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ORAL HISTORY 2 TRANSCRIPT

NORMAN H. CHAFFEE
INTERVIEWED BY JENNIFER ROSS-NAZZAL
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ROSS-NAZZAL: Today is January 25th, 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 interview, and she is assisted by Sandra Johnson.

Thanks again for meeting with us this morning.

CHAFFEE: It’s my pleasure. I’m honored to be part of the program.

ROSS-NAZZAL: Great. Well, this morning I think we should begin with your experiences in the Apollo Program. When did you start working on the Apollo Program?

CHAFFEE: Probably it was early on. I think, as I told you last time, I arrived at the Manned Spacecraft Center [Houston, Texas] in May of ’62, and by ’64, I was deeply involved in the Apollo reaction control system work. One thing that drove that was the fact that, although I was also working on Gemini, the reaction control system thrusters, their little rocket engines for Gemini and for the Apollo command module, which held the crew, were made by the same company, Rocketdyne in Canoga Park, California, and were almost identical.

In fact, when the contract had been given to Rocketdyne for the Apollo thrusters, the idea was that they would be identical, and therefore would save development money, and that type of thing, turned out not to be the case. But the fact that I was working on Gemini led me, really, into the Apollo ablative thrusters also. And then the thrusters on the service module and on the lunar module, which was contracted for later, were of a different type. They were hard metallic rocket engines, which were cooled by radiation. In other words, they just allowed them to get up to very high temperatures and then radiated away their heat and controlled their temperature by that mechanism.

So I would say by mid-'64 I was deeply involved in the Apollo reaction control system thruster, and my field was specifically working on the thruster, although I was also aware of technology and did periodically work on other components like the propellant tanks, the pressure regulators, the various valves, and the things of that nature.

ROSS-NAZZAL: You mentioned that the idea was to have the Gemini and the Apollo be identical for the RCS [Reaction Control System].

CHAFFEE: Right.

ROSS-NAZZAL: But you mentioned that they weren't exactly. Can you explain that?

CHAFFEE: Well, it's, you know, who knows exactly, but Rocketdyne sold the concept of using a common engine, and the requirements for the two were such that a common engine could indeed have been possible. But they set up a separate organization within the Rocketdyne Company to handle the Apollo Program, and it seemed to us that thereafter those two groups of people never spoke to one another.

And in addition, on the NASA side, the Gemini Program Office was fairly self-contained and insular. They kept to themselves. And the Apollo Program Office engineers were a little more outgoing. Of course, they didn't have a mature program at that time. They were looking for all the help they [could] get. And the two designs, for a variety of reasons, slightly different requirements or whatever, diverged, and even though we fought, and I—particularly Henry [O.] Pohl, my Branch Chief, and Guy [Joseph G.] Thibodaux, the Division Chief, would fight very hard to avoid design divergence and manufacturing process divergence and this kind of thing, we just didn't seem to have control over that, and it did happen. So it ended up that although the engines were quite similar, they were also substantially different in their design, even though the requirements were not that much dissimilar, that type thing.

And, of course, every time NASA agrees to a change, from the contractor, there's something called a change order, and that means more money and more fee and all that kind of stuff. So that's a little bit of a cynical outlook, but I think that also drove the contractor side of the house, that if we can make it different, we can require some additional analysis and additional certification and qualification testing and all that kind of stuff, and then the more money we spend, the more fee we get.

ROSS-NAZZAL: What did you learn from Gemini that you applied to the Apollo Program?

CHAFFEE: Well, to start out with, we didn't. I talked the last time we spoke about the orientation of the plies in the combustion chamber of the Gemini, the laminations coming apart, and we learned to make those things at an angle and ended up going, I believe, to a 45-degree angle, and we went immediately to those kinds of things in the Apollo design. We also were able to come up with a better throat design in the engine and made it of a slightly different material so that we didn't have the cracking problem that we did in Gemini.

There were some manufacturing process kinds of things that we applied, so basically all of the things we learned from the Gemini Program went into the Apollo Program, and the Apollo command module thrusters were a successful program. For instance, the last time, I told you we learned about how to filter the system so that we didn't get problems in the valve, and we applied those kinds of things. We had some different unique problems in the Apollo Program that we had not encountered in Gemini, and that did cause us a substantial amount of grief.

ROSS-NAZZAL: What were some of those unique problems that you encountered?

CHAFFEE: Well, there was one I was just e-mailing some friends last night. There was two or three that I could comment on. There was a problem called the iron nitrate problem. Our oxidizer in this two-propellant system—we've got an oxidizer and a fuel. The oxidizer is a material called nitrogen tetroxide, N_2O_4, and we had learned early in the program that we had to have a small amount of nitrous oxide, NO, in that to control corrosion of the metallic parts of the system. And particularly if the nitrogen tetroxide was not chemically inhibited, with the addition of a little bit of NO, it would seemingly leech out iron from the alloys of the piping and the components, that type of thing, and form an iron nitrate complex chemical, which was not really very soluble at all in the nitrogen tetroxide, and it would crystallize and deposit out and would plug up filters and valve orifices and things of that nature.

And it took us quite a while and quite an interesting research program over a year, year and a half, to understand what that was. We could take systems apart and find this semigelatinous material with a little bit of crystal material in filters and that type of thing, but as soon as it was exposed to the air, it would dissipate. It would react with the air and go away. So it was very hard [to figure out what was going on].

In the first instances, people would take these components apart, and if you didn't look at them immediately, or didn't make note of the behavior, then you'd go back. The engineers would go back the next day and say, "Okay, let's see what you've got." The evidence had dissipated. It had evaporated or it had gone away. So it took us a while to figure out what was going on, and a chemical research program that we worked. I can't remember who we contracted with, but the Air Force, it turned out, was having some of the same kind of troubles with some of their systems.

And we did find out that if we doctored the nitrogen tetroxide with a little bit of nitrous oxide, NO, it inhibited the leeching of that iron somehow, and at the time I understood the mechanism, and now I've forgotten what it was. But it was a significant problem, and one of the things I did later on was to develop a computer program that allowed us to understand as you depleted a tank which maybe had been filled to 95 percent with oxidizer that had the proper amount of the NO inhibitor in it, as you depleted that tank, and more and more of the tank volume became open to vapor and gas, it turns out almost like a distillation process. The NO component in the propellant would selectively evaporate into the vapor phase and the [liquid] propellant would lose its concentration of NO and therefore would be less inhibited.

And so we had to be sure that when we got down to the bottom of the tank and we only had a few percent of N_2O_4 left in there that that quantity of propellant when it was in equilibrium with its vapor phase still had enough dissolved NO in it to provide the inhibiting characteristic. Well, that's something that typical mechanical and electrical engineers throw their hands up and say, "Oh, gosh, how do you figure that out?" But that's also called fractional distillation, and it's what chemical engineers do all the time. All these columns you see in refineries are distillation columns where you're changing the composition of a liquid phase and a gas phase.

And so I understood how to do that, work with vapor pressures and a characteristic chemical engineers called "fugacity" and things of that nature and was able to write a computer program to show what you needed to start with in the NO content in order when you got down to the bottom of the tank and only had your residuals left, it was still—it wouldn't rot out the bottom of the tank or something like that.

So that was an interesting thing that I enjoyed doing. And whether they still know about that program or not, they still do know about the iron nitrate, but they don't understand it. And interestingly, in this e-mail exchange I had yesterday with my old retired buddies, one of our colleagues who's also retired, had gotten an e-mail from a current engineer in the Energy Systems Division saying that this new plan to go back to the Moon and go on to Mars, they have decided to go back to systems using the same propellants that we used on Apollo. And they had heard about this problem with iron nitrate clogging up the system and wondered if anybody knew what that was all about.

And, of course, they got about five or eight responses from my colleagues saying, "Oh, yeah, we lived through that. That was terrible. We'll tell you about it."

ROSS-NAZZAL: It's great that you're still a resource for those folks.

CHAFFEE: So yes. In fact, in my capacity not only as a retired propulsion guy but as president of the JSC NASA Alumni League, I'm going to go back and gather these folks from the hill country and wherever they are and go over and talk to the current Division Chief in the Energy Systems Division and say, "Look, we'd be happy to come over and introduce ourselves, give you a little seminar, just top little headlines of what the problems were we suffered through and resolved and then let you know how you can find us if you want to talk in detail about the analyses we did, the insights we got, the testing we did to show that what these things were and how we resolved the problems and that type of thing."

