ORAL HISTORY TRANSCRIPT

RICHARD R. CARLEY
INTERVIEWED BY REBECCA WRIGHT
SAUSALITO, CALIFORNIA – 2 MARCH 1999

[*Mr. Carley reviewed, edited, annotated in detail, and approved this transcript for use.*]

WRIGHT: Today is March 2, 1999. This oral history interview with Richard Carley is being conducted in Sausalito, California, for the Johnson Space Center Oral History Project. The interviewer is Rebecca Wright, assisted by Carol Butler and Summer [Chick] Bergen.

Good morning again, and thank you for taking your time to visit with us today. We'd like for you to start by telling us how you became interested in aerodynamics and, eventually, space flight.

CARLEY: Well, I appreciate that introduction. I looked at your questions before this interview, and…[reviewed my thoughts on how I got interested]. [Starting] out as a youngster, my father owned bakery businesses…[I grew] up with family that knew something about business and starting businesses. On the other hand…fairly early in my life, I got interested in airplanes, and built model airplanes, [and] tried to make them fly.

…This was prior to the Second World War, there was a lot of [information] around about airplane development, and young people could read magazines like *Air Trails*…As you read those every day and you learned…[many] statistics about [various] airplane[s]…such as weight, horsepower, [armament, flight characteristics, etc.]…This interest…grew.

Then when I started to…say, "Gee, [this] is fun. I think I'd like to work on airplanes," …I started to follow up on them a little bit. Obviously, before the war [airplanes were]… the high technology [of the era]. Things like the [Vickers Supermarine] Spitfire [and the Rolls-Royce Merlin engine]. So I wanted to know who designed the Spitfire, and [how did he get started]…[It was] R.J. Mitchell. When I learned more about him and the fact that he had designed something like forty airplanes. [He had accumulated a lot of experience.]…He also designed [the] high-speed Snyder Cup racers. That was where the highest horsepower-per-pound engines were developed, and it was [what led to] the development of the Merlin engines…

Prior to airplanes being a high-technology issue that…pushed the technology envelope, it had been the railroads. When the railroads started up, they were the place where the technology forced the building of bridges and rough terrain and then the locomotives themselves, so that was the high-technology business. When Mitchell was growing up, that's apparently were he…[got] into the technical aspects of design. Of course, airplane…[requirements are] the other end of the spectrum, in that they've got to be light. [Loss of life and light weight has to be balanced and once an airplane is airborne you are committed.]

But one of the things that I learned, or at least subsequently, looking back on it as an influence, was the fact that you seldom do a good job on designing something the first time. It's very important, if you make [considered] decisions at the start, sometimes you make decisions that aren't good, and you get stuck with them. So you really sort of want to go once over it lightly and then go back over it again. That's a very important—there are pressures, usually, to…fix a design. "Let's get on [and] manufacture something." In my point of view—and I guess maybe it started in those days—that's…very [dangerous]—that leads to errors, it leads to failures, it leads to all the things that…prevent the program from going where you really want it to go.

…Even in that time, I was starting to be interested in not only…what the technical challenges were, but what's the way that you get organized to be able to address these challenges. What kind of working environment, I mean, what kind of facilities, [organizations,] what kind of things do you need to do. [How do you develop support?]

Again, the other aspect of that is that to me it became clear…[obviously later experiences reinforced] that, but it started from these early days…that when Mitchell designed the Spitfire, he understood every system on that vehicle, so you could have a consistent issue [design]. I mean you could bring it all together. Subsequently, some people have felt that you could design programs by just having an expert over here and an expert over here and somehow or other it was all going to get put together properly. But I've always felt that—or at least starting from my understanding of that issue, that unless there's some person on the program who really understands the total concept and stays there and has the competence to get his arms around this whole thing, it's going to be a miracle if it ever gets together somehow or other and gets to go where it's intended to go.

…I guess…[that covers the] early days…Subsequent to that…when I was in high school, I chose to go to the kind of high school that had shops…In those days, the way you built things, you either had machine shops and you machined things [gears and cams etc.] or you needed to weld [or bolt]…Either electric welding…acetylene welding [or soldering]. …Automobiles were…[popular], so you had to know how a clutch [and carburetors] worked and all the aspects of it, how if something was wrong with…[an automobile], [how do] you [fix] it.

