ORAL HISTORY TRANSCRIPT

CHESTER A. VAUGHAN
INTERVIEWED BY SUMMER CHICK BERGEN
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BERGEN: Today is December 16th, 1998. This oral history interview with Chester Vaughan is being conducted for the Johnson Space Center Oral History Project at the offices of the Signal Corporation in Houston, Texas. The interviewer is Summer Chick Bergen, assisted by Kevin Rusnak.

Thank you for coming and sharing your oral history with us.

VAUGHAN: You're very welcome. I'm pleased to be asked to do it.

BERGEN: Let's start back before you even went to college. Did you have any interest in airplanes or aeronautics prior to going to college?

VAUGHAN: Well, you know, that was a long time ago, number one. Number two, the airplanes at that time, back in the early fifties, late forties, early fifties, were not as popular as they are today. I lived out in the country and I worked on a farm, and occasionally you'd see some airplanes go by, and, yes, I was interested and thinking that some day I'd like to be able to fly in one of those, maybe pilot it. So, yes, even before college, you'd see those flying by and I was interested.

BERGEN: Then you went to Virginia Tech and majored in mechanical engineering?

VAUGHAN: That is correct. While I was there, actually before then, and I'll just do this for you, before I decided to go to VPI [Virginia Polytechnic Institute & State University, popularly known as Virginia Tech], I almost joined the Air Force. At the time I wanted to fly, so I went over to take the physical, and would have probably shipped out to San Antone [San Antonio, Texas] that evening. That's what they wanted us to do, if you passed the physical. What I really wanted to do was get into OCS [Officer Candidate School], so I could try to be a pilot. It turns out I have very flat feet and they disqualified me for that. "Hey," they said, "we'll take you as an airman and you can go tonight." I declined to do that. That was probably in June or July. And I wound up going to college in the fall. So I didn't do a lot of planning on the college on the front end, but it worked out.

BERGEN: While you were in college you began a co-op [Cooperative Education Program] at Langley [Research Center].

VAUGHAN: That is correct. One of the reasons for going to VPI, two reasons, really. One, is it's a state school from where I was born, so tuition was lower. The second was that they did have the Cooperative Engineering Program, which allows you to work some and help earn your way through school, and I needed to do that. So they were the two primary reasons for going to VPI. I was living, from a home standpoint, in Virginia, but much closer to N.C. [North Carolina] State, but I'd have to pay out-of-state tuition.

BERGEN: Did you have a choice of where you got to do your co-op?

VAUGHAN: A little bit, but the school was also very active in helping point you in the right directions. I was in aeronautical engineering, and NASA Langley—at that time NACA [National Advisory Committee for Aeronautics] Langley—was one of the prime spots that they liked to send the aeronautical students to. So I wound up going to NASA Langley, NACA Langley.

BERGEN: What was your co-op experience like there?

VAUGHAN: It was great. What happened at the Langley Center, at least in those days, is that they bring the co-op students in and they would rotate us through all of the different disciplines at the Center. So I started off and my first co-op work period was in a sixteen-foot transonic wind tunnel, and so I worked in that. They did all the testing at night, but what they needed was someone to help them with the data reduction.

In those days we didn't have those big computers and good ways of getting the data that we have today. So what we would do is take all the data on oscillograph draft paper that had to go to the photo lab to get developed. My job for that first work period was to pick up all of those rolls of paper and get them to the photo lab and leave them to get them developed and pick up the ones that they had done the day before.

What would happen on those, there was just like thirty-two lines of data, analog data on those, and a big round roll of paper, you know. What would happen is, the parameters would vary. The identifier on each of the parameters had a little dash in it and you'd have to go down about every two foot on the roll and see if it had crossed lines. What you do is measure the difference in deflection or the difference in the position of the line on the paper.

So the other part of my job during that, a lot of the job during that work period was to simply go down, and about every two feet down the roll of paper, label the lines, one through thirty-two. It turns out those lines, you could just do them by numbers, but the engineers that were working there were good enough to tell me what the parameters were. So it meant a lot more than just numbering them one through thirty-two.

The reason we did it that way is then they would take this data and take it over to an IBM card-reader and measure the deflection that the parameter had had, and it puts it into an IBM card. That's how we got the digital data. Then they could process it.

So I did that for about three months, two and a half months. But I learned a lot, knowing what parameters we were measuring, and then as I understood what the drag coefficients were and so forth, then you could understand what was happening with the models. So it was a good learning experience, even though it could have just been a routine dirty little job. [Laughter] But one somebody had to do.

BERGEN: Did you have any other co-op experiences that stick with you?

VAUGHAN: I had the opportunity to work in the Rocket Section. At Langley at that time—this was in 1958—Langley had a small group of people who were in a facility on site where they would actually—it was primarily in solid propellants, where we would actually take the ammonium perchlorate and a rubber-based fuel, and we would actually make the propellant, cast small rocket engines, and then take them out and fire them. So I had an opportunity to handle the propellants, and they were explosive.

One of the things I remember is that you had to wear the wrist straps and that sort of thing to prevent static electricity. Every once in a while we would have one that would, in fact, blow up, and, you know, you'd go pick up all the pieces and try to understand what happened. Fortunately, we didn't hurt anybody, at least at the times I was there. You just had to be careful, because you are dealing with a lot of energy.

But that was good training for me for later and kind of helped me to get to where I was going and eventually wound up.

BERGEN: Were there any individuals that you worked with as a co-op that made a big impression on you?

