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ORAL HISTORY TRANSCRIPT

NORMAN H. CHAFFEE
INTERVIEWED BY JENNIFER ROSS-NAZZAL
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ROSS-NAZZAL: Today is January 19th, 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 this morning. We really appreciate it.

CHAFFEE: Well, I'm glad to be here and honored to be asked to participate in this program.

ROSS-NAZZAL: Well, we're happy to have you here. I'd like to begin by asking you about your education. You have a bachelor's and a master's in chemical engineering from Tulsa University [Tulsa, Oklahoma]. What led to your interest in chemical engineering?

CHAFFEE: Well, I was raised in Tulsa, Oklahoma, which at the time was called the oil capital of the world. Now, unfortunately, that title has devolved to Houston, but back as a youngster, Tulsa was called the oil capital of the world, and although my family was not in the oil business—my dad actually worked for the U.S. government, Corps of Engineers, there in Tulsa—many, many of our friends and the parents of my friends, that type of thing, were in the oil business. So I always knew I wanted to be an engineer. I was technically interested in things, liked to take things apart and put them together, and see how and why things worked the way they did, and this type of thing. So I always knew I wanted to be an engineer because of my immersion in the petroleum culture there. It always just seemed right that I was going to be part of the petroleum industry, also.

But then in high school I first took chemistry and really found something that I could relate to, that I understood, really loved, enjoyed doing the experiments. Most of the other kids in the class seemed to be totally bumfuzzled by how you balance an equation, and to me it was just natural, you know, so I thought, "Gee, this is something that has really resonated with me." So rather than become a petroleum engineer, I decided, "I'll try to become a chemical engineer." So that was the genesis of that interest, and it turns out to have been a really good decision and it's served me really well throughout my career.

ROSS-NAZZAL: What did you plan to do initially with your degrees, before you decided to come to MSC [Manned Spacecraft Center, Houston, Texas]?

CHAFFEE: Well, my progression, now, I went to Central High School in Tulsa, which was a huge high school. It was a four-year high school. There was five thousand total students there, and my graduating class was over a thousand, so it was a huge thing. We were very modest in financial means, and it was pretty important for me to get a scholarship, so I applied lots of places. Was fortunate, because I had a really good academic record and participated in lots of extracurricular activities.

I received scholarship offers from a number of places, but the best offer I got was from what was called Rice Institute [now Rice University] at the time in Houston. At the time Rice was about two thousand students, so it was significantly smaller than the high school I went to, and it was all technical, just about, very little liberal arts, just enough so that the engineering programs could be accredited. The good thing about Rice was that if you were admitted—it was very selective; they only took four hundred freshmen a year—if you were admitted, it was free. There was no tuition. So I was admitted, and in addition to that, I was awarded the Union Carbide scholarship, which paid all my other expenses. So my dad was turning handsprings and everything, so it became a no-brainer that even though I had offers also from many other schools, that this was my best deal. Plus, it was a day's travel away from home and Tulsa and that kind of thing.

So I came down and went to Rice for three years in the chemical engineering program. It was a five-year program. Rice, unlike other schools which had typical four-year programs, Rice's degree program was five years, and it was typically eighteen to twenty semester hours per semester. So it was a tough and full agenda to get to be an engineer, and very theoretical in its content. I came down and did very well for the first two years. The third year I stumbled academically, and I looked like I was on a track to not do well my senior year, so I made the judgment that, well, I'm probably going to lose my scholarship, anyway. I'll just go back to the University of Tulsa.

So I did that, went back there, finished my degree, and I was fortunate enough to get a graduate fellowship at the University of Tulsa, something called the Parriott Fellowship. I went ahead and that funded my master's degree. It turns out that that combination of schools was really very good, because the Rice professors and instructors were all Ph.D.s, very theoretical, and I had an excellent three-year grounding in the basics of physics, chemistry, mathematics, electronics, that type of thing. Went back to the University of Tulsa, those guys were nowhere near as academically famous, but they had all been long practitioners in the actual industrial field, and so they knew all these hints about how to decide whether a pump was getting ready to fail, or whether a heat exchanger was too hot or too cold, or this kind of thing.

So I had three years of outstanding practical education up there, so I think I probably had the best of all possible worlds, totally accidentally. My final year there as a graduate student, I also worked part-time for an oil company in Tulsa at their refinery. It was a company called the SunRay DX Company, which I think later was merged into the Sun Oil Company or Sunoco or something like that. Anyway, I worked at their refinery and was on track to be hired by them, but during that year that I had part-time work with them and my final [graduate] year, it became clear to me that maybe working in process industry was not as exciting and as much fun as I thought it was going to be.

The plant was unionized. They didn't like the young engineers to mess with the equipment. They'd like you just to sit down and stay out of the way because they knew what they were doing. The most exciting times was when something went wrong or something broke, and then the engineers could figure out what do we do now, you know, that the product is not per specification or it's too hot or it's too cold or the pressure is too high or too low, and then you'd have to go figure out what to do about it. That was fun, but you were deriving your pleasure from the wrong things.

Well, about that time I learned that there was going to be this activity in Houston called the Manned Spacecraft Center, and I had always been a space fiction nut, would spend all my money in high school, spare money, buying these little science fiction books and reading stories by Poul Anderson and Robert [A.] Heinlein and various other famous [authors], just ate it up, and had always assumed that the thought of humans traveling in space was exactly science fiction. You know, it was fiction; never going to come about. Suddenly the Russians did their

Sputnik thing and the U.S. got interested, and then President [John F.] Kennedy announced the human space program, and it was going to be in Houston, which I dearly loved, having lived here for three years going to Rice.

So it turned out that I had interviewed—as just part of my final graduate program I had interviewed with a NASA interviewer, and [I] was talking to them about employment at Langley [Research Center, Hampton, Virginia] when I finally did get my degree. I was going to compare that with the refinery and see what I wanted to do. Langley did call me and make an offer, and I said, “Well, I’d really like to do it, but I’ve just learned of this new facility in Houston. Would it be possible that I could transfer there?”

They said, “Well, we’re not doing their hiring, but we will refer your paperwork to the group that’s in Houston trying to staff up now.” So I did get a call from a lady whose name I don’t remember now in Houston. She was down in the East End State Bank Building. You know, we were all over town at the time. I explained I was a chemical engineer, was about to get my master’s degree, but had some experience in the petrochemical industry, but would like to get in the space business because I was so excited about it, and I wondered if the NASA organization was going to need chemical engineers.

This lady said, “Well, I don’t know. Let me look at this staffing book that I’ve got.” And so she said, “Well, yes, we apparently are looking for a limited number.” She said, “Mostly we’re looking for mechanical engineers, electrical engineers, and aerospace/aeronautical engineers, but here is an area called Energy Systems.”

I said, “Well, do you know what that is?”

She says, “No, I really don’t. It’s very generic description of this kind of thing.”

So I thought for a minute, and I said, "Well, here I've been at the university level for seven years. I've taken thermodynamics, advanced thermodynamics, heat transfer, heater design, all of these kind of things, you know, electricity. Whatever energy systems is, I must know that." So I said, "Well, I just happen to be an expert in energy systems."

She says, "Oh, well, my goodness, let me get your file from Langley Research Center, and we'll let you know."

Well, almost by return mail I got an offer from them and then got a call from a fellow named Dick [Richard B.] Ferguson, who was a Branch Chief in the new group down here, the Space Task Group that had moved down, and they offered me about the same amount of money that the refinery was going to pay me. So I talked to my wife at the time. We had a just-born baby. This was in like February of 1962, and the baby was born in February 1962. So we talked about it and decided. She said, "Well, whatever you want to do is okay. I'm going with you."

So I called back and said, "Well, as soon as I complete my thesis and everything, I'll pack the car and come down. I'll be there." So I ended up arriving here and going to work on May the sixteenth, 1962.

That was quite a day. Again, I reported in to the East End State Bank Building, which is still downtown, I think, on the east side of town, and did all the being inducted in and taking the oaths and signing everything, and was assigned to a group in the Rich Building, which was out on Telephone Road, and I worked out there.

So the group I was brought into was called the Energy Systems Branch, and it turned out that they had responsibility for things like pyrotechnics, the propulsion systems on the spacecraft, the power systems on the spacecraft, you know, fuel cells and batteries and that kind of thing. That's what that was, and I was so fortunate to end up working for a guy in the propulsion area, which turned out to be a perfect match for me and my interests. My first boss—although he was not a boss at the time, was a group leader—was a guy named Henry [O.] Pohl, who ended up being the Director of Engineering here. During the time I was with Henry, he made a fast path to the top, and I followed along in his wake. [Laughs] So I'll stop there and see what you want to know next.

ROSS-NAZZAL: What were some of your first assignments with the Energy Systems Branch?

CHAFFEE: Well, the first thing was to figure out what is a rocket engine. …

When I got here, the Mercury Program was active. That hardware had already been designed. They had flown [Alan B.] Shepard [Jr.] and [Virgil I.] Grissom, and John [H.] Glenn [Jr.] had flown. I got here about the time of the fourth flight, Scott Carpenter's flight, and so I got to support that, although I had no idea exactly what was going on. But I helped look at the postflight data, and by doing that, I was able to try to kind of understand what the little propulsion system was.