In high school we had shops, and in addition to that, had matriculation [courses]. In the morning you took all your matriculation courses, and in the afternoon you took all these shops. You learned how to do at least a little bit about what where all the facilities that you needed. In those days that's how you made things…In addition to that, you really need to understand the processes. Anyway, that kind of helped quite a bit, because you'd actually done some welding, [soldering, machining, woodworking, electrical wiring etc.]…If you're looking at somebody else doing their welding, you'd at least had a try at it once…that helped a lot.

Then on the other side, when I was going to school, I was…interested in philosophy, strangely enough. But again, it's the same drive, it's to try to understand what is the history and what is it that makes people go or machines go. What propels all this? So I was involved with the debating, or at least I was a debating captain. One of the issues that I still remember is that one time we had to debate capitalism versus socialism. Then the next week we had to reverse sides. If you were on the socialism side, you had to then turn around [and support the capitalism side]. Which I thought was pretty good, because it gave you a better view of what it was all about.

So anyway, let's see, what's the next step? The next step, I guess, is going to the university, which, fortunately, because I lived in Saskatoon, the university was right there, that was possible. The university's [expenses] were low enough cost that you could save a little bit of money and buy your books [and pay the tuition etc.] It wasn't a prohibitive expense, which I think is a very important thing. There ought to be [policies that allow] those people who want to go to college…should be allowed to go and encouraged to go. Anyway, I was able to.

Again, at…university I took engineering. I somewhat wanted to take engineering physics. I started taking mechanical engineering, and I wrote a thesis. I think my mechanical engineering thesis was magnetic amplifiers, which was before the days of [electronic control]—or at least before the real development of electronics [these were used for logic and control elements]… [The use of] non-linear magnetic materials [allowed the control] of [small and very] large machinery with magnetic amplifiers…

[While attending] university I started to read *Scientific American*. Which was very good at…introducing you to what's going on in science at…[any given] time. I distinctly remember reading the article on the first transistor and the Bell Labs' investigation…I remember taking the article to my physics professor, saying, "What's this?" They had a little discussion and somebody comes along and says, "Oh, yeah, that's a surface effect. I don't think that's going to amount to much." [Laughter]…[I was interested] because I had tried to make some electronic things, and tubes in those days were…[not very reliable]…They weren't the most rugged things on Earth…

I found [when studying] mechanical engineering…[that there were] these great long equations for gear efficiency or something or other, but they were all filled with constants. All these constants were empirically arrived at, so it really didn't have much of an analytical basis to it. So then I started to get a little more interested in electrical engineering, because there had been enough understanding of the physical processes that you could actually put pieces together and write down an equation that represented what they were supposed to do and predict from your analysis what would happen and then go build it and it would happen.

I went to electrical engineering, and there my thesis was on analogs. This was… understanding the difference between, or the commonality in the equations between all the various phenomena, like heat phenomena or hydraulics or electricity. That all these…can be represented by series of…quadratics…was interesting…At that time we were trying to make analogs [or electrical networks to simulate heat transmission on turbine blade etc.] We didn't have [digital] computers…[and the analog computers were] crude…To do analysis, you had to almost find closed forms to your analysis to be able to get answers, so dealing with non-linear systems was difficult analytically…That [experience] gave me the idea to let me relate different kinds of physical phenomena together. I found that was very useful.

…In between there…[were summer] jobs. I had several different jobs, again aimed at…having a broad…[background]. One summer I worked in a lumber camp, and, strangely enough, in a lumber camp there is a lot of engineering…[For the] cold-decking operation, they design these machinery for bringing…[large trees] down from the slope of mountains and transporting [them to the ocean]…[They used] big steam engines…[in the ravines with large cables connected to the top of very tall pine tree high on the slope of a mountain with] cable mechanisms for arranging all this. Then…[they built] little highways into the place to be able to haul all this stuff out. So there was a fair amount of civil engineering to put all this together, which I didn't really get involved except I saw it. I saw what people had done.

Then one other summer, I worked as supposedly a management trainee, but what it really was, you replaced the people on the assembly line so they could take vacation. [Laughter]…If you have an assembly line…building refrigerators or stoves…you'd take [a] position when that [person] was on his vacation. When he came back, the next [person] would go on vacation and you'd take his position…[You would] go down the assembly line [positions and]…work each one of them.