VAUGHAN: Quite a few. The co-op coordinator, a fellow by the name of Mr. Cole [phonetic], that was at NACA at the time, really took us under his wings, not just me, but all the co-op students, and they took good care of us. It was good, because you were away from home, working in a different environment, and it was awfully good to have someone who would be watching out for us. Those guys would even, if you knew what courses you were going to take next quarter or semester, they'd help you order the books and those kinds of things, just to make life a little bit easier. So he's number one.

Then there's a gentleman you guys may have interviewed, a guy by the name of Mr. Guy [Joseph G.] Thibodaux.

BERGEN: He's on our list, but we haven't talked to him yet.

VAUGHAN: Guy was the section head for the Rocket Section when I worked for him in 1958. Then later, Guy came to Houston and was head of Propulsive Power Division down here. I worked for Guy after then for another twenty, thirty years, so, a long time. But both those men.

BERGEN: Wonderful. At the end of your co-op term and the beginning of your full-time employment, NACA was making a transition to NASA. Do you remember how that impacted you or what the people that you worked with thought about that?

VAUGHAN: Very much so, and, in fact, one of the things I did when I graduated, I graduated from the co-op group in—it must have been 1959. But it was in '57 is when the transition was going on and I was working as a co-op during those times. So, yes, I'm quite familiar with a lot of things that were going on at that time. The place I eventually wound up going to work full-time when I graduated was to a Space Vehicle Group, and Langley was doing mostly aeronautics. Space was just coming into the picture, and I went to a group that was called Space Vehicles Group. It was in the same division as the Rocket Section I'd worked in earlier, but they were specifically working on space vehicles.

A guy by the name of Mr. [William J.] O'Sullivan, who died a bunch of years ago at this point, was heading that group. That group was responsible for putting up the small very lightweight balloons or spheres. Initially they were five-, ten-, twelve-feet diameter and measuring the atmosphere, looking at the orbital decay, and therefore being able to measure the atmosphere density.

That's the same group that eventually put up the hundred-foot Echo balloon. I don't know if you guys have run into that or not. But at that point we didn't have the electronics that we had later. I think it was, we'd use large structures in space, large lightweight structures, to reflect radio beams from and then back to Earth. That was how we were envisioning that we would do the communications. As it turns out, you know, the electronics world caught up and surpassed and we put transponders and that sort of thing. So we do it in an active mode rather than a passive mode. But we did a lot of work in the early days on large lightweight structures for that purpose.

BERGEN: Was it your choice to go into the Space Vehicles Group, or is that just where you ended up?

VAUGHAN: A little bit of both. They needed people in that group. I had expressed the desire to go there, and Mr. Cole, as I mentioned earlier, tried to be sure his co-ops, when they graduated, were, number one, given a job offer, and, number two, had some say in where they went. Now, they didn't absolute say, but if you wanted to go an area that also needed that talent, they would help you get there. So I managed to do that.

BERGEN: What did you do when you first went into that group?

VAUGHAN: At that time, there was a group of guys that were working on control systems and on dynamics, and I was assigned to a more senior engineer at that time that was working on a de-spin device. Basically, the best way to describe it would probably be a little yo-yo system with two yo-yos. It sounds simple now, but basically taking weights on the ends of cables and using those weights in a yo-yo fashion, one on each side, or ninety degrees apart. By balancing the momentum, you could take—and most of our spacecraft at that time were spin-stabilized during the way up. So, during the boost phase they were spin-stabilized. But most of them, once they got on orbit, you'd like to have them de-spin. In other words, so that the attitude would be constant and would not be rotating.

We were looking for lightweight, simple ways to do that, and we developed a technique called a de-spin device. It's simply small weights on the end of fairly long cables, and after you get into orbit with the spacecraft you release the weights, let them unwind, and at the precise moment when the rotational velocity was zero, you release them. So the weights go spinning—not spinning, but flying away, and you wind up with a spacecraft that's essentially very stable.

Normally the control systems that they had on board at that time were coal gas systems, and it didn't take much coal gas to finish up trimming them out and being sure that they were absolutely stable and pointing in the direction you wanted them to do.

I wound up doing a research paper on that and used it to help get my first promotion. That's the other thing that you had to do at that time, is that you had to have papers with the Langley group. You had to go before a group of your peers and present that paper, and they would decide if you would be promoted or not.

We did work that technique out, put out some papers on it. Later on, that technique was used in a lot of the unmanned satellites and Goddard picked it up. In fact, I spent a little time at Goddard working with the guys up there after I graduated, even after I was in Houston, being sure that they understood what we knew about that. They did apply the techniques to quite a few spacecraft.

BERGEN: When you were in the Space Vehicle Group, at the same time the Space Task Group was formed, did they work, interact any, or were they separate entities?

VAUGHAN: They were separate entities, but we knew a lot of the people over there. Dr. [Robert R.] Gilruth had been division chief of the division the Space Vehicle Group was in, so there's a tie, number one. The Rocket Section was also in Dr. Gilruth's division at that time. It was called the PARD, or Powerless Aircraft Research Division. So from the organization standpoint, we had a lot of people who knew the people who were in Space Task Group. In fact, a lot of the discussion was, hey, who's going to go? Who's going to be a part of that? Who's going to stay?

Earlier on, I elected to stay for a while, and then as the Space Task Group grew and as the Mercury Program came along, I recognized that I'd like to be where a little bit faster action was happening. I transferred over to the Space Task Group, I think in October of '62, thereabouts. Stayed with it till about 1996, the equivalent.

BERGEN: How did you feel about the move of the Space Task Group from Virginia to Houston?