The systems that I was associated with for most of my formative years here is called the reaction control system, RCS, or attitude control system, and it's the system of many little, small, low-thrust rockets which fire in a short-pulsing mode and act to steer or to point or to hold steady the attitude of the spacecraft. So that's a completely different device from the larger engines that come on once and burn and launch you into orbit or burn for eight minutes to take you to the Moon or something like that. These are small, low-thrust kind of things on the Mercury. They were on the order of fifteen-, ten- to fifteen-pound thrust, etc. So that was the area I got involved in.

The first thing I had to figure out was what is a rocket engine. Well, I had been exposed to that in classical thermodynamics, and so I got out my textbook and there was lots of reports to read and I went to the library. We already had a good paper library here and got material. Of course, being involved with the hardware, you know, the guys who I was working with, and [they] said, "Here's the drawings. Here's the system. Here's why the system is designed and built the way it is. Here's how it worked." So I was able to work my through that and gradually just by hands-on work, you became knowledgeable in that area.

They were well along with some of the early design for Gemini at that time. The Gemini steering rockets were going to be a fundamentally different type. The small steering rockets on Mercury used a propellant called hydrogen peroxide, which is not like the 3 percent solution you gargle with. It's a 90 percent solution of pure hydrogen peroxide, which is H_2O_2, and it's an unstable material, tends to come apart to form oxygen and water. When it's in that concentration, if you use a catalyst like platinum or something like that—which is the way the little rockets worked—you squirted this concentrated hydrogen peroxide on a platinum screen, it came apart, released heat, and made steam and oxygen, and that squirted out the nozzle, and that gave you your propulsion.

On the Gemini thrusters, they were going to stay in orbit much, much longer, up to two weeks, and do substantial maneuvering, because one of the goals of the Gemini Program was to demonstrate the ability to rendezvous and dock. So the requirement was going to be for a fair amount of the rocket engine firing to change orbits and come in and slow down and dock and that type of thing. So instead of the single propellant, they used rocket engines which, number one, were larger in size, and they had two sizes on Gemini. They had a series of—and I can't remember the number now—twenty-five-pound-thrust rocket engines which were in the nose of the Gemini, which controlled it during its reentry.

Then on the back end of the Gemini, in a piece that was similar to the service module of the Apollo, they had the on-orbit maneuvering system, which was a system of a hundred-pound-thrust rocket engines, and instead of using a single propellant that would come apart catalytically, they used a more classical combustion system that had a fuel and an oxidizer. The fuel was a unique material called monomethyl hydrazine, which was a material I wasn't very familiar with. The oxidizer was a material called nitrogen tetroxide, which is very similar to very concentrated nitric acid. Those two chemicals have the property of being hypergolic, which is a term that means when you squirt them together, they ignite and burn on contact and don't need an electrical igniter or something like that.

So they [were] perfect for this activity that requires very short burns. You know, you could just open the valves a short period of time, and by short I'm talking about a few milliseconds, maybe twelve to fifteen to twenty milliseconds, and have just a tiny slug of fuel and oxidizer get squirted into the combustion chamber of the rocket engine, where it would bump together, ignite, and burn, squirt a little plug of hot gas out the nozzle, and that gave you your push to control your attitude. So those things were very interesting.

The other requirement that was completely different and an area where I started making an impact was that these engines were described as being ablative engines, meaning that they had very thick walls made of a material that would burn and slough away during the operation of the rocket engine, and that was the way that you controlled how hot the outer shell of the rocket engine got. These things were buried down inside the outer mold line or the outer boundary of the [vehicle], so you had a limit on how hot the outer cover of the engine could be at the end of its operating life, because there was other equipment around there. So the walls of the little Gemini thrusters were on the order of an inch to an inch and a half thick, made out of a plastic material reinforced with fiberglass, very much like the material used to make speedboats and that type of thing.

It was a delightful process for a chemical engineer to try to understand. When you put in this huge amount of heat into the inner surface and begin burning the plastic and melting the glass fabric, and that generated hot gas, which percolated out and helped to restrict the heat coming in and carry away the heat coming in, and that was a technique to keep heat from getting into the wall of the rocket engine. So you had a tremendous heat flux there, which in a rocket engine the amount of heat per square centimeter or square inch going in is a huge amount, because the gas combustion temperature is on the order of fifty-five hundred degrees Fahrenheit. The velocity is very high, so there's a scrubbing action, and that really drives the BTUs [British Thermal Units] into the walls of this rocket engine.

At the same time, the plastic is pyrolyzing or charring and coming apart and creating all these chemicals, which are bubbling out and acting to block the heat coming in. The glass is melting and moving around, so you're absorbing heat that it takes to melt the glass, and it's acting as an insulator, but then also some heat is being conducted through the part of the plastic that hasn't burned and charred up yet, so it's just a wonderful problem for an engineer to work on. How do you figure out how long this thing is going to last? Is an inch, the wall thickness, enough? Is this the plastic right? Have I got the glass and the plastic mix right? And all that kind of stuff. Those were the kind of things that mechanical engineers and electrical engineers all look at and say, "How do you do that? Who knows anything about that?"

So I was able to raise my hand and say, “I do. I know about that.” [Laughter] So one of the contributions that I helped to make, although it was a minor thing, was to figure out some of the design parameters as to how the orientation of the reinforcing glass fabric in the wall ought to be to keep it from what we call delaminating, you know, coming apart and making big cracks between these layers of glass fabric and that type of thing. Henry Pohl at the time was very proud of me and very happy with some of the suggestions I made, the “Well, why don’t we do it this way instead of that way?”

At that time we had the marvelous capability at the Center, because our division, which was called the Propulsion and Power Division at the time, had our own set of test facilities here at JSC. So we could go out and commercially buy stuff, take it down to our own test facilities, and completely independent of the prime contractor and the subcontractor, do our own design, our own fabrication, that type of thing.

Although I’m probably jumping ahead, I consider the success of the Apollo Program was very heavily a product of the fact that the government engineers had the opportunity to evaluate the hardware that the vendors were saying was going to do the job in our own test facilities, independently, and not only that, to go and do alternate designs of our own that we could offer up to a Program Manager, saying, “Look, you know, Rockwell or McDonnell Douglas is having trouble, and they don’t want to do it this way, but we did it this way, and look, it works 30 percent better,” or something like that.

So we had a tremendous leverage on our contractors, both prime and subcontractors, because of the fact that we had hands-on experience and could get our own hardware built. We could design it,; we could go to our own shops here in Houston, get the stuff built, and then go to our own test facilities and get it tested, and then use that as a cudgel on the Program Manager and on the prime contractor and that type of thing. That capability, unfortunately, really has been decimated over the years, and we've gotten to the point where—although I think it's rebounding now, it got to the point where NASA engineers were book smart, but their experience was gained by looking over the shoulder of the contractors, and they didn't have personal, hands-on experience.

I came out of an environment where I was comfortable in a petroleum refinery, and I understood big industrial equipment and that kind of stuff. Came down here, within a year I was completely comfortable with small aerospace-type stuff, high precision, high set of requirements, exotic chemicals and processes, and this kind of stuff, and because it was a very esoteric field, in a year or two I was probably one of the best-known experts in the field of these small rocket engines around. In a worldwide community that might have been fifteen or twenty people, I was one of them that when they wanted to know something, they'd come ask Norm Chaffee, "What do you think?"

Some of my best memories are in those times, being taken by my Division Chief, who was a fellow named Guy [Joseph G.] Thibodaux [Jr.], and my Branch Chief, who was Henry Pohl, and my Section Head, who was a guy named Chester [A.] Vaughan, all outstanding guys. Something would come up, and we'd have to go talk to the Program Manager or something like that, and I'd go up and there would be this room full of senior folks. I'd get to say what the division thought, and I understood that my division management stood behind me and had understood what I was going to say and agreed with it. I was always given a very polite, thoughtful audience by the people, thanked, and often I was challenged and probed and this kind of stuff.

But the thing was that as a twenty-five- or twenty-six-year-old kid, here I was talking to the Gemini Program Manager, Charlie [Charles W.] Mathews, or something like that, and he accepted what I told him. Then we'd use that. We argued with the contractors a lot, because they had their own experts, and we had our professional disagreements, that kind of stuff. But it was such a blessing to come and be in this new incipient organization that wasn't hidebound organizationally or bureaucratically, in an area that was just developing its technology and that kind of stuff, where as a young man I had an opportunity to become an expert in an area where there were few experts, and have my expertise evaluated and considered and accepted, in most cases. As a result of that, I gained rapidly more and more responsibility and that kind of stuff. I'm sure in no other set of circumstances, hardly, could I have had that wonderful experience that I had as a young man, being given all that I could do and more, and feeling like I was taking advantage of it and doing what I wanted to do.

ROSS-NAZZAL: What an exciting time.

CHAFFEE: It was. And it's still an exciting time, it's just tougher to penetrate. I'm so excited now that we're going to go back to the Moon and go to Mars, that if I could hang on another twenty years, I'd love to be here. [Laughs] That's why I'm doing education outreach now. I want to let teachers and young people know that this is your exciting time. We're going to go back. We've been there before, but everybody's forgotten it, and we've forgotten how to do it. If we're not careful, we're not going to be able to go back to the Moon. But then going on to Mars is something that's really outstanding.

When I go out and talk to schoolkids, particularly in middle school, I tell them, "Somebody of your age, somebody that's now in grades six to ten, will be the first person to step foot on Mars. It may not be an American. Unless we work very hard, it won't be." And I said, "It may not be a male, like Neil [A.] Armstrong. There's absolutely no reason why it should be a male." So I look out at the class and say, "You kids, if you want to be the Neil Armstrong of Mars, whether you're male, female, Black, Anglo, Hispanic, Indian, Asian, whatever you are, if you want to do it, you can do it. But you've got to work your butt off." And I say, "You'll have a lot of fun if you want to follow that path."

