guts of that, but as a propulsion guy, the propulsion stuff was done by Marshall, or [it] was the propulsion associated with the Apollo spacecraft, which had already been done and was behind us.

So the only involvement I had was after the launch of the laboratory where the external sunscreen was ripped off by the aerodynamic forces of the launch, [as] they got the laboratory to orbit. And the concern was they weren't going to have the environmental protection, particularly [for] thermal control of the laboratory that they needed. And the crew was scheduled to go in a short period of time, I can't remember, but it was a week or ten days or a two weeks. And Pete [Charles] Conrad [Jr.] and a couple other astronauts whose names I can't dredge out of memory right now were to go up, and there was almost an Apollo 13-like reaction at JSC to figure out what we could do to recover the situation and to preserve the ability of the laboratory to serve as a laboratory for thirty, sixty, ninety days and not overheat or overcool or that type of thing.

One of the things I remember (that I wasn't involved in) was that our Technical Services Division, which was responsible for the manufacturing activities that we had, and were very, very capable, was led by a good friend of mine, Jack [A.] Kinzler, who is a currently active member of my Alumni League. Jack and his wife are still around. He came up with basically a parasol or an umbrella that could be poked out, in a folded-up state; could be poked out the side of some port that was in the laboratory and unfolded and essentially act as a sunshield and serve, at least partially, the role that this sunshield that ripped off during launch would have served. I didn't work on that. It was just interesting to see that.

The part I did work on was that they were very concerned [about] this parasol and [a] solar array which hadn't fully extended when they extended it from the laboratory and that was one of the things that Pete Conrad and his crew were going to have to work on to try to get the thing to go ahead and fully extend and lock into place; and they took up some big bolt cutters and that kind of thing to cut the cable that they thought was restricting its full deployment.

They were concerned about the command service module as it approached the laboratory for docking to the docking port, that the exhaust gas, we call it the plume, from the reaction control jets on the service module, as you approach the laboratory, in order to brake or to slow down, you have to fire the four jets or at least two out of the four that are facing the laboratory just to slow your velocity or diminish your velocity as you approach. In a vacuum, that gas doesn't have any ambient atmosphere to mix with and slow down its velocity or absorb its energy, so that gas hits whatever you're going to dock with.

So, number one, they wanted to use very short firings or pulses as we talked about before of the ten or twelve-millisecond variety. But the concern was that the gas dynamics, you know, just the density and the velocity of the exhaust gas as it came out of the reaction control system engines and washed over the laboratory might tear up the parasol or damage the solar arrays or cause some problem with this fully un-deployed solar array. So one of the suggestions that was made was that we take a service module reaction control system thruster, hundred pound thrust, and take it over to our large chamber A thermal vacuum chamber in the high-bay area of Building 32 and put it down on the floor and point it upward and fire it upward. And in the upper part of the chamber, we would put a parasol or something that simulated the parasol and both take pictures and try to measure forces and temperature, heat transfer to [it].

Well, the fellow that had controlled those facilities—chamber A and chamber B in Building 32, are world-class facilities, truly wonderful things to see and were some of the very first things that were put in place when the Center was built down here in Clear Lake [Texas]. The guy, who was a Division Chief and controlled that, another dear friend, a guy named Jim [James C.] McLane, who had been the first Division Chief of that activity, was fully supportive, but he was also very, very concerned because, as I've told you in our previous discussions, the reaction control engines, when they fire short pulses, make this incomplete reaction product, which is this goopy viscous material which also has detonable properties and flammable properties and that type of thing.

We already knew from our development testing in our own propulsion vacuum chamber that when we'd fire the engine in there in an attempt to create enough of this material to try to trigger an explosion of the engine and then figure out how to control it, that when we went in after a test or a series of tests into our vacuum chamber, this residue material, we call it goop, was all over the inside of the test chamber, on the walls, and [dripping] from wires and pipes and this kind of stuff, and we'd have to have a significant cleanup activity to keep that from becoming a danger. So we knew that when we fired the engines in big old chamber A that we were going to dirty the [chamber] up and that it would be a significant effort to clean it up afterwards.

And Jim McLane, bless his heart, knew that, and he just went ahead and after some mild statements of concern and protest, we went ahead. And on a twenty-four hour crisis basis, we did put together a service module reaction control system engine, built up a test [fixture], a mobile test facility that had a fuel tank and an oxidizer tank and a helium tank and all the necessary valves and regulators and that type of thing, did all the analyses on it, did the safety analysis, took it over, put it in the bottom of the chamber A, pointed it up, put a parasol up on top, instrumented it, and did do a series of tests.

And it was absolutely amazing to see, and, of course, it's quite close. The dimension is about ninety feet from the bottom to the top. It was amazing to fire one of these very short pulses of ten milliseconds or something and see the dramatic movement that it caused the parasol. It was quite dramatic. It wasn't just a little wiggle or something like that. It was like it got really hit with a big gust of wind on the Earth.

But the forces we measured and the heat transfer and that type of stuff seemed to be within the capability of the hardware to absorb it, and so we did go ahead with that [in Skylab], and as you know, it was successful. The parasol was put out and deployed. But ever since then, Jim McLane—and Jim's still with us today and also a member of the Alumni League and the American Institute of Aeronautics and Astronautics that I'm a member of—and we occasionally still talk about that, and he complains bitterly about the effort that he had to expend for two or three months in getting that chamber cleaned up and wiping the surfaces clean and getting it back to the point where it could be used as a thermal vacuum chamber, and not off-gas all this nasty propellant residue. So that was a very interesting set of activities, one that's probably little-known outside [Jim] McLane and his organization and the propulsion organization, but it was an important step to take in demonstrating before Pete Conrad's crew went that we could do this.

And this was done over a period of four or five days that the requirement developed, zap, we went off and built this test stand that was mobile and hauled it over there and got all the analyses done, got the safety people to buy off on it and went ahead and did the test, got the data and were able to tell the program office what the results were. So that was a lot of fun, but it was a short, short-term project. After that, we just kind of watched with interest as the program went on.

But by that time, I was already working heavily on Shuttle, RCS [Reaction Control Systems], that kind of stuff, because the [Skylab and Apollo] hardware that I was involved in was mature, was being used for the Skylab missions, and we did, you know, I did, to some low degree, support the flights, when they would launch until they were docked. And then when they came back, we would support the entry, like watching the hardware performance and that type of stuff. But other than that, we just became like the public, looking at the evening news and that kind of stuff, press releases about what was going on and that type of thing. Many other elements of the Center were much more deeply involved in Skylab than I and the propulsion [and] power people were.

ROSS-NAZZAL: What was your involvement in the follow-on program, ASTP [Apollo-Soyuz Test Project]?

CHAFFEE: A little bit. It was very interesting to us that we would do this docking thing with the Soviets, and I was one of the people who was a subject matter expert for the reaction control system, and there were two or three of us that would meet with the Russians. I never went to the Soviet Union. I did meet with them when their teams came here. That was still in times of the Cold War, and I was a little bit apprehensive about divulging so much because we had spent many years and a great deal of effort learning the ins and outs and the vagaries of these hypergolic bipropellant thrusters, both the ablative and the radiation cooled, and had solved these things, called the pressure spikes and the zots and the iron nitrate buildup in the pipes and the filters and all these kind of things. And we would just as soon, at least I would just as soon, let the Russians spend their assets and their time figuring that stuff out for themselves rather than just giving it all to them for free.

But that wasn't the policy, so we were—you know, I expressed some questions about how much do we tell them, and they said, "Tell them what they want to know." So at least a couple meetings I had with them, it was clear that the person who came and who was their representative for the reaction control system was a very knowledgeable person, asked very good questions. And we provided them with sets of drawings and operational information and experience, flight data and stories of things that had happened and that type of thing.

Then when it came my turn to ask questions, I think this fellow's name was Veria [phonetic] or Varsa [phonetic], or it's hard to remember now, he claimed that he had no knowledge of their systems, that he had just been given this list of questions to come, and that, unfortunately, he was unable to answer any but the most simple of my questions, but that he would take a—if I would give him a list, he would, when the next time he came back, he would have complete answers. So I gave him a list. Well, the next time that we met, it was a different fellow that came back, whose name I don't remember now, who claimed that, well, Veria had never told me that you gave him this list, and so he also had a detailed list of questions and stuff they wanted to know, but pleaded ignorance that he was unable to answer anything but the very simplest [things], you know, like how many thrusters they had, what was the thrust. But as far as their operational experience and had they ever involved, this, that and the other, he claimed he didn't know. So, that always bugged me a little bit.

Interesting story. Those guys were living around here in apartments somewhere, and I saw him one day. I left work late, seemed like it was in the spring or summer because it was still light at six-thirty or something like that. And there was a little 7-Eleven type store on Upper Bay Road in that first block of Upper Bay Road. I think there's one still there at that location. And there was also a grocery store across the street from that [location] that's not there anymore.

Anyway, I saw this Soviet guy walking along, he had a bag of oranges, and I stopped and hollered at him and he came over, and we howdy do'd for a minute. And I saw all these oranges, and I said, "You must really like oranges. Don't you have oranges in the Soviet Union?"

He was highly incensed. He said, "Of course, we have oranges in the Soviet Union. We could get oranges anytime we want to."

It was clear that this guy hadn't seen oranges in months, probably, and it was going to be a real treat for him, and he was stocking up on oranges, but he wasn't going to admit it, you know, that type of thing.

So that's about the limit of my experience with ASTP I did. We were involved in looking at the entry problem when the crew in coming back actually vented their command module reaction control system, vented their nitrogen tetroxide tank too early or too late. I can't remember now, but I believe it's too early. And it got sucked into the cabin as they were pressure-equalizing when the cabin—when the command module comes down through the atmosphere, it's initially at the 4.3 pound per square inch atmosphere that they fly at. Then as it comes down through the atmosphere on the chutes, there's an in-bleed valve that allows atmospheric air to come into the cabin and bring the pressure up to whatever the outside pressure is. Well, they were supposed to—I believe they were supposed to have dumped their oxidizer, their excess fuel and oxidizer before they opened those in-bleed valves, and somehow they didn't. When they did get around to punching the button to blow out as much residual oxidizer as they could in the tank, some of that vapor was sucked back into the command module cabin and created a problem for them, a concentration of nitrogen tetroxide vapor, which is a brownish, reddish-brown-looking vapor, which they inhaled and it did cause pulmonary edema in all three of them.