VAUGHAN: Well, I was still single at the time. It didn't bother me a bit. It was kind of exciting. Before the announcement was made, there were like three different sites: one in Florida, one in Houston and in California. Of course, the politics and the rest brought us to

Houston. But it's good, and I did move down and have enjoyed it, and glad I made that decision.

BERGEN: So once you moved to the Space Task Group, what were your first responsibilities and what groups were you in?

VAUGHAN: I was in a group called Energy Systems Branch. At that time the Mercury hadn't flown yet, but a lot of the work had already been done on Mercury and we were starting to concentrate on Gemini and Apollo. Some of the first jobs I did after getting into the Space Task Group was on Apollo and helping size in terms of weight and mass and thrust profile. They launched an escape system that we wound up using on the Apollo spacecraft, which is a solid propellant that sits on top of the capsule and it gives a signal if you've got a booster problem. It would yank the crew off along with the capsule and get them out of harm's way.

So I did some early work trying to understand what thrust time profile you need just to get the velocity and distance, then taking that profile, going back to some solid rocket designs that would provide that kind of profile. So I did a fair amount of work in that arena initially.

I also worked a fair amount in what we call pyrotechnics. That's the little initiative devices that allows you to start the solid rocket motors and other things.

BERGEN: Going back to the launch escape system, were you involved in any way in the decision on whether to use the escape rocket versus the ejection seats which were used in Gemini?

VAUGHAN: Not directly. What we were trying to do in Gemini, as I remember it, they were trying to get to more of the state-of-the-art things. If you'll remember, you wouldn't remember, but the situation at that time was that we had a lot of ejection seats. So that was a way, it looked like you could save some money and time in providing that kind of capability, and still provide the capability for the crew to get away and be okay.

In retrospect, different designers doing different things. You notice we had a tower on Mercury. We did not have one on Gemini and we had one on Apollo. Shuttle comes along and doesn't have any of those. Well, that's not true; it had ejection seats on the initial Shuttle vehicles. Different ways to skin a cat and all of them are okay, I suppose.

The escape tower provides a little more positive way and a little more personal protection for the crew. Fortunately, in all those programs we never had to use it. I believe they would have worked, and we proved they would have worked, but we did not have to use them, and that's great.

BERGEN: You were talking about working on the pyrotechnics. Can you explain a little bit more about what you did in that area?

VAUGHAN: Well, in the pyrotechnics world, at least early on, there were a lot of people who built little matches or squibs or what have you, and a lot of them were very sensitive, because they had to be. We didn't have enough energy in those, and so what happened was that they were very sensitive. Things like static electricity, if you weren't careful, could cause enough energy to get into the device and it would, in fact, set it off, which then would get a chain reaction going.

I remember we had an accidental rocket burning in one of the buildings at the Cape [Cape Canaveral, Florida], based on static electricity. I can't remember if it was in the VAB [Vertical Assembly Building] or not, but one of those. So what I was doing when I got involved with the initiators, is trying to come up with some systems that would be less sensitive and still do the job, so that they would not have the issues associated with early ignition.

See, one thing about the pyros is that they're just as bad if they fire early as if you didn't fire them at all, because you can get into trouble either way. You need to be very sure that they're going to react exactly when you want them to.

An example I could give you, for instance, is on the Space Shuttle today, we used pyrotechnic devices to separate, say, the orbiter from the external tank. We use them to separate the solid rocket boosters from the external tank. Number one, you don't want them turning loose too early, you don't want them turning loose too late, or hanging on. So it's very important.

Early on, we had difficulties with the sensitivity of those devices. We wound up using a system, both in Mercury and Gemini, that did have it, enough sensitivity and not being too sensitive, that they worked okay for us. We had a lot more applications in Apollo, and what we tried to do, in fact, what we did do, was standardize the use of those.

In Gemini we had, basically, whoever provided the function, say, if it was the solid rocket motor or so forth, they also provided the initiator and so forth. It turns out that you wound up with a lot of different people spending a lot of money and having different ways of checking them out and so forth. What we wanted to do was get to a standard. We did develop what's called today, they call it a—it may even still be called an Apollo Standard Initiator. No, it's called a NASA Standard Initiator now. At that time, we called it an Apollo Standard Initiator.

We developed that and wound up standardizing an all Apollo and we've used it as a NASA standard, basically, since the mid-sixties. Still using it today. NASA still has those made. They've got three or four vendors that can make them to print and supply them to all NASA programs. That way you have the same interface between the electrical system and your chemical energy system that you're trying to get started. It saves a lot of time and trouble, and it improves safety dramatically, and that's what we're trying to do.

BERGEN: Great. It looks like you, from what I can tell, you worked on Gemini and Apollo almost at the same time.

VAUGHAN: That is true. If you'll remember the—you won't remember, and I'll quit saying that in a minute, maybe. [Laughter] If you'll look back on the history, you'll see that the Gemini Program and the Apollo Program were both started at the end of the Mercury, or before the end of the Mercury Program. The Gemini Program was primarily sold as an extension to Mercury so that we could learn more, get more experience both with the humans and with the rendezvous capabilities. We didn't have that capability at all with the Apollo. So it was kind of sold as an upgrade to the Mercury.