ROSS-NAZZAL: I wonder if you could walk us through, beginning with the design parameters for Gemini, how you came up with, first, perhaps, the theory, and then the design and the testing.

CHAFFEE: Pretty much the theory is textbook, you know, structures and heat transfer and fluid dynamics and that type of thing, so it's just a matter of trying to figure out how do you apply the well-known engineering equations and stuff to a particular situation, a particular geometry, a particular material, this type of thing. So you start out, and on the Gemini Program, the requirements were already known, as far as what's the thrust level we need, because we knew what the mass and the geometry of the spacecraft was, and we knew from the control folks how fast they wanted to move, and that kind of stuff. So all that defines how hard you've got to push to make it move, which is the thrust of the rocket engines.

So basically all of those requirements were defined for me, or I wasn't part of the group that did that design. So I was given a set of requirements, and the problem was how do you take that set of requirements and select the geometries, the materials, and the other engineering details to make a product then that meets the specifications, and so that was the thing that we were focusing on, trying to figure out.

The rocket engine can be considered to consist of four or five key parts. The first part is the valve. In the case of the Gemini thrusters, now, where we've got both a fuel and an oxidizer that have to be introduced into the engine, and the little firing pulses are very short, you have to have a valve that operates very rapidly. It can't leak. It has to be electric, because that's the only way you can get a valve open and shut that quick. We use a device called a solenoid valve, which uses an electric field to pull a valve, poppet off the [valve] seat and let a little bit of liquid flow. So the valves are a key point.

Then there's the thing called an injector, which is like the showerhead in your bathroom, which takes the fuel and oxidizer which come in, and spreads it out and squirts it in little bitty streams into the combustion area of the rocket engine. The idea is you try to make a [stream] of oxidizer and a stream of fuel hit together so that they splash against each other and immediately burn and that type of stuff. Then there's the combustion chamber. This is this area that holds this hot gas.

Then there's an area called a throat. It's a part of a converging, diverging nozzle thing, but squeezes down the hot gas and forces it to then accelerate out through an expanding part called the nozzle. The nozzle is what you generally see when the public looks at a rocket engine, like the Shuttle or something. You see these big nozzles sticking out the back. The combustion chamber is back inside. You don't really see it or the valves or that kind of thing.

So I had a little bit of interest in the valves, but the thing I was really interested in was the injector and its design, how you distributed the fluid fuel and the fluid oxidizer around and forced them to come together in a way that made very efficient combustion, because combustion processes are something that chemical engineers study that no other body of engineering studies.

Then the combustion is a very violent process. You can imagine, for instance, the combustion in the cylinder of your car when you squeeze—you know, draw in some air, squirt in a little bit of gasoline, the spark plug goes off, lights it, and then you get this explosion that goes on in there. There's tremendously high temperatures, high velocities. It's running around against the wall. The higher the velocity of the gas is inside the combustion chamber, the more the heat transfers into the wall, and that kind of stuff. The materials you select has to survive the pressure surges and the high temperatures and that kind of stuff.

Then the throat design, because of the scrubbing action of the gas going out the throat—it's going at sonic velocity by thermodynamic definition—will tend to scrub and erode that throat. Well, your thrust of your rocket engine is set by the diameter of the throat, and so you don't want it to change dimension very much at all, or your thrust changes and can change dramatically.

There's also a thing called thermal shock, where it's if you get a rocket engine that hasn't been operated for a while. All the materials are cold in space. Suddenly you turn it on and fire it for fifty milliseconds or a hundred milliseconds, and sock it with a shot of heat. With fifty-five-hundred-degree gases, it's kind of like an ice cube; you know, if you take a real cold ice cube out of the refrigerator and drop it in a glass of tepid water, it all shatters. That's called thermal shock. So materials can thermal shock. We were using a ceramic material for the throat in the Gemini thrusters that very quickly would break up into pieces because of the thermal shock of the heating when they weren't used for a while, or even if it just cooled down to room temperature and then you hit it with something that's five thousand, five hundred degrees, and it is a big blow thermally to it and stresses it and causes it to crack.

So I was able to work—given the parameters of what we had to do and knowing that we wanted to have an ablative rocket engine, which is this thick-walled thing made out of this plastic and reinforcing layers of glass cloth, and had a ceramic throat that we wanted to preserve the dimension of the throat. Then the nozzle was also this ablative material, but the heat transfer out [in] the nozzle is much lower, because the temperature, when it expands, gets to be much, much lower, so your heat transfer is not a problem. So I was able to work with parameters of what is the plastic material that we use, what is the nature of the glass reinforcing fiber, what's the geometry of the chamber, what is the material of the throat that we want to maintain its dimension, how is it put in there and how is it retained, and then what is the design of the fluid injector that squirts the fuel and oxidizer in there.

Well, again, I was a young fellow, and there was lots of other smart people working on these kind of things, including the prime contractor and the vendors. But we were able, because we had our own facilities, to go design our own pieces and try things out. We would look at what the contractor did, evaluate their hardware. It turned out at that time the Air Force was doing a lot of research, and we had done a lot of research on ablative materials because ablative materials were also the heat shields that were used for Mercury and Gemini. Although the application is different, the thermal chemistry of charring the ablative material and burning it away—and if you go over to the Space Center Houston [Houston, Texas], now, and look at the heat shields of Mercury and Gemini, they're all black and burned and all that kind of stuff. That's sacrificial material that kept the spacecraft cool as it came in.

Well, one of the things that we came up with was the vendor was using an orientation where they just stacked up glass fiber, impregnated it with the plastic material, formed this what we call a billet. It was kind of a big cylinder of this stuff, and then they would machine out the internal contour of the rocket engine. But the plies, or the layers, went straight from the inside diameter of the rocket engine to the outside, and a lot of times because of heat shock, thermal shock, during the firing of the engine during its lifetime, those plies would begin to break apart and you'd have little cracks between the layers of the glass. Well, every time you had a crack running down into the interior of the wall, and every time you fire the rocket engines, suddenly it fills up with this high-pressure gas, drives that hot gas down into the crack, which takes extra heat down in there and tends to force the crack open a little bit more. So we were having failures where we were having the hot gas burn a hole through the side of the rocket engine before it had charred very much of the thickness.

So we looked at it, and one of the recommendations we came up with, and I was concentrating on some of these materials and processes at the time, was instead of just stacking this glass material up, why don't we lay it in at an angle such that the hot gas is running this way with the glass fiber running back that way, and it tends not to delaminate as much. That turned out to be very successful. We were able to work with the vendors, and they ended up adopting an angle of forty-five degrees, basically, as I recall; we may have actually gone to sixty-degrees, I can't quite remember that now, but an angle of the glass fiber laminations.

Also, there's various different glasses, and we went to a quartz fiber, which is a very pure glass, and essentially weave a cloth out of the quartz fiber. You hear the term fiberglass, and that's basically what it was, except that the fiber was a very pure quartz fiber, and that seemed to have the best melting properties and supportive properties and that type of thing. Then we used the plastic material called phenolic resin, that kind of thing based on the chemical, phenol, and that ended up working very good. We were using a silicon carbide throat in the Gemini throat piece, which we made a mechanical design that would mechanically hold the throat in there.

We never did completely solve the cracking problem, because ceramic materials tend to be very, very susceptible to thermal shock, just like this coffee cup. If I were to drop this into a bucket of liquid nitrogen, it would break it, because the ceramic coffee cup couldn't adjust itself to the fact that the outside was trying to shrink all of a sudden, you know, because it was getting real cold. So what we did was—unfortunately, but it's one of these practical matters—we defined a specification that said the throat piece can't break into more than twenty-seven pieces, and none of the pieces can be ejected or come out. [That] ended up working. We'd get these things back and look at them, and sure enough, they'd be cracked and that kind of stuff, but as long as the pieces didn't get ejected so that we changed the dimension of the throat, then we concluded it was okay. It was one of those things; we didn't want to do that, just at the time we ran out of time and money to solve the problem.

There would have been some other things to do, but the Program Manager, who has to be a practical individual, said, "Look, I've got a certain amount of money and a certain amount of time, and I've got to get on with this, and okay is good enough for me. It doesn't have to be perfect." In fact, there's a Program Manager's dictum called, "Better is the enemy of good." I've always remembered that and worked with that the rest of my career, that if you're in charge of something, it don't have to be perfect. It has to meet the specifications and meet the requirements and be good, and at that point, you quit working on it and go on to the next thing.

Another mode of operation we had at the time, because NASA had very generous national funding; I think in the later sixties the NASA budget was even 5 percent of the national budget for a year or two as we've homed in on the Apollo final development, much, much more, relatively, than the fraction of a percent that we get now. But there was very generous funding and when those of us that were working in an area had alternative concepts or alternative ideas, we could present them and be funded to go and do alternative approaches. You say, "Well, I think there's this other plastic material I'd like to evaluate. I want to go out and make some rocket engines with these six other different plastic materials and evaluate them in our test facility and use different glass fabrics and use different geometries and make some different injectors and that kind of stuff, and try this stuff and then look at the data and see what we've got."

