ORAL HISTORY TRANSCRIPT

HENRY O. POHL
INTERVIEWED BY SUMMER CHICK BERGEN
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BERGEN: Today is February 9, 1999. This oral history interview with Henry Pohl is being conducted at the offices of the Signal Corporation in Houston, Texas, for the Johnson Space Center Oral History Project. The interviewer is Summer Chick Bergen, assisted by Carol Butler and Tim Farrell.

It's so nice to have you here, Mr. Pohl.

POHL: Thank you.

BERGEN: Why don't you start out by telling us how you got interested in space flight in the beginning.

POHL: [Laughter] Okay. I grew up in the country, went to a rural school, and I remember while going to school, one of the proudest moments of my life was when we got electricity, and the reason I was so proud of that was that I wired up the house. After the war, REA [Rural Electrification Administration] came through and wanted to know if we wanted to sign up for electricity, and Dad turned to me and said, "Can you wire up the house?"

I said, "Sure." I wanted electricity.

So I did, and when they came to turn on the electricity, I was off at school. They checked it out, everything checked out okay, and we got electricity. The neat thing about that was most of the people around there got their houses turned down that day. They had gotten electricians out of Halixville and Yokum and Quero to wire up their houses, and they were following city code. Well, I ordered a book from Sears and Roebuck on how to wire your house for REA, and I memorized that book, and I did everything according to the book. So when they checked it out, we got electricity.

And that was kind of a start in wanting to do something other than farming. I had always intended to be a farmer, I really wanted to be a farmer, and when I got out of high school I did try to farm for a couple of years, but I couldn't make enough money farming to buy a farm. Engineers were making $400 or $500 a month. Land was selling for about 25, 35 dollars an acre then, and I thought I could go four years to college, get a degree in engineering, become an engineer and work four or five years, make enough money to buy a farm and go back to farming.

Well, the Korean War was kind of going on then, and when I got out of college I got drafted, and I happened to go to Texas A&M [University] one of two years when you did not have to be in the Corps. They were getting too many officers then, and so they decided it would not be mandatory to be in the Corps, so I elected not to go in the Corps. So I got drafted, did my basic training at Fort Bliss. Since I had a degree in engineering, they sent me to Huntsville, Alabama, to work with ABMA. That's the Army Ballistic Missile Agency. Since the GIs were freebies to the work force, they let us work where we wanted to work instead of sending us here or there. They let us interview for jobs and let us decide which organization we wanted to go with, and I remember I spent about three days up there interviewing, two interviews a day, and went back in and asked Sergeant Love if there was anything else. I remember he got very irritated at me. He says, "Are you going to be one of those guys that's not going to be satisfied with anything?"

I said, "No. There's lots of places around here where I could work, but if you would let me work where I want to work, I want to make sure I've seen everything."

He said, "Well, there's one more place, but we don't like to send GIs down there, since they don't treat them very well. They won't let them go to training, they won't let them pull KP, they make them work nights, make them work weekends, and all that kind of stuff."

Well, I wanted to interview them anyway. So he set up an interview for the next morning, and I went down there, and when I got out of the cab, this great big cross-eyed guy was waiting for me, took me in his office, and the first thing he told me was, "We have so much work to do around here, we can't possibly get it all done, and we don't have any need for anyone that don't want to work, but if you want to work, we're going to pull you in like a civil servant, give you the same kind of job, you'll work the same kind of hours as the civil servants work." And he says, "If you want to work out here, you've got to see a test on the Redstone today at eleven o'clock. Now, you're going to have to miss lunch, but you can get a sandwich out of that ptomaine box over there for a quarter, and that'll hold you over until you get back to the barracks tonight."

Well, now, here this guy was expecting me, on $75 a month, to buy my lunch, and I wasn't too sure that I really wanted that, but I did want to see a rocket engine firing. We went out to the power plant test stand, crawled around out there, and that little old engine looked pretty simple. It was the Redstone. We went over to this building about 1,000 feet away. The walls was two feet thick with one-foot windows, one-foot-thick glass windows. They was made out of twelve panes of one-inch glass. And they were barking those commands out over the sound power system there, and all at once that thing lit up, and that building just kind of quivered a little bit. It ran for 150 seconds. I had never seen so much power in my life in such a small package. I would have gladly given them my $75 a month to work on that thing.

So I went back and told Sergeant Love, I said, "That's where I want to work," and I did. I started out as test engineer on that stand with two other guys looking over my shoulders, Harry Johnstone and Jim Williams, both very, very sharp, very good, good people. And they gave me a lot of responsibility pretty quick. You know, it wasn't long until I was cleaning the stand and carrying a clipboard over to the blockhouse. They would pick it up and check off the test over there, and that lasted for a couple of weeks.

One morning Harry Johnstone didn't show up, and I waited and waited. Finally somebody said, "You'd better call Artley [phonetic]." So I called Mr. Artley and told him we were ready to run the test, but Harry hadn't shown up.

He said, "Didn't he tell you? He won't be there anymore. You've got to run it."

You know, that was the hardest 150 seconds in my life. It was so easy when he was barking out those commands, but when I had that responsibility of making sure that everything was right, it was a whole different story. But I took that stand over, and I guess I ran that stand for about six months or seven months, and I learned a lot, and I learned a lot from those technicians out there. They kept me out of trouble lots of times.

But I remember one interesting thing. Well, I remember several, but one of them was I ran a quarter-inch line down a pipe, a big pipe, down to an instrument on the engine, and I was afraid the vibration was going to fatigue it and break it, so I had the guys to wrap some safety wire around it about every two feet all the way up that pipe, pull it down tight against that pipe so it wouldn't vibrate. Well, Mr. Artley came by. I heard him holler, "Where's Pohl?" I knew I was in trouble. I walked out there, and he says, "What does it cost to run this test?"

"I don't know."

He told me. "What does this engine cost?"

"I don't know."

He told me. "What does this test stand cost?"

"I don't know."

He told me, and he said, "You've got it hanging up in the stand with baling wire." Well, I knew immediately what he was referring to. He said, "Fix it."

We delayed that test for an hour and a half while we put some big clamps around that big pipe, some little clamps around the little pipe, and clamped it together where it was real nice, very, very professional-looking, and ran the test. And I thought that was so absolutely stupid. That cost us an hour and a half and was totally, absolutely unnecessary. But I later began to realize that what that did was keep us on our toes. You know, it kept discipline in the system, and if you get sloppy in one area, you're going to get sloppy in another area, and sooner or later it's going to come home to bite you. That was a major, major lesson I learned.

All through the operations up there, that was the way that they operated. They kept a lot of discipline, a lot of professionalism in the system, and everything needed to be done yesterday rather than today. So we spent a lot of hours. I spent a lot of hours.

I remember one Saturday night about eleven o'clock at night running around up there on a stand. We had a LOX [liquid oxygen] valve froze up, and trying to get it thawed out, when one of the technicians came to me and says, "Henry, you ought not be up here tonight. You ought to be in downtown Huntsville having a good time. You're in the Army. They can't make you work but forty hours a [week]. I'd go to the IG [Inspector General] if I were you."

And I really couldn't sit there and explain to him that there was nothing in this world that I wanted to do worse than what I was doing then. I was up there that Saturday night because I enjoyed it. It was more interesting than anything that I could do. I was hooked on those rocket engines. We didn't really have a space program at that time yet. That was before we started out. That was in the [19]'57, '58 time—'57 time frame, I guess.

Anyway, after I left that stand, they moved me over to the component test lab over there, and I started designing and testing those scale model rockets, designed them for the Polaris, for the Atlas, for the Saturn, each one, the S-IVB, the RL-10, and we put those in vacuum chambers and wind tunnels and then used them to develop the launch pads, the deflectors in Huntsville for the static test tower for the launch deflector for Saturn IB, and then we put them in the wind tunnels up in Tullahoma, Tennessee, to get base heating data from those things.

That was kind of a fun kind of thing to do. It was kind of a one-man, or one-person, show. You did everything. You did the design work. I can remember sketching out on a piece of notebook paper changes for an injector, taking it down to the shop on the way home at night, stopping by the shop in the morning the next morning, picking up that part, bringing it out to the test stand, and having the technicians put it in the test stand and test that change that day, look at the data that evening, make more changes, take it back to the shop that night, and have the changes made and modifications made, and bring it back and test it the next day. So things moved very fast. We learned a lot very, very quick, and while it was a lot of long hours, it was all fun.

There was nothing that I—and my entire career has been kind of that way. You know, everything I have done has been more fun. I never did develop many hobbies because I always would just as soon be out there working on something as I would playing golf or something else that people do. That was kind of my recreation as well as my work.

Anyway, I guess along about [19]'62, I had a good friend up at Langley [Research Center, Virginia], and when they decided to put the space program here in Houston [Texas], or the Manned Spacecraft Center here in Houston, he talked me into coming down here, and I did. I almost went back to Huntsville. [Laughter] I got down here, and the division I was in was kind of—it was headed up by two very capable people, but they were used to the research environment of Langley where everything had to be very thorough.

I can remember going to staff meetings at night, or in the evening and run way into the night, where they'd spend hours talking about editing reports and who the editors are to be. They wanted to make sure that every report was absolutely perfect, it was well thought out. And they had grown up in a totally different environment than I grew up in, because the environment I grew up in, the Army was in competition with the Air Force, and we were trying to get things done on the Jupiter and the Redstone before the Air Force could fix their Thor or their Atlas. So time was of an essence to us there, and we didn't worry about a few misspelled words in a report. We didn't worry too much about if everything was verified. Come down here, and we were working at what I thought was a snail's pace, and here we were going to go to the Moon in seven years, and we just didn't have time for that kind of stuff.

Well, fortunately, I wasn't down here too long until they split that division up and made two divisions out of it, and brought a person by the name of "Guy" [Joseph G.] Thibodaux down from Langley to head up the Propulsion Power Division [PPD]. He had the same philosophy that I had, a very, very brilliant, very experienced individual that had one of the keenest minds of any people that I have ever dealt with. So we got along real good then.

Now, Mr. [Aleck C.] Bond and Mr. [Joseph N.] Kotanchik, that was my former division chief and deputy division chief, they developed some of the finest metallurgists and stress analysts in the world, brought young people in, and they had the tools and resources, and they were conducive to the kind of environment that they developed some of the finest young people that we have in this country even today. You know, NASTRAN was a stress program that we developed. It's used by the automotive industry, used by everybody in the world now. All of the petrochemical industry has—they've modified and improved it since the early days, but they used to design all of the stuff here and on the ship channel.