So we'll see. I'm hopeful that we can add some value and provide some shortcuts for the new effort to go back to the Moon and Mars. But interestingly, this new effort is looking more and more like Apollo revisited. So.

ROSS-NAZZAL: Absolutely. When we saw Mike [Michael] Griffin unveil that rocket, I thought, "Haven't we seen this before?"

Anyway, what impact did the Apollo fire have on your work?

CHAFFEE: That was a real shock. By '67, I think the date was like January 27th, '67, the systems at that time were mature. We had a mature spacecraft. In fact, the Apollo 1 fire occurred in a command module that was on the pad being tested out and would have been launched later in '67 if everything had gone well. I remember sitting at home that night and there came a flash on the TV, it must have been about eight o'clock at night or something, saying there had been a fire on the pad at Kennedy [Space Center, Florida] and that they feared that the crew had been killed and all that kind of stuff.

I immediately got on the phone and called my Branch Chief Henry Pohl and said, "What do you know about this? Have you heard about it?"

And he said, "Yeah, I've just been called. It's true, and they had a flash fire. The crew was not able to get out and they were killed. So now we've got a mess."

Of course my namesake Roger [B.] Chaffee was one of the astronauts who was killed. Roger and I were acquaintances, not quite friends, I guess. We never socialized but we did visit because we used to get each other's mail and each other's phone calls sometimes, and frequently when I was on a business trip, I'd be introduced as the astronaut Norman Chaffee because people didn't recognize the difference between Roger Chaffee and Norman Chaffee. And not too long ago, I did a Google search on my name and found several references to Astronaut Norman Chaffee, so the myth persists.

It was hard. I knew Roger and liked him. For a few years, I stayed in touch with his widow Martha, lost track of her now. His daughter, though, I know, is an engineer down at Kennedy Space Center, and I'm aware that she's down there and just a couple weeks ago talked to somebody on the phone that said they worked with her down there. So I know for a fact that right now she's still down there.

Anyway, that, of course, the Apollo fire, just put the kibosh on the Apollo Program. We had to go back and relook at all of the designs and revisit that. So number one, I was part of a process that went back and reevaluated all of the designs for proper requirements, all of the testing, all of the certification paperwork, and all of the analysis that went into the certification saying, "Yes, this system meets all of our requirements and has been demonstrated to be what we need," that type of thing.

But probably the biggest thing I got involved in was the flammability issue of the command module. The issue was that not only did they have this short-circuit in the command module that was the incident that caused the fire, but they found a little later on that a great deal of the material in the cabin was highly flammable, and of course the atmosphere inside the cockpit is pure oxygen. And if you've ever seen anything in its flammability behavior in air, which is like 20 percent oxygen versus 100 hundred oxygen, it's fantastic. I saw a technician one time who was smoking a cigarette, and he shouldn't have been, and he walked into an area with high-oxygen content in a test chamber where we had just done some testing with 100 percent oxygen. And why this guy was smoking, I don't know, but he was and he walked into this area that was probably still 80 percent oxygen, and that cigarette flared up and actually burned the end of his nose. So I mean just things in oxygen just burn at ten times the rate of and ferocity of something in a 20 percent oxygen environment.

So one of the big tests they did was they got a test command module, and they outfitted it with all kinds of different materials as a test to evaluate the flammability in a command module with these new nonflammable materials. And the testing was done in our division's test facility, the Thermochemical Test Area down in Building 353, and they would put a—in our subsystem chamber out there, they—no, I take it back. It was in one of the test cells they had this command module.

They would outfit it with the seats and all the interior stuff, put an ignition source inside it, fill it up with oxygen to the proper pressure, initiate a fire, and then we had it heavily instrumented and filmed where we could watch the progression of the fire, measure the temperature of the pressure, look at the smoke patterns, measure the chemical content of the atmosphere, that type of thing, and I worked on that for many months, helping out with those flammability tests and getting that data and evaluating it and this type of thing.

We had a funny incident, because to put the fire out, they had the command module hooked up to a vacuum evacuation system that's run by a system down there called a steam ejector, which uses steam at high pressure to pull a vacuum on a test chamber or something, using the Bernoulli effect. They get a fire started. Once they got all the data, they would put it out by opening this big gate valve and just essentially pulling all the air and atmosphere out of the command module. The fire would go out, and then they'd sit there and wait for a few seconds, let the temperatures all come down. Then they could open it up and go in, clean out all of the damaged stuff and the charred stuff and put in a new load of stuff and repeat the test, a week later, something like that.

Well, all of this was filmed, and we had a guy from Building 8 down there who was a photographer. And the big valve that hooked the command module to the evacuation system, it's, you know, great big valve, it's like eighteen inches in diameter, and when it closed, it made a tremendous clang or noise. So the photographer was out there one day, and we were getting ready for a test, and he was setting up his cameras inside the command module, making sure that the film was in it and they were oriented correctly and the electric cables were all hooked up and so he could turn them on and off and this kind of thing.

The test conductor needed to make sure that the main system value in the evacuation system was working, so he told one of the technicians, he said, "Cycle that valve." Well, when you do, it makes a big clang. Well, they cycled the valve and then, clang, you know, like a big metal-on-metal thing. Well, that sound came reverberating down that pipe into the—which is a big pipe, into the top of the command module where we were evacuating the atmosphere, and this photo technician was in there. Suddenly, he gets hit by this huge sound coming down into the command module. That guy came flying out of the hatch, and I mean he was three hundred yards up the road before we could catch him and tell him was what going on. And I guess the test conductor had just forgotten that he was in there doing that when he commanded that valve to be cycled.

Another interesting thing that I worked on, again, as a chemical engineer, one of the things that I was involved in was they were concerned about the flammability of water glycol solution, which was used to cool the spacecraft and all its electronics device, and it circulated around all over through the cold plates, which picked up the heat and the energy generated not only by the crew's metabolic load, but all the electrical energy of the equipment and that kind of stuff, and then went to an evaporator where it was cooled and that kind of stuff. Anyway, this working fuel was a mixture of water and glycol, almost like auto antifreeze is. Well, glycol is a hydrocarbon component, but has very, very low vapor pressure and a very thick, gooey stuff if you see it by itself, very viscous.

The feeling was that, well, the system is under pressure, and if we get a pinhole leak in one of these water glycol tubes that's running through the command module and we spray this stuff out into the 100 percent oxygen atmosphere, we may create a detonable atmosphere of oxygen and glycol vapor. Some of the contractor engineers and others said, "Nah, the glycol has such a low vapor pressure it will never achieve a concentration that's high enough with the oxygen to even be flammable or detonable."

So they asked me, and I said, "Well, I don't know. We're talking about a warm fluid, and I don't know what the detonation limits are. We can do some analysis, but maybe we ought to do a test." So we did some analysis, and it looked like it might not be at the flammable limit or detonable, but we decided to do a test anyway.

So we went out to our facility in Building 352 at the Thermochemical Test Area. We set up a test in a—we built a large Plexiglas chamber, probably about four feet on a side, where we could look in and see what happened. We created a liquid system with warm water glycol solution, just like is working in the command module. We put a very tiny orifice, like we had a crack or a leak or something like that, so this stuff could spray into the chamber, and then we had an igniter in there, basically a sparkplug, and we filled it up with oxygen environment.

Well, the day we were doing this was late in the spring, it was kind of cold, and so the test cell itself which was normally open to the outside, so you can exhaust just to the great outdoors, does have a garage door type thing on it to shut it down to protect the environment. So they had the garage door shut, and nobody thought much about it. So we did some tests. They were kind of noncommittal. And then we did another one where the glycol, we heated it up a little bit more, something like that, sprayed it into the chamber with the oxygen environment, triggered the sparkplug igniter, and kaboom, boom, I mean the thing detonated. And we blew out the end of the box, and not only that, we looked, and the garage door on the chamber was all bowed out and everything. I'm just glad nobody was in there.

But I was able to call my boss Chet [Chester A.] Vaughan and Henry Pohl, and say, "Yep, it's detonable." [Laughs] So not only does it burn, it can detonate. So that was an interesting situation.

ROSS-NAZZAL: So what changes were made as a result of your findings?