The result of that was that we were frequently smarter than the contractors on this stuff, because they had their one design, and they were trying to make it work. Sometimes they'd do preposterous things, in our view, to try to convince us that something was okay just because they didn't know what else to do. That time within NASA, at least in the area that I was in, because of the fact that we had leaders who had come from other organizations, NACA [National Advisory Committee for Aeronautics] in the case of Guy Thibodaux, who was the Division Chief; the Army propulsion area in the case of Henry Pohl; Chet Vaughan came from NACA; that kind of stuff. Those cultures were all heavily "You have to have personal expertise. You have to have hands-on experience," that kind of stuff. That was the culture that they were comfortable with.

So when they got a tabula rasa like me, that culture permeated. They said, "Well, if you think you'll get some better way to do it, go make one and try it. See what you got." So that's the way I grew up within the NASA culture, very hands-on. I could design stuff myself, get it built, get it tested in our facility. I worked with alternate contractors. They'd come in and say, "Well, we think we can do this better than the Rocketdyne Corporation is doing for McDonnell Douglas."

I'd say, "Well, tell me," and we'd argue, and I'd say, "Well, okay, I'm going to give you a chance. I'll give you $50,000. You build me one of those and send it to me, and I'll evaluate it," that kind of stuff.

As a result, the people like me were able to build up an excellent personal expertise in hands-on engineering that, even if I'd stayed in the refinery, it was clear to me that the culture there was not going to be that I was going to get to mess with the equipment. The union operators were going to get to do that, and I was going to look over their shoulder and suggest maybe do this or maybe do that. So it was a wonderful time for a young engineer, and I think the program, the Apollo Program and the early Shuttle Program, benefited from the fact that the majority of engineers working on those things had personal, deep expertise, understood the processes and understood the organization, that type of thing.

We have lost a great deal of that in the decades since then. But that was, in my view, that was why we were so successful in the Apollo Program, even though we had our failures and that type of thing. It's the lack of that experience that makes me fearful for the vision for space that's being enunciated now. I don't see in the young engineers now the same level of personal expertise, although I think our new Administrator [Michael Griffin] appreciates the fact that we have to get back [closer] to NACA type culture where the employees themselves are not observers of work, but doers of the work in a very deep sense.

ROSS-NAZZAL: Would you describe for us the test facilities of your division out at MSC?

CHAFFEE: Yes. We started out in the specification for the Center that we were going to have—the Propulsion and Power Division was going to have a series of test facilities that supported its entire range of responsibilities. That area is called the Energy Systems Test Area now. It was called the Thermochemical Test Area, TTA. It's now called ESTA. It was to have—let me see if I can remember this—it was to have a space vacuum chamber where we could simulate space vacuum. It had a fuel cell test area where we could do testing of cryogenic materials, not only the tankage, but the other components that were required to work with liquid oxygen and liquid hydrogen, and the fuel cell. We had a propulsion test facility where we could do rocket engine testing. We had a pyrotechnics test facility where we could do the explosive devices, the breaking bolts and things like that.

Then we had a fluids test facility that also doubled as an area where we could do—what's the word? I can't think of the word—but, you know, we could do weights testing and calibrate our instruments. We could clean materials, because all of the materials, valves and pipes and everything, have to be clean to a certain level. We had a cleaning facility over there and metrics, I guess, weights and measures kind of thing. Then finally in the Headquarters Building, we had a very, very capable machine shop. So for many, many years we were a standalone activity. We had some contractors that supported us, too. I can't remember whether it was Northrop at that time or not. But we had chemists and machinists. We had draftsmen, that kind of stuff.

So as a young engineer, I could say, "Well, I want to try a different injector design for this rocket engine." I'd draw it up myself, but I wasn't a skilled draftsman and couldn't necessarily make something that a machinist could relate to. So I'd do the best I could and then go talk to one of these draftsmen, and they'd help me figure out how to get it into what you call a working drawing that a machinist could get to. Then very early on I also learned to go down to

Building 8 and talk to these wonderful, wonderful NASA machinists. We have since fired all those guys, unfortunately, terrible, terrible error, in my view. But these were civil service machinists. Many of them had come from Langley, and they were what you call experimental machinists. They weren't making oil company gears and that kind of stuff. Very fine, tiny, aerospace precision parts and that kind of stuff.

I'd take a drawing down there. I learned after one bad experience. I'd take the drawing down there to the machinist and say, "Ernie, here's what I want to do, and here's the drawing I got. What do you think about this?"

He could look at it and say, "Well, how the hell do you expect me to make that? How am I going to drill this hole when I can't get the drill in here?"

So, "Oh, you're right."

So then we would work through that, and he'd say, "Well, if you split this piece here, we can still control the dimension, but now I can get in here and I can drill this hole, and when I'm through I can go in and do what I call deburr it to make sure there's no little metal edges hanging out and all that kind of stuff."

So I quickly figured out that there's a whole bunch of supporting people around JSC. There was materials experts. There was chemists. There was draftsmen. There was machinists. You know, people that knew how to do other things that I didn't know how to do as well as they did, and I was an idiot if I didn't go around and get their input in what I was doing. So, you know, bingo, the concept of the team thing dawns on you. You can't do this yourself. You're not expected to do it yourself. You're expected to use the resources of the organization.

So as a result, we did that, but the test facilities we had in the Thermochemical Test Area, which still exists as the Energy Systems Test Area, were absolutely wonderful, and I spent many,

many, many thousands of very productive and enjoyable hours down there during Gemini and Apollo. The Center didn't open down here until '64. But before that, because of the entrepreneurial spirit of Henry Pohl and Guy Thibodaux, we built our own little test facility up at Ellington Field [Houston, Texas], and very quickly, within a year after I got here, we were doing rocket engine tests with hypergolic propellants, which are also very dangerous and toxic and explosive and that kind of stuff, up there. If anybody cared; we didn't bother to check with anybody. The Center said go do it, and we could do it.

Later on, must have been in the late eighties or early nineties, we were barred from having those kind of chemicals at the current test facility, because we did have one spill in there and made a big cloud of nitrogen tetroxide, which floated over the fence line, and by that time we had Clear Lake City and the University of Houston-[Clear Lake, Houston, Texas] and all that, and there was a big uproar, and they said, "Nah, no more. You guys have got to take all that stuff and go to White Sands [Test Facility, Las Cruces, New Mexico]." Well, White Sands is also a wonderful place, and I've spent lots of good time out there, but it's a whole lot harder to go out to White Sands and check on something than it is to jump in your car and drive down to the Thermochemical Test Area and check on something.

So those facilities and the people that ran it saved the bacon so many times for us, by the fact that we could go down there and check something out. The vendor would tell us something, that this is the way something works, and we said, "No, it isn't. We have either run your piece of hardware or our own, and we know that's not right. You're blowing smoke." That was happening all over the Center, in the Life Support Systems, in the Guidance and Control, and everything. All the people, we had our own facilities. We could do our own work, evaluate what the contractor was doing, get a piece of their hardware and evaluate it. Absolutely critical to the success of the program.

ROSS-NAZZAL: You mentioned that you were working with highly combustible material. Can you talk about the testing of Gemini and how you were able to protect yourselves and the other people who were doing this testing?

CHAFFEE: Sure. The test facility, you know, if you build one, has what we call test cells, and in the case of a propulsion test, at Building 353, which has still got that number, in the Thermochemical Test Area, that was the propulsion test facility. We had six test cells, as well as a large subsystem test chamber, where we could have not just individual components, but put together a systemwide kind of thing. But you would basically get your hardware, and the technicians and the engineers would go out and build up a system exclusive of introducing any of the propellant. But you'd get your engine and your pipes and your valves and your pressure regulators and your check valves and relief valves and your filters and all the things that you needed in the system, and all your instrumentation. Go out there, build that up in the test facility, and then check it out to make sure it was working, either by just flowing water through it or gas, to make sure there was no leaks and the instrumentation was working and that type of thing.

We had two large storage tanks out there, one that held the fuel and one that held the oxidizer. When we were working with those tanks and then loading propellants in the tanks, that kind of stuff, everyone had to stay inside the facility or else just leave the area and go back to the office building. The people who were actually handling the transfer from trucks into the supply tanks were dressed in what we called SCAPE [Self-Contained Atmospheric Protective

Ensemble] gear. It was full suits with a breathing apparatus and that type of stuff, in case there was a spill or something like that. Once you introduced it into those feed tanks, then we had internal systems which would pipe the propellant over to the device that we were going to test and that type of thing.

During the test we were inside in the control room, which was totally separated and had air intake designs, all that kind of stuff, so if there was a spill, we wouldn't draw vapor into the control room and that type of thing. We had both windows that we could look out into the test cell and, of course, closed-circuit TV, and then all the instrumentation was inside, and we would sit there and watch all that and do the tests. So that's the way we protected ourselves.

I remember—I don't know whether you want funny stories, but—

ROSS-NAZZAL: Absolutely.

CHAFFEE: I remember one funny story, when one of our experienced test conductors was running a test, and one of the technicians was smoking a cigar. That was back in the days when almost everybody smoked, including me. Nowadays, you know, almost nobody around here smokes. But you weren't supposed to smoke during a test, because you were busy and that kind of stuff. Well, this test conductor was sitting in front of a big instrumentation display that had all kinds of electronics behind the gauges and this kind of stuff. But there was a place where you could walk back behind this display case of instruments. So the test conductor is sitting there; he's looking at all the instruments. He's got all the controls in front of him. This technician walked back behind there, and the test conductor was a relatively new guy who hadn't been in the—test conductor is like the Flight Director, you know—he's in charge of the test and the room. He went back behind this console, which was like eight feet high. You usually can't see back there. Lit the cigar, got a big lungful of smoke, and then blew it through the back of the console.