From there, after Thibodaux came in, I became subsystem manager for the RCS, Reaction Controller Systems, on Apollo, worked on the Reaction Controller Systems on Gemini. We had many, many problems on those things. There was very, very little experience in this country in starting rocket engines in a vacuum, and we used acid-based propellant that depended upon the chemical reaction to get it up to temperature to ignite, self-igniting stuff, and I had a little experience working on those things with my little rockets there in Huntsville because I used T—triethylaluminum [phonetic] to start them, which, when it came in contact with air, was self-igniting. And I had a little experience with trying to start those in a vacuum in Huntsville.

We had a lot of problems with that because you'd get a hard start, and it would blow up the rocket or burn something up. But I probably had more experience in that arena than anybody in the country at that time, any of the contractors or anybody. But we were having a tough time with those things. They were always failing, and we put some rings—if you look at those service module engines, you'll see the little rings on them. Mr. Thibodaux never did like those rings on there because they did nothing to make that engine strong enough. If you got a hard start on it, it was just going to fly all to pieces.

I remember one night about 2:30 in the morning, we had the service module out there in a vacuum chamber, in TTA [Thermomechanical Test Area] out here, and we were trying to run a fourteen day and night simulated mission to the Moon and back, and that involved about 175,000 starts or something like that. That whole chamber was getting coated on the inside with a substance that looked like rouge, and we went in there and took some samples through an air lock, and we pulled that stuff out, and you could put it on an anvil and tap it with a hammer, and it would pop like a cap pistol going off. So we got some of it in a vial, and I gave it to Mr. Chaffee, Norm Chaffee, and told him to take it over to Transportation and ship it up to the Bureau of Mines to Henry Purlee [phonetic] to get them to analyze it and see what it was. Well, he came back long faced late in the evening. "Can't ship it. We've got to find out whether it's a Class C explosive or not before you can ship it."

I says, "Now, how in the world are you going to find that out unless you get it to the Bureau of Mines?"

He said, "I don't know."

I said, "I do." I said, "Put it in your briefcase and get on an airplane and get up there tonight." [Laughter]

So he did. He stuck his little sample in a briefcase, and we went up there, and the next day he called back and found out that it was hydrazinium nitrate, which is similar to ammonium nitrate and similar to the stuff that caused the Texas City [Texas] explosion in the ship or that that guy used to blow up that federal building, that kind of stuff. You have to hit it pretty hard to get it to detonate, but you can detonate it, and that's what would happen in the combustion chambers. It would build up in there, and it would detonate. So we had to figure out a way to eliminate that a little bit.

Anyway, a couple of nights later in that same test program, one of the technicians came in about eleven o'clock at night and said that stuff was getting out of the chamber, it was all over the cars outside. We all ran out and got looking, and the cars were just yellow with kind of a brownish residue. We got cigarette wrappers, papers, and started scraping that stuff up, and I even signed a piece of paper that night saying I'd take responsibility for the test stand because the guy in charge of it wanted to shut it down, and we only had about a day or two to go on it to get our fourteen days, and I didn't want to shut it down. But I was able to convince them that we would get the chemist in in the morning and we'd start analyzing that stuff.

Well, we got our chemist in here and we got a chemist from Rice University and a chemist from the University of Houston come down. I even went home, washed my car off about 2:30 in the morning, went to bed, slept a little bit, came back out here, and went out to the test lab out there, and this one scientist from, I believe, the University of Houston stared in that microscope for a long time. Finally he straightened up and said, "You know, if I didn't know better, I would say that was pollen."

I said, "Son of a gun. That's what it is. It's golden rod pollen." We had golden rod all out there, and it had nothing to do with what was in the chamber, but it looked like the same stuff that was in the chamber.

We got through with that test, and the engines all worked on Gemini and worked on Apollo, and we came a long, long ways in a very short time in getting that hardware and finding out how to design that stuff.

But kind of the attitude back then was that when you had a problem, you know, you did what you needed to do to find a solution to it. You didn't throw up your hands and say, "I can't," or "It's not—" We've got to find a way to fix it. It's just like when people started worrying about astronauts living for fourteen days in space and how do you keep people healthy and how do you pick healthy people? We went to the medical community and said, "How do you pick healthy people?"

They said, "We don't know. We only deal with sick people."

So the first thing we did was implement a program looking at different people. You know, we had a pretty big program out here at JSC [Johnson Space Center] where we picked probably six or seven hundred people and put them through all of these tests and kept records on them. I know I was one of them. Every month I went over and rode a bicycle and did all that stuff. Up at Ames [Research Center, Mountain View, California] we converted some rooms up there into hospital rooms, just like a hospital, and we actually hired people to stay in bed for ninety days. You know, they sat there. They were mostly schoolteachers in the summer. They'd sit there writing or reading, laying there in bed, writing and reading, they had a TV up there, and the meals were brought into them and everything. Out of that program we learned that we needed to get people up quick, and as soon as they had an operation, you know, within a day or two now, bypass surgery, they've got them walking around to keep the muscles from atrophying and going bad. That was kind of the attitude that the program had at that time.

Anyway, after a while, Mr. Thibodaux came in one time and told me he was going to make me section head. I really didn't much want to be a supervisor. I liked the engineering part, and I didn't think I was very good at handling people and managing people, but I took over the section, and then a little bit later he came in and told me he wanted me to be branch chief and take over the pyrotechnics.

I said, "Mr. Thibodaux, I don't know anything about pyrotechnics."

He said, "That's all right. I know everything there is to know about them. All I want you to do is supervise the people. I'll take care of the technical."

Well, I took that over, and then when he retired, I took over the division. While I was branch chief, every year he'd come in and ask me to go to Washington [DC] on one of those short transfers up there because I could never get a better job if I didn't go to Washington [DC], because anybody that took a higher-level job had to have some experience in headquarters. I'd tell him I didn't want a better job; I had a great job. And I did. I always had a great job. Every job I ever had was a good job.

But anyway, when he left, Dr. [Maxime A.] Faget called me over there and told me there wasn't nobody watching the henhouse, and he wanted me to take over the division. So I did. And then after 51-L [*Challenger*] and Aaron Cohen got moved up to [JSC] Center director there, he asked me to take over the engineering side of the house. I first told him I couldn't do that. My wife was sick then.

A few months later he called me back over there, and he told me he wanted me to reconsider, and I didn't have the heart to tell him no, I wouldn't do it. So I told him I'd do the best job I could. So I wound up as Center director—not Center director, director of engineering. Sorry about that. I need to be thinking about what I'm saying. And then I had all the engineering there at the Johnson Space Center for a while.

I guess, backing up a little bit and reflecting a little bit on the people and the things that made the Apollo Program the success that it was is basically two things. One is that the attitude of the people that worked there was that we all wanted to beat the Russians to the Moon. So we had that competitive spirit of trying to do things quick, trying to do things safely, and figure out what we had to do to beat the Russians. So everybody tended to work together with each other for a common cause rather than worrying about their little niche in the world.

You know, I never did worry about who got credit for anything as long as the right thing was done. If the wrong thing was done, you just couldn't tolerate that, but if the right thing was done, if I could convince somebody else to take a position on something that needed to be done and let them get credit for it, that was great, as long as it was the right thing to do.

I think the other thing that probably had the biggest influence—and I know the operations side of the house got nearly all the publicity. Engineering is dull. It's not glamorous. It's not easy to put it out in front of the people. You know, when you're on a drafting or drawing board or working out complex equations and complex problems, it's not easy to present that in a way, but we were very, very fortunate in having picked up people from the research organizations, you know, Lewis Research Center [Cleveland, Ohio] and Langley Research Center.

You just look at the people that came down here and managed the Apollo Program. Every one of those people grew up in a laboratory doing things themselves, running out the calculations, developing the formulae, and doing things themselves. So when it came time to manage 200,000 people all over the United States and contractors of every persuasion, they could do it from the standpoint of having been there, of knowing what it took to do the job. You know, they could look at a problem and see how many man hours they thought it would take to do it, so they weren't easily snowed by something.

For example, every airplane that flew in World War II flew with what was called the Langley wing. Now, Langley never built an airplane, but they developed the formulae and the criteria for the airfoil or the airflow over the wing giving the most efficient wing, and they did that by cut and try and instrumenting in the wind tunnels there, different shapes, different forms, and really understanding the lift characteristics or the airflow over wings. The cowling that was used around the engines on all of the World War II aircraft was the Lewis cowling. You know, they developed a cowling that would keep the engines cool and yet minimize the drag, and every aircraft manufacturer that made these aircraft picked up those kinds of things and used them in their design. So these people had all been down the road in their particular disciplines.

Huntsville, the Armored Ballistic Missile Agency, which later became the Marshall Space Flight Center, used the arsenal concept to develop the rockets. They had brought those Germans from Peenemunde, Wernher von Braun and about a hundred other people, to El Paso [Texas], and after about three V-2s went into Mexico and one of them went into the Juarez cemetery over there, they wouldn't let them launch any more of those V-2s from El Paso, and they moved that whole group of people up to Huntsville, Alabama, to the Armored Ballistic Missile Agency.

Those were extremely dedicated, extremely sharp people. You know, I always was convinced that if the Russians would have captured them and the Russians would have given them a place to live and given them their meals and given them the tools to develop rockets, they would have worked just as hard over there as they did over here, because that was their desire. That's what they wanted to do, was build rockets.

Well, we did all of the design work, and we actually built the first ten Redstones and then contracted with Chrysler to build the production ones. We built the first Jupiters, designed them in-house, tested them in-house, and then, after that, they went on to a contract with somebody to build the production ones.

Saturn was done the same way, the Saturn IB. We built the first Saturns in-house. When we got that original contract, I thought that was the dumbest thing in the whole world. Mr. Haukohl come in the office one night, grinning from ear to ear, said, "We just got authorization from DAPRA, Defense [Advanced] Projects Research Administration, I believe is what it was called. Anyway, "We've got funding to cluster eight Jupiters together." Now that's going to be eight times bigger than our biggest rocket we had.

I said, "Mr. Haukohl, we stayed up all night last night trying to start one engine. We'll never start eight of them at one time."

He said, "Well, Henry, we might have to put a couple extra ones on there so they lift off if six of them lights up."