It seemed like when they were recovered around Hawaii and went back to Hawaii, they did have to go into the hospital for some medical treatment of some lung problems. That turned out to be, I understand, fortuitous for Deke [Donald K.] Slayton, because they, in doing his examination, they found an incipient tumor in his lungs and were able to take that out, before that ever became a problem.

So I did get involved with looking at the dynamics of—when you vent the oxidizer, what's the flow pattern, how does it distribute itself around the spacecraft, how much could have gotten in there, what do you think the concentration was? We made vapor solutions or vapor mixtures of air and tried to get the crew to say, "Yeah, that's about the degree of brownness that we saw in the cabin, you know," and try to relate that to what was the concentration they got in there. But that, again, was just the effort of a few days and that type of thing.

Both Skylab and ASTP, I was just very marginally involved in, was a very interested observer of what was going on, but my primary effort was working on the Shuttle, Shuttle Program at that time.

ROSS-NAZZAL: And you started working on the Shuttle Program in '68, you said?

CHAFFEE: Well, no, it was more about '70, 1970, probably, it was. I believe the last time we talked, I talked about the experience we had over at Marshall the night that Apollo 12 landed on the Moon, when all these Alabama folk bought Chet [Chester A.] Vaughan and I all these martinis. [Laughter]

So at that time, we were already trying to work on the Shuttle activity, and exactly when it happened, I don't remember, but there was a—NASA by that time had formulated this approach to new programs of Phase A, Phase B, Phase C, D, kind of thing. Well they were getting into some pre-Phase A concept definition studies. I think what they wanted to do was define both a Shuttle and a Space Station. Max [Maxime A.] Faget, who was Director of

Engineering, had come up with this concept for a winged vehicle. We still didn't really have the piggyback [launch] concept that we ended up with, as I recall now.

But there was a tiger team formed, and a colleague of mine named [B.] Darrell Kendrick, who had arrived at JSC from the University of Texas [Austin, Texas] about a month or two before I got here, so he predated me and actually stayed around and worked several years past the date I retired in 1996, a good friend and a really, really good, smart engineer, but one of these low-key guys who over the years made dramatic contributions to the program and probably will not show up in any kind of documentation or anything, other than a mention like this, but really, really a great guy.

Darrell and I got assigned to this tiger team, which was led by a fellow named Jim [James A.] Chamberlin, who had been the first Program Manager of the Gemini Program. He was a Canadian engineer, a brilliant guy, but absolutely almost impossible in his interpersonal relationships and ability to be a manager and get along with people and [to] direct people satisfactorily and that kind of thing, but an interesting, interesting guy. He formed the tiger team, and we met up on the third floor of Building 36 in that part of the building that has no windows and all that kind of stuff. It was kind of a Skunkworks. We kind of called it a Skunkworks, al là the Lockheed kind of group that they had for years out there, and we were sitting up there trying to figure out what are the systems and how do we want to approach the Shuttle, what are the requirements, what are our system options, what are our geometry options, this type of thing.

So Darrell and I were working on that probably from mid-1970 on, because I remember Darrell and I going forward with Henry [O.] Pohl and Guy [Joseph G.] Thibodaux to talk to Dr. Faget about options, and we had looked at an option for using a hypergolic propellant system just like we had with Gemini and Apollo. We looked at a monopropellant system, which just used hydrazine, which disassociates catalytically to create a hot gas and give you thrust, and we looked at a system where the fuel and oxidizer would have been hydrogen and oxygen, either stored as liquids or stored as high-pressure gases.

We particularly liked, over all the initial studies that we did over a couple of years, we particularly liked the hydrogen/oxygen system for the Shuttle because we had long experience in Gemini and in Apollo with the difficulty of dealing with the hypergolic propellants. They were toxic. They were corrosive. Any time you dealt with them, you had to do what we called suit up in an SCAPE [Self-Contained Atmosphere Protective Ensemble] suit, which was a fully contained suit with breathing air and that type of stuff. If something leaked, it was a big problem. If it leaked inside, you had to immediately go in and try to neutralize it. The nitrogen tetroxide is an acid, strong acid. The fuel, which was a monomethyl hydrazine-type material, is a very strong base, so they eat things up. So if you spill some, you've got to go in and remediate that right away, and it's just a pain in the neck to deal with the stuff, and any liquid system has problems of leaking.

So when you fill the system up, you use things like quick disconnects. You essentially plug the fuel connector into the spacecraft somewhere so you can fill up the tanks, and when you disconnect it, there's a little bit of stuff there that can leak. And if the valves don't seat instantly when you pull the disconnect apart, it vents a little bit and dribbles and smokes and, you know, just a pain in the neck to deal with and worry about. So we looked at hydrogen/oxygen and said, "Well, this stuff has so much going for it. They're both cryogenic. Oxygen is mildly cryogenic—in minus 280 degree Fahrenheit." We were very comfortable about keeping liquid oxygen in a tank for long periods of time. We had that technology. Hydrogen is tougher because now you're talking about minus 420 degrees Fahrenheit, and that's a much, much tougher job to have a tank, which no longer can be a tank, it has to be a dewar with special provisions to keep the liquid hydrogen in there for any period of time, keep it from boiling off, that kind of stuff.

But we looked at high-pressure tanks and decided, we could keep significant amounts of oxygen and hydrogen as gas just under high pressure or as a—if we went to a pressure above the triple point pressure as a condensed fluid [which] is not really cryogenic. So we were very taken with the oxygen and hydrogen. It had better chemical performance, better propulsive performance than any of the other options, significantly better than the hypergolic bipropellants that we used in Gemini and Apollo. Neither of them were toxic. If it leaked, didn't matter [from a corrosion standpoint]. The combustion products were water and steam and hydrogen and oxygen, so they weren't toxic. So it had a whole bunch of stuff going for it.

The problem was that it was difficult to store. It didn't have the density, so you needed big tanks. And it is not hypergolic, meaning that when you mix hydrogen and oxygen together, they don't automatically ignite like the hypergolic bipropellants do. So we were going to, for the engine, and a very critical consideration, we were going to need an igniter that would ignite the combustion propellants every time, so we're talking about something like a sparkplug or a glow plug in a diesel or something like that. This is a big deal for a reaction control system thruster because they fire hundreds of thousands of times, typically, so you've got to have an igniter. You're not talking about a one-time firing like a boost thruster or three, four, five like the orbital maneuvering system on the Shuttle or the service propulsion system on the Apollo or something like that. You're going to have to ignite these propellants reliably tens of thousands to hundreds of thousands of times.

So that was an important consideration, it was one more thing you've got to put in there. You've got to protect the igniter from being burned up when the engine operates, and if it has to operate for sixty seconds or five minutes or something, you don't want to burn the end off your sparkplug or something like that. So that was an interesting consideration. But after a lot of studies, we decided that hydrogen/oxygen was our number one choice. It was a technology we didn't have, and so it was going to create a very interesting technology development program prior to going out for selection of a prime contractor and then subcontractor vendors and this kind of thing.

The other thing that we really liked about hydrogen/oxygen was it lent itself to a totally integrated propulsion and power system for the spacecraft, so we could use hydrogen and oxygen for the reaction control system jets. We could use that same hydrogen and oxygen for the on-orbit maneuvering, you know, the bigger engines, the five, six, eight thousand, ten thousand pound engines we were going to need for orbit adjustment and entry and that type of thing. We could use hydrogen and oxygen for the fuel cells, and in the case of the Shuttle, we knew we were going to have an aircraft that was going to fly back through the atmosphere. It was going to need a hydraulic system or something of that nature or an electric system, something of that nature, to drive those control surfaces and the other things that were part of an airplane that would function as it came down through the atmosphere. And we needed some device to create the power that drove those devices, whether they were electrical actuators or fluid actuators.

And so we designed something called an auxiliary power unit, which was a typical device on an airplane, an APU we call it, that would provide the power to drive the landing gears and steering and the brakes and the flaps and all that kind of stuff. That was a more typical part of an airplane. Well, it turned out you could also use oxygen and hydrogen as the energy source for the APU. So it looked like a really nice system with some possibilities of integrating a lot of things and have reaction control, main propulsion on-orbit energy, and auxiliary power energy all be part of an integrated system.

So we spent, a year and a half, two years doing in-house detail system studies for that. We had very, very productive contracts, study contracts, with TRW [Thompson-Ramo-Woolridge] on the West Coast and with McDonnell Douglas in St. Louis [Missouri], and Darrell and I spent a lot of time looking, working with those two companies. We looked at high-pressure systems, we looked at low-pressure systems, we looked at cryogenic liquid systems, and I'm convinced we could have made that work. But that may be the irrational exuberance of somebody who was deeply involved in something that was a lot of fun, because as part of doing the systems studies, I was also involved with the technology in developing the thrusters, in developing the APUs, in developing the tankage, and looking at the, you know, what are all the aspects of the system, insulation of the lines, the pressure regulators, all the different system valves, the relief valves, filters, what are all the elements of this system if we went to a fully integrated hydrogen/oxygen system, and we were really for that.

But as the program progressed, and I'm not sure when we actually nailed it down, but at some point in there, the administration under President [Richard M.] Nixon approved the Shuttle but not the Space Station, which was absolutely the rational thing to do because we couldn't do two programs at one time. And there was a formal establishment of a Space Shuttle Program Office, and another dear, dear buddy of mine was the overall Program Manager, Bob Thompson, Robert F. Thompson. I think you've probably have interviewed Bob Thompson.

ROSS-NAZZAL: Yes.

CHAFFEE: He's one of the men in the program who I have almost unlimited admiration for as an engineer and as a manager and that type of stuff. Bob just lives up here off Pineloch, by the golf course, got a house on the golf course, plays golf every morning, and I talk to him frequently about things and I stop by and visit and have a beer and we talk about how things have gone downhill in the agency since we were worked there.

But I was truly a little fish down in the bowels of the ship, and he was the Program Manager, Level II Program Manager, so he coordinated not only the JSC activity but across the agency and did answer to people in [NASA] Headquarters [Washington, D.C.]. But he was also always so receptive of my concepts and personally kind to me and respectful of my opinions and acted like he thought maybe I knew what I was talking about and that he should pay attention to what I told him. And when you're a young engineer, that does a lot for your ego. Anyway, he was a great guy.

But the reality of the program set in once we had a formal program, and now you've got schedules and you've got budgets and not only a total budget but you've got a yearly budget that the Congress keeps whittling on, and so when we looked at the development cost of the hydrogen/oxygen system, although I thought we were well down the development road. We had built and tested in-house thrusters that worked very, very well, and we already had the fuel cell technology in hand from Apollo. We knew how to store cryogenic liquids. We knew how to build high-pressure vehicles. We felt like there was no issue building an APU that operated with hydrogen/oxygen. The larger engine would have been an issue, but we felt like there was some technology work done there. We felt like we could deal with that.