So this guy is sitting there, trying to do the test, and all of a sudden here comes all this smoke coming out through the front of the console around the instruments and through the airholes that are there so the instruments don't get too hot. [Laughs] "Hey!" He exploded with an expletive. All of these tests, because they're dangerous, have what we call a chicken switch, which is you chicken out; it's an emergency shutdown switch that you hit it and everything—all valves close, everything stops.

So he says, "Chicken switch!" and he hits this button [demonstrates] to shut the test down, which was going along just great. Everything was doing fine. We lost the data and the test, and we had probably half a day invested in getting the thing all set up and ready to go. In the end, the technician, I think he got suspended for a day or a week or something like that. But it's stuff like that that you remember.

ROSS-NAZZAL: How long did a test typically last?

CHAFFEE: It would depend. The little engines that I worked on, for instance in the Apollo thrusters, which were a hundred-pound thrust and very small—they're about fourteen inches long—the shortest pulse that they could fire was essentially ten milliseconds, which is one one-hundredth of a second. It was almost indistinguishable, and frequently we would have VIPs [Very Important Persons] from NASA Headquarters [Washington, D.C.] or somewhere come down, and they'd say, "Oh, you know, we want to go fire an engine for them."

So we'd set the thing up and go through the formal countdown. You know, "Fuel pressure okay. Oxygen pressure okay. Helium pressure okay. This valve open. That valve open. Relief valve okay." You'd go through your checklist and you'd go through your countdown to fire. So we'd go, "Five, four, three, two, one," [imitates noise], and that's it; it's a ten-millisecond thing.

The guy said, "It didn't work."

"Hell, yeah, it did. That was it." You know, great disappointment. So then we would go and fire for a second or do a series of spurts, "pop, pop, pop, pop, pop, pop, pop," machine gun kind of thing. We always started out with a single ten-millisecond burst, because the people would say, "It didn't work."

But it was a highly disciplined process, and when you test something like that that's dangerous, you don't just go out on the spur of the moment and say, "Well, let's unhook this joint and see what happens," or, "Let's try this." So everything is done by written procedure. So you start out with a schematic of all the equipment that's going to be out there, and then you figure out, "What is it I want to happen?" And you go through and you define, "Okay, I've got to close this valve. I've got to open this valve, and before I open the next valve, I want to read the temperature and the pressure to be sure things are the way I think they should be. Then I open this one, but then I got to go back and close this one. Then I want to read the pressure on the relief valve to be sure that's okay."

So you get all these steps and procedures. You write all that down, so you assign somebody. So you go, "You write the procedure for how we're going to do this." Then that gets reviewed by at least one other person, sometimes two other people. Then once they think it's okay, it gets reviewed by a team, including a quality person, and a safety, reliability, and quality assurance person, and that kind of stuff. So there's multiple levels of oversight here, and they may say, "Well, at this point, we need to stop, and the quality guy needs to go in and stamp that this is okay," or this kind of thing. How long has it been since we calibrated this pressure gauge? Is it going to be in calibration during the entire period of this test, or do we need to change that out?

Then there's a whole series of what ifs, emergency situations. If this happens, what do we do? If this happens, what do we do? If this happens, what do we do? That's kind of an engineering natural, except I learned that many engineers don't naturally think logically like that always. Some of them, they're so entrepreneurial and excited, they just want to jump from the idea to the end result. Oh no, there's fifty steps between here and there. We've got to figure all this stuff out.

So what it does is instills in you this very disciplined, step-by-step what am I trying to do, what does each step that I want to do require, and how do I describe it such that you can't do it wrong, and now how do I write it down so that nobody can misinterpret what it is that I meant. What checks and balances in here do I need for the quality guy to say, "Yes, you've done that. I've put my stamp on it," and this, that, and the other. This is a very important skill for an engineer.

When I go and talk to schools now, one of the things that I emphasize when I'm talking to kids that want to be technical, is you have to be a good writer, and by writing, I don't mean fiction. I mean, you have to be able to very explicitly figure out what it is you want to do in a step-by-step process and write it down so that somebody can follow your instructions and get it right and not hurt themselves or somebody else or this kind of thing. When I first started doing education outreach after I retired in the end of '96, the second or third year I was doing it, I got a call during Engineers Week from some school, and they said, well, this English teacher wants somebody to come out.

I said, "Oh, I'd love to do that," because English is a critical skill for engineers. So I talked to this gal over the phone.

She was apprehensive about, "Well, what can you do?"

So I said, "Well, do you do writing in your class?"

"Yeah. Well, yeah, that's part of it."

Said, "Let me come out and I'll teach the kids how to write something that's not a story."

So I went out, and this wasn't my original idea, but I took a loaf of bread, a jar of peanut butter, and a knife, and went out there and said, "Okay, kids. I'm a Martian. I don't have any idea what a peanut butter sandwich is or a knife or anything like that. I want you to write me a procedure to make a peanut butter sandwich, because you want me, as a Martian, to try and enjoy a peanut butter sandwich."

They said, "Oh, yeah, that's easy." So all these kids start writing. So you give them twenty minutes to write a procedure.

Then the teacher selects one, says, "Okay, well, now we'll try Johnny's procedure, see how that works."

Well, Johnny says, "Take knife. Dip into peanut butter." So I'd take the knife and grab it by the wrong end, you know, try to—you know, he didn't say, "Open the jar of peanut butter," so I try to stick the wrong end of the knife through the lid of the peanut butter. "Put on bread," so then I tried—it didn't say take any bread out of the wrapper or anything like that.

So pretty soon, you do some of these—they're all wrong; they leave out details—pretty soon the glimmer begins to go through, "Oh, I didn't think about that." It's a very educational thing. The teacher loved it, and I ended up doing several of those, because the word kind of got around that this was a neat thing that kids could really relate to that promoted disciplined thinking as well as complete assessment of a situation and that type of thing.

So one of the things I've really enjoyed as a retired person working with teachers and kids is the ability to take my experiences and my continuing excitement about space and space exploration and that kind of stuff, and bring that in a way that, for most kids, can turn on their excitement. You can't get them all. When you go to a classroom, there's always a few whose heads immediately go down on the desk and they go to sleep. They're not going to be interested in anything. But your reward is to look around and see these bright little faces and the excitement and the lights going off above their heads and that type of thing, of thinking about, "I can do that."

Anyway, the facility was important, and the way we protected ourselves was a very disciplined process. That has to be the case in any—you know, over the years there's been a number of accidents at the Center where people were even killed because of a breakdown in that discipline. I remember back in the eighties there was a battery that exploded over in the crew systems area and killed a technician, because they just hadn't paid attention to the first process they were using.

ROSS-NAZZAL: When the Center finally opened, where was your office located?

CHAFFEE: Well, I started out in the Rich Building. I think that's on Telephone Road, and it's still over there. Every once in a while I go by and look at that and think, "Golly, just imagine that as a big, old, windowless building on Telephone Road."

Not too long after I went in there and worked—that's where all of the Energy Systems people were—President Kennedy came down to visit. Suddenly, boy, here on Monday or Tuesday there was all this activity, and there was new carpet in the lobby, and they went and they spruced up one hallway, and they put new flooring in the hallway, painted the hallway, and it went down to this exhibition area where we had a mockup of what we thought a lunar lander might look like. The rest of the building was in pretty bad repair. Somebody said, "The President's coming!" I was really impressed with the way they fixed up only where he was going to go, you know, that type of thing.

But we were there for about a year, and then we moved to the Ellington facility. They fixed up a bunch of those old barracks down there and turned them into office buildings. We were there for a year. I guess it was early ['64] when our area first moved down, and the Propulsion and Power Division moved into Building 16, because I remember at the time, in November of ['63], when President Kennedy was assassinated. That's a memory that everybody who was alive at that time vividly knows where they were. I was in an office building in the barracks at Ellington in my office when we got that word. Then I think it was early in '65 when Building 16 was ready for us to move down and occupy it. Over my career I think I've been in so many different buildings. I've worked in Building 1, Building 16, Building 12, [Building 13], Building 17, Building 45, off-site in the Nova Building, Building 36, and Building 32. Oh, and Building 9. So a whole bunch of buildings as I've moved around during my career.

ROSS-NAZZAL: Yes, you've been all over.

CHAFFEE: Been all over the place, right.

ROSS-NAZZAL: When President Kennedy came down, did you get to meet him?

CHAFFEE: No, no. You had to stay in your office. Nobody could go down. But he had come in—no, this was a different time. He had come in and nobody got to see him. The Secret Service locked the place down, but Dr. [Robert R.] Gilruth, of course, met him, and Max [Maxime A.] Faget and the big wigs met him and walked him down the hall, showed him the lunar module. Seemed like that was the time when then he went over to Rice and made the talk in that Rice Stadium. That's the famous talk.

He later came back. In fact, it was on the trip that he was on to Texas when he was killed. He had come to Houston the night before. He flew into [William P.] Hobby Airport [Houston, Texas], and then there was a procession with his car down Broadway, going to downtown Houston to whatever he was going to. So I got my wife and our little girl, and we went over and stood on Broadway and watched him go by that night. Then that evening he went to some event in downtown Houston, came back to Hobby, flew to Dallas [Texas], and then the next morning was when he was assassinated. So I did get to see him that one time.