Shoot, within about six months we had one of those things light up on a test stand with people around it. [Laughter] We got those things where you just punch a button and they were gone, where before we had a real complex ignition sequence. We had to put an apple in the chamber, and you had to light it, and then there was solid propellant charges, burn a wire in two that opened the propellant valves, and then it had another wire going over there to the other chamber that would have to break to open the main valves, and we were at main stage then. That's all right when you've got a single engine, but when you light up a whole bunch of engines at one time, that wasn't very good.

I think I can even take a little credit for developing that ignition system, you know. Mr. Haukohl came in there one night and he told me that we needed a better way to start a rocket engine, and he suggested that I look at "T" [triethylaluminum] and I look at hydrazine. We had that H____ catalyst then. And I started working with that staff, and I spent about a week, and was about to get a migraine headache because I can't figure out what to do.

I go in there one morning, and I've got my facts all lined up, two column, the pros and cons of hydrazine and the pros and cons of triethylaluminum. I go in, and I said, "Mr. Haukohl, I can do this or I can do this, but I can't do both of them." I said, "This is the pros of this, and these are the cons of this. These are the pros of this, and these are the cons." I did it very snappy, and when I finished, I said, "Now, Mr. Haukohl, what do you want me to do?"

He got up and put his hand on my shoulder, kind of patted me on the shoulder a little bit, and says, "My son, I'm sorry, but I cannot tell you how to do your job," and walked out.

So I go on back to the test lab, and I get my technicians out there, and I'm talking to them, and I said, "I just don't understand Mr. Haukohl. I got this problem, and I went to him for some help, and he just told me that he couldn't do my job and got up and walked out."

This one technician told me, says, "Oh, that 'T,' that stuff is no good. We used it in our flame-throwers in the Second World War, and when we'd pull the trigger, we'd just hold it down and hose out all the propellant. There wasn't any use saving it, because it wouldn't ignite the second time." He says, "Those nozzles just all stop up."

I said, "How'd you clean them out?"

He says, "Oh, it's easy to clean out. You just unscrew them and slosh them around in some diesel. That diesel just dissolves that stuff and washes them right out. Then you can screw them back in. But you've got to get out of the tank to do that."

Well, a light bulb lit up. I said, "Hmm, I wonder if we couldn't put a little diesel or rocket fuel in there behind the 'T' to flush it out."

So we got our boiler side gauge and went out there and made this little test stand right quick with a little two-element simple injector and chamber, and we'd bring a little "T" up in the boiler sight gauge – [Recording interrupted] – RP-1 down on top of it, and we ran about 200 tests there over a period of about two weeks, never had a misfire at all. That was passed on to Rocketdyne, and that's what they used to start the H1 engines, the H1 engines [Saturn] 1B, Saturn V. As a matter of fact, every LOX kerosene engine in this country was started using that system.

Now, I have recently had the privilege of looking at the Russian engines that were used on the Russian moon program, and it turns out they used the exact same system designed almost exactly the same way, and I didn't have any idea that they were doing that. I don't know whether they found out what we were doing or not and copied it. I don't think so, because I think their work probably preceded ours a little bit, and I think they were using that, probably, before we started using it.

Anyway, we both used the same ignition system, and it was a good system, and it worked very well, and if Mr. Haukohl would have picked one of those systems, I can almost guarantee you it would not have worked. Even if he would have said, "Let's go with 'T,'" it most likely would not have worked, because what I would have most likely have done at that time was designed it exactly the same way that it was designed for the flame throwers on the tanks. And had it not been for the experience that this technician had with it and passing that information on to me, it probably still would not have worked, because I most likely would not have thought about that. But it worked very well. That's just one of the ideas.

I remember building up a test stand out there at one time—and I'm going back to the early days and some of the things that happened when we were young. I was building up a test stand, and we were running so many tests I couldn't get out there and get measurements, and we had a whole bunch of pipes we had to run, quarter-inch pipes we had to run from a valve box down to our little model over there. So I looked out through the window and estimated the length here and here and here. Well, it was off a little bit, and we bent up about twenty of those pipes, stainless steel pipes that way, and was out there putting them in, and Mr. Haukohl come out there and stood around. Those people didn't seem to do much. You know, I didn't think they did anything. They mostly just stood around, stood around and watched what was going on, but one of the technicians came to me and says, "Henry, you're in trouble. Mr. Haukohl's rubbing his chin." Wasn't long, he'd take his finger and do it like that. So I come walking over to him.

He says, "Henry, you can do better than that."

I says, "What's wrong?"

He says, "Those tubes you're putting in out there, they're catawampus."

I said, "Well, we couldn't get out here and get the measurements, so I just guessed at them and I missed them a little bit, but that's all right. You know, there's enough length on that pipe where—in fact, it comes out over here and it's got a little 'S' in it. Come over here is all right, and it's not completely straight up here. It's pulled down a little bit." I said, "That's all right."

He says, "Henry, fix it right."

So we tear them out, throw them away, have them all nice and neat, you know, to come out straight, straight, straight. That was another example of keeping a person from getting sloppy. You know, if you get sloppy in one area, you're going to get sloppy in another area, and that kept us on our toes, kept us from getting sloppy.

It also gave us the experience so that when we started going out dealing with the contractors that we had contracted with to build this hardware, to pass judgment on what they were doing. You know, too often in the federal government, the people that they have overseeing a project really don't have enough experience or know enough about that job to pass judgment on it, and they're totally at the mercy of the contractor, and sometimes the contractor didn't have a lot of experience in those areas. I would say, if the one thing that made Apollo successful, it was the fact that all of the management, from the top all the way down, had been down that road, had the experience of working in the laboratories, developing the equations, they knew how to do those kind of things, so when they went out and dealt with the contractor work force, which built up, you know, from nothing up to 250,000, you pick lots and lots of people that's not very experienced, they could supervise that activity and pass judgment on it and make decisions based on their own personal experience.

You know, most of the things you do in the engineering world is somewhat intuition. You have to have a feel for it. You really don't have time, most of the time, to do the analysis, to run out the equations before you make a decision. So to make a decision, if you've been down that road before, you've got a feel for what you need to do, and I think that was one of the really, really big things that paid off.

The other thing that really helped us here at the Johnson Space Center was the fact that these people from the research laboratories of Lewis, Langley, and ABMA that came down here understood the value of having good equipment and good facilities. They did not spend all of that money on our test facilities here because they had the money. They spent the money on those facilities because they knew that they had to train a lot of people, a lot of young people, and they knew that those people had to have the opportunity to test their ideas, to test their theories, to be able to fail.

Now, I know Gene [Eugene F.] Kranz got a lot of publicity for saying, on Apollo 13, that failure was not an option, and there are times when a failure is not an option, but if you're not allowed to fail, then, by definition, you cannot succeed. You have to be able to make a mistake, and if you make that mistake in time, you've got time to fix it, but if you never do anything and time goes away, when you finally find out that you did something wrong, it's too late to fix it.

You know, I tell people that I still remember the answers to nearly all the problems I got wrong in college. I don't even know the questions of the ones I got right. So if you can get something in that laboratory, if you can run a test on something early on and it doesn't work, then you've got time to fix it. If you want to be absolutely certain that it's going to work before you ever build it, before you ever test it, then you'll never get to the point of where you're going to test it until it's too late to fix it.

So that's one of the things that really helped us a whole lot in the Apollo Program is that we were able to get things in test early, prototypes, even things that we knew sometimes wouldn't work, but at least it gave us an idea of how to change something, how to modify something, how to do something different. And we did something. We didn't just sit around.

Now, you know, one of the biggest problems I see out there with the agency now is that they're so afraid that they're going to make a mistake, that they do not give the young folks an opportunity early on to accept the responsibility to go do something. It takes too many reviews, too many processes that you have to go through to get something done, and time gets away from them. And then you find major problems or major flaws in the design very, very late in the program. I guess if there's one thing that I could change, it would be to be able to let the young folks that's coming into the agency now to have the responsibility to do something in their own right and do it quick and do it early. That's kind of a summary of where I've been.

BERGEN: Okay. I do have some questions.

POHL: And that's totally off the cuff. I didn't take any time to think about it.

BERGEN: That's fine. That's fine. During your time at the ABMA, did you actually ever have an opportunity to work with Dr. von Braun or any of the German scientists?

POHL: Yes. Well, my boss for most of the time that I was there was a guy by the name of Guenter Haukohl, H-A-U-K-O-H-L. It turned out he was not one of the Peenemunde guys, he was a test pilot testing the V-1s and those rocket-powered aircraft that they had under development over there.

Karl Heinberg [phonetic] was my lab director there, and I interacted with him on a daily basis. Kind of a high-strung guy, had a tremendous amount of experience. He was test director on the V-2 over in Peenemunde.

POHL: I remember when the Redstone shut down on the pad down there on one of the early unmanned Mercury flights. I saw him come out of his office, and he was running down the hall. He would take two steps with one foot and kick his other foot out to the side. Come running down in the office and says, "Guenter, we're through. We're through. It's all over. We might as well quit. We might as well go home."

Mr. Haukohl took him by the shoulder, pushed him down in a chair, and says, "Now, Karl, tell us what happened." And that's when he started telling us about the Redstone shutting down.

I tried to run von Braun out of a test lab one night. [Laughter]. It was just about sundown, and this old '51 OD Chevrolet with two little red flags, two-star flags on the front bumpers, but I didn't notice those kind of things, come up from the piney woods. We had a gravel road that came in from the Tennessee River back up from the back. It come driving up there, and it came up in back of that lab, and I walked out there as far as I could walk out with the sound power system and hollered at him, "Get that thing out of here. Don't you know you're not supposed to have cars in here? Get it out!" waving at him. He just turned, come driving right up to me. He got ten feet of me before I realized who it was, and, of course, then I got tongue-tied, I couldn't talk.

He got out smiling, said, "Get somebody to move the car around to the other side of the building and tell me what you're doing here."

So I take him over and showed him my little engine and told him all about it. I remember he wanted to know what the heat transfer was on the combustion chamber, and I told him. Well, he had to convert it from English units to metric units in his mind a little bit, and then he says, "That's pretty high. How do you know it's right?"

I told him, well, I took this big piece of copper and machined out the inside of it for the contour of the combustion chamber, and then I cut rings and slots along the outside of it so I formed these little squares of copper, about eight-inch by eight-inch squares of copper, and I knew how much each one of those squares weighed, and I could put a thermocouple on each one of those pineapple cores there. By measuring the temperature rise of it, I could calculate how much heat that copper is being absorbed.