In fact, McDonnell-Douglas in St. Louis—well, it wasn't McDonnell-Douglas at the time, it was McDonnell in St. Louis, used the basic same concepts in a lot of ways that they used on Mercury. So the distinct differences is in the escape system, but an awful lot of the rest of it were very similar. A major difference is in the propulsion area for the spacecraft propulsion. On Mercury we used what we call a mono propellant system that was a hydrogen peroxide system. On Gemini we went to the propellants that we're using today on the Space Shuttle for the on-orbit propulsion and that's the hypergolic propellants. Yes, we started them both at the same time and worked them at the same time.

The other thing that's important to note is that none of them lasted forever. The Gemini Program, it was probably started in about '62 and was over in '66, thereabouts. I don't remember the exact dates. So three, four, five years and we were finished with the whole program. Of course, Apollo started in about the same time. We didn't finish it until the, what, the mid-seventies, or thereabouts, but we had the first landing obviously in '69.

BERGEN: Talking about Gemini, you worked on the solid rocket motors for the Gemini land landing retro rocket system, is that right?

VAUGHAN: I did. You must have saw that in my résumé. Well, what we were trying to do again was to provide more flexibility for the Gemini spacecraft. Obviously, landing in water makes you have to get out the naval fleet, and with the different abort modes and so forth, it was difficult and expensive to do that. Dr. Max [Maxime A.] Faget, who was probably on your list, if you haven't interviewed him already, was looking at—well, let me back up.

Two things happened. One, early in the Gemini Program they had a “Rogallo wing” [named for Francis M. Rogallo, a Langley engineer who developed the initial design], which was an aerodynamic device almost like a parachute. It was not a parachute, but served a similar function. It would be packaged in a small volume, deployed, and then allow the Gemini spacecraft to land on land with some capability to steer it and to land it softly. Part of the issue with that is that the impact velocities were too high and what we looked at is some ways to soften it. One of the ways to do it is the same way the Russians do theirs today, and have done it for many years, is use a retro rocket to slow the velocity down to near zero just as it touches down.

Under Dr. Faget's direction and leadership, and Mr. Thibodaux's, we designed a solid rocket motor that would do that. We ran the analysis to see what thrust time profile you would need to do that job, and then we designed the grain, a solid rocket motor grain that would provide that profile. In fact, we then put that under contract to the Thiokol Company. It was in Maryland at that time.

For the flight weight design, they had some pretty hard-to-get materials from a casing standpoint. In other words, the container around it. And because we were in a hurry and we wanted to do some development work, another guy and myself designed an alternate case made out of less exotic material. Went down to one of the industries on the ship channel, and they build those for us and we shipped them to Thiokol and they loaded them with propellant and we used those early on for the development testing. So, yes, we did work that a little bit.

BERGEN: It seems astronauts weren't necessarily enchanted with the idea of using retro rockets. Did you ever get that impression?

VAUGHAN: Not directly, but retro rockets have some issues and part of the problems that you have is detecting when you should ignite them. If you ignite them too early, and you wind up reducing the velocity to near zero, but you're still ten feet off the ground, and then you free fall the next ten feet, you can impact and do a lot of damage. So, you know, the timing was very sensitive.

We were also having troubles with the Rogallo wing and we'd spent a lot of money on that, but trying to get it to deploy, package it in a small enough package that we had to have it in for it to fit in the spacecraft, get it deployed and have it operate reliably at that time was also very difficult. I think that probably had more to do with doing away with the land landing and going back to the water landing, more than the retro rocket aspect of it. But it was a combination of things. And time. You know, we just didn't have time at that time to work all those issues and get them flight-worthy and still do it in less than three years.

BERGEN: Did you actually do some testing with your retro rockets?

VAUGHAN: Oh, yes, we did. In fact, we did some static firing tests on the runway at Ellington [Field]. Later on we did some in what was called later the thermochemical test area, which is located on site. So that was doing a component test, just the rocket motor by itself. Then we also put those rocket motors in some Gemini spacecraft. Not real ones, but not mock-ups, even, but they were test articles representing what the Gemini would look like and what it would weight, and actually dumped them out with parachutes and so forth.

Now, another way to do that job and still land on land instead of using the Rogallo wing is to just use a normal parachute and take your licks on a little bit of where you land. I mean, they didn't have the steering capability that we have today. In fact, today the X-38, the planning for the X-38 Program—and I don't know if you guys are familiar with that or not—but it's using a steerable chute which allows it to come down and land on land. It's reducing the velocity enough with the chute, making the chute big enough so that the touch-down velocity on the ground would be low enough that it would not cause damage. They're also using an air bag, I think, to help them with the landing. I'm not certain about that, but I believe they are.

BERGEN: You were on a Gemini Ad Hoc Pyrotechnic Committee. How did you get involved with that?

VAUGHAN: Well, I told you earlier that I had been working the initiators, the pyrotechnic devices. It really was not just the Gemini. I don't remember how the words were stated, but what we were doing was trying to do technical work for both Gemini and Apollo. I'm sure it was called Gemini at the time, because that was more pressing. What we were trying to do was to be sure that we had devices that were non-sensitive enough to be okay and to try to get them standardized.

You may not realize, but if you use things like radar and point the radar toward a pyro device, the electrical wiring on the pyro device—so what you're basically doing with a pyro device in the first place is taking electrical energy, some current and amperage through a wire, heat up the wire, and have that hot wire then initiate the chemical reaction. But what can happen when you take things like RF and expose these devices to radio frequency energy, the electrical wiring can act like an antenna and actually pull in some current and have it flow through these devices.

At that time, early on, they were sensitive enough such that some of them would occasionally go off. What we were trying to do is to be sure we didn't have any that were that sensitive. They were not all standard on Gemini. We didn't get to that until we got to Apollo. So what we were doing is working through and trying to be sure that we were doing the right testing and to be sure the sensitivity of those tests and the devices we were using were okay.