Another good feature of the hydrogen/oxygen, if you stored this stuff as a high-pressure gas, you didn't have to worry about getting it out of the tank. You don't have a blob of fluid floating around in zero gravity where you don't know where it is. It's just the contents of the tank are high-pressure gas, and when you open the valve, it squirts out whether you're on the ground or in zero gravity or whatever. But the realities of the technical risk and of the likely program cost, as well as to some degree the size of the system, because hypergolic bipropellants are very dense materials and you can store a lot of energy in a small tank. For instance, nitrogen tetroxide has a density of over ninety pounds per cubic foot. The hydrazine is around fifty-six, fifty-eight pounds per cubic foot. So you can put a lot of propellant in fairly small tank.

If you're going to even high pressure, and we were talking six, seven thousand pound [pressure] storage tanks, you need big tanks. They themselves then become bombs, because the energy in a stored tank, if that gets penetrated by a meteorite or something like that and releases all that energy, you've got a big problem.

But anyway, the Program made the decision that we can't take the technological risk that we can actually do this integrated [oxygen/hydrogen] system and we can't afford the money it would take to develop it and demonstrate that it's going to work and probably we can't tolerate incorporating the size of the system that it's going to take.

So they went back to what we knew, which was the hypergolic bipropellants for the Shuttle, and by late '73, early '74, I think I was deeply involved then in developing the technology for a much larger engine. And the engines on the Shuttle vehicle, because of its size and its dimensions, were now not the one hundred pound thrusters that had come out of the Apollo Program. The thrust size nominally was eight hundred and seventy-five pound thrust, so these were much, much larger engines. Not only that, because the thing had to fly back through the atmosphere and go through entry aerothermodynamics, they couldn't be hanging on the outside of the vehicle like the service module thrusters were. They had to be inside the mould line and protected from the plasma of entry.

But they also had to be reusable. The engines in the Apollo command module were buried within the mould line, but they were one-shot engines. We could design them for a ten-minute total operating life, and then when the command module landed, it never had to do that again. These flights, on the other hand, we thought we were going to fly this thing a hundred times and the requirement to certify and qualify the hardware was times four, so we had to demonstrate that the hardware was good for four hundred flights or say that we got to change them out every so often in order to meet that.

So it was a technological challenge to come up with a larger pulsing reaction control system engine, one that was buried but still had an essentially unlimited life. But, we had immense confidence in our ability to do that, and the prime contractor, which was selected, again, was Rockwell, and we had been working with those guys on the Apollo Program. We knew all of the RCS guys there. And it turned out that the contractor that was selected to provide these engines was the Marquardt Company, which had built the service module and lunar module [reaction control] engine[s].

Let me digress a little bit. There's some interesting activities, because as technology efforts for a year prior to selection of the Rockwell contract and their subsequent selection of their vendor [Marquardt], we were running a heavy hypergolic bipropellant technology program, and we selected a six hundred [pound level]. We weren't quite sure what the thrust level was going to be, and so we selected a six hundred pound thrust technology demonstration program and went out for competitive bids and had kind of a Phase A, Phase B kind of thing. I was very deeply involved because by that time I was very well versed in that technology and, not only that, I always had a bent for program control and administration and have always been fairly facile with words, and my love and my ability to think logically would apply to program documentation and everything. So I was able in a very short time to go off and write a detailed specification for the engine, a detailed set of requirements that supported the specification, and all of the statement of work and all that kind of stuff, and then work with the procurement guys to put together a competitive procurement package.

And then I was chairman of the evaluation committee that read the proposals for this thing, and as I recall, we got four, maybe five, proposals. We selected a couple for a competitive evaluation, and then after evaluating that, we were going to down-select to one company for more in-depth design and fabrication and demonstration capability. And that worked out to be a selection of the Bell Aerospace Company up in Niagara Falls, New York.

I was also working on a lot of the ancillary technology, and I think we've talked earlier about the oxidation resistant coatings that have to go on these refractory metals, that type of thing. We knew that the thruster incorporated into the Shuttle was going to have to be a metallic thruster. It was going to have to be designed such that it could be buried within the exterior mould line of the vehicle, and we were going to have to manage the combustion process and the cooling of the engine so that it wouldn't overheat in a buried installation, all of which we knew was going to be fairly tricky but ought to lend itself to a straightforward engineering approach, and it did.

But the Bell Aerospace Company was selected for this technology program. I was the Program Manager, spent many, many months going back and forth between Houston and Niagara Falls. [It was] the first time I'd ever been to Niagara Falls. It was so nice that one spring when I had to go up there I took my wife with me, and we took a little honeymoon after the business meetings were over, that type of thing.

But I worked with the Bell Aerospace guys. We came up with a very nice, nice piece of hardware. It worked very well at the six hundred pound level, had good performance, looked like they could manage the heat. It could be buried in the mould line. We had some valves that would work. We had decided that we didn't need the very, very short ten or twelve-millisecond pulse of the Apollo Program, and we ended up homing in, working with the guidance and control people on what we called the minimum impulse duration of forty milliseconds. So the minimum firing of a Shuttle thruster was to be forty milliseconds, but since the valves were a whole lot bigger, you still needed—that was a significant engineering design challenge to build a valve that would handle the flow associated with six hundred pounds of thrust and which would go from fully closed to fully open and back closed again within forty milliseconds. So that was interesting. Anyway, we came up with a very, very nice program at Bell Aerospace.

Another interesting story, and I hope you don't mind if I digress. The guy who was the Program Manager at Bell, his name was Melvin, and I can't come up with his last name right now, but he had never been down to the Gulf Coast, and, of course, I went up there and they came down here for program reviews. And the first time he came down, we had a nice program review and he did a good job. We broke for lunch, and I said, "Let me take you over to a well-known place where we like to take our friends, over on the Bay, called Maribel's." So Maribel's was very well known at the time. I think it's still over there, but at the time it was a well-known watering hole for the engineers. It was a good place to go for a long lunch or after work for a beer or a drink or they had good food. And it was run by Maribel whatever her last name was, who had been Miss Las Cruces in 1825 or something like that. Anyway, she was well over the hill, but she was a delightful, delightful gal, wonderful hostess and a good conversationalist and everything.

Anyway, we went down there, and he ordered a plate of shrimp. Well, apparently in Buffalo [New York] when you order shrimp or shrimp cocktail, they peel them and devein them and bring them to you all ready to go. Well, down here, typically when you order a plate of shrimp, they just boil them and dump them on the plate and bring them out. So he got this plate of shrimp that had been boiled and were ready to be shucked and eaten. The heads were off, so it was, you know. But he looked at that for a minute, and I think I got a hamburger or something, and he picked up a shrimp and just put the whole thing in his mouth and started chewing and begin—you could see his frown developing on his forehead and all that kind of stuff. And he kind of gulped and swallowed.

I told him, I said, "Mel, let me show you a better way to do that," and so I showed him. I said, "You break the tail off, and you peel this outer stuff." But apparently he had just never encountered that in his eating experience, wherever he'd been before. It was a funny story.

So he was an interesting guy. He was a good engineer. I did have a little trouble with Bell divulging all of the information when we got to the point where Rockwell was going to go out for their competition. Rockwell had been selected as the prime, and they were going to go out in a competitive manner for selecting the vendor that was going to provide the reaction control system thruster to them. They were going to do the system themselves, and they were going to buy the components, the tanks and the valves and then put it together. They were going to be not only the prime vehicle contractor but the prime RCS system contractor themselves. And the technology program that we had developed with Bell was government [property], and all that information was government information, including stuff on coatings and materials, and we had looked at a lot of different materials, molybdenum alloys and columbium alloys and tantalum alloys and various coatings and all as part of this effort with Bell. And they were very reluctant to release all of this information to their competitors who were going to be involved in this competition that Rockwell was going to run. So Rocketdyne was going to be one [competitor], and TRW was going to be one, and, of course, Bell was going to bid. They were very reluctant. I had some legal trouble. I'd say, "Send me the information because we want to make this fully available to all of the other people. This is government bought and paid for information. We want to be sure that all of the vendors have access to this information, not just Bell Aerospace."

The package they sent me, I knew, was significantly incomplete, and I had to lean on them, and in fact had to go clear to the top of the Bell technical organization to say, "Look, you guys have got this information. I know you've got it because I've seen it, and now you need to divulge it in a way that I can provide it. If there's anything in there that's your own internal R&D [Research and Development] that you paid for that I didn't pay for, take that out. But you need to give me all of the information the government paid for, because we are going to give it everybody who wants to bid on this contract."

So at the time I got to that level, they recognized what their legal and ethical responsibilities were and did that, but it was—that was kind of a first for me to be involved in that kind of an issue and I, frankly, had stomped them to make them do that.

The follow-on to that was because of the fact that the Rocketdyne Division of Rockwell was going to be a bidder for this thruster, because they had built the command module and the Gemini ablative thrusters, which were buried installation, and so they said, "Well, we also know how to build metallic thrusters," and they had this kind of stuff, so were an obvious supplier.

And the Program Manager said, "Well, we have to be very careful here because Rocketdyne is a sister division of Rockwell Downey [California], and we know that there could be some pressure at the corporate level to, why don't you guys pick Rocketdyne, and we'll just keep it all in the family." So what they did was they told Rockwell, "We're going to put a NASA individual on your source selection committee," and that ended up being me. So I was told, "You go to Rockwell Downey for six months or however long it takes, and you're going to be part of their source selection committee and on the technical committee."

So as a result, I was deeply involved in writing their procurement package, their specifications, their statement of work, all of the supporting document requirements that go into—so you've got to supply this list of two hundred specifications on how you package, how you select materials, how you provide lubricants, all the stuff that goes into a procurement package. And I went out there and was very carefully briefed on don't interject your feelings, because Rockwell was a little goosey about me because I had been the manager of the Bell activity, and they said, "Well, this guy's prebiased against Rocketdyne. He likes Bell because that was his technology winner." So I had to go out and overcome that initial bias.