CHAFFEE: I'm not sure that I remember. I don't know whether they changed the composition or controlled the temperature or what, but they did change so much. Everything that went in there now, they had to evaluate for its flammability characteristics, down to things like wiring insulation and all of the covering, the multi-insulation layers, that type of thing were changed from things that were flammable to things that were nonflammable, that type of thing, you know, all of the materials.

And they worried about things like outgassing, which is the products. Either the products that naturally evaporate from a material or which are caused if you heat it up and it starts burning or something, what is the chemical identity of the material that is released in the cabin, and is that toxic and that type of thing. I wasn't really involved in a lot of that. I was involved in the flammability testing in the command module and that one particular test, which was whether the water glycol was flammable and detonable and that type of thing. How they actually resolved that, I don't quite recall.

But the command module ended up working very well on all of the missions. Once we got that system certified, that propulsion system, which again used the ablative engines, twelve of them, at ninety-three pounds thrust, and they were all buried down within the outer mould line of the vehicle, because they can't be on the outside because they'd burn off during entry. But they all worked well, and you can now go over to Space Center Houston [Houston, Texas] or some of the other museums where these things are on display and see the holes where the engines [fired] through the outer boundary, and they all look in good shape and that type of thing.

The one problem area that I probably worked the most on and am the most proud of as far as my contribution had to do with the metallic RCS thrusters that were used on the service module and the lunar module. The service module had a series of sixteen hundred-pound metallic thrusters organized every 90 degrees around the outside of the service module in clusters of four. If you looked at the service module as an upright beer can, each of these clusters, which was like a little box with four rocket engines sticking out of it, there was one thruster pointing up, one thruster pointing down, and one thruster pointing to the right, and one thruster pointing to the left. We called the little box, we called it a doghouse, because it looked about the size of a doghouse for a small dog. And it had these four engines poking out of it, and there was one of these things, one of these doghouses with four engines, every ninety degrees around the outside of the cylinder, which was the service module.

Then inside, right underneath the doghouse was the rest of the system, which was separated from all of the other systems, so all of the tankage for the fuel and the oxidizer, the helium pressurant, the filters, the isolation valves, all the instrumentation, all that kind of stuff, was all tied to that single doghouse and four thrusters. It was all on a panel. It was about three by seven or something like that. And then those panels were installed on the service module every 90 degrees around there so that each of the service module thruster installations was separate. The four were not interconnected with one another.

My area, again, was the rocket engine or the thruster. These thrusters were made by a company called the Marquardt Company, which was in Van Nuys, California, and they had been making radiation coolant thrusters for the Air Force, but they had been working at a lower thrust level, twenty-five pound thrust, and they'd built a successful twenty-five pounder for the Air Force for an Advent Program, which was an Air Force satellite. And they were selected by North American Rockwell to build the hundred-pound thrusters for the Apollo service module.

And they made them out of a metallic material which was 99 and a half percent molybdenum and half a percent titanium, so it's, you call it, moly-half-ti [phonetic], was the patois for what that alloy was. Molybdenum is one of the four refractory metals that exist in the periodic table. There's a material called columbium, which is sometimes called niobium also, but I call it columbium. Then there's molybdenum and there's tantalum and tungsten. And all of those metals have working mechanical properties that are good up toward three thousand degrees and have melting points up above five thousand degrees. So you can use them in a high-temperature application, like a rocket engine or that type of thing, unlike the command module thrusters, which had to use this thick-walled ablative material and absorb heat by just burning away the walls for a one use application.

The service module engine was out in the open. It would get hot up, to twenty-three hundred, twenty-four hundred degrees was its maximum temperature and would just reject heat or cool itself just by radiating away. And radiation is proportional to temperature to the fourth power, so the hotter it gets, the more effective it is at rejecting heat from itself. The problem with molybdenum was that the combustion gases inside the combustion chamber are oxidizing by nature and the high-temperature molybdenum is very susceptible to being oxidized and forming molybdenum oxide, which could then be sloughed off and dissipated. And so you had to protect the surface of the molybdenum in order just to keep a chemical reaction from occurring on the inside surface of the combustion chamber that would just rapidly eat its way through the wall of the combustion chamber, and it wasn't a melting reaction. It was a chemical reaction that was eating up the molybdenum and then removing it as molybdenum oxide with the gases that were [exhausting from the combustion chamber].

So you had to put a protective coating on this molybdenum. The one that the Marquardt Company utilized was a material called molybdenum disilicide, and it's made by a special process. Molybdenum disilicide is a material called an intermetallic compound. It's kind of a semi-ceramic. And the effect would be that when the oxidizing gases attempted to attack the molybdenum disilicide, it would preferentially attack the silicone in this molybdenum disilicide, form silicone oxide, which is glass, basically, and it would melt that very thin layer of silicone oxide which formed, and that then provided a protective coating on the inside and would last for a long, long time.

Over time, you would also remove the silicone oxide. But if you had a pretty good layer of—and I'm talking a few thousands of an inch—of molybdenum disilicide on the inner surface, it would provide hours of protection for the engine against this oxidizing hot environment that is created by the combustion gases. But the coating was a little bit artsy in the way you put it on there. It had to be done in a high-temperature furnace and with the special techniques and pressure and temperature and controls and this type of thing.

And nobody understood that too well, but the Air Force had done a lot of working [in] that area, and so I got interested in that, and my assignment to that thruster led me into working on oxidation resistant coatings for the rocket engines, and I read all the literature. I funded some independent studies at various companies. We did some studies in our laboratories over a couple of years. I worked heavily with the Air Force and talked, went out, and visited these companies that were experts in this field and including the Marquardt Company who made the thrusters and the company who applied the coating as their coating vendor and that type of thing.

And as a result, I became one of probably a few people that knew a whole lot about these oxidation resistant coatings on molybdenum, and we were able to suggest some chemical additions. I think we put a little vanadium in there, and my memory grows dim now, exactly, but instead of just putting silicone in the chemical materials that we used to generate the coating, we doctored it up with some other stuff that allowed this coating to be more adherent, more viscous so that it lasted a longer period of time. We completely characterized the process for forming the molybdenum disilicide coating in the high-temperature chamber where that was done.

So that was one of the areas in which I knew quite a bit. I did talk to the Air Force. I consulted a little bit for the Army, that kind of stuff, over the years when they had similar problems with some of their hardware. But we did ultimately come up with a coating that was very protective and worked very satisfactorily for the molybdenum thrusters on the service module. And although in development, we had burnthroughs where the coating failed and that kind of stuff, during the program, we never had a burnthrough [in flight] that we were aware of.

Now, the other problem with molybdenum is that it's very brittle. It's a brittle material, particularly when it's cold. And if it's suddenly impacted by a high load or an impact load like if you took a molybdenum thruster and chilled it down like it might be space if it weren't heated, and took a ball peen hammer and gave it a sharp rap, under certain conditions, it would just shatter like glass, almost like a ceramic. That became a problem because we found that we had a phenomenon that became known as the pressure spike. And when you fire these reaction control system engines for repetitive pulses with limited time off in between, there's a lot of reaction product forms in the combustion chamber during the period when the pressure is building up inside the combustion chamber or when the pressure is decaying after the valves are shut.

Under those conditions, you do have fuel and oxidizer vapor inside the combustion chamber, but it is at low temperature. And they do react, but they don't react in a fully-combustion-type reaction forming all these hot gases. So at the start and end of each pulse, there is the potential for forming a small amount of just a chemical material where the fuel and oxidizer go together and they form a material called an adduct, which is a flammable and detonable material, and it's a precursor to the full combustion process, but it doesn't go ahead and go the combustion. Reaction stops at this intermediate point, and this material is a very, very high-temperature or high-vapor pressure viscous material, and it stays inside the combustion chamber.

So over a series of many, many short pulses, you're forming just a little bit of this stuff every time. As an example, a hydrazine-type material is the fuel in this case on the service module. The fuel is a material that we used from the Air Force called aerozine-50, and it was a 50/50 mixture of the chemical hydrazine, which is N_2H_4 and another version of hydrazine, which is called unsymmetrical dimethyl hydrazine, or UDMH. And UDMH is basically hydrazine where you've taken two of the hydrogen atoms off and replaced them with methyl CH_3 radicals. So you had this mixture of hydrazine and unsymmetrical dimethyl hydrazine and repetitive short pulsing, which these engines primarily operated in that mode. They rarely fired a very long pulse. It was typically trying to hold the spacecraft in a particular orientation or move it very slowly one way or another, so they were firing on the order of short millisecond pulses. The minimum firing an Apollo service module reaction control system thruster could put out was ten-millisecond thrust, and in that case most of the firing was getting up to pressure and then falling off from pressure, and there was almost no part of the combustion at the full combustion pressure.