You have to remember we were using radar at the time to track. So we did have the radars focusing on the vehicle, so we had to be sensitive. And some of the ships have some pretty high-level radar as well, and some of them were getting fairly close. You wouldn't want them to get radiated even after they've landed and have pyro devices going off.

BERGEN: True.

VAUGHAN: But that work eventually led to the Apollo standard initiator also.

BERGEN: You did a great deal of work on reaction control systems. How did you get involved with that?

VAUGHAN: Well, it turns out as the Space Task Group grew and then the Manned Spacecraft Center grew, and growing into both the Gemini and Apollo roles, what we were trying to do is use common disciplines. It turns out that Mr. Thibodaux's organization grew. We had both the pyro devices, as well as the small liquid in the same group. I eventually wound up heading what was called Reaction Control Section, became section head of that. The Pyro Group was next door to me, so I continued working some, but not exclusively in that group.

As the bi-propellants, as we were trying to get those on line, we also had a lot of difficulties with those from a design standpoint and development standpoint. I wound up being selected as section head for the Reaction Control Section, probably in, what, '64 that things says. '64 or '65, somewhere in that area. So from that point on, even though I still worked with the Pyro Group some, that was not my primary area. So I worked for the next twenty years or so probably with reaction control stuff, small rocket engines to maneuver in space with.

BERGEN: What kind of experiences in your background helped you in working with your reaction control system?

VAUGHAN: Well, the combustion aspects of solids versus liquids. I mean, you got to get the fuel and the oxidizer together to get the combustion system going. Once you get it going, a lot of what happens, including the heat transfer and how it behaves inside the rocket engine and how efficient it is, there's a lot of similarities between those. So some of that was a natural follow-on.

The other part of that was, we didn't have anybody in the country that had any real experience with hypergols at that time, very limited. Like I say, the Mercury had used a single propellant, the peroxide.

At the time, the Air Force was trying to do some work on Dyna-Soar. I don't know if you guys have run into the Dyna-Soar Program. They were trying to use the bi-props as well, so the country was trying to develop the bi-props, but it was pretty new and we had to learn a lot about it, and we did.

At that time, we were burning up—I mean, you'd build a small thruster or a small rocket engine, and most of them were pretty simple. They were what we called a pressure-fed system. In other words, just using pressure to push the liquids through some valving into a combustion chamber and they are hypergolic, the ones we were using. The reason we wanted to use hypergolics, we knew we'd have to have a lot of starts and stops, and that's why we were using liquids rather than solids. I mean, if you're trying to start and stop a lot, the solids, once you started, you don't have any way to shut it down.

In fact, I could digress a little bit. The solids people at that time even wanted to try to have one that would start and stop. So what they'd do is almost like little cartridges of solids and then a chamber that they'd rotate them through, again, trying to get control of the spacecraft in that mode. Unfortunately, he would depend on how many pellets you had and you just didn't have the flexibility you have with the liquid.

But at that time we had thrusters made out of materials and injectors that were putting it in there. You'd have combustion instability. We'd have hot spots in the engines. You'd wind up burning a hole through the engine or the injector, and we were doing a fair amount of that. It was taking a lot of work in a lot of different places in the country to solve those kinds of problems. New stuff. No one had done it before.

BERGEN: You worked on the RCS [Reaction Control System] for Gemini and Apollo.

VAUGHAN: Yes.

BERGEN: What were some of the differences, or something you learned from Gemini that you were able to apply to Apollo?

VAUGHAN: A lot. As I said earlier, we were doing them generally at the same time, although Gemini had to fly first. Gemini propulsion and the way the Gemini Program was managed was a little bit different in the propulsion area than most of the rest. Because a lot of the Center was involved with the Apollo Program and it was a bigger program, and because

Gemini was expected to just be an upgrade of Mercury, the program office, in most cases, was taking care of all the disciplines in Gemini as opposed to using Engineering Directorate at the Center at the time.

One of the things, because we were having so many different difficulties with propulsion is that that whole discipline got pulled over and applied to Mr. Thibodaux's division and it came down to my section to provide the technical support for Gemini and Apollo. We at one point were trying to use this same thruster for the Gemini system as we were for the command module. So that's the other tie that got them together. We were trying to make them common thrusters, again to save money and to use the best that we had for both programs. It turns out we used the same concept, but they were not identical. Again, we were able to make additional improvements in the Apollo one that we didn't have time to make in Gemini, although they were not safety issues, you know, we had to fly, I mean, as long as we were safe.

BERGEN: What were some of those improvements you made for Apollo?

VAUGHAN: In Gemini we were having some difficulties getting this same performance from all the thrusters. We would periodically wind up with thrusters that would put out about 10 percent of its capability, as opposed to all of its capability. It was okay for Gemini because the thrust level was reasonably unimportant. In earth orbit you weren't worried about overcoming disturbing torques very much. You just had to provide a delta-V, and if the thrust level is smaller, you can get that delta velocity just by burning the thruster a little bit longer.

In Apollo we knew we were going to have to overcome some disturbing torques as we were landing on the moon and the gravity forces become larger as you get down close to the moon, and we knew we would not be allowed to let the thrust decay on different thrusters. We had to work through. That was probably some of the primarily things.

Others is that the numbers of cycles on the valves, the duration total time was about the same, but the burn time on the thrusters and the number of cycles on the thrusters were dramatically different between two programs. So that you could tolerate a little less superior design on one than the other.