I learned a lot then about unions and how unions deal with management and [what]…the union people['s concerns were]…Should we really work fast, or should we work slow, or how do we protect ourselves against [those] who come in and say, "Work faster." [Laughter] …One of the things that came to me then was is, well, I sure don't want to get involved with this kind of negotiating with unions and trying to get things done with management saying, "Work harder." The union's trying to say, "Well, don't work too fast, because if you work too fast, everybody's going to have to really kill themselves every day. You've got to reserve some time there for problems."

So anyway, I thought, well, gee, I'd better go work in engineering where I'll be working with engineers…[where there] won't be all these problems. Of course, there's just as many when you work with engineers. [Laughter]

So anyway, there were several other things I worked in the summer. I worked as a draftsman with an inventor one summer, which was very interesting to see how the inventor worked. I wasn't much of draftsman, I don't think, but it was an education for me.

So anyway, then I…graduated eventually and took a job with Canadian National Telegraphs…working on lining [up (putting in service)], or installing telephone systems, long distance, and carrier systems. These were C [and N] carriers. Eventually we got the

Lenkurk 45-A carriers. [I did the testing for the first installation at CNT]…[These were] just modulation systems for putting more than one channel voice on an open wire or a cable…

Going back to the university…[during that time] I got very interested in something called cybernetics. Cybernetics was…[being developed at] MIT [Massachusetts Institute of Technology]…[by] Weiner [a mathematician]…It was this idea of feedback. You'd try something and send a little signal back. If it's not agreeing with what you want to get done, you close a loop on it. If you put a very high gain in there, you can make sure that the output is what you want it to be to a very high degree of accuracy.

Later on, of course, I learned a little bit more of the basis of control systems…Black [at] Bell Labs…made it possible for telephones to transmit [clear voice] across the country… If you keep amplifying a signal; you amplify not only a signal, but [also] the noise…If you imagine in those days you had to have a repeater every forty-five miles and you're going to go three or four thousand miles, you're going to have a lot of repeaters…you're going to amplify a lot of noise. You have to figure out some way to solve that. I guess Black was the guy who did that, and he originated the whole idea of feedback control systems, feedback control. Later on there was something called superheterodyne, which is a little bit different, [which] was really more related to radios. But anyway, that concept was something that I started to pursue.

…When I did go [to] work in the telephone business…all our equipment was basically Bell-designed and Western Electric [manufactured]…which is very good equipment. I mean, it was really well designed. Bell Labs did…fundamental work, on connections, materials, and we got to read, [apply] and understand [the concepts]…Really all we were doing was a little bit more like catalog engineering. You take these engineering assembl[ed] systems out of some design components that had come out of Bell Labs. So I was a little more eager to get more at the basic design aspect, but I still learned a lot about reliability and the various ways of achieving [it]—if you want a telephone, it's not much good to you if it doesn't work half the time. Pick it up, it's got to work every time.

And the thinking processes that go through people to achieve that, how do you make sure the signal will get from here to there not just once in a while, but every time on…a guaranteed basis. So Bell Lab tried all kinds of ways. They put in redundant lines, so if one line went down, then another one would pick it up. They also tried this business of in-service monitoring…With a system that's running, and while it's running, without taking it out of service, you can assess its ability. Are the tubes starting to lose their emission capability, or are there changes, so that you could tell before the failure occurred that it was about time to replace a part or something. So they did a lot of that and the C carrier system, you used to be able to do a lot of on-line monitoring. This concept, though, of how do you make things reliability was…very important…in pursuing servomechanisms and automatic controls.

…I kept on getting more and more interested in this automatic control business, so I finally said…I'm not going to do any automatic design here in the Canadian National Telegraph, even though they were treating me very well…I had my first supervisory job, supervising linemen and…[engineers testing] a cable in Newfoundland that had been put in…by the United States to provide communication to a base in Newfoundland [during the Second World War]…[CNT] were going to…use it as telephone carrier cable. We had to measure all its characteristics, but in order to do that; you had to open up each of these connections.

…In Newfoundland, the weather's very humid and you can't open up these cables when the humidity is high…I learned again this issue of, you've got working people and they have unions and they have rules about your work. If you work for a half an hour, you've got to get paid for the whole day. Besides, you don't want to use all the work up, because you want a job tomorrow. On the other hand, as a…[manager], you're trying to get this work done as quickly as possible. It's this basic conflict between somebody looking for job security and not wanting to use up the job. On the other hand, the other side of that coin, we really want to get something done…That was a little bit frustrating, but in the same time I was still building up to this idea I really wanted to work on control systems.