He said, "Very ingenious. Very ingenious," and went on off, got in his car and drove off someplace else. But he was bad to do that. He'd give them fits over in the manufacturing area because he was always over in the manufacturing area looking around and watching what was going on. Everybody thought that he ran the Center there, but von Braun didn't run that Center. Eberhard Rees ran it. Von Braun much preferred to spend his time trying to convince somebody to do something.

He was a big proponent of Space Station. He had that big circular Space Station with a counterrotating hub in the middle of it. I remember telling him one time when he was talking about this thing, he was telling us people can't live in the absence of gravity, so we're going to have to build an artificial gravity out there, and if people's going to live on the outside out here and it's going to have this counterrotating hub in the center, there's going to be zero G, and they'd go down these spokes every day to work, and then they'd go out to the outside to live in the gravitational field.

I said to Dr. von Braun, "It won't work." I said, "There's no way we'll be able to balance that thing. He's going to be jumping all over the sky up there." Because I used to try to balance car tires, you know, by jacking them up and spinning them, putting weights on them.

He says, "See these two rings out here? We've got to have lots of water out there," and he says, "this is the fresh water tank and this is the septic tank." And he said, "We're going to have each one of those half full, and that fluid is going to go to where it has to go to balance the Space Station."

When I was working on those little rockets there in Huntsville, we worked on a project there for a few months called Nova. Now, Nova was five times bigger than Saturn V, and the way they had the scheme laid out was Saturn IB was going to be a million and a half pounds of thrust. Saturn V was going to be five times larger than Saturn I, and Nova was going to be five times larger than Saturn V. We actually took these little model rockets and stuck them way down in a water tank and fired them down in the water tank to work out the deflector down under water and try to keep the bubbles from destroying the rocket as it came up the side of the rocket, to try and duck that stuff out far enough away from the rocket so you didn't get the collapsing pressures against the skin of the rocket while it lit off.

We were actually going to take this thing out in the Gulf and sink the first stage down and sink the second stage down, and then the third, fourth, and fifth stages were going to be up above the water. I guess we would have continued in that direction a little bit had it not been for President [John F.] Kennedy deciding to go to the Moon, and, of course, when he decided to go to the Moon, that put a hold on all of that stuff.

That's kind of interesting, too. You know, we had a rocket down on a pad at the Cape [Canaveral, Florida] ready to go in orbit before the Russians put Sputnik up, when word leaked out about it and Secretary [of Defense] [Charles E.] Wilson put a freeze on all Redstone launches, pulled all of our rockets back from the Cape, sent a team of people down to Huntsville to see if we could launch a rocket from Huntsville. I guess Colonel [John C.] Nickerson got court-martialed over that. But had we been successful in doing that, there would have been no missile race. You know, we would have been ahead of the Russians because we would have had one up. Even though it was a tiny little thing, we would have had one up there before they had one up there. Had there not been missile gap, it's quite likely that [Richard M.] Nixon would have been elected President, because I think that missile gap was just enough to tilt it over, over the other way.

Von Braun was pushing very, very hard for a Space Station. It most likely would have went in that direction rather than going to the Moon. And I've often wondered if we'd be better off today if we'd done that or if we're better off today than we were. I really think that going to the Moon and the Apollo Program did more to help the quality of life of humans than probably any project that we've ever had. It worked somewhat like the Manhattan

Project in that it was an urgent thing, you had a very definite goal on the end, but there were so many, many spinoffs.

You know, you can't imagine all of the things that came out of that program that actually made life a little bit easier, a little bit better. You know, some of the things we probably don't need, but you look at this watch with that LCD crystal. It is so cheap that you don't buy batteries for them. When the watch plays out, you throw it away. The technology was around, but it was not in a form that you could use it, or at least the theory was around. But on the lunar module we needed displays that used almost no electricity because batteries were very, very heavy and weight was a premium. So NASA just—we needed it, and we just paid for the qualification, the development qualification of that technology, and used it in some of the displays on the lunar module, and it just caught on like wildfire, integrated circuits. You know, those things would have come about, but probably not nearly as quickly as they did.

You have to realize, when we started out going to the Moon, we used Freidan [phonetic] calculators for all of our calculations. You remember you'd pull that lever and punch the numbers in the thing and always had a problem with them going into a do-loop and have to plug and turn them off and then try to start over. But vacuum tubes, all of our electronics was run by vacuum tubes. When we first started, the guidance system or the computers for Apollo, we had them strung with vacuum tubes. Some of the vacuum tubes was a foot high. They occupied a whole building down there, and by the time we went to the Moon, we got them in a foot by foot by a foot and a half, I guess. All of those kind of things—I was talking about NASTRAN a little bit earlier. You know, there was another spinoff that changed the way that we design things, changed our automobiles up.

But, yes, I did have a fair amount of experience with Dr. von Braun. He came around quite frequently. I remember one time when I was working on the deflector for the launch pad. They had come up with a design that had a certain angle to it, and when I put it under my little model and tested it, all of the exhaust gases came back up under the base of the rocket. By making a few quick changes, I found out that we could change that angle just a little bit on it and the flame would scour down and go out.

Well, I went up and told Mr. Haukohl about it and then went and showed him the film, and then I went and talked to Mr. Heinberg about it. We went over that, and I got a call a little bit later that morning that they were going to go up and talk to von Braun and wanted me to go with them. So I go up there and get in the car, and we drive over to headquarters and walk in there. I don't have one piece of paper with me. When I get in there, we sat down at his table, Mr. Heinberg says, "Henry has something to tell you." And that's the first time I knew I was going to brief him on that.

So I get a pad, and I just sketch on a pad what happens with the design that they had and what happens when you change that design. He just goes over and picks up the phone and calls [Kurt H.] Debus down at the Cape, tells him that I've got some data up there that says the design is wrong and to change it to such and such and that I would supply the details. Done, just like that.

BERGEN: You worked on the Redstone, and when it came time to actually put a manned spacecraft on top of the Redstone, how did you feel about that?

POHL: I really had a great deal of confidence in that Redstone. I was test engineer on the rocket engine on that thing early on. I personally went in and filed all of the curvature on all of the control valves that control the thrust level of the rocket and tested those, take them and test them and file on them some more, and test them, file on them some more and then you file too much, throw that blade away and get another blade and start over again on it, because the production Redstones had a tendency to hunt. Their thrust would go up and then it would go down and little bit, go up and go down a little bit, because the flow of the pintle [phonetic] on the valve was not uniform. And by reshaping that a little bit, you could get it to where the flow was proportional to the position of the pintle from end to end. So that was one of the modifications that I made there in the lab on those valves for each of those manned programs.

We did some things on the program that I thought reduced the confidence level a little bit. Like they came out with this edict that we had to have dual seals on all of our mechanical fittings. So we had to go in and machine all the AN fittings with a little groove in there and put a little O ring in there and put them together so you had redundant—well, that satisfied a paper thing, but it wasn't as good. If you had a metal-to-metal fitting and you had it tight, you didn't have to worry about it leaking, but if you had an O ring in there, you could always cut the O ring, or the O ring could seal for a little bit because it's flexible and pliable. Then when you hit it with a lot of pressure or vibration, it could come loose.

So we did a few things like that that I didn't like, but, by and large, I had a great deal of confidence in that Redstone. It was basically a V-2. Not many people realize that, but the Redstone was a modernized V-2, used the same propellants and the same kind of turbopump [phonetic], the same kind of gas generator. As a matter of fact, we even used some V-2 valves on it because we couldn't get the American-type valves to work quickly, so we just copied the valves that were used on the V-2.

The injector and the combustion chamber was different. The V-2 used the showerhead out of the German showers for the LOX injector on the thing. They were standard. They were off the shelf. They built thousands of them, I guess, so they were easy to come by, and that's what they actually used. They'd take that little showerhead that they used in the German showers and used them for the LOX injectors on the V-2.

BERGEN: After Alan Shepard's mission, President Kennedy announced the goal of sending a man to the Moon and returning him safely to Earth by the end of the decade. What did you think when you first heard that challenge?

POHL: [Laughter] I thought that was the dumbest thing I'd ever heard in my life. I mean, you have to appreciate where we were back in that day and time. Like I said, we still had vacuum-tube technology. Transistors were just coming into being a little bit. The Atlas was the biggest rocket that we had at that time, and we were probably still having seven failures out of ten flights with it.

Gemini flew with—I can't even think of the name of the rocket it flew with now—Titan. The Titan was just coming off the line. Most people don't realize that either, but we started out with the Titan I that used LOX and kerosene, and it had the most complex rocket engine I've ever seen in my life. As a matter of fact, I don't think they got a single Titan I out of sight. Back in that day and time, if the rocket got out of sight where you couldn't see it, it was classified a success. But they lost both their test stands at the Cape, and they lost both of their static test tower stands in Denver.

So they were going to be down a long time now because they had to rebuild it, and they went back and proposed that they change from LOX / kerosene to storable propellants of kerosene 50 and nitrogen tetroxide. That gave them time to rebuild their stands, gave them time to redesign their engines, and they went from one of the most complex rocket engines that was ever conceived by a human to one of the simplest. And that Titan II engine was a very, very good, very reliable engine. It was just coming on line. I guess Atlas at that time was producing maybe 225,000 pounds of thrust, and it was going to take something many, many times larger than that to even think about going to the Moon.

We had been testing for about six or seven years the—I believe it's called the M1 engine. It was a million-pound thrust design, but it was just an engine and just the combustion chamber, and they had only had very, very limited success with it. And then President Kennedy comes on and says we're going to go to the Moon in less than ten years. We got an awful lot of things on our plate. We've got a lot of things we've got to learn. We've got a lot of things we've got to do. I was amazed at how quickly a lot of that stuff came together. That F1 engine worked beautiful. We had almost no problems with it.

By that time we had a good group of rocket people in this country that had a fair amount of experience. They had gone through Redstone, Thor, Atlas, and Jupiter, all the same people building that same hardware, and then the H1 engine was a vast improvement over the Thor, Atlas, Jupiter engine, and they were able to learn from that and then design and build the F1 engine. So that came together very, very good. Matter of fact, the whole Saturn project came together good. All the Saturn stages came together good. They had very few problems with the upper stage engines, the J2 engines. So that part came together really good.

We had a lot to do in the command service module, lunar module, and we had a lot to learn on those programs, some of the things that you would think that you ought to know, but you just don't think of everything. For example, most people don't realize that in space, the part of the vehicle that's facing the sun goes to a temperature of about 250 degrees. The part that's facing the shade or deep space goes to a temperature of about a minus 250 degrees. That's about a 500-degree differential that you're working with between those two.