But we learned a lot from both that we applied. We tried to apply all of that to the thrusters on Shuttle. Now we're able to use them for long, long periods of time and quite reliable, really. Did a great job last night. We used those, what, down to about 60,000 feet, so you use some of the primary RCS thrusters on Shuttle to pretty low altitudes, not just space.

BERGEN: You were talking about that there were some inconsistencies in Gemini and there was one particular mission that had severe problems with the thrusters.

VAUGHAN: The thrusters? It would have been Gemini VIII, wouldn't it?

BERGEN: Yes.

VAUGHAN: Yes. Remembered well. On Gemini VIII we had a thruster stick open. I'll tell you up front what happened so that you can understand it. Basically what happened is that we have the little solenoid valves that you provide a 28-volt source to the solenoid valves and then it opens a pocket and allows the fluids to flow through and then go into the combustion chamber and provide the pressure and the thrust.

In the Gemini Program what happened was that, again, because of the electronics, we were doing the switching. In other words, the complete electrical circuit, you've got to get a positive and a negative going to the coil to get it to open. It's a lot easier to switch on the negative side. You don't get the arcing. You don't get the kinds of things going on. With the electronics that we had at that time, it was a lot easier for them to make the interface to put the switching on the negative side.

What that means is, if you have a short—what that really means is that the valve is hot all the time and all you have to do is complete the—if you had a short and complete the ground, then the thruster would open and fire. That's in fact what happened. We made a mod [modification] in the system between there and Apollo and did the switching on the positive side, which means that if you had a short, nothing happened until you applied the power on the positive side, because it was already at the ground.

But in any event, what happened on Gemini VIII is that the thruster had a short and we went back later and confirmed that that was what was going on. We couldn't do that at the time, only the fact, you know, the guys, Neil [A.] Armstrong didn't know exactly what was happening. Only thing he knew was that he was gaining rotating, and the longer the thruster burns, the more velocity you get. So it would keep on spinning them up.

The guys took all the troubleshooting they knew to do. The only other thing he knew to do was to manually—I can't remember if it was manual or a pyro device, but basically to close off the isolation valve to the whole system that would stop the propellant flow. So it would block the flow to the thruster and then it shut itself down. It's like going out and turning the water off at the main water line to the house, right? You've got a faucet flowing in the house that you can't get turned off, one of the ways to go do it is to turn off the water. And that's basically what he did. That didn't stop him.

Remember you've got to do something else to get stopped, so he was still spinning, and what he did is he activated the system that we used for entry, and used that system to stabilize the vehicle. Close call. Very close call.

BERGEN: We've heard some say that that was the closest call we ever had.

VAUGHAN: Very close call, no question. I mean, they're very lucky in a lot of ways. Some good design, too, and we had the capability to shut off the water, the main, okay? We also had another system that would provide entry. We had enough capability in that system that were redundant to also allow him to use that on orbit to stop it from spinning and still have enough to be able to come home. So it wasn't all luck.

BERGEN: True. Then you were able to make that modification which would make Apollo safer?

VAUGHAN: We did that mod, but prior to the Apollo. I can't remember for sure, we'd have to go back and check the records. We may have already have made that mod before we ran into that, but it sure reconfirmed it if we hadn't. We may not have. We may have made that as a result. I think we made it before. You know, everybody knows you don't want to do this at a house either. So if you're doing wiring at the house, don't ever wire it up so just to short the ground. Gets your refrigerator hot. Because if you do, you touch it, you get shocked. So, wire it the other way. We knew that, but the electronics and switching circuits just were not available in the size and weight that we had to have on Gemini and Mercury—Mercury was wired the same way—to allow that.

BERGEN: You mentioned Mercury just briefly. Did you have any experience with the Mercury RCS?

VAUGHAN: Very little. Little bit, in that they were flying it—they weren't flying it. It was basically already developed and they were flying it after I was over in that group. There was a guy sitting at the desk next to me who was responsible for that, so frequently we talked and I understood what was happening. But I didn't do much personal work on Mercury. Some, but not a great deal.

BERGEN: Did you work on the orbit attitude maneuver system?

VAUGHAN: Yes, and that's called, it was orbit attitude and maneuvering system or the OAM system for Gemini. It's the same system as we've been talking about. In fact, it was that system that had the short and caused the vehicle to spin up. The reason it was called the orbit attitude and maneuvering is that the Mercury only had attitude-control capability. It could do no delta-V orbit changes and stuff like that.

In Gemini, we did put in a capability to do the rendezvous. So that was new for Gemini. So we had the maneuvering part in now, so we had the orbit attitude as well as maneuvering. We've had that capability in all of them since. We didn't always call them orbit attitude and maneuvering to get the nomenclature back to RCS, but we had the capability in all of the modules on Apollo also.

BERGEN: There were other later Gemini missions, IX, XI, and XII, that had some problems with reduced thrust performance from multiple thrusters, and McDonnell did an investigation on this. Do you know what the outcome of that was?

VAUGHAN: I sure do. I was at McDonnell when we ran a test program. After the program was over, we did a lot of work trying to understand that before the program was over. I told you earlier that one of the things that was happening is that we would frequently have reduced thrust. Didn't matter from a performance standpoint dramatically, because the torque was not that important. Again, though, with the upcoming Apollo, you know, program management is wiser than the engineering in many, many cases.

During Gemini, had we announced that we had a major propulsion problem with the thrust being low, somebody probably would have stopped us from flying the rest of the mission. And it really was not an issue that would keep you from flying.