…In Toronto…AVRO Aircraft [A. V. Roe Aircraft Company]…[was] working on airplanes. …I did consider working for IBM, but at that time they didn't have quite the reputation they… [had later]. So I applied to AVRO Aircraft…when they were asking me questions and how to design [an item], I would just…[remember what] I'd read from Bell Labs. [Laughter] So that worked out very well, because what Bell Lab did was very good.

…The first job I did with AVRO Aircraft was designing electronic systems to study landing gears and landing problems, what the landing impact velocities were and the basis phenomenon of landing. So you're designing control systems that would—in those days instrumentation was—telemetry was kind of nonexistent. In fact, in the early days of airplane designs, you had an instrument panel in your airplane that would have an altimeter, [compass] and an RPM indicator [etc. We were also using galvanic photo recorders with 50 or 100 channels.]…

So what they would do is take [an] instrument [panel with a clock] and mount it in the back of the airplane. Mount a [movie] camera [to] take pictures of it. Then…people [after the flight]…would [read] each of these frames of these pictures, write down the time, write down the readings of each of these instruments. This all had to be done manually…That's how you found out what was actually happening during a flight…

I was designing electronic[s]… It's one thing to design it and make it work in my lab, but when I gave it to somebody else [to manufacture several]…they wouldn't always work… You'd figure out, well, why…Then you learned that you have to put tolerances in these things, and that something that just works nicely on your lab isn't adequate. You have to go back and say, well, what happens if this piece doesn't perform exactly like this one did? Anyway, that was a quick education in that area.

…At the same time I was still pursuing my automatic controls…At that time at AVRO Aircraft we were working on sort of three basic projects. They had already designed what was called the CF-100, which was…an interceptor for…the DEW line [Distant Early Warning line]…to protect Canada [as] and part of the North American Defense System. It was very high-performance and a very reliable airplane.

I think it was a very well designed airplane. I got to appreciate it more, and when I was working on the instrumentation of the landing system, I started to work on the control system, or at least understand how it all worked. So I started to slowly…get a little exposure to it in flight control.

…All aircraft companies…[made] proposals…The Canadian military [and] U.S. military… [put] out requests for proposals for various things, one of which was vertical takeoff airplanes. So AVRO decided that we were going to [submit] a proposal…[I did the design for the control systems for] AVRO's proposal…Another proposal prior to that was for doing surveillance…[AVRO] proposed…a small…model airplane, equip[ped]…with cameras and do surveillance. This was in the fifties. Since I'd built model airplanes…I was working on the proposal, and on that proposal my job was to put together the control system. It was my first [attempt at]…configuring a control system. I remember, just from that point, this is where I can start from scratch, know the whole thing, [and] put together a control system concept. So that was the first try…

[At] AVRO Aircraft and [I was] doing instrumentation and slowly working into control systems. I was going to say AVRO had three major projects. They built the CF-100, which was a twin-engine high subsonic rocket-firing interceptor with very good handling characteristics.

The [second] project was an aircraft competitive with Boeing's jet aircraft and AVRO actually built a four-engine transport aircraft. They actually have pictures flying it over New

York. I think it was something like two weeks after the first flight of the Boeing 707 or something like that. Anyway, they were working on that project.

…[The third project was a] supersonic fighter airplane, which was the CF-105. We were just doing the early work on the CF-105. In those days they made models and they would take them down to Chincoteague, which was a [NACA] facility on the Eastern Shore. They would put [them on] Nike boosters…to get up to high speeds to get aeronautic data. In order to get the aeronautic data, you had to put instruments on board, and instead of this time having photo recorders, like I described earlier, they actually had telemetry. So you'd build this little model airplane, you'd put these instruments on it, and then you'd transmit the data back and try to interpret what that meant in terms of aerodynamic coefficients.

So I went over and talked to the people who were doing flight control, said, "I really want to do flight control." They says, "Okay, you can join our organization, but we'll put you over here at instrumentation at the start." So, okay, that's all right with me.