Then interestingly, the guy who was in charge of it from the Rockwell side, who was also the reaction control system supervisor, at least at the thruster level, was so overwhelmed with work and had never worked a procurement before that he really was kind of clueless about what to do, and I became—I was careful never to undercut him in meetings or in public or anything like that, but he pretty much figured out early on that my only job there was this procurement, whereas he had fifteen other jobs, too, that I knew what I was doing, that I had broad experience and more than he did, as a matter of fact. And I became the de facto kind of technical leader of this activity, not the de jure, and I was very careful never to give any implication, particularly because we did lots of reviews for the Rockwell management. I just kept my mouth shut and that kind of stuff, but Paul was very careful always, too, when he got a question that he had no clue about, he would say something, say, "Well, before I say something, let me make sure Norm, is comfortable with this," and then I would give the answer, and then Paul would then lay claim to it, say, "Yes, that's right."

And so it was a very interesting process. I had to be in Downey for a long time. It was over four months, and I think I did what NASA wanted me to do. I made sure the process was fair and open and that they had a set of requirements documents that weren't biased to any particular vendor, and then they obeyed their own rules and followed their own procedures satisfactorily. And I was absolutely amazed when the proposals came in and we started reading the proposals, because I was on the evaluation committee. When that came in, the Bell proposal, the design of [their proposed] thruster looked nothing like the thruster that had been the product of the technology program I had done with them for a year and a half, and I just couldn't believe that they had decided that with the little bit of maturity that we had demonstrated and the good results that they wouldn't have just built on that experience. The darn thing looked completely different, had a lot of flaws in the design, and it looked like some people who hadn't been involved in the previous program and must have played a strong role in the writing their proposal, and as a result, they were not a strong competitor.

And the Marquardt Company, which did a good technical job, was again selected. They had a good design. They had a good program proposed. Their costs were attractive and, among other things, the Rockwell guys, many of whom had worked in Apollo on there, were comfortable with Marquardt because they knew the people involved, and they were the same guys they worked with during the Apollo development. So Marquardt ended up getting the job.

But I always felt like I played an important role in making sure that was an open and honest and fair competition and that the government, through its prime vendor, got the best subcontractor and the best deal that it could, and it was good experience for me to be involved in that and see the procurement culture of a commercial company rather than the procurement culture of NASA, because as a young engineer I had been involved in probably fifteen or twenty procurements, because we did lots of little research contracts and I'd write the statement of work and evaluate the proposals and have to defend it and all this kind of stuff. So I mean I was very comfortable with that kind of process on the government side and was interested to see how it worked on the corporate side. That helped me and served me in very good stead because, as we'll talk about later, I was involved in some other major procurements at a very high level and those kinds of experiences and insights were really valuable to me, and to the government I think. So that's how we got into developing the thrusters on the Space Shuttle Program.

Then I was kind of anointed as being within JSC being the thruster manager to work with the Rockwell guys and the Marquardt guys. So I spent a lot of time at Marquardt, and we also, as I've told you, we had our own test facility here, and we not only had our own hardware that we built and hardware that was left over from previous technology work, but one of our goals early on was to always get the vendors to give us an early piece of hardware so that we could bring it in and independently evaluate its performance and not just have to take their word for it. We'd do our own tests, select our own test conditions, have our own instrumentation, all that kind of stuff, and they had no control over what we did with it.

That pretty much continued up through, you know, about '76, '77, kind of thing. I was the primary thruster engineer at JSC although Chet Vaughan was also an expert, and Henry Pohl was. Henry is an amazing guy. He's an expert on everything, and if he's not, he very quickly becomes that, just an amazing, amazing engineer. But we ended up—we had the same kinds of problems in different flavors on this larger thruster that we did on the smaller Apollo hundred pound thruster.

The larger a thruster gets, it's more conducive to a condition called combustion instability, and what that is is an oscillatory combustion such that if you look at the pressure in the combustion chamber. For instance, on the Apollo thruster it was designed to operate at a hundred pounds per square inch internal pressure in the combustion chamber. If you were to look at the pressure data measured by a pressure transducer that measured that pressure in the combustion chamber, the engine would light, the pressure would come up to a hundred pounds, it would sit there, and it might wiggle a little bit between ninety-nine and a hundred and one. But it would sit there, and when you closed the valves, it would drop down to zero.

When you get combustion instability and that, the larger the volume and the more inhomogeneous the propellants can mix in the thing, you get this. If you look at a pressure trace, it varies wildly up and down, sometimes like a sine wave but not necessarily. It can be an arbitrary shape of wave, but the pressure will go up and down. And if the nominal combustion pressure is supposed to be a hundred pounds, it may oscillate between twenty pounds and a hundred and eighty pounds and oscillate at a high frequency around the nominal pressure that you're trying to reach. But when that pressure oscillates as it goes up and down, it essentially scrubs the wall with a varying velocity of gas inside the combustion chamber and never allows the buildup of what you call a boundary layer or a stable layer of cool gas that you want to maintain against the wall. If the pressure is oscillating, you can never establish and maintain that layer of cool gas on the wall, which keeps the thruster within manageable temperature limits for the material system.

So one of the results of this combustion instability is that you burn a hole through the side of this metallic thruster. The temperature just goes up so much it overwhelms the ability of the protective coating and then it melts the metal or oxidizes it and you just make a big hole in the side and now you've got a mess. So combustion stability was a problem we worked on very much and had to incorporate things called acoustic cavities. They're essentially in the injector plate that I've talked about before that is the equivalent of a showerhead in your shower. That's where the fuel and the oxidizer are distributed in small streams into the combustion chamber with the hope that you get a very homogenous mixture that's reacting that doesn't have a higher concentration here and there that would cause this pressure oscillation and that type of stuff.

You put little holes in the face around the edge of the injector, and they act very much like the holes in acoustic tile in the ceiling, with the sound wave, which is what this is. The pressure wave goes in there, gets damped out, and doesn't reemit. So it's a way to damp out the energy of this pressure oscillation and keep the pressure oscillation either stable or at a much lower level such that you don't increase the heat transfer to the wall of the thruster by scrubbing and surging and discombobulating this layer of cool gas you'd like to have along the wall. So at the eight hundred and seventy-five pound thrust level, we were big enough that we started getting into some combustion instability problems. We hoped we wouldn't, and we hadn't made accommodations for that to start with. Later on, we had to go in and figure that out.

But the development was fairly straightforward. Given that, the valves had to be a little bit bigger, and we had some valve design issues to make the large valves open at the forty-millisecond pulse, that kind of thing, and then the thermal design such that it could be buried within the mould line of the vehicle and yet still have an outer surface temperature that was compatible with all the stuff that had to be around the outside of the thruster, that was it.

So by ’76, late ’76, we were pretty mature with that. There were still some issues to be developed. And about that time, I think it was, that Chester Vaughan, who I figured was going to keep me from ever advancing into any kind of a management position because he was smarter than I was and was only a year older, and suddenly he was selected to go off on a special assignment and do a special job as Branch Chief of the Thermochemical Test Branch within the same division, and suddenly the position of Section Head was open. And that’s when I really realized from a personal development and management capability situation that you can never, in organizations like this or probably in any organization, you can never foresee your own career path. You might have some ideas and some desires and work toward a particular avenue, particular direction, particular goal, but you can never foresee what’s going to happen.

And that time, that was a real eye-opener to me that’s saying, “You never know what kind of opportunity’s going to be presented to you.” You don’t know where somebody that you think is blocking your path is going to be rewarded with a better job or a special job or something’s going to happen over in another area of the organization that they need your capability that you never would have thought about, this kind of thing. And so although it had always been my personal instinct to always work hard and be prepared and try to wear the big hat and be aware of everything that was going around me and not just work on my little area, but be very aware of how it fit in, what the interfaces were I had to deal with, and take a very broad view of how my work contributed, which I always characterized as being ready for something. You know, when opportunity knocks, you need to be ready.

I felt like I was ready. I did apply. It was a competitive personnel selection. Several of the people that worked for Chester, as well as other people, applied, and I was fortunate enough to be selected to be the Section Head. At that point, then, my responsibilities continued to be the thruster, but now I also had a lot of other detailed responsibilities within that section. So I had to worry about tanks and filters and system valves and the thermal control stuff and the vibration environments and all of these kind of things that were the responsibility of the Section Head.

And it was a very large section in Henry Pohl's branch. I think it was called the Reaction Control Systems Branch. There were two sections. One had the reaction control system, and the other had pyrotechnics. The Pyrotechnics Section was headed by a fellow named Mario [J.] Falbo and had about four or five people in it, and that was a very focused technical group. They had a lot of components, but the technology was all the exploding kind of stuff, the exploding bolts and the separation devices and that kind of stuff. They could kind of go off and do their own thing. So Mario and his people were off doing that, and Henry knew what they were up to.

But the Reaction Control System Section had, at one time, seventeen people, and so it was a very interesting experience for me to go from being responsible only for my myself and my own performance and being a colleague, to go from being Norm the colleague to being Norm the supervisor. And it was a painful transition that—luckily one that I survived, and I read lots of management books and that kind of stuff. I had participated—the Center had been good enough up to that point to send me to management training, and I had been to a personal development program over two years that the University of Houston [Houston, Texas] put together and was lucky enough to be in a program with some people that I had very high regard for and [who] then ended up attaining high position within NASA, that kind of stuff. But I really learned a lot from this University of Houston management development, and it was called the management development program and [I] got the equivalent of a master's. It was a certificate. Got the equivalent of a master's in public administration, although they didn't—they give a certificate and not a degree, kind of thing.

So I had some awareness of these kind of things and of organization behavior and how to deal with people and that type of thing, but it was still a very painful transition for me to go from being just a friend and a colleague to these other sixteen people to being their supervisor, and now I had to critique their work. I had to write their performance evaluations. I had to go over their job performance, had to make assignments, and when there was a plum assignment, I had to pick somebody and aggravate somebody else, and this type of thing.

I remember the first week I was a supervisor; my first problem that almost overwhelmed me was two guys. When I moved into the supervisor's office, I had had a window seat [where I previously sat]. Well, two guys in the room, both of the equivalent seniority, both wanted the window seat that I had vacated. So I had to figure out, how do I do this? Why do they argue about this? Why can't they just figure out?

It took me probably a good two, two and a half years to fully internalize this concept that when you're a manager, you can no longer be a personal engineer to the same level that you were when you were an individual performer, and although I fully, fully believed that a technical manager can never give up a certain amount of personal technical expertise and responsibility. When you're an individual performer, your responsibility, I'd say, is 90 percent technical expertise and 10 percent manager. And when you end up as a program manager or division chief or assistant division chief or something like that, it's essentially reversed. It's probably 90 percent management, but you also need to retain that small degree of personal technical expertise that you yourself are responsible for. I believe personally you can never fully give that up, and that has been my experience.