When we took our lunar module and put it in a thermal vacuum chamber out here and started shining the simulated suns on one side and the other side exposed to liquid helium temperatures, it was literally tearing itself apart. The side that would expand was getting hot. The side that was getting cold would contract. The thermal-induced stresses in it were just tearing it apart. So, very late in that program, that's the reason we wrapped it all in that goldized and anodized aluminum mylar, to insulate it so it didn't break itself up. If somebody would have been thinking, you would have thought about that, but it wasn't thought of until very late in the program, after we got some test data down here.

Like I said earlier, one of the keys to the success of this program was we did have a lot of testing. We tested everything, and we tested it on more than just one sample. So there was not too many things left to chance, not everything you can thoroughly test on the ground. It's very, very difficult to test for a very long period of time the absence of gravity on the ground, and it's just not a person's intuition to think in terms of no gravity. You have no idea what all gravity does for you. You know, you a pot on a stove and put water in it and heat it and the water gets hot. If you had zero G in there and you put it on the stove, you'd form a bubble right down in the bottom of that pot, and that bubble will just keep growing, push water up, and the water would stay cold. You put gas in your automobile, and you drive until the tank's empty, and it sucks the gas out of the bottom of the tank. Well, you put rocket fluid in a tank and in zero G there is no bottom. The bubble, the gas bubble, is going to go right where you don't want it, and when you open the valve, then you're going to suck the gas out. So you had to come up with some kind of device that will keep the propellant over the outlets in the absence of gravity or in a slight negative G.

Those are some of the things that we had to learn along the way or had to think about. You know, you stayed up lots of nights just laying there thinking about all of the things that could go wrong and could come back to haunt you, and we were very, very fortunate in that we thought of most of those.

BERGEN: There were definitely a lot of things to take into account. Since you did work on so many of the early rockets, do you think that if we had chosen direct descent as opposed to lunar rendezvous and orbit rendezvous, that we would have been able to make it to the Moon in that decade?

POHL: I think it would have been possible. I think by the time we got down to the point where we did earth orbit rendezvous that we would have developed the techniques to put two or three large vehicles in orbit and join them together and then send that on to the Moon with a direct landing.

Of course, you know, when we first tried to do that, and I've forgot which Gemini flight that was now, but they were going to catch that target vehicle and rendezvous with the target vehicle, and these astronauts were used to those Corvettes, and when you want to pass somebody you push the foot to the floor and zoom around them. Well, they line up behind this target, they're going to catch it, and they shove the throttle forward, and the more they burned their rockets, the further behind they got and the further behind they got, and they couldn't understand why for a while. What happens is, when you're in orbit, your altitude is determined by the velocity of your vehicle, and you know the centrifugal force just balances the pull of gravity on it, and the faster you go, the higher the orbit. Well, when they'd shove the throttle on, they'd accelerate the vehicle, the vehicle kept going up into higher orbit, so you got one vehicle going through a small orbit and the other one trying to go through a larger orbit, and he's got a long ways to go. So when you want to catch up with somebody in orbit, you decrease the velocity a little bit and let it fall down a little bit smaller. It's like a race car driver going around the inside of the turn. You know, you're always better off going inside than the outside. We learned that, but we would have learned those kind of things.

I think it would have been more expensive. It would have certainly taken a lot more launch capability down at the Cape than we had because we would have had basically twice the launch capability down there to launch those big rockets that we had, because we would have had to put some very big machinery in orbit and had to put it in orbit very quick. You know, you launch one, and then just as soon as that one launched, you'd have to launch a second one. But yes, I think we could have done it.

BERGEN: You were at the Redstone arsenal when it was changed to Marshall Space Flight Center. Did that make any noticeable change in what you did and how you did it?

POHL: Not immediately. For the first year or so there was really no change, and I had an option of going over with the Army side or going over with the NASA side. They gave us all an option of staying with the Army or going with NASA, and I chose to go with NASA. Kept doing the same thing I was doing the same way that I was doing it, but very shortly after that, I'd say within a year, year and a half, after they formed the Marshall Space Flight Center, they came out with this decree that they were going to hire all contractors to run the facility, and they started laying off technicians or, as they retired, they started backfilling them, and they moved all of the technicians out of hands-on work into paperwork kind of positions out there, and we brought contractors in to run the facilities out there.

That was one of the major things, to tell you the truth, that influenced me to come to a Manned Spacecraft Center because my friend at Langley told me that we were going to use civil service technicians to run our facilities down here. Well, I hadn't been down here very long until they decided down here they were going to contract all of that, too, and have contractors run the facilities, and that's all right. It's just that it doesn't give you the same flexibility as it did, and it's a little bit of what you get used to. I could do a lot of things working with the civil service technicians that I couldn't do in interfacing with a contractor, because I'd get out there and I'd torque bolts, and I would take things loose, and we worked together on things just like that. Well, now, when you've got a contractor running a facility, you've got to write a piece of paper to get somebody else to go do it, and you can't just get out there and get in that layout and do things yourself.

So that was probably the biggest change that took place. They changed from what was classified as an arsenal concept where everything was developed in-house and then you contracted out for the production of it, to having the contractors come in and run the facilities. That was kind of a political maneuver, and I suspect it would have come about even with ABMA because there was a lot of opposition building in Congress for an arsenal-type operation then. Congress thought that the money ought to be given to industry, to the contractors, rather than hiring government people with the overhead and everything that is associated.

Plus a lot of people have the idea that civil servants didn't work as diligently as the competitive environment out there. I never really found that to be true. I'm sure it's true in some areas, but like I say, there in Huntsville, I had never worked with a group of people that was more dedicated, that was more capable, and that worked harder than those people did. That's all they thought about, was building rockets. That's all they dreamed about, was building rockets.

Von Braun wanted a Space Station out there in the worst kind of a way. That was his big dream, and he used to talk about doing anything and working for anybody to get the resources, to get the money, to develop the technologies that was needed to put a Space Station up there.

BERGEN: Seems like it would have been an exciting environment to work in.

POHL: It was. That was a very, very exciting time and exciting place to work, and, for the most part, that was true down here, too, when I came down here and started working on Apollo, although I would not in any way, shape, size, or form want to go back through the Apollo Program.

BERGEN: Why is that?

POHL: I just couldn't do it. I'm too old. [Laughter] But that's all I did for seven years. That's all I thought about, day and night. That's all I dreamed about, day and night. We had so many problems, and you never had enough time. You always got all of this stuff piled up in front of you, but a lot of it was fun kinds of things, too.

I guess I remember in [19]'65 making fifty-one trips out of Houston, and the reason I remember that is there's fifty-two weeks in a year, and that means that I lacked one of making one trip a week, but there's some weeks where I'd be in California, Florida, and Washington [DC] all in the same week. So that would give me three weeks or two weeks where I didn't have to travel. Travel at night, I always traveled at night, leave here after work. Work all day at a contractor's facility, catch a six o'clock flight back home. In California coming back this way, that puts you home late, be at work at 7:30 or eight o'clock next morning.

Mr. Thibodaux, I was talking about him a while ago. He used to come in there and tell me I was working too hard. He always took the attitude that work was not measured in the number of hours that you put in; work was measured in what you produce. And I remember one time specific. It looked like we were going to have to put those coal chutes on the lunar module, those thin, metallic slides that they've got under the rocket engines on that thing to direct the exhaust out, because after we put all that insulation on there they were afraid it was going to burn that out, and those things were heavy, and weight was extremely precious on it. We had this guy that was our analytical guy there in the division that was supposed to run out the numbers and decide if we had to have it or not.

Mr. Thibodaux had promised Joe [Joseph F.] Shea [Apollo Program Manager] that we would have it like on a Monday. Well, Monday we didn't get it; Tuesday we didn't get it; Wednesday we didn't get it. I went in to Mr. Thibodaux and I said, "Guy, we've got a problem. You promised Joe this Monday. Here it is Wednesday, and this guy tells me that he wants to run one more case. He still don't have it, and I don't know what to do about it.

He's working as hard as he can work. He was in here Saturday, he was in here Sunday." I said, "He's here when I come in in the morning. He's here when I leave at night."

Guy looks at me real hard right in the eye and said, "You obviously don't understand work. Work is force times distance, and as far as I'm concerned, he hasn't done a thing." And he might as well not even been there. He hadn't done anything because he hadn't produced anything. And that was a good way to look at things. We usually sometimes think we put in our forty hours a week, we've done something, but unless you've produced something, you've done nothing.

One of the neat things they had when I was in Huntsville was every single afternoon the whole time I worked out there, I had to, before I went home, take ten or fifteen minutes to write a one-page summary of what I did that day and turn it in. The section took that from all of us and condensed it into one page that was passed out the next day to the branch, the next day to the division, and the next day up, and every day von Braun got from every laboratory a one-page summary. It always stayed one page, but it moved up the line. I thought that was so stupid, to have to take time out to write down what I did that day, and sometimes it was pretty hard to write something down, because sometimes a day goes by and nothing gets done. That really did help you think about what you were doing and what you were producing and what you can put down.

I remember one time I was test engineer on a powerplant test day and then I wrote in my activity report that I had to get a flame permit from Safety, and they required me to drain the fuel tank before they would issue the flame permit, and I thought it was a whole lot safer to have the tank full than it was to have it empty, because if you've got it empty, you've got it full of explosive vapors. If you've got it full, nothing can happen. Of course, it's way up there anyway.

Boy, the next morning Gordon Ortley [phonetic] called me up, and I mean he just chewed me up one side and down the other side for agreeing to do something that I knew was not safe. I was the one responsible for that facility, and I didn't let anybody do anything, regardless of what their job was, knowing that it would be less safe than—I said, "Well, you know, I wanted a flame permit, and I thought it was all right either way. I just thought it was dumb that they required me to do that."

But they read those things, and they passed them on up the line. But it really did help us, and I think that's one of the very good things that every young person needs to learn to do, is to sit down the last ten or fifteen minutes every day and write down a little bit about what you accomplished that day. Of course, there's some days when that's awful hard to do.

BERGEN: That's a good philosophy. When you first came to the Manned spacecraft Center in Houston, what exactly were your responsibilities?

POHL: Well, I came down here with the understanding that I was going to help them design the test facilities. I had been involved in testing there in Huntsville, and there is a lot of things that I would have done different up there if I could have done it over, having been down that road one time. This friend of mine that was in Langley kept talking to me about coming down here, so I went up to Langley and talked to him about that, and the guy that I talked to then showed me some of the designs, some of the plans, and I came down here with the understanding that I would be involved in the design of the test facilities.