So anyway, my job then was to learn about or to do all the calibration on all the inertial instruments that were in this vehicle, and you had to figure out what environment it was going to have…[Using a centrifuge etc., I] calibrated all these instruments. In order to get the calibration, you had to operate them in the [operating] environment…I learned a little bit more about inertial instruments, what they did, how you measure them, and a few little fundamentals about that. But still I was interested in doing design and control system…

Debranski was…[the chief] aerodynamicist. Anyway, I talked to him and he said, "Well, you can come over and work on it." They had rudimentary Boeing analog computers…Because Boeing was in the aircraft business and were trying to solve the same problems, all aircraft people tried to make equipment [to do]…design work. One of the things was to build analog computers [for]…stability analysis and other kinds of things.

AVRO had…Boeing computers when I joined it. Again, I remember the first thing I had to do was to make an analog simulation of [the] longitudinal characteristics of [the

CF105]—this was really not much more than a simple quadratic. It was a little bit more than that, but not much more. But I still remember the first time I put the stimulation together, Debranski brought in some people, and we had this little cockpit and we had this thing hooked up, and it wouldn't work. [Laughter] So Debranski looked at me and says, "Better work the next time." [Laughter]

So again I learned the second lesson, or learned again the other things about things have to work, and in order to make sure they're going to work, there are certain things you have [to do]…wishful thinking [does not help]. You have to really examine all the reasons why they won't work and make sure they aren't going to occur. So that was a good lesson. Fortunately, Debranski didn't fire me. He gave me a second chance. I think we actually did make a demonstration and I think it did work.

Anyway, then he allowed me to come in and work on the control systems. Then about that time, like all organizations, people change, and for some reason or another Debranski decided to leave the company. Actually, when I joined this, there were two other people [in the control group]…At the same time they had hired a contractor…Honeywell…This was…the beginnings of our control system analysis for the AVRO Aircraft, for the electronics part of it…

[James A. "Jim"] Chamberlin, who was [Debranski's] boss, started to come in and spend more time with us because Debranski wasn't there anymore, so somebody had to organize these young kids…That's when I first met Chamberlin…We gradually got…better at our simulations [and analysis]. We…bought more elaborate analog computer equipment [Electronic Associates] and we expanded our analog facility…[During] that time, I…decided we ought to really have a really good airplane simulation…

At least I proposed that we put together a simulation. In the early days of simulation, we had [limited] ways of generating functions. In fact, the earliest function generators…were called a shadow mask with DuPont tubes…A mask [was placed] in front of an electron tube and you drove the beam so that it would follow the [edge of the] mask… [One] whole tube [was needed] just to get one [non-linear] variable. So it was…hard to simulate airplanes…[Airplanes require] a number of [static and rotary] derivatives…

[I decided to use something different.] We'll use servos…[to drive a series of potentiometers.] We'll use…tapped pots, so that we could make functions [of] two variables… We could have functions of…altitude [and Mach number]…or [1/2 rho V squared.]…Chamberlin said, "Okay, go ahead if you want." So it was really more…just go do it.

Fortunately, we had a lab next door that could build things, so you'd just go over there and say, "Well, would you put this together?…Then we'd take it back and check it out. So we built a simulator and the idea of the simulator [was to represent the aircraft mass properties, aerodynamics, control surfaces and actuators, control system electronics and the hydraulic system volume and pump response characteristics etc. The system would cover the flight envelope. Zero to Mach 2.5 and altitude zero (including takeoff characteristics) to 70,000 ft.]…

[I] learned enough about the analysis of Moog valves and how they work and the dynamics of them so that we could simulate [their characteristics]. So we had some pretty good hydraulic simulations…We had a [complete] electronic simulation with a cockpit.

Then at the same time, the hydraulics people were…build[ing] their full-scale systems and test them with what we called a B-1 test rig. It was being built over in the hanger just across from…our engineering building. So the way we actually evolved this was we did everything on the simulation, and then as we got hardware, functioning hardware in test rigs, we would replace that part of the simulation, we'd run wires over there and drive [the test rig thought the simulation.]…So we eventually ended up where the computer would drive P static [and P total, using computer signals to drive the pressure pump's supply]…the pitot tubes on the vehicles so that the airplane thought it was flying and the control system thought it was flying, so we could actually check out the whole control system as it was installed in the airplane.