So you'd make a little bit of this additive material, which was the hydrazine combining with the nitrogen tetroxide, and it formed a material called hydrazine nitrate, which ultimately could be a crystalline material, but there's also water formed in the combustion process, and so it tended to be this gooey gelatinous-type gummy stuff that would collect in the rocket engine, and if you didn't have a longer pulse once in a while that would burn that stuff out of there, if you just kept going with the pulses on a statistical basis, at some point you'd collect enough of this stuff in there and, for reasons that we never could adequately predict, at some point there would be a critical mass of stuff in there, and you'd get a single short pulse whose energy was enough to detonate that. It didn't just burn it. It detonated it. So it was an actual detonation.

And it would spread the—it would destroy the combustion chamber because molybdenum is, again, this brittle, can be brittle, material, and when it took a sudden pressure surge or pressure spike inside because a significant amount of this hydrazine nitrate, which had accumulated over five hundred or eight hundred or a thousand pulses, suddenly detonated then it would destroy the engine. And worse, it created high-velocity shrapnel from the fact that the engine exploded very, very bad. And the astronauts were totally against that, as we were, so we had to figure out what was going on there.

My colleagues and I worked on that a bunch for—probably from when we detected that maybe in late '64, early '65, up until, you know, fairly late in the, you know, '67 kind of thing before we really felt like we had gotten that understood. And the guys I worked with on that are the guys like Carl Hohmann, Bernard [J.] Rosenbaum, a guy named Julian Jones who later left and went to EPA [Environmental Protection Agency], Jim [James] Wiltz who's not with us anymore. But that was a really interesting problem to understand what was going on.

One of the techniques we undertook just to figure out phenomenologically what was happening was we used the little injector that we had that sprays the fuel and the oxidizer into the combustion chamber, but we took off the molybdenum combustion chamber and replaced it with one made out of Plexiglas that we could see through. And what we would do, we would put this in a test chamber at vacuum, because that was critical, and we used this vacuum evacuation system that I told you about that we used to evacuate the command module. It was really down there so that we could do high-altitude testing of rocket engines at altitude simulations above a hundred thousand feet of simulated altitude.

And we'd put a camera in there and take pictures. We'd pulse the engine a number of times, and we'd watch the pictures. Well, the heat input from one of these short pulses is low enough that the Plexiglas would last a long time. If you turned the engine on and just let it run, it would eat up the Plexiglas pretty quick. But if you just had these little short pulses the blip-blip-blip kind of thing and tried to take pictures of it, you could see the fire, then go out, then fire, and then go out, and this kind of thing. Of course, we were instrumenting the temperatures and pressures and that type of thing.

But it turned out that you couldn't tell that much. You'd watch this thing, in one frame you'd see the brightness of a combustion, and then the next flame, it was out, because the whole process was only ten milliseconds long. So we decided to try to up the speed. We kept upping and upping and upping the camera speed to be able to get multi frames of what was going on, and every time we did that, we were able to learn more and more from that.

Ultimately we were using cameras that took up to a million frames a second in order to try to understand it, and in that case, you can't feed film through anything that fast. What you do is you have a cylinder that's got a strip of film in it, and you start it spinning at a high speed, and then you try to open the lens of the camera just at the right time. And at a million frames a second, if you get fifty or a hundred frames on this strip of film in this rotating film case, you're getting less than a millisecond's worth of data. So sequencing and timing of when you open the lens and when things are happening, was critical, and so it took us a lot of time.

We blew up lots and lots of Plexiglas chambers without exactly getting the data that we wanted. We finally did, though. In over a two-year period, we were able to figure out what was going on. We documented the formation of this hydrazine nitrate material. We were able to make some just in the laboratory and characterize its properties and its chemical signature in various analytical techniques, mass spec [spectrometry] and absorption spectrometry and that type of thing.

We did find that we could fire the rocket engine, and what we did, we would go in the test chamber, our large subsystem test chamber, at Building 353, turn a rocket engine up, so that if we formed this stuff, it would collect down inside the rocket engine. So we'd fire a number of times, drop the pressure, rush in there real quick, get that thing, look in it, and see if we saw this liquid gelatinous material in there, and if we could, try to capture it, get it into a sealed container, and get it into the lab where we could see what it was. So that's how we were able to do that.

Then we, both in our laboratories and under contract with various people including [The University of] Denver Research Institute [Denver, Colorado, and the U.S. Bureau of Mines Explosives Research Center, Pittsburgh, Pennsylvania] we did a real good program with them where we showed how this material was made under low temperature, low pressure conditions and what its chemical properties were and all that type of thing. Really, some groundbreaking research they were doing and where we were characterizing the chemical reaction rates and what we called the kinetics and that type of thing, some really good work there.

Anyway, we came to understand that this material was going to form, that it was just a characteristic of the way you had to operate these pulsing thrusters, but that we could control the amount that built up. If the engine stayed warm, it tended to dissipate the material, because when the engine is not firing, it's in the very, very high vacuum of space. So if the engine was kept warm, it seemed like that was a controlling factor. We couldn't keep the material from forming. We could keep it from accumulating by adjusting the temperature of the thruster, and that was the final technique and solution to solving that problem. Although the manufacturer, the Marquardt Company, did some other things, like they tried to strengthen the combustion chamber of the molybdenum by instead of having just a straight-walled cylinder, they had a couple of bumps on it where they essentially built reinforcing ridges that went circumferentially around the outside of the combustion chamber. We called them knobs. They had knobby combustion chambers that looked knobby when you looked at it from the outside. I don't believe that those were very effective, but they were part of a configuration that ended up being qualified and then certified. And so then that's what flew.

But as far as I know and anybody knows, we didn't ever really lose a thruster. We had problems with valve leaks and this kind of thing, but we never had a detonation during a program of the engine that caused a problem. When we went to the lunar module, which was contracted for a couple years after the service module, again, they were directed to use the Marquardt Company to provide their thrusters. And again, it was one of these cases where they had a similar kind of thing.

They didn't have the doghouse configuration, but they had a pod structure of four clusters of four of these radiation-cooled rocket engines. And the Marquardt Company also made them, but Grumman, working with us, did two significant things that made the pressure spike problem a little less severe. They changed their propellant and they went from this mixture of hydrazine and unsymmetrical dimethyl hydrazine to a single chemical that was not mixed called monomethyl hydrazine, all part and parcel of the same thing. But it turned out that the chemical properties of monomethyl hydrazine were less likely to form this additive compound, this monomethyl hydrazine nitrate, than the hydrazine that was used in the aerozine-50 on the service module.

The other thing was that they changed the design of the combustion chamber and went away from the brittle molybdenum half-titanium alloy to a [columbium] alloy. And [columbium] has a melting point, oh, a couple hundred degrees [lower] than molybdenum. I think it's up around fifty-[two] hundred or fifty-[four] hundred or something that its melting point is, but it now has the property that it is a ductile material. So when it took a pressure spike, rather than shattering, it would just give a little bit and then come back. And it's kind of like punching a fat man in the stomach, it just, you know, you're going to go "oomph," but then when you withdraw your hand, it pops out.

So we did over the years use stainless steel chambers to measure the height and measure the amount of pressure that we would generate in these things, and we were able to calculate due to pressurized rates and high-frequency high-response instrumentation, piezoelectric pressure transducers, this kind of thing, that we were getting internal pressure momentarily inside these chambers of over fifty thousand pounds per square inch when we got a detonation. And there's still around somewhere, I think Henry Pohl may have it or Chester Vaughan may have it, a fairly thick-walled stainless steel version of this, of the combustion chamber for the service module thruster. It's a little bit smaller than your coffee cup there in diameter and had a fairly thick wall, probably a quarter of an inch thick, and had a little square, what you call, "boss" when we screwed in a pressure transducer. We had one detonation in that thing that essentially took it from a straight-walled cylinder to where it looked almost like a softball. So it deformed that stainless steel due to that momentarily, probably less than a millisecond, pressure pulse of up over fifty thousand pounds per square inch that instantly deformed that stainless steel test chamber into something that was almost round rather than a cylinder. And that just amazed everybody when they saw it. You could imagine the amount of energy that it took to essentially instantaneously do that.