Well, by the time I got down here, because there was a conflict between the Manned Spacecraft Center and Marshall at that time, the Marshall folks didn't want to let me go, but they couldn't hold me if I got a raise down here. The Manned Spacecraft Center people didn't want to give me a raise until I got down here. So, you know, they had a little battle that went on for a few months, I guess from November to about April of the next year.

Well, in the meantime when I got down here, the Manned Spacecraft Center had already gone through reorganization, and the guy that I hired on with no longer had the responsibility for the test facilities. So that's when I picked up the reaction control systems and started working on the spacecraft side of the house rather than on the test side of the house, but he did let me continue working with the design of the facilities and working with Brown and Root in the design of those facilities. I was able to contribute an awful lot to that because I had just built some facilities in Huntsville, vacuum facilities and altitude facilities, things like that, and I knew some of the things you could do and couldn't do by learning the hard way.

I took my little rocket and put it in this huge container out there and fired it, and all of the exhaust hit the bottom, bounced right back up, and came right by the chamber and killed all the vacuum around the engine and burned everything up around the engine. So I had to put me a nice smooth deflector in there to turn it down, this big long pipe out there to the other tanks out there. So when I looked at their facility that they had designed out here, they had an engine firing right up against a wall. I remember telling Mr. Ferguson, I said, "Dick, you can't do that." I said, "That exhaust is going to come back and hit that engine in five milliseconds, and that's going to be too quick to get any data."

Mr. Ferguson says, "Henry, don't make those accusations unless you can verify it."

I said, "Well, that's easy to do. It's square root KGRT, and K is such and such, G is such and such, T is such and such, the square root of that."

He sits there with his pencil, and he's real good at that, rounding out numbers. "Hmm, you're right."

So we went and redesigned those facilities. I never did tell him. He just thought I was brilliant because I knew that. I never did tell him that I had been down that road, had tried, and made that mistake and learned the hard way. But I did help them with the design of all of those facilities out at TTA, the instrumentation, where it was set up, and the big vacuum chambers over there and some of that kind of stuff. But that lasted probably over a period of about a year and a half off and on.

The rest of the time I was working more on the spacecraft side of the house and the design of the RCS for the Apollo command service module [CSM] and Gemini [unclear] and RCS and then the lunar module, and had a fair amount of influence over the design of those systems very early on. For example, we used the same rocket engines on the lunar module that we used on the command module, which was a really hard-fought battle, because the contractors, each one, wanted to develop their own thing, and I couldn't see the need to pay for the development twice. We used the same propellant tank design for both of them, so that saved us a lot of time than having to develop two things. It turned out that the Gemini and command module rocket engines turned out to be almost identical. They had different mounting brackets, but the internal construction was, for all practical purposes, the same.

BERGEN: Were you able to learn from the problems in Gemini that you were able to apply to Apollo, or were they just simultaneous?

POHL: You know, it turned out to be almost the flip side of that. We ran into problems on Apollo on the command module engines before the problems was recognized on Gemini, and that was primarily because we weren't—engineering here in Houston wasn't following the Gemini Program as closely. That was being managed primarily with the project office dealing with the prime contractor, who was dealing with the sub. So the reporting back through that chain was not as crisp and short. So we actually found the problems on the command module side of the house, and then when we started looking at it, we discovered that we got the same problems over on the Gemini side of the house.

So when that happened, then they kind of folded both of those programs together and gave engineering the responsibility for the engineering on both sides of the house. So when that happened, then we started bringing the designs closer together. We'd find a problem on one side, we'd fix it on both sides of the house almost at the same time. So it turned out that worked out very good.

We had a little more time at the end on Apollo to make some changes that we never incorporated on the Gemini side of the house, we just reduced the mixture ratio, reduced the efficiency a little bit on Gemini and used it. And there were some things we did on Gemini that we didn't incorporate on Apollo. Gemini had a little bit different application. Their engines would burn for long periods of time and not as many firings. The Apollo engines had ten-millisecond bursts, or twenty-millisecond bursts, very short pop, pop, pop kind of things, and in a vacuum the ablator would ablate different. But, yes, they complemented each other.

BERGEN: What were some of the challenges in designing these propulsion systems?

POHL: Well, I talked about the formation of hydrazinium nitrate and that stuff detonating in the chamber and just causing that chamber to fly into a million pieces when you'd try to start it.

BERGEN: You never told us how you resolved that problem.

POHL: We were able to change the injector design so that we could put a little confined area in the injector so that when we brought the propellants together initially, they would stay compressed so that the temperature didn't go down so low. You know, when you dump a propellant—like if you dump water in a vacuum, it immediately flashes, and about a third of it will go to a gas and two-thirds of it will go to ice instantly. Well, any of these liquids, when you dump them in a vacuum, that's what happens. Part of it vaporizes and the other part gets very cold until it freezes.

But if you've got a chemical reaction that's trying to take place, especially an acid-base reaction, you know the speed of that reaction is a function of the temperature, and if you're dumping it into a vacuum where your base temperature is going down very rapidly, then the reaction is slowed down, and you accumulate a whole lot of it in the chamber before it would ignite, and when it would ignite, then it had already formed these nitrate compounds, and you get a detonation.

So what we did, we came up with a design with a little precup in there that would keep the propellant compressed long enough so that it would heat up enough to start the basic combustion reaction, and that worked very well.

On the command module and on Gemini we used what was called a splashplate injector where the two elements came together and hit another plate very close to that, which formed kind of an orifice in there, and we never really had that problem. Plus the ablator was much stronger, and it was a good absorber of energy. So we never had the problem on Gemini or command module that we had on the service module early on. But that's the way we fixed it.

We had problems with the expulsion devices on the propellant tanks. We'd tear a little Teflon bag that we had on the inside, and that's another one of those areas where sometimes your rules get you in trouble and create more problems than they fix. We wanted everything double, triple redundant, and so what we did was take three of those little Teflon bags and stuff it inside one of the propellant tanks, then you put the propellant on the inside of the bag and you squeeze it out by putting helium on the outside of the bag, and that squeezed the propellant out.

Well, what happened is that they used three-mill bladders in the tank, and when you would push the propellant out and that bladder all wadded up down in the bottom, and then you try to fill it and that propellant running in there, it would pull the—and when you get up to the top, now it's too short because it's all doubled up on the bottom, and the weight of the propellant hanging down there would break one of those plys and then break the second ply and then break the third ply. It's like taking an I beam that you want to pick up and you put three ropes on it and you tie them all at different lengths. All three of them would hold, but one would come up and it'd break, the next one would come up, it'd break, the next one come up and it'd break. So we changed that and made, I think, a sixty-nine-mill single bladder. It no longer satisfied the redundancy criteria, but at least it worked.

I guess the altitude starting of those engines, getting good coatings on the combustion chamber was not a problem. We would have flaws in the disilicide coating and little wormholes would develop through the combustion chamber. It would oxidize out. You'd see a little tongue of white smoke from the outside of one of them when you were running it, and after a while fire would start shooting out through that hole because it would just eat a little hole right through, where you didn't have coating on it, it would just eat a little hole right through the combustion chamber.

It took us a while to figure out how to test those chambers to make sure that you had good coatings on them, that you didn't have any flaws in them, which turned out to be a very simple thing. For example, we just put them in this chamber with this induction heater, and we'd heat those things white hot with air in there and look for smoke, and if you saw smoke developing someplace, you'd throw it away. If it passed that test, it was a good piece of hardware; you didn't have to worry about it.

BERGEN: When you became chief of the Dynamic Systems Branch in 1964, how did your job change?

POHL: Well, I just picked up the pyrotechnics in that change. That was all of the explosives. That's the one I was telling you a little bit earlier about when Mr. Thibodaux asked me to take over that job and I told him I didn't know anything about the explosives. He says, "Oh, I know everything there is to know about them. I just want you to manage the people, and I'll keep you straight technically." Well, you know, it wasn't exactly true. I had worked a little bit with solid propellants, and I knew a little bit about them, and I had worked a little bit with PETN [Petaerythrite Tetranitrate] and HNS [Hexanitrostilbene] and RDX [Cyclotrimethylenetrinitramine] and those things in Huntsville, but not a great deal.

So that worked out really good. We had some good people in that section kind of on opposite ends of the spectrum. We had this one guy that you had to kind of pull the reins in on him a little bit all the time because he was always—he could do more things wrong and yet get things right quicker than any human being I had ever seen in my life. He would just charge, and he'd go off this way, and he'd realize that's not the right thing to do, he was going right back this way as hard as he could do, and this way, and he would come up with a solution before anybody could do it quicker, and having done three or four wrong things first. So you had to kind of keep him.

The other guy that was absolutely brilliant when it came to explosives, he came from the naval research labs and had the same kind of experience with explosives that I was talking about the Langley folks having in aerodynamics. He understood that stuff backwards and forwards, but he was an extremely cautious kind of a guy, and I guess when you work on explosives all your life, you learn to be cautious because those things have a habit of blowing up on you. But he was brilliant, and he kept us out of an awful lot of trouble on that side of the house.

I can tell you a good, interesting experience along those lines. The subsystem manager had a problem and couldn't go to one of the FRRs, one of the early Flight Readiness Reviews down at the Cape, so I sent this young man down there that had been working for us for a few months, and I told him, "I want you to take a look at all of the N-rays." We took a neutron radiograph of all of that hardware because it was the flip side of an X-ray and the explosive would show up as a black or dark image, and the metal would come in clear. I wanted him to look at them.

Well, I get a call at home at eleven o'clock at night. Tom's on the phone. He's got this problem. They won't let him see the N-rays. I says, "Why not?" He's not a certified N-ray reader. I said, "Well, don't sign the flight readiness statement."

He says, "Can I do that?"

I says, "I don't know. You can try."

Next morning, I mean, everything broke loose. They got him tons of N-rays to look at, and we got these little explosive trains that cut the service module loose from the SLA [Spacecraft Lunar Adapter], split the SLA down in four pieces and cut it loose from the S-IVB, you know, to expose the lunar module out there. He got looking at those things. He says, "What's all of these little spaces in here, these bright spots in here?"