So, anyway, we built this simulation and the simulation was—I think we ended up with something like 120 functions and two variables, derivatives so we could put in all the static derivatives and all the rotors derivatives. So we had a pretty elaborate simulation. In fact, it was a good enough simulation that the people from Cornell Aeronautical Labs came down to review our simulation. They brought in their data and we put it in the simulation. They didn't tell us very much about what they thought of the results, though. [Laughter]

At the same time that we were building this and we were using this simulation [for control system analysis] Honeywell was designing the control system and they were doing analysis [in the]…old way of doing it, which was very slow. We could run all our simulations in a few days, whereas Honeywell with their—they didn't actually have any simulations capabilities, so they were doing it analytically. So it would take them much longer to evaluate different aerodynamic derivatives.

…[We held frequent] meetings [with Honeywell]…Chamberlin…arrange[d] these meetings…We'd divide up [the analysis]…I was doing the longitudinal and the other two…were doing the lateral [analysis]…At the same time, I decided I'd take some postgraduate work, go to the university and try to learn a little bit more about control systems. I met a Dr. Hamm, who…[was a] MIT graduate…After Black and his [work] at Bell Labs, MIT [Instrumentation Laboratory developed the technology]…Brown [and Campbell]…were very good at servomechanisms [and wrote textbooks]. MIT did a fair amount of servomechanisms work during the Second World War, gun tracking [etc.]…

The control system in the AVRO [CF105] aircraft was a pretty advanced control system. It had faders in it, and it was a normal acceleration and side acceleration [control system]…[We measured] the side forces with accelerometers. We had a normal acceleration command system. Since it was a delta airplane…the airplane actually [loses lift] when you change the elevator settings. It was kind of an advanced—well, it was a very advanced flight control system and a very effective one. We had [very] good handling characteristics.

As we were doing all our simulations and learning more things, the airplane was being built as actual hardware, and more analysis problems were coming along. [Analysis of the gyroscopic effect of the engines, dynamic loads etc.] We didn't like the way the environmental system was working; we couldn't control the temperature in the cockpit properly. So we decided we'd make our own…We [decided]…to design this new environmental control system controller with transistors…We did our first design. Of course, in those days, transistors were pretty unreliable. I mean, they never manufactured two transistors that had exactly the same characteristics. [Laughter] The characteristics would drift, so the first tries at making solid state electronics were less than [perfect] however, we made one.

Then other things occurred before we actually—we were getting ready to put it into production, but I think we would have had a lot of problems trying to make that, because the state of the art wasn't really quite along far enough to make reliable components. However, we learned a little bit more about transistors and how to use them in the systems. In the hydraulic systems, we needed to get better performance out of them, so we were doing more simulations.

I remember one time…that we couldn't get the [actuator] band[width] we thought we needed…However, we wanted to improve the response characteristics of it, so I'd do some simulations and I'd say, "…if we change this spring tension from so and so on these valves, we could…[get the response we want.] The good thing about working for a company like AVRO with Chamberlin as my boss, I'd just say, "Chamberlin…we can change the spring." He'd say, "Okay."

The next day, the hydraulics people would put a new spring in there, and we'd have test results from the actuator…[the same day]. So everything was very quick. You know, when you decide that you're going to do something, you do the analysis and then you found out very quickly as to whether it worked or not, so that was a great advantage, because instead of having a lot of talk where people debated this and that, you know, you might have your debates, but you were going to find out what the answers were. [Laughter] So you had to caution your debate with a little common sense.

WRIGHT: I guess at some point your work started to shift then at AVRO?

CARLEY: Well, by that time I was supervisor of the control system analysis…We had a group of about twelve of us…and then we had a lab next door [led by Leonard E. Packam]…We had an analog facility and then we had a digital computer [in the office directly] below us [in the engineering building]…We used up at least thirty or forty percent of its time, because we were verifying all our simulations. The digital computers were so slow, it was painful. But we at least did most of our static analysis using the digital computer to confirm the [simulation]results [and to run test cases]…We'd find errors in both systems, either something wrong with the digital [or the analog]—but we sort of digitized exactly the same equations that we had on the analog.

So again we learned [about the effects of (Ronda Cutta) numerical integration, scaling,]…and other kind of problems that lead to differences between analog results and digital results…

Then due to some politics within the Canadian Government, the program got canceled. By that time, though, we had flown the airplane to pretty well all [through] its [entire] envelope, which was very [high] supersonic and pretty high altitude… We had flight characteristics and we were in the process of trying to interpret flight results as compared to simulation results….