Anyway, the lunar module thrusters changed fuel to monomethyl hydrazine, changed the alloy to a [columbium] alloy, and the thrusters behaved beautifully for the Lunar Module Program, worked very well.

ROSS-NAZZAL: You mentioned working with the military and also with contractors. Did you work with any other NASA Centers on propulsion issues?

CHAFFEE: Yeah, a little bit. But in general, in those days, boost propulsion, launch propulsion, was at the Marshall Space Flight Center [Huntsville, Alabama], and the spacecraft propulsion was at the Manned Spacecraft Center, and so those two are fairly different technologies. The boosters are hundreds of thousands and millions of pounds of thrust. It's one firing or two kind of thing. The kind of stuff we're talking about, completely different propellants. It's these hypergolic toxic propellants, many firing short pulses, tend to not be long burns, that kind of stuff. So the technologies and the expertise are completely different. I did work some over the years, really didn't work much with the Marshall Space Flight Center except on the S-IVB upper-stage of the third stage of the Apollo; I'll come back to that.

But I did work with the Jet Propulsion Lab [Pasadena, California] some, because they were using some of these small thrusters for their satellite programs, and in fact, gee, I can't remember it now, but there was an early orbiting mission of the Moon where it took pictures of the lunar surface, Lunar Orbiter, I think it was called, went into orbit around the Moon. Its main engine was one of the RCS service module hundred pound thrusters that we used for our steering thrusters and used in short pulse mode. Jet Propulsion Laboratory bought one from Marquardt and used it as their primary engine, and so it made long firings. It fired for midcourse corrections on the way to the Moon and then fired a significantly long pulse to bring and drop that spacecraft down into orbit around the Moon.

So you know, over the years they had various satellite programs where they would have problems and needed help with steering engines or they used hardware from our vendors or something like that, so I did work with Jet Propulsion Lab some and had some good contacts out there. There was a guy named Dave Evans [phonetic], who was head of their propulsion area that I worked with a lot, and I have no idea what's ever become of Dave, whether he's still around or not.

On the Marshall side, they had one requirement for the S-IVB third stage of the Saturn V launch stack that it had a unique capability. They didn't—the third stage used a single J-2 engine, liquid hydrogen, liquid oxygen propellant, two hundred thousand pounds thrust, and it had a restart capability. So on the first stage of the Saturn V, you turned the engines on, they fired one firing. That was it. You dumped it into the Atlantic Ocean. The second stage fired, that was five J-2s at a total of a million pounds of thrust one single burn used up all the propellant, dumped it into the Atlantic off the coast of Africa. And the third stage came on, and it gave them about a two-minute kick that put them finally into Earth orbit, and then they would go into Earth orbit and do their navigation and figure out exactly where they were and do the calculations to see when they needed to make the translunar burn, and then the third stage would relight, as I recall, for five or six minutes, and give them the velocity to escape Earth's gravity and be on the proper trajectory to intercept the Moon.

The problem is that once you get into Earth orbit and you turn that third-stage engine off, it's now in zero gravity in Earth orbit, and so the liquid hydrogen, liquid oxygen, they're in the tanks, but they're not at the bottom of the tank where the outlet pipe is. They're just floating around in blobs in there somewhere, and there's always been conjecture about what configuration did this blob of propellant have, but you couldn't just start the engine because likely as not, there was no liquid over the outlet pipe. The engine would either just get—would [get] vapor or helium pressure [or] something, and you'd have the possibility of an explosion or a turbo pump running overspeed or something like that.

So before they could start that J-2 engine on the third stage, they had to make sure all the propellant in those liquid oxygen, liquid hydrogen tanks were down in the bottom of the tank, and so they had two smaller rocket engines called settling engines, which used hypergolic propellants and were very similar to our command module ablative engines. They were contained within a little pod on two sides of the S-IVB stage, single engine pointing backwards or downwards. And what they would do, they would fire [those engines] for a few seconds, which would give the stage a little bit of acceleration and essentially because of that it would force the floating blobs of liquid oxygen, liquid hydrogen to go down into the bottom of the tank. And then when you were satisfied that that was down there, then you could start the big engine. Then it created lots of gravity force, or force keeping the liquid over the outlet pipe.

Well, Marshall went out on their own to develop that ablative thruster. They had some problems, and we did work with them a little bit to help define, just bringing our expertise from the Gemini Program and the Apollo command module RCS Program to their program, and I think we helped them significantly. And as far as I know, those systems all worked, worked just fine.

Over the years, I did consult a little bit with the Army Redstone [Arsenal, Huntsville, Alabama] people on the RCS problems they had in satellite programs I can't talk about, but they had issues with some of those things where the hardware was similar or in some cases identical to what we used. They had some propellant problems like the nitrate problems that was clogging up the–iron nitrate problem. I couldn't get the word out. So you know, not a lot, but I did consult some with the Army, and they were appreciative and that type of thing.

One other Apollo incident that comes to mind was that they had an incident after one of the flights where they landed in the Pacific. They would take the command module to some place, and they would have to deactivate the propulsion systems because the hydrazine fuel and nitrogen tetroxide oxidizer are toxic materials, flammable, detonable, that kind of stuff. So they wanted to get all that stuff out of the tanks, anything that was left, get it out, and then [neutralize] the tanks and make sure that it wasn't dangerous for people to work around and that kind of stuff.

So in this particular case, and I don't remember which flight it was, it may have been 14, Apollo 14, they landed in the Pacific and they took the command module. They got it onboard the carrier, took it to Hickam in Hawaii, Hickam Field in Hawaii, and they had a hangar over there where they were deactivating it. What they would do would be to drain as much fuel and oxidizer out of the command module tanks as possible and then introduce another chemical in there which would neutralize anything that was left. And then they would drain that out and blow it out with helium and that kind of stuff. I'm not sure I remember all of the process, but [it would] make it safe so that the workmen could then work around the spacecraft without fear of encountering any toxic gases or in case a valve was inadvertently opened or something like that.

Well, they had a problem. They offloaded the oxidizer into a tank out at this hangar in Hickam Field in Hawaii. Then they put it in a big tank, and then they put in this material that was supposed to neutralize it in the tank so that they could go dump it somewhere, get rid of it safely. So even though they weren't neutralizing it in the command module, they drained off several gallons' residual out of the tank and were trying to neutralize it. And they had some kind of a problem, and I can't remember whether they had an explosion or a fire or the pressure ran away with them and they vented everything out through the relief valve on this tank or not, but they couldn't figure out what was going on, so they asked us to take a look at it.

And somebody who wasn't a very good chemist had written a procedure for doing this, because it called for draining off the amount of residual nitrogen tetroxide into this ground servicing equipment tank, GSE tank, and then calculating how much you had in there, and then you would add a liquid or a water solution of something that was supposed to neutralize it. And the material they chose was a material call triethanolamine. The guy, whoever it was who calculated how much you put in there wasn't a very good chemist, because if you want to satisfactorily neutralize the N_2O_4, you would put in at least twice the amount of triethanolamine that you'd need to neutralize it, make sure that you had plenty to neutralize all of the nitrogen tetroxide that was in there.

The amount that they ended up putting in that they thought was going to neutralize it ended up being about, as I recall, it was about 15 or 20 percent of what you really need to neutralize all the N_2O_4. And as a result, they had this solution of triethanolamine sitting in there with an abundance, an overabundance of nitrogen textroxide. Well, the triethanolamine that you put it there, it does its thing immediately. It immediately reacts with the N_2O_4 and forms this product that's safe. But then in the absence of—and what happens is you're adding a nitrate to the triethanolamine from the N_2O_4 and that's what does the neutralization. But if it's not all neutralized and there's an excess of N_2O_4 in there, it sits there for a while and then another N_2O_4 says, "I think I'll add on there, too," and so it then starts continuing to react and the triethanolamine nitrate adds another nitrate, adds on to the amine, and now you've got triethanolamine dinitrate. And then pretty soon it sits there and adds another one. Now you've got triethanolamine trinitrate, and at that point you have essentially got a solution of TNT.

Not only that, the trinitrotoluene is—you know, trinitro-anything is unstable, so you've got a triethanolamine trinitrate type of thing. Not only that, it's an exothermic addition. So all of these neutralization reactions are exothermic, so they release heat when the reaction happens. So the thing starts cooking, and it's heating itself up. Well, pretty soon, you're going to get to the temperature that trinitro-whatever doesn't like to stay together, and it comes apart. So as I recall, they didn't have an explosion, but they had a runaway reaction that got—the tank got real hot and was venting and this kind of stuff.