I think I've seen all the other—President [Lyndon B.] Johnson was here several times. [Richard M.] Nixon was here at least once after the landing, lunar landing. [Ronald W.] Reagan was here. I don't remember whether Jimmy [James E.] Carter [Jr.] or President [Gerald R.] Ford were here. But President [William J.] Clinton came, walked right past me when I was over in the robotics lab. He walked by and looked at the robots and that type of thing.

ROSS-NAZZAL: After President Kennedy was assassinated, what was the mood like at MSC?

CHAFFEE: Well, it was shock, just like it was all over the nation and the world. There was no feeling that there was going to be any impact to what we were doing, because we were already committed as a nation and a government to do this. But, you know, great sadness that this had happened. Of course, everybody, during that week, everybody was just glued to the TV watching all the things that happened and that type of thing. But it's incredible that that's been over forty years. I can't imagine how forty-plus years could have passed since that time. But, there may have been some rededication to the program, but there was such incredible dedication already, it's just hard to say how you could have dedicated—the workforce at that time was so committed to the job that it was almost the classical thing of, you know, they talk about Apollo divorces and that kind of stuff. I mean, people that just never went home. They lost track of how many children they had or what their wife's name was and that type of thing, almost. The job was the thing.

In my case, I did the same thing, and now as I look back, I have very mixed feelings, because I missed a lot of quality time with my wife and my kids that—when something came up, they'd call me from the test facility, if I wasn't on the late-night test shift, and say, "Well, the thruster—little rocket blew up."

"I'll be right there," and whatever I was doing, I'd drop it.

I remember one time my parents had come down to visit from Tulsa, and we were having dinner. They called up and said, "We've got a problem."

So, "I've got to go." Zoom. Spent the next sixteen hours down there trying to figure out what happened and what to do about it and all that type of thing. So there was lots of—you know, what we didn't appreciate was probably misplaced sense of responsibility at the time, or an imbalance in your personal time. In the people now I see a much better sense of balance a lot, and I think the current generation has a sense that "I have a life and my family has a life and NASA owns part of me but not all of me."

ROSS-NAZZAL: Can you give us a sense of a typical workday for you? Obviously, you were working long hours and working evenings.

CHAFFEE: Typically, I'm not a morning person, so I never came in real early unless I had to, but it always depended on my level of responsibility. I rarely went home at four-thirty; it was always six or seven o'clock before I got away. I worked almost every Saturday and some Sunday afternoons; after church I'd come in and that type of thing.

At one point in my career, in the mid-eighties, after I had left the Propulsion and Power Division, I'd grown over the years, and I was Deputy Division Chief in the Propulsion and Power Division when the first Space Station Program was approved by President Reagan. The first guy who was Program Manager was a Flight Director, very well thought of, a fellow named Neil [B.] Hutchinson. He called me and asked me if I'd come over and be his Technical Assistant in the Program Office. By that time I felt like I had wrapped my arms around the propulsion and power technology and understood that; had been Deputy Chief for some years, and it didn't look like I was going to get to be Division Chief, because Henry Pohl was the Division Chief.

So I accepted that and went over and worked with Neil. He put me to be—I didn't stay as his Technical Assistant. He had a requirement for somebody to be the Chief Engineer for Integration, and that responsibility was take all the stuff that all the NASA Centers were doing, including Kennedy [Space Center, Florida], and make sure that when we put all this stuff together on orbit, it would work as a successful Space Station, not only in its final configuration, but when we took piece two up and attached it to piece one, that worked okay. Then when we took piece three up and attached it to pieces one and two, that worked okay. And that each step was a satisfactory design, because you never knew when you were going to have to wait for significant periods, which has happened, before the next planned piece came up.

Well, that was an overwhelming job. The politics, which were a very important part of the job at the time, were such that at that time I did go in early. I was going in like at six in the morning and coming home at ten at night. That was at least five days a week. I worked almost all day Saturday and Sunday afternoons. By that time my kids were up in high school and going to college, and they didn't care whether I was there or not, but I did miss some time with my wife and I missed some other kinds of things.

It depends. I think people in the early days had a misplaced sense of responsibility to the program at the expense of their families. As I said, that has changed over the years. But still there's people today, when you've got a big job to do, that the work tends to overwhelm any other sense of responsibility in your lifetime, and you focus on that. As with any crisis, you can be in crisis mode only for so long before you crash, and then you've got to back off and that type of thing. But in some of the programs we stayed in crisis mode an awful lot.

ROSS-NAZZAL: Let's take a break. We need to change out our tape here.

CHAFFEE: Okay.

[Tape change]

ROSS-NAZZAL: Did you have any involvement with the Agena?

CHAFFEE: A little bit. I'm trying to think back. We had the angry alligator part of the Agena, where [the shroud didn't separate from the docking area], and there was an issue at the time, which was another area where Henry Pohl always gave me a lot of credit, and I think more than I deserved; I really—because that's so—damn, that's almost forty years ago.

But there was a problem with the Agena engine that was going to drive the thing that we were going to dock with [the docking adapter/target]. The Agena was a practice docking target that we were going to use one of the Geminis to show that we could find and co-orbit and then dock with this thing. Well, at one point the thing, the docking adapter or whatever they called it at the time, which I don't remember, had a problem with the Agena engine, and there was a big question. The Agena was an Air Force device, and we had worked with them. I think a group from NASA had worked with them to make sure that the reliability of the Agena was such that it met the requirements of a manned program, such that if we got close to it and all that kind of stuff, there wouldn't be a problem. But the Agena engine, as I recall, in one of the flights either exploded or had an explosion [on initial ignition on orbit or during launch] that disabled it such that we weren't able to complete the mission.

Then there was a big investigation as to what happened and why. This was an area that turned out to be very fortuitous for me, because it's another area where chemical engineering and people who have been trained in that field have kind of a natural affinity for thinking about the process that must have occurred there that led to this difficulty. As I recall now, and this may be wrong, but it had to do with the sequence in which the fuel and oxidizer valves opened on the Agena rocket engine.

When you squirt a fluid into a vacuum, the stream of fluid doesn't retain its integrity as a stream. If you take a water hose, a garden hose, and squirt it out in your yard, you see this stream of water going out. But if suddenly you were in a vacuum, you'd just see this fog coming out the end, because the internal pressure of the water blows that stream apart immediately. So when you first squirt the first little bit of propellant into the combustion area of a rocket engine, it doesn't come in as a coherent liquid stream. Coming through the injector, essentially it's boiling is what it does; it vaporizes into that vacuum environment. Until enough has vaporized to bring the up the pressure inside this combustion chamber, only then do you start seeing liquid come out, and that takes several milliseconds for that to happen.

It turns out that on the Agena they had had a start sequence for the engine that—I believe this is right—they opened the oxidizer valve, oh, like forty milliseconds or something before the fuel valve, the idea being they'd pressurize the inside with vapor, but they didn't really understand what happened then after the fuel came in.

On occasion, on a statistical basis, it was a low probability, but on a statistical basis, the fuel would come in and find this combustion chamber full of cold oxidizer vapor and wouldn't immediately do its hypergolic thing. It wouldn't ignite on contact, because the reaction rates, which is called chemical kinetics, are such that just the right conditions weren't there, with the amount of chemicals being together and the temperature and the pressure, to start enough of a reaction that it would ramp up the temperature release enough to get the combustion process going. This didn't happen until the fuel had been squirting in there for several milliseconds. By that time you had a chamber that was full of unburned stuff, which is the equivalent to having a bomb. So when it did ignite, when the conditions were right for an ignition, it—boom, it went off, and as I recall, the pressure was enough that it blew the combustion chamber partly away from the injector so that the engine then didn't work.

We were going back—well, when you work your way through the physical chemistry of all these processes, which we had known about, but on the little engines, it didn't really matter that much. Turned out later it did. But the Gemini—I mean, the Agena was the first instance of a significant instance leading to program failure of a phenomenon called the [ignition pressure spike, or "spike"]. The [spike] is an unintended explosion due to a delayed ignition after you introduced both fuel and oxidizer into the rocket engine. …

Henry Pohl, who was my boss at the time, got assigned to the working group with the Air Force to figure out what had gone wrong with the Agena. He called me back and we were talking about it, and we were talking about the physical processes of the sequential introduction of the oxidizer and the fuel, and the vaporization, and the chilling effect when you evaporate a liquid, and all this kind of stuff. He said, "Well, could you do some calculations on that?"

So I said, "Sure," so I went and did it, and that's the kind of thing that mechanical engineers and electrical engineers and aerospace engineers, you know, "How the hell do you do that?" But that's what chemical engineers do, so I was able—the various heats of vaporization and heats of reaction and all that kind of stuff, go and built up this scenario and called Henry back in a day and said, "Well, here's some real insight into what I think might have happened there."

So he got them working on that and then some other people went through and refined what I had done in a day, and lo and behold, they decided that's what the problem had been. By altering the sequence in which the valves were opened, they changed that such that the conditions were not prevalent to allow that to happen again. Now, it turned out, jumping ahead into the Apollo Program, we also, it turned out, had [pressure spikes] in the Apollo Program. Same propellants, same kind of vacuum starts, and in particular, these little pulsing engines that I was always working on, many times in the Apollo development program we blew up these thrusters that we were testing in the Thermochemical Test Area, or the vendor, which was a company called Marquardt Company out in Van Nuys, California. It was the vendor to Rockwell for that. They would blow them up.