So when I got to Section Head, it needed to be about 50/50 because I was still going to have thrusters and that kind of stuff. I found myself on the verge of having severe medical problems with stress and things like that, trying to be the supervisor of all these people to try to get my arms around all the things that they knew so that I could successfully manage them, not realizing that I didn't have to know everything that they also knew. I had to know them and what their capabilities and foibles were and this kind of thing.

Then the other thing I did was I always have held myself to a very high standard, and the work product that I feel responsible for and the standard that I put on it, I consider to be very high, and my supervisors always did, too, and I applied that same standard of quality and performance to these other sixteen people that I was supervising and in my previous experience had never really had opportunity to critique their work in the same way that you do if you're the supervisor and responsible for signing something they've written or concurring in it or something like that.

So I found out that these people have different levels of capability, greatly different levels of being able to think logically and communicate well, and this type of stuff, and I'd find myself taking work home every night. I always tried to be a good daddy, and I'd play with the kids and make sure we had our baths and read stories, and I'd give my wife a little time and say, "What did you do today?" And oh, yeah, and that kind of thing. But then after the ten o'clock news or something when she went to bed to read or something like that, out came the briefcase and I was sitting at my desk till two-thirty in the morning doing work every night. And I was working every Saturday some and every Sunday some, and the stress was—you know, I'd sit there and I'd correct the grammar on these memos that people had written and all this kind of stuff.

And it suddenly occurred to me one night I was sitting there at two in the morning working on a detailed technical report on a test that one of my better engineers had written, but his presentation of the results was not very good. It didn't flow well in a logical format. His conclusions weren't easy to pick out and this kind of thing. I was sitting there reading, it suddenly occurred to me, I said, "Here it is, it's two a.m., and I'm sitting here rewriting this guy's memo, and he's home in bed asleep. What's going on?"

So I just put some notes on it, said, "I can't figure this out," and some guidance, rather than doing it myself. And I think that was the epiphany for me as a manager, that I couldn't do other people's work anywhere near as well as they could do it, but I could help mold it and craft it to where I could add my level of expertise and awareness and make it a better product, and that's really what my job was. So after that, things got a little bit better as I gradually internalized that and learned how to be a manager, rather than a technical overseer and redoer of everybody else's work. But it was painful.

In fact, medically, in my early days because of the fact that in the Apollo Program as such a young man I had so much responsibility given to me, which I loved. You know, my psychological bent is I loved to be in charge, and I cannot stand a control vacuum as many NASA managers can[not]. If I go to a meeting and there's no clear-cut agenda, it just aggravates the heck out of me. And if there's something comes up and somebody says, "Well, what are we going to do" and everybody sits around looking kind of puzzled and this kind of thing, I cannot abide that kind of behavior. And whether I know anything about it or not, I can instantly form an opinion, and I'll take charge if somebody doesn't slap me down.

I'll say, "Well, okay, listen, here's what we're going to do. Here's the blah, blah, blah. Here's the ground rules and here's the considerations, and you're going to do this and you're going to do this and you're going to do this, and okay. Anybody got any questions? If not, meeting's adjourned."

And the first time I went to some management training, I took one of these things that characterizes your personality behavior, and gee, I can't remember the name of the—

ROSS-NAZZAL: Myers-Briggs?

CHAFFEE: Myers-Briggs is one of them, and that was a real eye-opener. But I went to another one up in [Wallops Flight Facility] Wallops Island [Virginia] later on and got a profile that said, well, I was very confident in myself, I didn't care what other people thought about me. I didn't particularly need back-patting and constant encouragement and all that kind of stuff. And I looked at that. Then we each had an individual session with the person who was giving this little one-day seminar, and said, "Gosh, I didn't realize that psychologically I was like that." I said, "That sounds pretty bad."

He says, "Oh, no, that's the way 85 percent of NASA managers are." So, I guess they just knew with the culture it was just the way because we did have a technical culture of being able to disagree with each other strongly, to limit our disagreements to technical issues and then we could violently confront one another technically over issues and then go out and be the best of buds and have a beer and all that kind of stuff, but, you know, berate one another and say, "You goddamned idiot, you cannot do it that way. You don't know what you're talking about," to that level type of thing and then get it worked out and go off and still be friends, that type of thing.

Early in the Apollo Program when I got a lot of responsibility for thrusters and this kind of stuff, it had overwhelmed me and I started having heart palpitations and that kind of stuff and, was always a little overweight and pudgy. I'm a lot pudgier now than I was then. But I went to the doctor and they said, "Well, you know," and I did stress tests and electrocardiograms. And "can't find anything wrong with you. Nothing the matter with your heart." But I'd get these pressures in my chest and my arm would hurt and the heart would palpate. One night I had my wife take me to the emergency room. I knew I was dying. I actually went in and held my kids because I knew I was never coming back from the hospital again. Took me over there, put me on the machines, and "Nothing the matter with you." So then the doctor said, "Are you under some kind of stress?"

I said, "Well, I don't think so, but maybe I am." So he gave me some valium at the time. This was about '66 or something like that. And I took valium for a month, and yeah, they'd make you feel really good, you know, that kind of thing. But then I said, "Hey, look, I either got to figure this out and deal with it without having to use this crutch, or I need to go back and work in a refinery or do something else. I don't want to go through life like this." So I quit taking them and was able to psychologically figure out and work with the stress of myself as an individual performer.

Then I had somewhat the same kind of experience when I became the Section Head and had to worry about dealing with and being able to critique my colleagues who were now my subordinates and that type of thing. And it's very difficult to learn how to constructively critique and direct people without damaging their own self-concept, or you can't tell them they're idiots or something because "This is excellent. I was looking for this aspect of it. I wondered, do you think that's an important aspect?"

"Oh, yeah."

"Well, why don't you see if you can work that in there somewhere, and maybe this thought that's down at the bottom, maybe this needs to be someplace else, because it more logically goes with this other thought that you've got in there, which was very well done by the way, and I'm really proud of you." And you kind of learn to do that.

And I might tell you that I had a wonderful model for that because my father was the Director of Human Resources for the U.S. Corps of Engineers at their regional office in Tulsa, Oklahoma, where I was raised. And he was a marvel at dealing with people and making them feel good about themselves, never challenging their concept of self-worth, but still not letting them get away with anything, and all his life he was so diplomatic but firm with people.

And another guy that taught me that is Chester Vaughan, who is just a great dealer with people. He can be very tough when he has to be. He's one of the guys that I've lost more arguments with than I've won. There are very few of those. But Chester could bring you in. When he was my supervisor, he could bring you in and let you know you weren't doing what he expected you to do or he was disappointed in something you've done, and when it was all through, you didn't feel bad about it. You were so grateful to him for having pointed out the error of your ways and showing you how you could be better at everything. You'd go out all charged up and determined that I'm never going to disappoint Chester like that again.

So those, my dad and Chester, were two of my models, and now I could be tough and demanding when I had to be, and in times in my career when I had to initiate procedures to fire people, I didn't feel bad about doing that. I'd gone through the steps and done what I felt like was logical, and it was harder than hell to fire anybody at NASA or in the government.

But my first step into supervision was a very interesting transition of how you look at organizations and people and how work gets done and what your individual responsibility is, but I felt like I learned it. And I regretted the loss of being able to have that personal professional technical expertise that I had had as a rocket engine guy, and I never had that again. But at least

I had knowledge of the system and how it worked and the things that had to be done regardless of whether it was a tank or a rocket engine. I wasn't an expert on cryogenic tanks, but I knew the steps that had to be gone through engineering wise, and I pretty well knew if somebody had done their job or not and were trying to pull the wool over my eyes or not and that type of thing. So you just have to be able to live with that kind of insight and expertise, and occasionally go find the Norm Chaffee of tanks and ask him, "What do you think about this? Because I don't know what to make of it," that type of thing.

But anyway, from '76 to about '80 or '81 I was a Section Head and had responsibility for not only the thrusters but the entire RCS system under Henry, the tanks, and the valves, the pressure regulators, the filters, the entire system, the thermal control aspects of it, deciding whether the hardware could withstand the vibration environment of the launch and the loads and all that kind of stuff. The tank was another one where I was able to bring personal expertise back to bear. I don't know are we about to run out of [tape].

ROSS-NAZZAL: I think we need to stop and change the tape and then we'll talk about the tank.

CHAFFEE: Let's stop and then we'll talk about the tank next time.

[Tape change]

ROSS-NAZZAL: Okay. So you were going to tell us about the tank.

CHAFFEE: Yeah. I wanted to talk about the Shuttle reaction control system propellant tanks. And as we talked about, we'd made a decision at the start of the program to go to the hypergolic bipropellants and monomethyl hydrazine and nitrogen tetroxide as our fuel and oxidizer. The size of the tanks was significantly larger than the Apollo propellant tanks. In the Apollo propellant tanks we used Teflon bladders, and I talked about this last time we visited, which were surrounding a standpipe. You put the standpipe down the middle of the propellant tank, which was kind of a cylindrical tank with hemispherical ends. You put the propellant inside the bladder so that if you put helium pressurant on the outside of the bladder and they increase the pressure and it tended to squeeze the bladder down around this standpipe, which had little holes in it through which the propellant could flow out into the distribution pipes, everything. So essentially it was like taking a balloon full of water and sticking a straw down through the neck of the balloon and then sealing the neck of the balloon around the straw. And then if you squeezed the balloon, the water all comes squirting out of the straw. That's the same principle. The standpipe becomes the straw; the balloon becomes the bladder that's got the propellant in it; and then the helium in the tank is around the outside of the bladder, and it squeezes this bladder down around the standpipe, and it squeezes it out.

The Apollo experience was also a one-shot deal. All you had to do was get the bladder fully expanded inside the tank. Then you filled it up with propellant, you squeezed it down once, because it only went on one mission, and that was it. So we very quickly figured out early on that we, number one, because of the size of the tank the bladder had to be so big that when you tried to fill it up with propellant on the ground, it tended to all bunch up down at the bottom of the tank. The balloon acted like a water balloon. If you hold it up, it's kind of an oblong shape, teardrop shape. The fluid in the bladder would form that kind of a shape on the ground as you filled the tank up, and it tried to pull the bladder lose from around the top of the standpipe, and then the launch loads and that kind of stuff, we weren't able to figure out how to make a bladder big enough to work in these much larger tanks.

After looking at various ways of doing things, we went to what we call a screen acquisition system, where you use a set of screens which are almost built like—I don't know quite how to describe them—like a two-by-six or something like something about that shape that were put into the tank, distributed through the tank, in a series of what we call galleries, and these things had some structure to them. But instead of solid walls, the walls were made out of a material very much like window screen, except that the screen was very, very, very fine, and in one dimension there would be like—and these were metallic, stainless steel screens.