So we went into the laboratory, and in less than a week we're—after we looked at what their procedure was and how much triethanolamine they had added to this tank, we were able to go into the laboratory and create those same conditions in a beaker. And, we controlled so that when you'd get a beaker of nitrogen tetroxide, put in a small amount of triethanolamine and instrument it, and you could see it. It would sit there for a while and all of a sudden, boy, it would take off. You'd see it start to bubble and the temperature going up and all that kind of stuff. And then we'd stop it, arrest it by cooling it, putting in water and that kind of stuff, and then go and analyze the products, and we could see that we were getting the dinitrate and the trinitrate and that type of thing.

So those kind of things were fun for somebody like me who was a chemical engineer who understood and had been trained in those kind of areas and were areas that mechanical engineers and electrical engineers and aero [aerospace] engineers didn't have a clue about. So those were kind of things that I felt like, oh, I probably made a difference by being there. If it hadn't been me, it would have been somebody else, but I can take pride in the fact that I kind of understood probably what was going on and was able to show that that indeed was the case.

ROSS-NAZZAL: Let's talk a little bit about a couple of the Apollo missions.

CHAFFEE: Okay.

ROSS-NAZZAL: Where were you when Apollo 11 landed on the Moon?

CHAFFEE: I was at home. Actually, I worked all of the missions in what you call the Mission Evaluation Room, which was the engineering backup room. And on Apollo 11, I was—let's see, the landing was in the afternoon Houston time. I think I worked the four a.m. to noon shift in the Mission Evaluation Room, and I was at home sitting on the floor of my family room, where I still live, with my kids sitting on my lap and my wife watching what was happening. And that's, you know, I'll never forget that. My daughter was seven, my son was five, and gee, I was only thirty-two, so I was still just a kid, that kind of stuff.

But I started to really get puckered when they were landing, and Neil [A. Armstrong] looked down, decided he couldn't land where the thing was headed for. He had to move it because he didn't want to land in that boulder field. And you know, I knew all of the mission rules and all that kind of stuff, not only for the RCS but for the service propulsion system and for the lunar descent stage, all that kind of stuff. We'd been over and over that kind of thing. So when they started talking about how many [seconds] they had till they were going to have to abort because they were at the propellant red line, boy, I was really starting to sweat it, wondering if he was going to get to a touchdown situation, because they seemed to be going so slow, down three [feet/second] or down five or something like that. And it seems to me they were down within ten seconds of having to abort that mission when they got the contact light.

And then they said, “Houston, the Eagle has landed,” that type thing. So that was a memorable time. I’m not sure my kids remember that. They remember it because I tell them about it. I think they think they remember, but whether they actually do or not, [I don’t know.] But for any adult who was alive, watching that at the time and particularly somebody who had participated, it was something that you never forget.

However, that was not my most memorable thing. Apollo 8 was my most memorable. I was actually on shift that night, and just as a precursor to that, I was absolutely amazed when after only one flight, you know, Apollo 7 with Wally [Walter M.] Schirra [Jr.] and his crew and everything went reasonable well, although there were some, there were plenty of anomalies and that kind of stuff. But it looked like the command service module was ready to go, and that was in October. They turned right around and said, “In December, we’re going to circumnavigate the Moon.” Well, it turns out that that analysis had been going on for a while, and apparently there was—we were still in a race with Russians at the time with some feeling that there really was a credible Soviet attempt to land people on the Moon to beat us. They, whoever it was that pays attention to these things, had evidence that there was a Soviet launch in the works.

And so from late summer on, they were looking at, well, what can we—we don’t want to be beat again, I guess, what can we do? And they had looked at, you know, if Apollo 7 goes well, can we credibly go ahead and do a circumlunar navigation, actually go into orbit around the Moon and then come back. Not land, but do that. And, it was--what an amazing decision, although I didn’t think so at the time, made sense at the time.

And today we're in such a risk-averse situation with our own management with the public and with Congress that we could never recreate the Apollo Program situation again. But with one successful two-week flight, they decided to fly the Saturn V for the first time and go into orbit around the Moon and then come back. And all this stuff had to work. They had to go into this orbit around the Moon successfully. They had to kick themselves out of lunar orbit successfully, and they were going to land at a higher velocity than they'd ever landed because the return velocity from the Moon is several thousand miles an hour higher and that type of thing.

Anyway, that was an exciting mission. I was on station at the propulsion station, keeping track of the RCS data when they went into orbit around the Moon, and that was really exciting. But when they got the TV pictures and they came around the Moon and Frank Borman started reading from the first chapter of Genesis with this TV picture of the surface of the Moon and the Earth in the background, that just broke me up. That's something I still get choked up about and will remember. I've got that picture in my study at home of the lunar surface with the Earth in the background and a quote from Genesis 1:1.

ROSS-NAZZAL: It was an amazing time.

CHAFFEE: Yep. So that was my most memorable. And probably Apollo 17 when they came back, the last time, and I knew for a fact that in, you know, ten or fifteen years we'd be back and [it] didn't happen, so.

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

[Tape change]

ROSS-NAZZAL: Okay. So we are back. Could you tell us about your involvement with the Apollo 13 mission?

CHAFFEE: Yeah. That was a memorable flight also because of the accident and the thing that happened, the explosion of the oxygen tank. That was one of the technical responsibilities of the division that I was in, the Propulsion [and] Power Division. We had responsibility for the fuel cells and the cryo [cryogenic] tanks. So, we were deeply interested in what had happened there and probably had some culpability in not having caught the fact that that heater thermostat stuck on and overpressurized that tank.

But anyway, I was at home when I heard that that had happened, and I got called to come in and went in to work and was not involved in the activity that's depicted in the *Apollo 13* movie and all that kind of stuff. But every subsystem was trying to do something, and what they told us was that in order to save power they had had to essentially power down the command and service module. The crew was going to live in the lunar module, go around the Moon long enough to get back to the Earth, but the electrical power from the service module was gone. We were operating on batteries out of the lunar module and out of the command module, and in order to save enough battery power in the command module batteries to allow a successful entry, they had to just really minimize the amount of propellant [they used and] the amount of energy that they took out of those batteries. They said, "We've got to shut down everything we can shut down, so we're going to shut down the heaters on the propellant lines of the command module reaction control system."

Well, it turns out that that was no problem for the fuel, which in the command module was monomethyl hydrazine, which has a freezing point of minus 60-some-odd degrees Fahrenheit. But nitrogen tetroxide freezes at 12-degrees Fahrenheit. So there was some concern that for the—I can't remember now whether it was four days or five days or whatever it was going to take this guys to go around the Moon and get back—that without any heaters at all that we would have warm-enough propellant. It wouldn't freeze the oxidizer in the lines and then the system wouldn't work and you wouldn't have any control during the entry process.

So the entire period, I was working on thermal analysis because that had been another area that had been assigned to me, thermal control of the propulsion systems. I was kind of the thermal control guru in our organization, although we had a complete organization over in Structures and Mechanics Division that was responsible for thermal modeling. I was the one that had to oversee that and make sure that the models were correct and I understood what they were predicting and what the data said and monitor the tests and this kind of thing.

So we were working on techniques for how can we assure that these rocket engine systems in the command module needed for control during the entry, were not going to get so cold that the oxidizer lines froze and then the system wouldn't work. So we were doing some very heavy analysis and that type of thing. They decided that they could use this technique called barbeque mode where they turn the spacecraft side to the Sun and rotate it slowly about one revolution per hour so that no one side is always in the shade and no one side is always facing towards the Sun. And the other thing—so we did that.

And then the fact that the propellant in the tank, which was a big blob of propellant, it was sitting there at 75 degrees or 70 degrees or something like that, and if you get a big tank of something that has a thermal inertia and it takes a long time for a big tank full of stuff to cool off, as compared to a three-eighths-inch diameter line, which is running around. It's almost like a fin or something. It cools off really quickly. So we had no concern about freezing anything in the tanks, because that's a big blob of stuff—a lot of pounds of warm propellant, and there was going to be no way it was going to cool off to anywhere near an area that we would be concerned about over the four-day period or whatever it was that I can't remember.