The thing that I worked the most on over the Apollo years was trying to figure out what's going on there and how can we solve that problem. We did figure it out, and we did get it solved, and I wrote several well-received technical papers that were given at conferences on that kind of stuff. I consider that one of my areas of expertise, and I still get calls now from people saying—in fact, in the past year or two I've gotten a call from a young engineer who probably wasn't born at the time I was working on that, who's now working over in the current Energy Systems Division, saying, "Do you remember this thing called a [pressure spike]? Can you tell me what that is?"

I said, "Yeah, I'll be right over. I'll tell you about that."

Because all that documentation that we wrote down, the reports and that kind of stuff, God knows where it's all gone to. It's in an archival place somewhere. The people who knew about that are retired or dead or somewhere else, and so the young staff now just doesn't have this base of experience that—we didn't know it, either, you know, but we learned it and figured it out, because we had facilities and capability to go look into it ourselves. Without our own facilities, we would have been hard pressed to figure it out. We would have been at the mercy of the contractor for them to say, "Well, we think it's this."

And we would have only been able to nod our head and say, "Yeah, you must be right," because we don't know anything different.

ROSS-NAZZAL: How closely did you work with McDonnell and Rocketdyne?

CHAFFEE: Very close. Not so much with McDonnell. They were the Gemini prime contractor. But Rocketdyne was their reaction control system, subsystem, vendor, and they were the ones that were trying to build these ablative rocket engines that had the glass and phenolic resin that were delaminating and burning through and having cracked throats and all that kind of stuff. We worked a lot with them, and I spent a fair amount of time with my people out at the Rocketdyne facilities in Canoga Park, California, looking at what they were doing in their facilities and in their manufacturing shops and all that kind of stuff.

You know, how you built this thing, we suggested doing it a different way, and how you did that was a different manufacturing challenge, so it was a big impact to their manufacturing process to say, "We don't want to stack these things up as wafers. We want to stack them up as cones," or something like that. Rocketdyne, as soon as they thought they had an answer, they wanted us to accept that answer and go ahead, and we were a little bit more—I don't know what the term is—weren't always convinced that they had done everything that needed to be done.

I remember one other story, and you may want to edit this out, but it's true. I didn't go on this trip, but Henry Pohl went out to Rocketdyne for a late program review late in the development program. Rocketdyne claimed that they had solved this problem of burning a hole out through the side of the rocket engine, and one of the ways you demonstrated that with an ablative rocket engine was you sawed the thing in half lengthwise, so you could pull it apart and look at the wall thickness and see how far in the charring pattern had gone and whether there was any cracks and this kind of stuff. So Rocketdyne presented and said, "Well, we've done six or eight rocket engines going through the complete test cycle; they all look good. Here's the half of the engine from these eight that we've tested to demonstrate that it all looks good."

So Henry sat there. "Yeah, that does look good." He says, "Just for kicks," he said, "would you bring me the other half of these engines?"

Suddenly there's this worried look, as Henry tells it, on the face, and there's all this whispering and stuff going on. They said, "Well, one of them is lost. We can't find it." They brought in some of the others, and they looked okay, too.

Well, during lunchtime Henry said, "I'm not going to lunch. I'm going to go out and just walk through the machine shop." He went out there and he found this half of an engine laying out in the machine shop somewhere that had this hole burned in it. He picked it up and looked at it, and said, "Well, what's the number on this one?" They told him, and he went on back to the conference room, and the other half was in the conference room as evidence that everything had gone well. So he says, "During lunch I was out in the—I saw the other half of that one you said you couldn't find." He said, "Why don't you go out there and get that and bring it in?" So they did, and of course, you know—well. [Laughs] So, a little bit of obfuscation that was going on.

But those are some of the kind of things that happened. They were under pressure to get on with it, and I'm sure they thought, "We'll get this fixed if we can just get NASA to give us the go-ahead," and that type of thing. Later in the Apollo Program I had the same kind of experience with the vendor when it was my responsibility to go out and conduct these reviews and that kind of thing.

ROSS-NAZZAL: Did you ever participate in any flight readiness reviews [FRR] for Gemini?

CHAFFEE: Yes.

ROSS-NAZZAL: Can you tell us about that?

CHAFFEE: Generally I was just sitting in the back, because, again, I was just a bull pen engineer kind of thing. I went to one or two of them, and it may not have been the formal program one, because I think they had them in St. Louis [Missouri]. But we would have a pre-FRR at Houston, where everybody who was going to go to the meeting would get their ducks lined up with the Program Manager, Charlie Mathews, or something like that, and I went to two or three of those as a sit against the wall in the back of the room in case they want to know something about what you do, or something that you know about.

In the Apollo Program I went to several out at Downey [California], but that's a time for another story or—

ROSS-NAZZAL: Okay.

CHAFFEE: —something like that. I did work on a lot of Gemini flight analysis, where I was there for all of the flights. At that time we had a Mission Evaluation Room that we would sit in and look at data, and help the Mission Operations Director [MOD], Flight Director, and Controllers, and that process continued through Apollo and Shuttle and all that kind of stuff. The MOD folk knew very well the hardware and how it operated and the mission rules and all that kind of stuff, but the engineers had this detailed hardware knowledge, including all of the funny things that had happened and what happened that one day when the pressure got too high or too low or this kind of thing. We had all that data plotted up, and this what we call off-nominal or off-limits kind of conditions that we had tested. Frequently, again, the value of having our own hardware, our own test facilities, we could ask the what if questions, and the contractor would frequently say, "That's not in the specification, and we're not doing that test."

And the Program Manager would say, "You want me to spend $250,000 to go do this thing that's never going to happen? No way. I haven't got the time or the money to do it."

So we could say, "Well, we think we need that data. We've got an engine. We've got a test facility. We've got people. We're going to go get that data."

So in all these programs we had reams and reams of notebooks and notebooks of data. What happens when the fuel feed pressure and the oxidizer feed pressure are not right? The fuel is too high; the oxygen is too low. The other way around, they're both too low; they're both too high. The valves open slow, or they don't open at the same time. We want to know what happens under all these conditions, so that if we're ever faced with that contingency, we can give proper advice based on some data to the [Mission] Operations people.

So we had all this kind of data, and so it was very valuable for us to sit in a separate back room and look at all this data that would come in, and if we saw something that the Mission Operations people, we thought either they'd missed or they were misinterpreting or they were trying to make a decision that we didn't agree with, we'd make sure that we had an opportunity to make our input. Those facilities and the ability for us to get that data on our own and be independent experts, independent of the prime or the vendor or MOD, was absolutely critical, in many cases, to making sure the right thing was done.

ROSS-NAZZAL: How well did your engines perform during the Gemini Program?

CHAFFEE: Very well. The biggest problem we had was one of system cleanliness. There was two big problems. The first one was cleanliness, and I'll talk about that. The second one was we almost killed Neil Armstrong, because he had a thruster that ran away on him.

System cleanliness was one in which we just did a dumb thing. In conjunction with our contractors, we had filters in the system in the wrong place. We didn't have good filtration [capacity] of the propellants upstream of the propellant valves, the little valves that open and shut to allow the fuel and oxidizer to enter the combustion chamber of these little engines.

When you build these systems, it's inevitable that there's contamination. Machine dust, metal particles, slivers, burrs, that kind of stuff, get down in these components, and no matter how hard you try to clean those things and wash them and that kind of stuff, you can't get all that stuff that gets down in crevices. You're putting pipes together, and you're welding them or brazing them, and that generates something on the inside that breaks off and gets down in the pipe, and then when everything is said and done, you flush all kinds of fluid through these things to wash out all this dirt, but you can never get it all.

What happens, if it were to stay down in a crack, that would be fine, but when you go into zero-gravity, stuff that's down in the crack comes floating up out, and they still see that in the Shuttle. They clean those Shuttle cabins just as good as they can, and every crew will tell you as soon as the main engines shut off and they go into zero-G, they see all this stuff come floating out into the cabins and all that kind of stuff that's been in cracks and crevices. That happens inside these closed fluid systems, too, and we didn't have the proper design of the system filters, and the valves that control these engines are very precision devices, so if you get a little piece of contamination down in there, number one, it can cause the thing to leak or stick open or leak or something like that.

We didn't have good system filters upstream. We had little filters at the valves, and all this stuff that was dirt and contamination of the system would come down and collect in these little filters right in front of the valves, which protected the valves from leaking, but the filters would get so full of stuff, it's kind of like the screen filter in your kitchen faucet. Every once in a while, do you ever take that off, and it's full of boulders and rocks and sand and all that kind stuff, where the water is not coming out good? Same thing happens in these little valves. There's all this contamination, and pieces of stuff, metal filings and dust and stuff, was washing down, filling up these filters, and it would restrict the amount of fluid that could go through there into the engine.

So the thrust of the engines, we would evaluate how crisply the Gemini capsule responded to the rocket firing, and say, "It's acting like this thing is only putting out fifty pounds of thrust instead of a hundred pounds like it's supposed to," or something. That kept happening and happening, and we finally figured out that it was a contamination problem that we had; that we didn't have the filters in the right place [and with the right capacity]. Well, that came a long at a time where we were able to get that realization into a better design of the Apollo filters and how we did that, so that never was a problem. Then the same guys were around when we designed the Shuttle system, so we had all those filters done right. So system cleanliness in any liquid or fluid system is always, always, always a terrible problem that you have to pay extraordinary attention to, because a little piece of junk can get in something and totally mess up the system to the point of a disastrous occurrence.