In one dimension, there would be like back and forth there would be eight hundred wires per inch, linear inch, and then the other dimension like fourteen hundred wires per inch. So it was opaque if you tried to look through it, but if you'd try to blow through it or something, it would pass liquid or gas or something like that. And we used the surface tension effect, which is very similar to something if you've ever seen standard window screen, which has little grid sizes maybe a millimeter in size or smaller. If they get wet, each one of those little squares will form a little meniscus of liquid that's formed by the surface tension of the water, and it takes a little bit of pressure to blow that film of water through.

The smaller the little square becomes, the greater the pressure it takes to punch the air or the gas through to break that surface tension hold. And so we were able to use the technology that had showed that if you got a screen that was very, very fine such that the little open areas in between the weave of the screen was small enough that when you wetted that screen with either nitrogen tetroxide or monomethyl hydrazine, that it would take two to three pounds per square inch of pressure to punch through and blow the gas through that. So it acted as a barrier.

And the concept was that if you had a blob of propellant in the tank that was floating around in the tank somewhere and when it got down to only a small amount of liquid in the tank, you really don't know where it was. So you had these pipe-like things all around through the tank that were made out of this screen material with the idea that wherever this blob of fluid or amount of fluid was, at some point it was always in contact with a screen surface somewhere. Then you'd put in helium into the tank. There's no bladder now. It's just in there mixed with the propellant. And when the helium pressure goes up, it will pressurize the area that's on the outside of these screen pipes, and anyplace the liquid is in contact with the screen, the liquid will flow through at a much lower pressure drop than the gas trying to punch through the meniscus of the little water surface tension or the propellant surface tension.

So if you could keep the pressure differential between the helium inside the tank and the liquid that's flowing out and going through the pipe, if you could keep that at no more than a pound or a pound and a half of pressure difference, that it would be the liquid that would preferentially flow through the wetted screen and not the gas. If you ever got the pressure too high and the gas poked through and broke the meniscus of the surface tension, then you would preferentially get gas flow through that spot. It would dry the screen out, and now your rocket engine doesn't work because it's not getting propellant, it's getting helium gas, which doesn't provide any significant thrust.

So it was a significant design problem. We had a guy who was a good engineer named Dale [L.] Connelly, who worked on that and came up with many, many, many really creative ideas. Darrell Kendrick also worked with Dale. Dale was a guy who didn't have a whole lot of confidence in himself. I had to work a lot with Dale as his supervisor to get him to, you know, when he had good ideas, which he did, to push them and defend them and not be so easily beat down by people who would challenge him and that kind of stuff. He was an American Indian kid, later on left the agency and went to work for the Bureau of Indian Affairs and then later got back into the technical field with the Air Force technology program. I hear from Dale occasionally, he calls me, and that kind of stuff. A really, really good engineer and was critical to the development of this tank, which was a real challenging kind of thing, not only to find something that worked in zero G with a small amount of propellant in the tank to guarantee that when you opened the valve it was liquid propellant that came out and not helium gas.

But now how do you demonstrate that this thing works when your only testing can be done in a one-G environment where the liquid is sitting down in the bottom of the tank, but you've got these screen pipes all through the upper section of the tank, too, and how do you keep them wet and how do you keep them from drying out and all this kind of stuff?

Well, that ended up being what you call a mass transfer kind of process. The evaporation of material from one phase, from the liquid phase to a gas phase, and at what rate does that occur and how is it affected by the temperature and how is it affected by the degree of saturation of the helium with the—you know, has it got a hundred percent humidity of propellant in it such that nothing is going to evaporate, or this kind of stuff?

Once again, that's a chemical engineer's problem. That's the kind of thing that chemical engineers do, and so when we got into those kind of issues, that was another area in which my own personal expertise could be brought to bear and I could help Dale and Darrell and the Rockwell contractors because when they, the contractors, proposed systems for calculating these kind of things and doing design stuff that I knew wouldn't work, just from my experience as a chemical engineering student. So I had—by that time, I was thirteen, fourteen years out of school, but I got out my mass transfer books and my thermodynamics books and all that kind of stuff and had to go back and do some real research, but was able to make a real contribution about how do you calculate some of these important parameters of how do you wet the tank, how quickly might the screen dry out and the liquid film evaporate, because there's not much mass there, you know, a little meniscus of fluid, that type of thing.

There's some esoteric dimensionless numbers that chemical engineers use in calculating those kind of things, and I was able to go back and figure out some analytical techniques and ways of approaching that and were able to show that those things, in fact, probably were better analytical techniques than what other people were trying to propose and felt good about it, because as a manager some time I wasn't really quite sure what I was doing when I was dealing in other areas of other people's real expertise. And now here was that 50/50 or 40/60 area where I could once again go in and be a personal technical expert with something that I knew about and really contribute to the solution of the problem.

I talked about one other one that occurred about that time a little bit earlier and was in trying to figure out what, how much NO, nitrous oxide, you put in the N_2O_4 to keep the final solution concentration from being too low and this inhibiting material that would keep it from attacking the iron in the system, the iron nitrate problem. That's a problem basically of fractional distillation, which is, again, what chemical engineers in the process industry study.

So those are some of the major, major problems. As we got to the end of the Shuttle Program, one of the areas that I was responsible for supporting was the auxiliary [power] unit, which at that time was essentially a propulsion device. It used a material called hydrazine. Its formula is N_2H_4 and it's a monopropellant device. And we used essentially a hydrazine rocket engine in which you drive liquid hydrazine through a single valve across a catalyst bed of a platinum type, platinum metals family material, causes it to dissociate into nitrogen, hydrogen, and ammonia at a temperature of about seventeen hundred and fifty degrees Fahrenheit and then exit as a hot, hot gas. And you can use that as a rocket engine, and many satellites do that.

In this case, we were using it as a turbine driver, and so we would take this rocket engine, which was built to drive a turbine, and we had a little five and a half inch diameter turbine, which the exhaust from this rocket engine would be ported into the turbine with its blades, this kind of stuff. It would drive the turbine and then the gas would go ahead and exit from the turbine, but then the turbine [shaft] drove a box of gears which ended up driving the hydraulic pumps which were used to control the aerodynamic surfaces, the speed brake, the body flap, the wing flaps, the landing gear deployment, the nose wheel steering, the brakes.

All those kind of functions were driven hydraulically, and you had to have something. You had hydraulic fluid and then you had hydraulic pumps, and you had to have some energy source for the hydraulic pump, and the auxiliary power unit was that source. So it used a hydrazine rocket engine to drive the turbine, which drove a gear train, which then drove these pumps, and a very, very interesting system.

We had a lot of trouble developing that and making it work because the gears had to be lubricated. It was all very high speed. For instance, a little five and a half inch diameter turbine typically ran at about a hundred thousand revolutions per minute, or rpm. The pumps ran and [pumped] hydraulic fluid at a pressure of three thousand pounds per square inch. So it was a very complicated system. This gear train had to be lubricated but it had to be lubricated in zero gravity. So we had to come up with a zero gravity lube system, unlike [a] transmission. It's kind of like a transmission with a series of gears that went from this very high-speed shaft that was coming out of the turbine down to something of a lower speed that drove the hydraulic pump, very much like a transmission.

The transmission in your car has several quarts of transmission fluid in it that serves as a controlling material and also a lubricant and this kind of stuff, same thing in the differential in the rear axle or front axle of your car. And when you're running around on the ground, that lubricant is always laying in the bottom and you can just fairly easily design a system just to be submerged in this kind of thing. Well, you don't want your gears submerged in this in space in these high-speed systems, because the drag on the gears and the energy it takes just to churn that fluid, it takes so much energy you can't tolerate that in your design. But you do want enough lubricant in there to keep all the bearings and the surfaces that are rubbing together, to keep them fully lubricated.

So you have a little reservoir of lubricating oil, you have little jets that squirt oil on all the critical bearings and rubbing surfaces and all that kind of stuff, and the question is how do you make all that work in zero gravity? How do you grab the oil, get it back down into a tank, into a lube oil pump that then pumps it up through these little jets that squirt on these, and so that was an interesting problem, too. You also had a problem of potentially leaking a little bit of the hydrazine fuel over into the gearbox where the oil was, and when you did that, it was a disaster because the hydrazine would react with the [oil] and form a [thick] grease instead of an oil. The grease was not particularly lubricating, and it would gum everything up and stop up the filters and all this kind of stuff.

The company that was building this device for us was a company called the Sundstrand Company. They were up in [Rockford], Illinois, west of Chicago, and I spent many, many weeks up there. We had a couple of guys that were working on the auxiliary power unit.

Dwayne [P.] Weary was the APU Subsystem Manager, and a guy that worked with him, a very, very good engineer, a fellow named Bob [Robert J.] Villemarette, who later left NASA and went to work for the Army over at Fort Polk [Louisiana]. He was from that area, and he and his wife wanted to go back there. A great engineer was Bob, but Sundstrand was in a lot of trouble.

Rockwell was having trouble managing them and getting them to do what needed to be done and this type of stuff, and so the situation got schedule critical, and so Henry Pohl decided we needed to have a constant NASA presence up there with the Rockwell staff, and so we would alternate for two or three-week shifts up there. Duane Weary would go up for two or three weeks, and then Bob Villemarette would go up for two or three weeks. And then to help save their family life, I'd go up for usually no more than a couple of weeks and take a shift and that kind of stuff. So once again, I was able to get some personal expertise, really get down into the technical depths of this kind of thing.

Well, one of the challenges we had was that the Orbiter Program Manager at that time was Aaron Cohen, again another delightful guy, but he was under tremendous pressure at the time, both schedule and cost control. And he had all these problems going on in various systems, and he wanted to check in personally on them every day to see how things had gone. Well, whoever was at Sundstrand had the responsibility to call Aaron Cohen's house every night at ten-thirty and tell him what had happened that day, what progress had you made, what problems had you had, how did things go, was there something that you could make him feel good or hopefully make him feel not too bad even though something had gone wrong or something like that.

And it was very interesting and every night at ten-thirty that phone wouldn't ring one ring till the phone would be picked up at the other end and the voice would say, "This is Aaron Cohen."

I'd say, "Hi, Aaron, this is Norm. We did good today." Or "we had a little problem today but it's going to be okay," or something like that. You very quickly learned that you tried to be truthful with him, but he reacted so much to bad news that you tried to—if it was bad, it was bad, but you tried to keep it in perspective for him, give him an honest assessment. And I'll talk some more about managing Aaron later on, but Aaron was such a hands-on manager, so interested. And every night you were up there, at ten-thirty you called Aaron and told him what happened, and he was always very grateful. And I know, to jump ahead, he later became Director of the Center.