But we decided that we got warm propellant in the tank. The propellant in the lines that's running around the periphery of the command module feeding all of these twelve thrusters is going to cool off a whole lot quicker, and so just to be sure that we didn't get anywhere close to the freezing point, if we would fire these engines short pulses every once in a while, it would keep warm propellant flowing a little bit out of the tank into the lines and down to the engines, in addition to which the heat that was put into the engines would provide a little heat to the valves and that kind of stuff, and, as a result, the system worked just fine.

But my contribution to Apollo 13 was essentially thermal analysis and how could we keep the command module RCS system alive without the system heaters having to be used, and it worked well. The guys came back in and were saved. Everybody had a great celebration.

And you know, that's one other thing I'll talk about. The celebrations we had after each flight were essentially legendary. You see the pictures of the Mission Control Room where Dr. [Robert R.] Gilruth and Chris [Christopher C.] Kraft [Jr.] and Gene [Eugene F.] Kranz, and the Flight Director is Glynn [S.] Lunney, and the flags comes out, the cigars come out, that kind of stuff.

But within hours after a mission was over, essentially NASA Road One would be shut down and there would be—back in those days there were was bars and pubs and stuff along NASA Road One out there that were immediately populated and stayed populated for many, many hours as people celebrated the return of a successful completion of an Apollo flight. And I remember being in a place called the Wagon Wheel, which is no longer over there anymore, and being extremely happy, along about two or three in the morning sometime.

Interestingly, just another aside, and I don't know whether this has any part in oral history or not, but I was working with Chester Vaughan on a program with the Marshall Space Flight Center for something called a common engine at the time of Apollo 12, and we happened to be over there in a very difficult negotiation with the Marshall folks about how we were going to do this program. And Chester and I were part of the team that was defining the requirements, and it was going to be Marshall was going to do it. We had resisted that, but they got the nod to do this program. But they assigned Chester Vaughan and I to work with the Marshall technical people to make sure this engine, which was going to meet several Marshall requirements and several JSC requirements, that we had all our requirements in, that we were happy with the paperwork and the requirements and the contractual documentation and all that kind of stuff.

Well, the people we were working with had a different outlook on how to do all this than we did, and so the negotiations were difficult and we were over there on the day that Apollo 12 landed on the Moon. We had worked quite late into the evening and went out to eat late. I think it was probably ten or eleven o'clock when we were eating dinner at a barbecue place or something over there, went back to our hotel and went in the bar and were watching the—I think the landing had not occurred at that time, and we had several drinks and everything. Well, the bar closed at midnight, and I think they gave us about twenty minutes' slack or something like that. And Chester and I were still anticipating the landing of the spacecraft, or maybe they had landed and we were waiting for them to get out or something like that. I don't really remember, because I had had several drinks at that time.

But the bartender said, "Why don't you guys"—and we were about the only guys in the bar. He said, "Why don't you guys come with me. There's another place over here in Huntsville that stays open after hours, and all the bartenders and the musicians from all the places that close at midnight, they go over there after they close up and we have a jam session. And it's called Napoleon's Nook."

So we said, "Gee, that sounds like a lot of fun. Let's go over there." So we went over there, and had a few more drinks, and sure enough, all these other bartenders and musicians from the other places in town showed up and the music was rocking and they had the TV on and all that kind of stuff. So, after a couple drinks over there, Chester and I let it be known that we had personally designed the Apollo spacecraft and this entire thing was our baby and that kind of thing, at which place these people in the bar didn't know any different. They recognized we were NASA people, and so everybody wanted to buy us a drink. So we had several more drinks as we were watching Pete [Charles] Conrad [Jr.] and his buddies walk around on the Moon.

Anyway, it turned out that about six-thirty the next morning, we were still having drinks, and so we just went across the street, got breakfast at a waffle house or something like that, went back to the motel, washed our faces, shaved, and went back to work to negotiate with the Marshall guys, and I have no [memory] of that day at all. But it ended up we left that day with a good agreement. [Laughs] So maybe it made us more ornery or something like that.

But I know my stomach was upset on the way back, and I figured it was all those olives I had in those martinis that had upset my stomach, because I think I must have had twelve or fifteen drinks over the course of a—from ten o'clock at night till seven the next morning, something like that. That's back in the days when I could drink. I no longer drink, but and if I do have one, one does me in.

Just funny personal stories of the Apollo Program, an amazing program. I'm sorry that the agency didn't pick up and go back. As you well know, we truncated the program after [Apollo] 17. We had 18, 19, and 20 ready to go, and they're now sitting as display items at JSC, Marshall, and Kennedy. As a matter of fact, a couple of years ago when--you know, I believe it's Apollo 18 that's in front of JSC, is the only one of the spacecraft on display that is a really flyable thing and doesn't have part of it being a mockup or something like that.

Two years ago when they contracted out finally to refurbish JSC's Saturn V, which is now well along the way, they gave the contract to [Conservation Solutions]. That guy contacted me and said, "I've been given your name because you're president of the NASA Alumni League, and we're trying to get information about materials and processes and paints and things that were used in the booster stages and in the spacecraft."

And I told him, I said, "Well, I can find you all kind of guys that know about the spacecraft, because that's what we did at here in Houston, but the booster was all Marshall guys, and I'll have to scramble some to get you some information about that."

And he said, "Well, we've got some drawings and we've got some process specs [specifications] and that kind of stuff, but, we really need some help to try to understand what we're dealing with so that we can make this—you know, not damage anything as we clean it up and make it authentic as possible as we restore it."

As a result, I was able to talk to some people here I knew who were at Marshall at the time who were both retired and worked for contractors and this kind of stuff and was able to get this fellow in touch with JSC materials and process experts and Marshall materials and process experts to really help them out in understanding what they were dealing with and what the materials were, why they were selected and that kind of thing. I later invited the guy to come down, this has been almost two years ago, to talk to one of our Alumni League dinner meetings about what his plans were, and [it was a] very interesting meeting. There [were] a few guys at the meeting even that said, "Hey, I can help you. I know this guy or I know about this."

Now this March, since they're essentially finished with that, I'm going to have a joint meeting between the Alumni League membership and the American Institute of Aeronautics and Astronautics, and the guy's going to come back and tell us, "Okay, this is what we did and the shape that it's in," and all this kind of thing. So I'm looking forward to hearing the story now of how they did use the information that we were able to supply to restore that thing, because it's amazing piece of gear.

The entire Apollo Program was an amazing accomplishment, and things since then have been, too. In 2002, I guess, it was the thirtieth anniversary of the Apollo 17 landing, you know they landed right at Christmastime, right before Christmas in 1972, and I was president of the Alumni League then. And we threw a thirty-year anniversary bash for celebration of the final splashdown party for the Apollo Program, and I got a hold of Jack [Harrison H.] Schmitt and Gene [Eugene A.] Cernan. They came in and—do you remember the name of the third crewman?

ROSS-NAZZAL: Ron [Ronald E.] Evans.

CHAFFEE: Ron Evans. Ron was gone but I got a hold of his wife out in Phoenix [Arizona], and she came. And we had almost a black tie bash over at the Space Center Houston, had about six hundred people show up and the crew came and we had—Max [Maxime A.] Faget was going to come. Turned out he was sick that night. And he was going to talk. Gene Kranz was the master of ceremonies, and Gerry [Gerald D.] Griffin was there and that kind of stuff, had a really, really nice program and celebrated with a nice dinner and a nice retrospective, the thirtieth splashdown party of that.

I'm still in touch with Jack Schmitt, had an e-mail from him just Monday of this week, I think, and I see or talk to Cernan every once in a while. I haven't heard from Mrs. Evans. She was sick. But anyway, that was my last gasp for Apollo, to throw the last splashdown party for the Apollo Program over at Space Center Houston, and it was quite an affair, quite an affair.

ROSS-NAZZAL: What are your memories of what the mood was like at the Center when the Apollo Program finally did close down in '72?

CHAFFEE: Well, I think it was gratitude, pride. At that time, the shutdown was not, on Friday you were working on Apollo and on Monday you weren't. It was, for the engineers, pretty much [over] after '67, '68, and after the fire was resolved and all that kind of stuff, the engineering side of the house, we supported the flights but the development was all done. And so very early on, in '69, '70, I was already for most of my effort was off of Apollo. I supported flights because I understood the systems and that kind of stuff, but I was working Shuttle at the time and trying to develop the Shuttle technology and all that kind of stuff.