"Oh, shoot. That's breaks in the powder train." And some of them was a quarter inch or longer where you didn't have any explosive touching each other. We had two of those things in there. Then I came unglued. I mean, we're getting ready for a launch, and you obviously can't fly that junk, and how did he get by? Well, there was—I forget now, but there was twelve or thirteen people that had signed off on the certification statement on that hardware, and I got them to read them off to me and the phone numbers, and I gave the list to the secretary and had her start calling them, starting from the last one that signed the work up toward the first one that signed it. Every single one of them was telling me that they just checked to see that it had the other guy's signature on it.

When we got into the lab that produced the explosives, the quality guy at the company told me that he checked to see that DCAS [phonetic] has signed it. Now, DCAS is the government inspector, and the government inspector had signed it. If it's good enough for the government inspector, it's good enough for him. I got a hold of the government inspector, and I learned the next day when I got out there that he had a trach [tracheotomy], you know he had to put his thumb over his—the hole to talk, but he told me, says, "Well, I don't know anything about N-rays, but we get this certification from the company that takes the pictures, and he certifies that they're good N-rays."

Well, I knew what I was going to get when I called him. He said, "Yes, we had a lot of quality problems up here a few years ago, and they put me in charge of quality control, and I personally look at every single N-ray that goes out of here, and I make sure that it is a good N-ray."

I said, "What do you look for?"

He said, "Well, first thing I do, I look to see if the film is fogged." He said, "Sometimes the film is foggy and it's not clear." He says, "I put a gray scale block on there so I can check the gray scales to make sure that we've got the proper exposure on it, and I look to see if it's in focus."

I said, "Well, what about the hardware that you're taking pictures of?"

He says, "I don't have the foggiest idea of what we take a picture of."

Based on his signature, which was on something totally different, looking for something totally different, we were able to pick up eleven or twelve more signatures certifying that that was good hardware. That just gives you an example of how little things can slip through if you're not really on your toes. And I didn't send this guy down there with instructions to look at N-rays with the expectation that he was going to find anything or there was anything wrong. I just wanted the guy to have the experience. He hadn't been with us very long. He'd taught physics in high school for a few years after he got out of college. I just wanted him to have the experience of looking at a bunch of N-rays to see what they looked like and see what the hardware looked like when you looked at it through neutron radiographs.

We delayed the flight—big, big problem—until we got some new hardware made, and what happened is, when they were drawing it, they had the capstans too tight and they'd stretched it too much, rather than rolling it down over the—the way they make that stuff, they put that explosive in a big block, and then they squeeze it down, make a tube out of it, and then they run it through this machine that draws it out a little bit longer, and then they get it long enough where they can run it back and forth on capstan, and each time they just draw it out, roll it out a little bit thinner and thinner and thinner so you wind up with something that's the size of a pencil lead of explosive running down through the center of it with a little silver around the outside of it, or lead, depending on which ones you're working. But that's a good example there of one that—

I'll tell you, while I'm talking about those kind of problems, the one that really haunted me, and I still shudder when I think about it, is the problem we had on the titanium propellant tanks. You know, we had three thanks that were supposed to go through a thirty-day test pressurized in the propellants that we were using. Twenty-eight days into that thirty-day test, one of those tanks developed just a tiny little leak. It was just bubbling out a little bit through a crack in that tank. Well, they cut that out, and they sent it off to one of these high-powered testing laboratories.

It came back with a report that said it was caused by a fingerprint on the inside of the tank, and the salt from the fingerprint caused intergranular corrosion that worked its way through the grains of the tank. Well, that looked like it was okay to me, and it was a very nice lab that did their work. I signed off on it, shipped it back.

Well, I had two guys working for me, Mr. [Darrell] Kendrick and Mr. Ackerman [phonetic], and I sent those two guys up to Bell to do something, and they came back and told me, says, "Henry, we can't let them get away with that. That was not caused from a fingerprint."

"Why not?"

"It would take a monkey to get his hand in there at the point where that hole developed." You've got this little hole about that big around, the tank's that big around, you've got to stick your arm around it and reach down inside of there. Well, there was logic they used.

I looked at it, and it's got a weld right around the middle of it. I says, "Oh, Jim, somebody was getting ready to weld it, they just saw a speck in there, reached in there with their finger and picked it off and welded it."

It wasn't long, they came back in the office. With all the fluids they put in there, I'm surprised it didn't dissolve the whole tank, much less dissolve any fingerprint before it went into heat treatment. So that was ruled out. I was still inclined just to let it slip. I mean, you know, it didn't appear to be that big a problem. Every morning, without fail, Mr. Ackerman was in my office, "Henry, we've got to do something. Henry, we've got to do something."

I finally told him, I says, "Okay, Jim. Give me a proposal. Tell me what we ought to do."

Well, they came back and told me that they thought we ought to put ten tanks in test, in a thirty-day test, and if those ten tanks went through that test, then we would write it off as a random failure and it'd be okay.

"Well, that's going to cost a lot of money." I went out to try to get North American to agree to it. They wouldn't. Said to come back and go to Dr. Shea, the program manager, and convince him that we need to spend the extra money for those ten tanks and put them in test, and we did.

You know, I think about seventy-two hours into test, one of those tanks exploded. It just busted wide open. Before we could get the pressure down on that bank of ten tests, two more of them blew up. That was three out of the ten, and they did it really, really quick. That took a long time and a lot of effort. We had every research lab in the United States involved in that. We had Langley and Lewis and Huntsville and JSC, Rockwell, Bell, everybody working on it. And come to find out that what happened was the Air Force changed the manufacturing process on the propellant because some of the rocket people were fussing because sometimes N-204 was brown and sometimes it was green, and sometimes it was very light-colored, and sometimes it was very dark. Well, what happened is it gets a little water in it, and it changes color.

Well, the manufacturer that's producing the N-204 found out if you bubble oxygen through it for a long time, you can fix it where it's always the same color and it's always good stuff. Unfortunately, what that did was it freed up hydrogen. So now, instead of having water in the propellant, you had hydrogen, free hydrogen, in the oxidizer. Now, hydrogen causes many, many problems with titanium. It'll cause titanium just to change to dust in no time, and that's what was happening. When they changed the manufacturing process of it, that hydrogen, then, was causing intergranular corrosion through the titanium tanks, and that would have been the last time we would have had any of that hardware exposed to long-duration testing until the first Apollo flight, the fourteen-day Apollo flight, and it would have caught us on that program. It may have caught us on the Gemini Program because by the end of the Gemini Program they would have been using the newly manufactured propellant.

But it's those kind of things that causes a person to lose a lot of sleep at night. I mean, how close we came and how close I came to letting that one go, and had it not been for the persistence of two very young guys that were dedicated to a cause of not letting something slip through, we would have missed it. I would have missed it.

BERGEN: It shows how valuable everybody who participated was. Were there any other incidents of testing that stand out in your mind?

POHL: Well, you know, that's probably one I need to go off and think about a little bit. There was tons and tons of them, but they don't always come to mind right at the time. I told you about the goldenrod pollen.

I'll tell you what. I will think about that some, and I might come back out here sometime and we'll talk about it, because that's probably pretty good.

BERGEN: That would be great. If it's okay, why don't we talk about some of the missions, the Apollo missions.

POHL: Okay. That's been a long time ago.

BERGEN: First, I'd like to ask how the Apollo fire affected the people that you worked with.

POHL: I'll tell you, I, for one, felt very, very bad about that fire, because there was another one of those instances where a light bulb lit up and I didn't follow up on it. I was out at Downey [the North American manufacturing plant in Downey, California] one time when you crawled in that command module just to look in there. I had absolutely no responsibility in that area at all, but I says, "John, you can't put all of that stuff in there. It'll burn." You know, they had come up with this Velcro-like stuff so people could stand up and not stick, and they had it full of what I thought was combustible stuff. Having come from the test facilities there in Huntsville, where we had almost nothing in this world that was compatible with pure oxygen—you know, it would burn almost everything. There's a few metals, monealin [phonetic] and some of the nickel steels won't burn. Copper won't hardly burn, but most everything will burn to some extent, and all of those plastic-type materials is very, very bad to burn in a pure oxygen environment.

I even followed up on that and called somebody the next day and talked to them about it, and he says, "Oh, that's a new fireproof material."

POHL: And I didn't check out to see what it was or follow up with it, follow through with it, and it turns out in 100 percent oxygen environment it was not, but nobody really tested it. There again, they were thinking about a different environment, a different set of things, and let that fall through. So, you know, I had the opportunity to at least maybe not get it changed, but I had the opportunity to at least make an issue of it before that happened. Of course, you've got to have a fire, you've got to have a spark, you've got to have something to start it. It's just one of those things.

It did, on the other hand—you kind of hate to say it, but it did give us the breather that we needed to fix an awful lot of problems in the program that, as bad as that was, may have been worse than that, because if we started on a journey to the Moon and had lost a crew and not been able to have the evidence back home to figure out what went wrong or how it went wrong, it would have probably been a real big set-back to the program. At least this one, you had the hardware there, you had the evidence there, you could see it through and figure out exactly what went wrong, and just buying that time allowed you to go back and look at all of the other areas in the program and just go through it and reassess everything to see if there's anything else like that that was lurking in the woods.

So, from our standpoint, in the Propulsion Power Division, it bought us the time on the agents and some of the other things. We kind of redesigned the whole service module propulsion system to fix some of the deficiencies in it that made it a much better piece of hardware.

BERGEN: So when Apollo 7 finally came around and launched and was successful, how did your engines fare?

POHL: Good. Good. As a matter of fact, after Apollo 11, I was at one of those big balls downtown, or celebrations they had in one of those big hotels down there, and Joe Shea was all the way across the room, saw me, come walking over to me and says, "Henry, those RCS engines worked, every durn one of them." And you can thank Jim Ackerman for that. That was a good feeling, and they did. We didn't have any failures on any of those.

We had a problem on Skylab. Like I said, we had these redundant seals in there, and on those long Skylab missions, that rubber seal would deteriorate, and two of them developed leaks, and then we were able to correct that for the next flight. But the RCS worked real good. Matter of fact, all the propulsion systems worked very good.

We had that one real bad mishap on the oxygen tanks on Apollo 13, and that one, too, was caused by not a complete understanding of the system and the hardware and the way that we were using it. What happened down there on that one, as I recall, is they had a delay in the flight, and so they needed to empty the tanks. So instead of using twenty-eight volts on the heaters in there to heat the—the only way you can get the propellants out is supercritical, is heat them up and blow them out as gas. So you need to put a lot of heat in the tanks. Well, they've got those heaters in there, and they're twenty-eight-volt heaters. Well, they're in liquid oxygen or liquid hydrogen so they're very, very cold. They're not going to get hot until you get all of this stuff out.