When he retired, we had a nice party for him and a program over at Space Center Houston [Houston, Texas] in their big IMAX auditorium, and his son got up and talked. And his son talked about, "You know, every night the phone would ring from seven-thirty until midnight with people calling in to tell Dad all this stuff," and said, "some nights he was so distraught he couldn't go to sleep, and other nights he was just giddy because something had gone well," or something like that. Then he looked at the audience and said, "Did you any of you ever hear of an APU?" And of course, we always had lots of problems to worry Mr. Cohen, and so I had to really laugh to myself, said, "Yeah, I know about calling Aaron about the APU every night at ten-thirty."

But that was another—it later came out good. We had a lot of problems. We blew up APUs and we had a design requirement that said if the turbine ever overspun, you know, got to going so fast that the centrifugal forces overcame the mechanical strength of the blades, and in that case it comes apart. And, that happens in aircraft engines and that kind of stuff, and it throws blades through the engine cowling and into the cockpit and through the side of the aircraft and all that kind of stuff, tremendous energy in these things.

Said we've got to have containment, so we had this big, heavy ring of—I can't remember whether it was stainless steel or tungsten or what it was now, but big, heavy ring of metal all around the outside of the—where this blade, where this turbine was spinning at a hundred thousand plus rpm, such that if it ever overspun and came apart that it would break into pieces, but you would keep all the pieces in here and it wouldn't go crashing through like shrapnel through the back end of a Shuttle because there was going to be three of these APUs in the backend of the Shuttle for redundancy.

And so one of the tests we had to do was demonstrate that that APU could come apart and that the containment design feature would, in fact, keep all the pieces inside the box. So the way to do that was, number one, you purposely make a small mechanical flaw in this turbine wheel such that it's weakened slightly and provides a preferential place where if it's going to break, it's going to break there, you know, like making a notch in something that you're bending. And then we took the speed up to a point where we calculated the centrifugal forces would be so high in this spinning wheel that it would cause the wheel to break and come apart.

So we went out and did that. I wasn't there at the time. I think Bob Villemarette was at Sundstrand when they did that test. But he called back and said, "Containment didn't work. Pieces went everywhere. I mean they were all over the test cell and outside the test cell and rolling down the hill and all this kind of stuff." So we had some work to do there. But it all came back together and worked well.

The hydraulics system was another interesting challenge. I never knew anything about hydraulics systems, but we got into the development of the hydraulic system, which once again, is a fluid system, and it's something that not only chemical engineers but mechanical engineers are very comfortable with dealing with the flow of fluids and this kind of stuff. But the hydraulic system, as I said, works the wing flaps, the body flap, the rudder speed brake, the deployment of the landing gears, the brakes and the nose wheel steering, had all those functions, very complex system, ran around all over the place. Each hydraulic system, and there's three of them on the Shuttle, each hydraulic system has one hydraulic pump which puts out pressure at about three thousand pounds per square inch, has an APU that drives the hydraulic pump, and then these pipes that go all over the place to all of these locations that have to be driven hydraulically.

The bad thing about hydraulic systems is that they all leak. And anyplace you go, you know, you go out to the airport and look where the airplanes park, you always see these little puddles of hydraulic fluid underneath the areas where their hydraulic actuators and things are because you just can't help it. The way you put the system together, the joints are what you call swaged ([which is] crimped mechanically) rather than welded. They'd just seal them and leak a little bit. So we were always worried about the hydraulic fluid, which goes all over the Shuttle. Like I say, there's three different systems for redundancy, and the geometry of each system is different because some functions are fed only by one system and some functions like the elevons and that kind of stuff are fed by all three systems so that you can lose a system and still have a hundred percent functionality. In some cases, you could lose two hydraulic systems, and if you didn't have too much demand on your hydraulic system, you know, too much disturbance as you were coming in, you could still control it satisfactorily, on a good day, as they say, with a single hydraulic system.

But we were always worried, and occasionally you'd get a spewing leak and hydraulic system would spray out inside somewhere in the Shuttle and then it would get the insulation wet and the inside insulation blankets and everything around there would get wetted with hydraulic fluid. And you've got to go in and clean this stuff up, and it was a real mess. Luckily, early on we figured out that this special hydraulic fluid that we decided to use which was [a] Department of Defense fluid that they used in aircraft that were subject to being hit by enemy fire, and it was specially selected because it was supposedly nonflammable so that if your hydraulic reservoir or a line got hit by a machinegun bullet or something like that, it didn't immediately start a fire. Well, that was all relative. Because of our Apollo flammability experience, we immediately, when we went to this better hydraulic fluid, went and did a test where we made a pinhole in a pipe, put it under three thousand pounds per square inch, sprayed this stuff out and then tried to light it with an igniter, and, boy, it would make a blowtorch.

There's no doubt about it that when you atomized this stuff in an atomizer that way that it was indeed flammable. So we knew it was flammable. If you just wet the table with the stuff and tried to light the pool of it with a match, it wouldn't burn that way, but under pressure if it spewed out as a fog or something like that, it was very, very flammable.

But we found out that the materials that they used, and this hydraulic fluid was not a natural-occurring material, it was a manufactured hydrocarbon material, a mixture of many, many different things to provide all the different lubricating and temperature-resisting properties that you wanted [in] your hydraulic fluid. Among other things, it turned out to fluoresce in black light, and when we found that out, it became a whole lot easier when we had a leak somewhere inside the vehicle to go in with a black light and we could shine it around. And anyplace it glowed, "See that, we've got to clean that up, or replace that insulation blanket or something like that."

So that was an interesting—we had a test facility where we did hydraulic testing here in Houston for pumps and APUs and that kind of thing, but the big system test facility that served to develop and qualify and later certify the Shuttle hydraulic system was what they call an iron bird out at Downey. And they had a special facility set up whose name—I guess I thought I would never forget that, but I do. I can't come up with the name of it now, but it's essentially a geometrically correct hydraulic system but without all the fuselage and the wings and all that kind of stuff. But the APUs and the hydraulic pumps were all in the right position. The line runs are all bent the same way. They've got the wing actuators. They've got the landing gear. Everything is geometrically where it's going to be, and they can then run this system on the ground and operate the various things that need to be operated, the rudder speed brake and the body flap and the elevons and all this kind of stuff and see what the characteristics of the system are how far does the pressure drop, how can we drive the elevon as quickly as we need to, and number of degrees per second against a certain aerodynamic load, and all of the things that you need to verify about the performance of the system.

So that was done in this system whose name I can't remember out at Downey, and I spent a lot of time out there. And we hired a guy about that time who had worked at Douglas Aircraft, a fellow named Wes [Charles W.] Galloway, who had been a supervisor in their hydraulic system and was immensely knowledgeable about hydraulic systems, and he came in in the mid-seventies and worked for us for a number of years, and he was key in helping us to oversee that Rockwell effort out there and make sure that we understood what was going on, that Rockwell did everything we felt needed to be done. [We] had several problems that Wes contributed to, things that he developed as far as figuring out what the [pressure] pulsing environment of the system was where we were going to—were we going to overstress the lines and the joints and stuff due to the high-frequency pulsing and this type of stuff. Really an interesting guy, he was an older guy, probably fifteen, eighteen years older than I was, and was one of my first experiences in supervising a guy that was significantly older than I was and who had been in a fairly high-level supervisory position in previous stages of his own career. So that was interesting and we butted heads a little bit, but we got it sorted out and had a very productive working relationship. And the two of us together, we worked out a way to make that Rockwell facility a whole lot more productive and ended up getting a nice cost-savings award for that, which I've still got somewhere and value very much.

So the first Shuttle flight was an amazing kind of thing. When we finally got to the point where it was time to go and launch this thing, it was a puckered-up situation. When STS-1 lifted off with John and Crip, Bob [Robert L.] Crippen and John [W.] Young, and this kind of stuff. And the problem with the people in the energy systems area is that you know all this stuff that is essentially a bomb, you know everything that can possibly go wrong. You've studied for years all the what-ifs and what if this and what if that and all this kind of stuff, so you know all the vulnerabilities. And you know you've done the very best job you can to create a system that is very, very tolerant of failures and off-nominal conditions and that kind of stuff, but there's still red lines that if you go beyond this, you're going to have a catastrophe or a disaster or a real serious problem.

So when that thing lifted off and got into orbit, first of all, that, you know, whew, we made it into orbit. The main engines worked, the orbital maneuvering system engine worked and the APU worked and all that kind of stuff, and we got into orbit. Then being in orbit is actually a very benign situation because you're in an area where you can define your environment very well. Things, the conditions, aren't changing rapidly. If something goes wrong, you've got some time generally to figure it out, although on occasion, if you lose critical redundancy, you might have to say we've got to come in as quickly as possible, something like that.

And then entry, again, is another time where you have no tolerance for things are going wrong. You're coming in, and you've got—and of course, we saw that with *Columbia* you've got a limited amount of time and you're going to be on the ground, whether it's in one piece or a lot of pieces and that type.

So when STS-1 landed, it was an amazing kind of thing and really, really a good feeling to have gone through that. By the time that happened, I had, I think, about '81 got—let's see, well, right after that, I guess, Guy Thibodaux retired, after the flight of STS-1, and Henry Pohl became Division Chief and Henry, bless his heart, took me up to the Division Office with him as his, essentially, Technical Assistant, and I was responsible for the whole division's technical responsibility, which is essentially all Shuttle and some technology work at that time. And I spent a couple of years doing that kind of thing.

To go back to STS-1 when it landed, and that was April of '81, I bought from Rockwell a mahogany model of the Shuttle. They had these in their gift shops, very nice thing. It was a little pricey, but I bought one and then went around and got both John Young and Bob Crippen to sign the wings of it. It's about ten inches long and eight inches wide or something like that. So John Young signed one wing and Bob Crippen signed the other wing, and then I had it lacquered so that the ink wouldn't come off, and that's still a treasured memento of mine.

The other thing I did, which means a whole lot to me, is that the week after they came back, *Time* magazine's cover was a picture of the Shuttle at about forty-five degree landing angle coming in, and so I cut that cover off and went around and got personal signatures with little personalized words to me from, number one, Chris [Christopher C.] Kraft [Jr.] who was the Director of the Center, Max Faget who was Director of Engineering and father of the design, Bob Thompson who had been the Level II Program Manager, Aaron Cohen who was the Orbiter Program Manager, Bob Crippen, and John Young. And I've got that framed on the wall of my office at home now, along with a lot of other nice mementos that I've accumulated over the years.