So there was just kind of a natural transition. We were grateful that we had gotten through Apollo 17. We hadn't lost anybody. We'd had one problem with 13, and we didn't fly the last three, but there wasn't a lot of feeling of denial or dissatisfaction with that. That might have been a good decision. Those flights were expensive. We'd been there six times successfully. We'd had one near-bad experience and, you know, to say, "Well, you guys have done that, been there, done that, now let's quit," and on top of that, the public after Apollo 13, you know, after that it was kind of, "Well, why are they showing this Moon stuff? Where's my soap opera, you know what I mean? I watched Apollo 11. That was exciting. Now let's get on, you know. Where's Lucy and Archie Bunker, and I want to see that program. I don't want to see this news special." So there was kind of a natural transition.

I don't think there was a loss of or a downbeat in morale or anything. Staffing was greatly reduced because in, you know, at the peak of the Apollo Program, I think JSC had over five thousand civil servants, and by the end they were even having RIFs, reduction in force and that kind of stuff, much, much down into the thirty-five hundred staff level kind of thing.

But the engineering side was deeply involved in the Shuttle Program, and I personally was and that type of thing, and so, you know. And the other aspect of that was we fully expected that having been to the Moon with that momentum and that knowledge and that technology, it wouldn't be very long before we'd be back, and that was the expectation. I figured by '90 we'd be on the way to Mars. You know, here in '72, give us eighteen years, boy, we went to the Moon from a standing start in less than seven years. And I figured given that demonstrated ability and the expertise we had built up, going to Mars was going to be tough but certainly doable. And I still think that.

ROSS-NAZZAL: One of the projects you worked on was a project on lunar soil. Can you talk to us about that and how you got involved with that?

CHAFFEE: Yeah. That was a material where the idea was can you live off the land by making something useful out of lunar soil, and some of the soil they brought back was high in a concentration of a chemical called ilmenite, which was an iron titanium oxide, and it turns out that the oxygen in that oxide is much less tightly held chemically than in typical oxides. Like iron oxide, which is rust, iron and the oxide are very tightly bound, and it takes a lot of energy to break them apart. Ilmenite, with the introduction of the titanium, it turns out, heated modestly, maybe up to like a thousand, eleven, twelve hundred degrees, the oxygen starts being released and you can capture it and either pressurize it or condense it as a liquid or, that kind of thing.

So there was some thought that, "Gee, if we can find this ilmenite, and this wasn't an Apollo application, but now when we go back to the Moon, we could go around, mine this ilmenite, land some place where it's a fairly high percentage of the lunar surface fines," and it's also magnetic, so it turned out it was easy to separate from because it has iron and easy to separate. So I worked with a group that was trying to demonstrate that there was a viable chemical process, manufacturing process, for mining ilmenite and turning it into gaseous or liquid oxygen on the surface of the Moon.

I worked with a guy from Structures and Mechanics Division who was head of this project, Dr. Dick [W. Richard] Downs, really interesting guy. He's been gone now for probably, fifteen years or something like that, but really a brilliant guy. We had, I think Dave [David S.] McKay was involved in that. He's still a planetary geologist. But we showed that you could take lunar soil with ilmenite in it and with a magnetic drum you could separate the ilmenite out so you could concentrate the ilmenite where what you were dealing with wasn't like a half a percent or one percent. It was a high percent, and, again, I don't remember what it was that we could get up to.

But then you threw this stuff in a pot and heat it up, and you could heat it up basically by using—you could get it hot enough by using sunlight in a solar collector where you focus the solar energy from a big mirror on this reactor and then the stuff would just cook off and then you capture the oxygen that's being emitted from this mixture of ilmenite and compress it and put it in a tank or if you can refrigerate it, you can chill it to a liquid and keep it in a liquid tank. And we showed that that could be done and documented, what the design of a process would be, made a design of a little lunar surface device, could have been, if you wanted to go the Moon and scoop up some of this stuff. But we did the whole process on the surface of the Earth. We'd showed that you could refine ilmenite from lunar soil, put it in this thing, heat it up, the oxygen would come off it, you know, just chemistry.

And there was a report written on that, and I can't remember the name of it or exactly what year that was, but must have been '70 or early seventies, '70, '71, like that. And interestingly, we applied for a patent on that process, and the Patent Office turned us down because it said it has no earthly application because ilmenite is not known as an Earth's chemical, and "We don't give patents for things that are only applicable to other planetary bodies." Well, it turns out later on they changed their mind somehow, and some other group went in and did patent that later on. So, I was always kind of aggravated that—I thought, well, at some point, they're going to go back and do that, and old Norm would like to have a few thousand a year out of assigning that patent to somebody. Although it would have been a NASA patent, of course, but I would liked to have had the recognition or something. But someplace or other I still have the letter from the Patent Office explaining why they were denying the patent so.

ROSS-NAZZAL: Well, I think this might be a good place to stop unless you want to tell me about the Apollo Program, any other aspects you think we might have overlooked.

CHAFFEE: Okay. Gee, I think that's—that probably covers it.

The only other interesting thing on the Apollo Program was one of tankage where we used, because the nitrogen tetroxide and the hydrazine material were tough stuff, we kept them in bladders inside of a tank, and it was like putting your stuff inside a water balloon and then squeezing on the water balloon to get it to come out to get out the pipe. So you'd have a standpipe down the middle of this tank that had little holes in it, and it was, the standpipe was, surrounded by a Teflon bladder, and you'd put the propellant inside the bladder, and then provide pressurized helium on the outside of the bladder, and it would squeeze the bladder down around this standpipe and force the propellant out through these little holes in that end of the pipes and that kind of thing and feed the engines and this kind of thing.

When you washed, when you cleaned the tanks, number one, we had some problems with the bladder but those were more mechanical problems. They didn't fold up right and they'd get funny three-cornered folds and it would rip and this kind of stuff, and also the bladder acted differently in one gravity than it did in zero gravity, which would put some pressure on the bladder and just kind of—you can imagine, if you got a pipe going up and down and bladder around it in zero gravity, the fluid is distributed equally from the top of the pipe to the bottom of the pipe. But if you're doing a ground test, it's all sitting down at the bottom in kind of a blob like that, which stresses the bladder. And understanding those problems and figuring out how to handle a ground test and that kind of stuff provided some challenges, and I worked some on that kind of thing.

But there was a system called the triflush [phonetic] that we use when you had to clean the system out and wash the propellant out of the lines and everything. We used a three chemical thing. We would first—let's see, now, I'm trying to remember. We would first use a water rinse, and then so you'd wash the systems with a water flush because the propellants were all soluble in water, and get everything out that you could. But then you didn't want to leave the water in the system, so you washed that out with, I believe it was, methanol, methyl alcohol, in which water was essentially infinitely soluble. And then so you'd flush it then with methanol to get the water out. And then we flushed it with something else, and I can't remember whether it was acetone or what it was. I just—that's gone from memory now. But there was a third fluid, something like acetone, that would dissolve the methanol and get it all out. But then this third chemical had a very high vapor pressure and then it would evaporate, and you could just blow warm helium [or nitrogen] through the system and get it out. So then you ended up with a system that was completely dry and didn't have any propellant or water or methanol or acetone in there. At that point, it was in a nice safe state.

Well, it turned out that in the tank, we weren't getting all this stuff out, and it would—and the tank was titanium, which everybody knew nothing corroded titanium. But it turned out that after flushing and blowing helium through there and deciding by measuring the vapor content of the helium that was coming out, "Oh, okay, we've gotten everything out. There's nothing coming out but helium." We were wrong, and there was still a little bit of moisture. Not moisture in the sense of water. But left in the tank there would be a little bit of a mixture of maybe some propellant, some water, some methyl alcohol and some acetone left down in the tank. And of course, in one gravity, it was sitting down in the bottom, bottom of the tank. And over a period of time, for reasons that I don't recall now, that attacked the metals in the bottom of the tank, including the titanium and would cause a weakening. It was essentially corrosion where that when you went back and pressurized the tank it would break.

And so the figuring out what to do about the triflush was something. I think—the bottom line, they stopped the third chemical and, anyway, we resolved that and we modified the process and the chemicals we used to avoid the titanium tank being corroded. So, one other thing of, "Chemistry is important to these programs," so. [Laughs]

ROSS-NAZZAL: Well good. Well, we appreciate you taking your time to meet with us.

CHAFFEE: Okay. Great.

[End of interview]