I do remember that the deputy Center director sent our division a letter, by the time Gemini XII landed, which was, I believe, in December, maybe '66, asking us to come tell him why we weren't going to have that problem on Apollo by the first of year. And I wound up working all through the Christmas holidays trying to do that.

Part of what we did after that was, in investigating that, we did take a flight-like system and it was built up by McDonnell, and we took it out and loaded it with propellant. Had a facility near McDonnell-Douglas or McDonnell in St. Louis. We were able to demonstrate what the problem was. The first firing that we tried on that flight system wound up getting a small little bead of small little metallic particle in an orifice that partially plugged that orifice.

VAUGHAN: The thrusters, because you can't make the flow resistance exactly repeatable in every unit to unit, you have variations, we put in what we call a trim orifice, and that way you can very precisely control it. It's a very thin little small hole, if you like.

On these small thrusters, to have the right flow rate and to get the right delta pressure across that thing, it turns out that was around 28/1000ths in diameter. Not a very large hole. It's a .028. It turns out that we were brazing the tubing. It's a gold braze, only you don't have to use the gold part. That's the only way we knew to braze at that time, and it's still a good way. But in the brazing process you could sometimes wind up with a small amount of the braze material getting to the inside of the tubing. Even though overall the tubing was very clean, had to be for the bi-propellants, the hypergolic propellants, otherwise it would react in the tubing and blow the tubing out.

What was happening then and what did happen, we were lucky enough to see it on the ground test that we ran after that, is that we got a very small piece of gold material, braise material, it came up and it partially blocked the trim orifice. It turns out in the design at that point in time, we were putting the filters downstream by the trim orifice. We knew we had very small, much smaller than 28/1000ths and some of them going down as small as 6/1000ths, very small holes in the injectors, trying to get the fuel and oxidizer broken up so it would get together and combust and react. So we knew we had to have good filtration protection for those, and we provided that in Gemini. But we put the trim orifice on the systems side, rather than on the other side of the filter, and it caught us. It would reduce the thrust, about 10 percent of the time it would reduce it. More often than not, we'd have three or four out of the, I think there were sixteen thrusters, three or four of them would have reduced thrust. I'm convinced at this point that that's what happened.

We didn't have any thrust degradation in any of the larger thrusters, the hundred-pound thrusters, and they had orifice diameters that were in the order of 70/1000ths, 70 to 80/1000ths. So it was a very small particle, but it was enough to cause blockage. In the Apollo design, we also moved the trim orifice to the other side of the filter, so that if we did have some contamination coming down, even large emits, it would not block and cause problems.

I was at McDonnell-Douglas when we ran system level tests and, in fact, when they actually ran it and also when we took the hardware apart and went out and looked and sure enough there it was. [Laughter] The orifice was blocked.

BERGEN: Well, it's getting close to ten, and before we end I wanted to ask Kevin if he had any questions for you up to this point.

RUSNAK: Yes, I did have a couple while we're on this topic. Gemini had two control systems, the OAMS and also the reentry control system. Was your branch responsible for both of those?

VAUGHAN: Again, recognizing we were responsible for the technical aspects of those, McDonnell-Douglas actually did the design and built them, so I don't want to overstate what we were responsible for. But the answer is yes. And those two systems looked a lot alike. The thrusters, for instance, the twenty-five-pounders is what we used in the orbit attitude maneuvering system were essentially the same design as what we used for the reentry system. We made those independent, such that, number one, you'd have a fresh system to come home on and you'd know you wouldn't use up that propellant even if things went wrong on orbit, number one.

Number two, we actually, using pyro devices and cutters, dumped the OAM stage off before entry. So it was not even there. I mean, if we hadn't have done it that way, we would have had plumbing lines running in a lot of places you wouldn't like to have them to, just to get the thrusters located where you wanted them to be able to do both functions.

We provided a similar capability in Apollo, but we did it with a service module. So if you looked at the service module on Apollo, that's basically the equivalent of the OAMS service on Gemini. So the nomenclature is different. We had more systems in Apollo and larger systems, because we had to have more capability, but the same basic function.

The come-home system for the command module was essentially different thrust levels because of the different vehicle sizes, but very similarly designed in a lot of respects and was not activated until you were actually ready to come home. So from that standpoint, the systems were very much the same, so you see a lot of design similarities. And, in fact, the command module thruster was almost the same part number, it wasn't quite, but very close. Design-wise you couldn't see the difference. There were small differences in terms of certain features, but it was identical almost to the Gemini OAMS maneuvering thrusters, which were the hundred-pound thrusters. So again, trying to get commonality.

RUSNAK: Backing up a little bit, you were talking about Gemini land landings. I believe they were pursuing two different approaches, the paraglider, the earlier one, which is the Rogallo wing you mentioned, and also the parasail.

VAUGHAN: Right.

RUSNAK: Were they planning on using retro rockets for both of those systems and were you involved?

VAUGHAN: My memory is a little hazy, to be honest. I suspicion, now that you reminded me, that the Rogallo wing probably did not require, it may have had small enough velocities. The Gemini vehicle may have had wheels on it or skids. It had skids, now that I think about it. It had skids on it that would allow it to kind of land with some lateral velocity, and that's what the wing was trying to do. So I believe that the retro rockets would have only been used with the parasail system. So, yes, I stand corrected on that. You're right.

RUSNAK: I just wasn't sure.

VAUGHAN: I don't think the Rogallo wing one would require that. Now, we had enough other difficulties and people were also concerned with the vehicle tumbling and various other things, depending on how the skids touched down. But the retro rockets was for the other parachute system.