But you know, I have told my kids very carefully, I said, "When it's time to go through Dad's stuff and wonder what to do with this now that Dad is gone, do not put this stuff out on the curb for the trash man. There is an aftermarket. If you don't care anything about it, it means a lot to me, but if you don't have any particular attachment for it or the grandkids don't, for heaven sakes, go to e-Bay or something like that, because there's people that will pay you some significant money for stuff like this." So I have been very careful in stuff I've written for my kids and stuff I've kept that says, "This is the background of this stuff, just wanted you to know where it came from and what's behind it, that kind of stuff, so that if you want to keep it or throw it away or sell it, at least you've got this information. By the time you're faced with that decision, I won't care, but just wanted you to not look at something and wonder what is this or what meaning does it have, or what did it mean to Dad, why did he keep it, or that kind of thing."

So I continued working for Henry at the Division level till about '83 and working the first several flights of the Shuttle and had some interim early assignments in there as gradually we phased into thinking about the Space Station and what were we going to do about the Space

Station. But to go back to the Shuttle Program and Aaron Cohen, who is an amazing man and also I think a dear friend and a man that I just have immense respect for for the pressures that he withstood. As far as I know in a major program like that, he is the only Manager that survived clear through from the pre-Phase A or Phase A kind of thing clear through implementation of the program. He wasn't relieved of duties halfway through because of being over schedule or over cost or stuff. And I just can't imagine the pressures he was under during his years as the Orbiter Program Manager. I have some sense because I used to call him at ten-thirty at night from Illinois to tell him what was going on.

But one of the things we did, the Propulsion and Power Division, had a weekly meeting with Mr. Cohen on like Wednesdays at noon at lunchtime, it was lunchtime meeting, and he'd bring his lunch and that kind of stuff. And it was just to brief him on what was going on on all of our subsystems that the division was responsible for that we had subsystem managers that were responsible to him for. We had the main propulsion system, the orbital maneuvering system, reaction control system, the pyrotechnics, the batteries, the hydraulic system, the APU, the fuel cell, the cryogenic storage, all of the things that were liquid systems that were bombs in waiting kind of thing, and so we always had a lot of bad news.

And my job when I took over this job as Henry's Technical Assistant was every week, I had to gather the inputs from all the subsystem managers for this weekly briefing to Mr. Cohen and look them over and put together a coherent briefing and decide which order and this kind of thing that we were going to do that in. After one bad experience, I learned that Aaron's tolerance for bad news was limited and that if you went in and the first thing you told him was something that he took as very grim or bad news, that he would become so upset and obsessed with thinking about that and immediately trying think, "Now, what am I going to do about this and how can I solve this," and all this kind of stuff that he didn't hear anything else you told him for the rest of the hour.

And on occasion, literally, although Henry would go, I was essentially the master of ceremonies of the presentations all of the subsystem managers would present. Henry and I, if there was eruptions, we would be the buffers and we would explain what the subsystem managers really meant and what that really meant to Aaron and the program this time. But he would literally, in some cases, when he had perceived the knowledge to be very badly, he would literally put his forehead on the table and be looking down at the floor [at] his shoes. And Henry Pohl, in trying to put things in perspective for him, would get down underneath the table so that he could look up at him, look him in the face, and say, "Now, Aaron, it's not that bad. We know what caused it and what we have to do about this."

And Aaron would just be just destroyed with this knowledge and so "down" that—so I very quickly learned that there were some people who came in and would give me a very balanced picture in their report from their subsystem of what good had happened, what progress was made, what that meant, what unanticipated bad things had happened, and what that meant and what the recovery plan was going to be, and, in general, where were we with where we needed to be. And I didn't have to mess with their stuff very much.

Some people wouldn't tell the bad news, and I had to beat that out of them. I'd say, "Look, you know, Aaron is the Program Manager. He has to know about this, so even though we're going to get beat up, we've got to tell him, but we've got to tell him responsibly so that, if it's horrible, we need to tell him it's horrible. If it's a failure but we know how to fix it, then we need to try to put a positive face on it and tell him we're going to work on it."

But the guy who was the main system [propulsion] subsystem [MPS] manager, not the rocket engine but all of the piping and components and stuff that go with the main engine that take the liquid hydrogen, liquid oxygen from the external tank down to the engines, was a fellow named Phil [Phillip E.] Cota, another amazing engineer, local Houston guy. He had so many components that he was responsible for, I have no idea how he kept them all straight. He always knew everything that was going on down to the nth degree. He knew the materials that were involved. He knew all the specifications. He knew all the testing that had happened and everything like that. And he was so thorough that he would come in and almost to the point where if some technician at Rockwell had gotten a splinter in his finger during the week, he'd report that as a bad-news kind of thing.

So I had to severely filter what Phil would put in and say, "Look, we don't want to trouble Aaron with stuff that he doesn't really need to know. If it's just we spilled something or we accidentally drilled a hole where it didn't need to go, or something that we can in the course of business—we're going to take care of, we don't need to tell him that. Let's don't put that in. Let's find in your five-minute pitch, let's find the really significant items and give him the information he really needs as a program manager on those things. Tell him the good stuff first," and then I learned to put Phil last in the presentation, so I never had the same sequence of subsystem managers presenting, that the charts would come in, and I'd sit down and go over it with each of these people.

And the best ones, the ones that had the best news and successes and great progress and we'd overcome a failure that we'd had the week before or the month before, something, all those went first. Now, Aaron was glowing and smiling and happy to hear that. Then the bad news went last, and the very last was always Phil Cota because he always seemed to have this long list of terrible things that had happened. Even though some of them weren't of significance, they were of tremendous significance to Phil, and he wanted Aaron to know about all this stuff and so he—it was interesting learning experience there of how to responsibly manage this kind of thing and the people involved in it such that you got the results you needed to have, results that were responsible to the needs of the program, but still not overreact.

I still am in fairly frequent contact with Aaron by e-mail, and he climbed on me a bunch. When he had bad news and I had to go tell him something about the Shuttle that he didn't want to hear, he could really get on you and challenge you. And I remember a time or two he—one case, I told him that the seals that we had in the elevon actuators weren't the right ones, that all of our tests showed that even though the actuator guys who were not our division thought they were going to be okay, we didn't, and we thought something had to be done and that kind of stuff. He didn't know how to resolve the argument between the people who were responsible for the actuators and the guidance navigation and control area and us folks who had the hydraulic system that provided the driving fluid for this. And I'm up there telling him, "Aaron, you've got to change seals. We've got to do something else," because that's what my people were telling me, and I'd looked at the data and believed.

And I'd take them up there and we'd give this—he got so mad at me one time that he said, "Look," almost shouting, "I want you to go out to"—in fact he said, "Get your ass out to Downey today, and you get with the Rockwell guys and the Rockwell hydraulics guys and the Rockwell actuator guys and you get this worked out."

I said, "Well, okay, Aaron, I'll get out there first thing in the morning."

He said, "That's not what I told you. I said I want your ass out there this afternoon." And this was at noon, something.

So I said, "Well, yeah, I mean, Aaron, I've got to make some arrangements and that kind of stuff."

"I don't give a damn. Get your ass out there."

So I said, "Yes, sir." So I called my secretary and said, "Quick, get me some travel orders and a plane ticket, I've got to go to Downey tonight." And I called my wife and said, "Quick pack a bag, I'm going to be gone about a week."

And then he sent another amazing guy, named Don [Donald D.] Arabian, out to go with me and make sure I did the right thing, and Don was another guy. He essentially was the leader of the team to try to get this [figured] out. He was going to be Aaron's eyes and ears and as a referee on the spot, and so he went out and we did get the thing worked out satisfactorily. We changed one of the seals, kind of thing, to a different type of a seal, to a T-seal.

But Aaron when he was upset and didn't want to hear things could really, really challenge you and then tell you to go fix it, in no uncertain terms, and you felt like you better go do that. I used to work him a little bit. I can't remember whether I talked about this or not, but when I had an issue for his control board, again, when I was Henry's Technical Assistant, I knew the division was going to have to go forward and make a recommendation for something that we needed more money or more time or something that the Program Manager doesn't want to hear, that if you went to the Control Board, which Aaron chaired that had representatives from Engineering, from Mission Operations, from Safety, Reliability, and Quality, from all of the major organizations sat on this board as advisors to Aaron, although on any Control Board there's only one vote and that's the Program Manager's.

But he listened to these people. If I had an issue that I knew was going to be contentious and that Aaron didn't want to do, I made sure that I got my technical arguments all lined up ahead of time and my first up, of course, was Henry Pohl and then Max Faget, to make sure that they thought we had a responsible position and that they would stand behind it. But then I would go around and I would go over and I knew who the Mission Operations Directorate rep [representative] was on the board. I'd go over there and, you know, I'd call him and I'd say, "Look, Dick," and Dick [Richard A.] Thorson a lot of times was on the board, "I've got an issue that's going to come up next week. I need to come over and talk to you about it, make sure you understand what's going on." So, I'd go talk to him and then I'd go talk to SR&QA [Safety, Reliability, and Quality Assurance] guy, and other people that were going to be on the board and make sure that they understood what the issue was, what our recommendations were, and why we felt like the recommendation had to be that way.

Then likely as not, when it came up before the board, Aaron would be resisting. He didn't have any more money. He didn't have any more schedule time, that type of thing, resisting, trying, "Well, I don't really think I need to do that. I think we can get by without that." He'd ask Max Faget, "Max, what do you think?"

Max said, "Oh, Aaron, looks like we've got to do it."

He'd ask the MOD [Mission Operations Directorate] guys, "What do you think?"

"I think we think that PPD [Propulsion and Power Division] is right, we really need to do this." SR&QA said, "Yeah, yeah."

And so I'd kind of run the table on him. Didn't win them all because sometimes the people would say, "Norm, you're looking at it very parochially from your division standpoint, and we don't think you need to do that." Didn't win them all, but I think I won a lot that I wouldn't have won if I hadn't gone around and prebriefed these folks and had them to the point where they thought they fully understood what the issue was and what we felt like was needed for the good of the program and why. So I think that also—Henry and I would work a lot on doing those kind of things to try to realize this is the real politic situation. This is how you make things happen because of the situation and the people that we're dealing with, and we'd adapt to that kind of thing.

So that kind of got me to the end of my Shuttle career.

ROSS-NAZZAL: Okay. Well, I think this might be a good place for us to stop and pick up with Space Station next time.

[End of interview]