So they put them on 120 volts. Well, that's ten times more heat going in there, and you get them emptied a lot quicker doing that, and they did that. Unfortunately, they had a safety switch in there that was designed for 28 volts, and when they put the 120 volts in there, it welded that switch together. Now, when you get in the absence of gravity and you turn those heaters on, the first thing that happens, a big bubble forms around the heater. So you've got this switch in there that disengages the heater and then turns it back on and turns it off and turns it back on. Unfortunately, that switch is welded together. So the heater just stayed on and got hotter and hotter and hotter and finally started melting some of the insulation and started a fire inside the tank and burned a hole through the tank, so you lost a tank.

Well, we got three of those on there, so we're still okay, except the way they did that thing, they manifolded all of them together with check valves, and the check valves had been sitting there chattering for three or four days and the seats had all eat out of the check valves. So when that tank developed a hole in it, all of the oxygen from the other tanks just ran out through that tank. Bad design. Somebody ought to have caught it, but nobody did, or ought to have made sure that all that hardware would have taken the 120 volts, number one. Number two, if they would have checked those check valves after running a realistic duty cycle in a realistic set-up for a long period of time, they would have probably picked that up, but they didn't run the right qual [qualification] program on it, didn't have the right set-up for the qual program, and didn't catch it.

Fortunately for us, though, that mission turned out okay. Again, the only reason we made it back home is that we had thoroughly, thoroughly tested that equipment. We knew the limits on all of that equipment, and we knew the thermal limits, we knew when it quit working, how far outside of the spec you could go before it would quit, we knew what kind of duty cycles we could run on things.

For example, I did all the calculations on the temperatures on the RCS on the command module myself because people wouldn't give. You know, everybody wants to protect his thing and let somebody else take all of the risk. Well, in a situation like that, on the trajectory we elected to come back with, it was about twice as long as we had battery power to live on. So we had to cut our energy consumption in half in order to make it back home. Well, to do that, you've got to turn every heater off that you don't absolutely have to have, and you have to let the temperature limits go to just as low as you can let them go.

Well, I calculated when we could throw the service module off and how cold the RCS-ers are going to get, and I gave myself four degrees above freezing on it. I missed it two degrees. It got two degrees colder than I thought it was going to get. So we cut those margins pretty dad-gum close, but we had electrical power when we came back home, and we had oxygen when we came back.

You know, they lost everything. The lunar computers were down. Nothing was up and running, and they had to point to the stars and get fixes on the stars. Well, they had the service command module hanging on that lunar module so the CG [Center of Gravity] was way back here. When the computer started, it was up here so it didn't maneuver right. It would twist around every kind of way. So they wouldn't get very good fixes on the stars. They'd get close to it and punch it in. Then we decided we had to make a burn real quick because we were going out into space and we needed to kick the thing up so we'd get on a free return back so that the gravitational pull of the Moon would pull it back to Earth.

And that really, at that time, when we made that decision, didn't look like it was a very good option to bring the crew back alive, but it looked like it was a very good option to bring the crew back. You know, at least if they didn't make it alive, at least they wouldn't go off and be lost in space forever. Plus it gave us a whole bunch of things now that we could work on to try to start correcting, but we didn't know whether we were going to the Moon or going into space. We didn't know if we had it pointed in the right direction when we made the burn on that engine or not. We were going to have to track it for a while. Well, they called up and told us, "Well, I've got to go to sleep." He did. He crawled in that tunnel and went to sleep. He didn't know whether he was going to the Moon or where he was going, but that was one time when I realized the discipline that the crew had to do what needed to be done at the time that they needed to do it.

BERGEN: That was an amazing time, amazing accomplishment getting them back.

POHL: We were very, very lucky, too. I never did think too highly of them spending all the money they were spending on those big computers out here. I thought we were spending way too much money on those things at the expense of other things that was more critical than those were, and we were really pushing the technology and pushing the development of computers, buying and spending it, buying the very latest and the very best that could be built and then developing the software and hardware for it.

But that is one night when that paid off and paid off big. I'm thinking a little bit from memory now, but I would say within an hour and forty-five minutes after that oxygen tank blew up, we were presented with five potential trajectories, different combinations of things that could be tried, all of which had the potential of bringing the crew back, from hotwiring the service propulsion system and turning the vehicle around and burning that propellant out and turning it around and burning it out at descent stage and then burn it out of the ascent stage, that would have braked it and caused it to come right back to Earth and we would have been home in a day or less, through doing different combinations of things.

The combination we chose, like I said, looked like it had the best chance of bringing the crew back to Earth. We had a lot of things we had to work out the details of, the consumables so that we had enough oxygen, we had enough battery power, and those kind of things, but those were all things that you could work on. Knowing the limits of the hardware, we had a pretty good feel of how much we could give on everything, and we had good test data to back it all up. That's really what saved it, saved the day, was a very, very good test program, a very good development program, a good, thorough understanding of the limits of every component in that vehicle.

BERGEN: Earlier, you mentioned Apollo 11. Can you tell us about your memories of Apollo 11?

POHL: I guess the first thing that I was really concerned about was [Neil A.] Armstrong was dilly-dallying around out there, and he wasn't coming down, and we got run out of propellant. I thought he ought to get that thing on the ground, but he was just as cool as an icicle, and when we had thirteen seconds worth of propellant or something like that left when he touched down, not very much. But once we got down, I had all the confidence in the world that we were going to be able to make it back home.

By that time, I had a lot of confidence in the hardware that I was responsible for. I thought we would lose an RCS engine or two on some of the programs, or some of the flights. I didn't expect them to be all totally error-free. The pyrotechnics I had a great deal of confidence in that hardware all working exactly the way it was supposed to. So, from my standpoint, I thought we were ready to go to the Moon, and I thought it would be successful.

BERGEN: It was.

POHL: And the night when it landed, we had people walking—I came driving down NASA Road 1 here that evening, or my wife did, and we just had to poke along because those streets were just solid people, wall to wall people. You were threading your car down through people just walking across the road everywheres out there. Everybody was celebrating. When I got off work, I went out and went home, and my wife had the car loaded and we put the kids in it and pulled out to the country. So I didn't go to any of the celebrations. I just went to the country and relaxed for a while, because it had been a long, long two weeks, not much sleep. Matter of fact, I lay down in the back of the station wagon and went to sleep.

BERGEN: Most of your work had been done to that point. You kind of had to watch.

POHL: Yes.

BERGEN: Is there anything else from the Apollo era that stands out in your mind, that you have special memories of?

POHL: You know, I think that's another one of those that I need to just go back and reflect on and think about a little bit, and maybe we could touch on it some later date a little bit, because it's been a long time, and a lot of water run under the bridge since those days.

BERGEN: Okay. We're kind of at a good stopping point before we go into Shuttle, but before we end, I'd like to ask Tim and Carol if they have any questions. Do you have any questions, Carol?

BUTLER: I have one question. You were talking about the pyrotechnics briefly there and that you had a lot of confidence in that system. Then looking at Apollo 12, when the rocket launched and it was hit by lightning on the launch and all the rest of the systems were able to be checked out, but the parachutes and the pyrotechnics for releasing the parachutes, there was some concern about that. Were you involved at all in that?

POHL: I didn't have any concern about that. I know a lot of people did. You know, you could kind of worry about damaging the material in there, but that was so remote. But the pyrotechnic circuits that we designed and used for Apollo was as immune to electrostatic discharges as it was humanly possible to make them. We were so concerned about that.

I'll tell you a little story about that. We started out with dual brakes for our initiators, had two circuits going in, Circuit A and Circuit B, and we had a premature firing on one of those things out at White Sands. We were up in Gilruth's office one night talking about the problem, and Mr. Thibodaux was there, and I had a young man by the name of Bob Robinson working for me, very, very sharp young guy. Dr. Gilruth made the suggestion that we take that project over and GFE [Government Furnished Equipment] that hardware and put this young man overseeing it. Bob spoke up and says, "Sir, I cannot accept that assignment." He says, "I was on a ship one time, holding a rocket in my hand when it took off, and I swore then I would never be a party to anything that's sensitive to EMI [Electromagnetic Interference] or electrostatic discharges."

Dr. Gilruth turned to Mr. Thibodaux and says, "GFE it, and put this young man in charge of it."

So he was really sharp on those circuits. I mean, he gave the Russians the fits on ASTP [Apollo-Soyuz Test Project], and you'll probably talk about that a different time. But they were concerned that their radar was going to set off our pyros, and so Bob one time asked them what frequency and power level, and they gave him a number, and Bob came back over and said, "We've got to run some tests." We had a tester out there that would go about half the frequency and half the power level they had quoted, but we cranked that thing up as high as it would go, and we fired three of them off, bang, bang, bang. Then we went back, and Bob told the Russians, "If you're hit with that power level, every one of them will fire, and yours will, too." That came as a big surprise to them. The last time I saw those people, they were still trying to get the formulae that Bob had used to calculate that firing level of those things, and they never did know that we just ran a test on them.

But we made those things as immune as was humanly possible to make them from external disturbances. I don't think that you could have had a skin strike of lightning on that thing that would have wiped out any of those circuits or pyros. There's always steep places where things can get in. You know, there comes a place, someplace, where from the command or the computer you've got to get into some of this protected circuit, but it was just pretty good. Everything was in a Faraday shield.

BUTLER: And then they did work every time.

POHL: About 212 of them on every flight.

BUTLER: I did have one last question. You mentioned that on Apollo 11, you wanted Neil to put it down, land it, but where were you at the time? Were you in the Control Center?

POHL: I was in Building 45 in the MER [Mission Evaluation Room]. You know, Engineering had their own little room over in Building 45 over there, and we called it the Mission Evaluation Room. That's where most of the engineering took place, and things fed back and forth. I was in there. I was in there for nearly all of the Apollo flights, pulled a shift in there.

BUTLER: Thank you.

BERGEN: Tim, no questions? Is there anything you'd like to say in conclusion?

POHL: Well, I guess the one thing that I could say is that in the entire history of humankind, there have been very, very few people in this world that had the opportunity to work on a project such as the Apollo Program and to have as much fun in their career doing the kinds of things that they wanted to do as I have.

BERGEN: That's wonderful. It was great to be able to say that at the end of your career, to say that you enjoyed it.

POHL: I wouldn't want to do it over, because I'd just screw it up worse the next time around.

BERGEN: We thank you for coming and sharing with us this morning.

[End of Interview]