RUSNAK: Yes, the record was kind of vague on that. Okay. That's all I had. Thank you.

BERGEN: Before we wrap up, we talked about a lot of problems that the reaction control systems, OAMS systems, had on Gemini, that you learned from and were able to apply to Apollo. Did you have any problems on Apollo?

VAUGHAN: Apollo went very well. The answer is yes, we had problems, but for the most part, by the time we actually got there and finished the development programs, the only thruster that we lost, if my memory's correct, was in the first lunar module vehicle that was unmanned. I don't remember if that would have been Apollo—it wouldn't have been 9. It may have been the 201 Series or something. It was not a Saturn V launch, so the first unmanned Earth orbital test of the lunar module system, we had one thruster fail. I assume it blew up. We had a lot of them blow up during the development phase.

And the reason it failed is that when we flew that mission, we had some difficulties when we were getting ready to separate the descent stage from the ascent stage. So what happened was that they were doing it automatically through the computers, and the computer timed out before we got firing to the signal to the ascent engine, and it took the gang a little while to figure it out. Not too long. What happened was, they had a timing device in there that didn't have enough time. Too much lag. So they did it manually.

When they did, in fact, separate, somebody forgot to update the computer to tell it now it's got a much, much smaller mass and inertia to control. And the control system was set up such that it would control the descent stage and the ascent stage of the lunar module, or just the ascent stage. I mean, a lot of differences in masses and mass properties and moments of inertia. So we didn't update the computer just to say you've got a much smaller mass after they separated it manually.

So what happened was, the computer says, well, I'm over at my dead band and I need to move over to the other side, or least I don't want to go out of orientation. Assuming that it had this big mass properties, it'd say, give me a thrust level—the thrust level is always constant. Give me a thrust duration of so much time. Well, the computer would tell it and the thruster would do it, and what would happen is the swing [unclear] would get to the other side very quickly. In fact, it was doing that so quick, we burned up one system's worth of propellant very quickly.

When the pressure, after we used it all up, when the pressures went down enough, even though the computer would ask for a long firing and it would get a long firing, but we were getting reduced thrust, if you like, because it was starved for propellant. So the equivalent was getting back to a smaller impulse and it got the vehicle back under control.

Fortunately, we had the other system. Only one of them was active at the time and we had redundant systems. So we had the other RCS-only lunar module to take care of it. During that time we lost one thruster while it was at the low thrusting. It's good that we only lost one. That's the only one, I believe, we lost in the whole program. Now, we had some regulators failures and some other component failures at various times, but for the most part, the systems worked very well.

BERGEN: Great. I have one last question before we close up, about the ASTP mission. Do you have any comments on that problem that they had with RCS during that mission where the crewmen forgot to close the valve?

VAUGHAN: I could comment a few things. You know, obviously that's what happened. The checklist. I believe it was in the checklist. And it's a systems engineering question. What happened was is that the in-bleed valve was too close to where we were dumping propellants and it, in fact, pulled in some of the propellants as we were dumping it.

Interestingly, at various times in the program, we didn't land with residual propellants in both tanks, because the propellants are hypergolic. If you had a tank rupture and did that, you could wind up with a pretty big explosion and fire. So what we did at various times in the program for different reasons, we wound up initially venting the oxidizer, and then we went to fuel and then we went back to oxidizer later. If you look at your sequences very well, I can't remember the exact missions.

The reason we vented the oxidizer, I'm not sure why, but the reason we stopped doing that is we had some degradation with the parachute lines with the oxidizer in 204 causes some small amount of degradation, reacting with and causing some degradation of the parachute lines. So we eventually decided to stop doing that and go the other way, again still learning and running tests. We elected to dump the fuel.

Then on Apollo 14, I don't know if you've looked at this or not, but on Apollo, I believe it was 14, at the landing we lost one chute. It turns out that the fuel, even though it did not degrade the parachute lines, it caught fire with the oxygen in the atmosphere and was actually burning. We burned the lines on one chute. For the other two or three missions, plus the Apollo-Soyuz mission, we went back to the other. We said, "Well, we get some degradation with the oxidizer, but we're not going to burn the lines off." You could burn them all off.

General Stafford and the group missed it on the checklist. From a system design standpoint, we should have—it would have been better to have the in-bleed valve at a location where even with the omission of that you wouldn't have gotten the stuff back in.

The other thing I could comment on on that was that obviously after that happened, you know, everything was great, we thought. At the landing, everything was fine. We knew that the guys had been exposed a little bit, but they were okay. It was about two or three days later before they actually got sick. If you check the record, I think it was a couple days later they had pneumonia-type symptoms and had some difficulties with their lungs, which medical doctors wanted to know was, how much exposure did they have.

We have a test lab on site and we did go out and mix up some oxidizer and some volumes in really long glass tubes. What we were trying to do was get a feel based on their description looking through it, because we were protected, but trying to get a feel for how much exposure that they actually had. We even got one of the crew, I think it was Vance. Was it Vance [D. Brand] that was on that? Yes, I think Vance came out. One of them came out and we had different mixtures of the oxidizer with air and said, "Well, tell us about how it looked. Was it this brown or was it this brown?" So we were able to make an estimate of how much exposure the guys were exposed to, and it was a pretty good amount. They were lucky.

BERGEN: Well, it looks like our time is about up.

VAUGHAN: Time's up. Okay.

BERGEN: It's been wonderful listening to your history.

VAUGHAN: Good.

[End of interview]