The second one was that we hadn't paid close enough attention to the way that the electric power was sent to the propellant valves on the little rocket engines, and these little twenty-five-pound-thrust rocket engines that were up in the nose of the Gemini, and I think there were eight of them—may have been ten, but I think there were eight—each of them had a fuel valve and an oxidizer valve, and they were wired such that they were hot. In other words, there was always power going to the valves, and the way you fired the valve was closing a switch on the other side of the valve, which then would let electricity flow through the valve which caused it to open. Well, it turned out that if you then have a short circuit somewhere, then it lets current flow through that valve and the valves come open, even though you didn't command it. You've got a short circuit somewhere, so the rocket engine turns on.

That happened during Gemini VIII, I think, which was Neil Armstrong and—

ROSS-NAZZAL: Dave [David R.] Scott, I think.

CHAFFEE: Dave Scott, I think, yes. That thing started spinning up, and they got going to quite a rate. I think they were going almost one revolution per second until Neil figured out that he must have a runaway thruster, and he reached out and he pulled the circuit breaker on the system, which then turned it off. No power could get through. Then he was able to use his landing steering system in the back to stop that rotation. But as I understand it, at the time and even still, they were close to the point where the tendency would have been for them to black out, that kind of thing.

So as a result of that we looked at those electrical schematics and said, "God, that was a dumb way to design that kind of thing." So on all subsequent systems we made the thing where the valves were not hot all the time; that in order to fire the valve, you closed an electrical switch upstream of the valve, which then sent electricity through the valve, and that way a short circuit downstream of there didn't affect the inadvertent and uncommanded operation of that engine.

So those two events, the key things that remain in my mind, the design of the ablative chambers and coming up with a design that allowed the engines not to delaminate and to go ahead and meet their ten-minute total firing lifetime kind of thing with the number of pulses, and working on the cracked throat pieces in the rocket engines and finally having to compromise, saying, "Well, we can't keep them from cracking. We can't figure out how to do that. We just are going to capture the pieces in the design so that we don't throw any pieces out or eject any pieces," and then it turns out that it doesn't look pretty, but it works. It's good enough. So those were the things.

The other thing for me on the Gemini was that was the first system in which I had a total system concept and began to understand the totality of not only the rocket engine and valves itself, but that I had a propellant tank. I had all these lines. I had filters. I had control valves. I had pressure regulators. I had a helium bottle. It took all of these things to make a complete system. Because up to that point I was just—rocket engine was my piece, and somebody else was working on the tank. Somewhere along in there it occurred to me that, well, if this other stuff doesn't work right, it impacts what I'm working on. I need to understand how this other stuff works and all that stuff.

So, one of the things I have always maintained in my career is I do what I call wear a big hat. I try to always see the big picture. This is my responsibility, but what can affect me, and what effect does what I'm doing have on what somebody else might be doing and they don't know about me, that kind of thing. So that's always been—I still work in the Mentor Program, and that's one of the things I tell the young people that I talk to, and even some of the older people. Wear a big hat. Always read the Center activity reports. Keep up with what's going on, not only in your area; what are you colleagues doing, what's the other divisions doing? What's the politics of the program? What's the budget situation of the program? You can't do the best job that you're capable of doing unless you're aware of all this environment that your work is immersed in.

ROSS-NAZZAL: Do you have any other memories of any Gemini missions, other than Gemini VIII's, that stand out?

CHAFFEE: Well, the one where the Agena didn't work, which we've already covered.

I think that the final docking missions, and again I lose track of the crew, but we had two Geminis up at once, and I think Wally [Walter M.] Schirra [Jr.] was in one. I don't know whether he was with [James A.] Lovell or somebody like that I can't [remember]. We finally got two Geminis up, and they came together and essentially got nose to nose. That was quite a thing, because I was still all agog about the fact that both of these things are going 17,500 miles an hour, and they're going to come together and remain motionless. Of course, if you think about it as an engineer, it's no different than being in an airplane going 500 miles an hour and that kind of stuff, and you're touching your fingers together or something, even though both hands are going 500 miles an hour. It's all a relative kind of thing. So, just the fact that it was successful.

I guess one other area in which my particular expertise—I remember this. They worried about opening the [hatch] of Gemini, and if there was a propellant leak, which, when you leak the propellants in a vacuum, as we talked about earlier, it not only creates this fog, but part of the propellant freezes and it forms these snowflakes of propellant. So the question was we're going to open the [hatch], and I think Ed [Edward H.] White [II] was going to get out and use this little handheld propulsive thing and float around, that kind of stuff. The question was what if we get out there and we find out we've got a propellant leak, you know a little one, or we develop one from one of these thrusters, and he gets these snowflakes of propellant on his suit, and to come back into the Gemini, close the [hatch], repressurize, these things melt and evaporate. Now we've got a potentially toxic vapor inside the cabin. What do we do about that?

So all the SR&QA [Safety, Reliability, and Quality Assurance] guys and the program guys and everything were all concerned. Henry said, "Norm, what do you think?" and Chester, Chester Vaughan. So I did a calculation—it ended up taking just a couple of days—that said, look, I'm going to make an assumption of how much propellant is—you know, I did it independently for fuel snowflakes and oxidizer snowflakes; that if they come back in, if they've got one gram on them—which would be a huge amount to have it adhere to the [suit]—come back in and it melts, given the volume and the pressure, what would be the concentration of oxidizer or fuel inside? I figured that out, and it was more than they wanted. It would have potentially caused some problems.

I said, "Okay, now we've waited, and we're going to make the guys keep their suits on. We've repressurized the cabin." Calculated how long it would take for the snowflakes to melt and evaporate in that environment. Said, "Now if we now dump the pressure in the cabin again and then repressurized again, what would the resulting concentration be then?" And it turned out that if you depressurize the cabin another time and the stuff was all evaporated, and then repressurize the cabin again, there was almost none left. So I said, "Even if there's a little bit left over that didn't melt or was somewhere else or something like that, then that would be a safe thing." As a matter of fact, they had enough consumables that if they then took their suits [off] and smelled around and said, "Oop, we smell something funny that smells like oxidizer or fuel," that they could do it again, and it would be okay.

So that was something that I feel like I was fairly key in coming up with a real problem that nobody had thought of at the time. But we were getting valves leak from the engines during the program, and the astronauts were reporting snowflakes and that kind of stuff, and we figured out what it was. It was the propellant valve leaking and making these snowflakes that would float around the [spacecraft]. The question was if we've got that and they get on the suit and get back in the cabin, what can we do about it, and it turned out to be a simple fix.

ROSS-NAZZAL: During Gemini, did you work at all with any astronauts? Were they assigned to your section of the spacecraft?

CHAFFEE: Not really. I was still a fairly low-level engineer and down in the bowels of the ship, and I could go to meetings and see them and talk to them in the context of a professional exchange if there was something about the reaction control engines that was coming up and the astronaut wanted to know about it, but—in fact, one of my—again, a funny story.

One of my first memories is right after I got here in May of '62 they had said "Well, we want you to start learning about the Gemini propulsion system." So I did, and after about a month they said, "Well, there's a meeting at the Gemini Program Office," which, turned out, was at the Veterans Administration Building downtown, because we were spread over ten or twelve different areas [around] town at that time. Said, "Item number eight is something about the engine, and want you to go down and just take notes. You don't know enough to really contribute, but go down and see what the issue is and bring back the report."

So I went down there, and turns out that the agenda item before me was on the reliability of zippers in the Gemini spacesuit, and John [W.] Young was there—[a] young guy. I was just sitting there waiting for my agenda item to come up, but paying attention, trying to understand what was going on. The issue was that there were several zippers in the suit, and the number of linear inches they had calculated was such that the reliability of the suit for leakage now wasn't met, because there was too many inches of zippers that could leak. So the recommendation from McDonnell Douglas was that the crotch zipper, which was supposed to be used when they had to defecate, because they had these sticky bags that they were supposed to stick on themselves when they had to poop; they would eliminate the crotch zipper.

Well, John Young just went berserk. He got up and made this impassioned speech and all that kind of stuff about being able to make zippers better and all that kind of stuff, but his bottom line was that "you've got to do better, and if you can't do better, then we're just going to still have a crotch zipper, because I ain't crapping in my pants." [Laughter] I think that when I got back to my office, the first briefing I wanted to give was that "John Young is not going to crap in his pants." [Laughter] And, "Oh, by the way, they talked about the rocket engines, and here's what I think they said." But I've always admired John for—you know, he's a very outspoken—he'll say anything, and over the years I developed a very good, close friendship with John, and am still a good friend with John. Don't see him as much as I used to, but I'm currently the President of the NASA Retirees Association, and he's one of my silent members. He's a member and pays his dues, but he doesn't ever come to any meetings or—but I run into him occasionally. I think he's still working.

ROSS-NAZZAL: I think we saw him the other day in Building 1.

CHAFFEE: Yes, he's still got a phone number, and he's still on the global e-mail and all that kind of stuff. I ran into him over at the Museum of Natural Science [Houston, Texas] not too long ago. He was giving some kind of a program for them over in the planetarium. That probably pretty much milks the Gemini Program.

ROSS-NAZZAL: Okay, well, I think that this would be a good place to stop and next time we could pick up with Apollo.

CHAFFEE: Sure, Apollo was great.

ROSS-NAZZAL: Good. We look forward to it.

[End of interview]