But during that process, though, I guess I should go back to one other step, which is kind of very important to the subsequent NASA activities…Anyway, we had this concept… We did the simulation and then the next thing was, is that we lay out flight plans for what you were going to do with a flight on any given day…Jan Zurakowski was going to be our pilot on the first flight. I had spent an awful lot of time in the simulator learning to fly it, because, first of all, you've got to see if it's working right, and really trying to understand the control system and make sure that the control system does what you'd really wanted it to do. So I became fairly proficient at [instrument] flying.

At the same time I was taking flight training, so I actually soloed in an airplane. I learned to fly an airplane, because I thought, if you're going to do flight control systems, you ought to at least know how to fly an airplane…So I was pretty proficient on the simulator. So the next step then was to try to convince the test pilot that he ought to spend some time in this thing and to learn characteristics of the airplane before he flew it.

We did get Zuro out. First he was somewhat reluctant, but he started to spend a fair amount of time and he was very interested in flying an airplane near its stall characteristics so he could see what it was like. He used to sit there for quite a few hours just playing around with it to its various altitudes just close to the stall characteristics. Anyway, he did that, and, like I said, on the first flight we actually ran the simulation. We didn't have very good displays in those days, so we had a small little TV, it was like a TV tube, except you couldn't put all the pictures we can on TV. You could only put little lines. Anyway we had a little representative of an airplane and we could move it around so that it was a little bit like an attitude control. We could put a little altitude data on it.

So anyway, we mounted that on the outside of the window of the actual airplane. You could sit in the airplane, and since we were feeding the pitot static and total data, you could go through the whole flight plan. So we did that prior to the first flight, the morning before the first flight. So we had all the airplane, we felt, pretty well checked out. We knew the control system. So Zurakowski flew it. It took off and flew nicely on the first day, and he came back and landed. The first thing he said to me was, "It's a lot easier flying the airplane than it was a simulator." [Laughter] Which I thought that's exactly what I wanted. If he could fly the simulator, we didn't have any worry about him flying the airplane. But I'm not sure how he really meant that. [Laughter] I've often wanted to ask Zuro as the years went by what he really meant. [Laughter]

But anyway, that idea, though, of simulation and trying to get pilots to understand the flying characteristics of the vehicle well enough, and then to observe their performance… We had…four [or] six of these eight-channel recorders, so you could pretty well [monitor]… all the [flight] variables…[We] put them all close together so you could look around at all of them at the same time. So you could watch pretty well what was going on with the flying, how the maneuvers were being done, how well the airplane was performing…

Well, first of all, on the airplane they'd made a big step forward [on instrumentation] on the CF-105. We had what was called a PCM telemetry system, which was pretty advanced telemetry system…We set up…a little command center [for monitoring] the flights. So we set up recorders in this command center and we set their variables up and scaled them so we were identical to what we had on the simulator. So before every flight, we would run the simulator and then we had these great long tables with these recorders. So we had the simulated results here, and then as the flight came out you could compare the flight, the actual flight with what your simulator results were. So we knew pretty well [how the flight was going].

In fact, there was one time when…[the telemetry people removed the accelerometers to calibrate their system] without telling [us]…They [replaced] one of our packages… backwards. It was a bad design that you could physically do that, but they did. So the end result is they had the accelerometers hooked up in reverse. So when…the control system [was engaged] in flight, the airplane darn near did a loop. [Laughter] Of course, the pilot didn't think very much of that. [Laughter]

However, because we were monitoring all this, we were pretty well able to deduce exactly what the problem was before we landed, so when it landed, all we'd do is run out there and look at, oh, yeah, somebody—then the question was, "Who did it and why?" But still that sort of, in my mind, at least, firmly confirmed that there was a lot of benefit in real-time monitoring.

So we set up this whole deal, and in addition to that we had our flight control, our flight operations people became familiar with that, and we trained a lot of people in how [to set up and run real time flight monitoring]…

So anyway, they canceled the program. So then what do you do? In the meantime, I had been reading my *Scientific American* and *Aviation Week*, which everybody did in those days. There had been this notification that they were going to create this new agency, something about space…There was actually, I think, ads looking for engineers. By this time, we had pretty well had our flight control system was working and things were going along. As organizations get a little older, there are a lot more politics go on. Anyway, I was getting a little restless.

WRIGHT: How old were you at this time?

CARLEY: Oh, I don't know, twenty-eight or something like that. Somewhere around there. Yes, well, I don't like to remember age. I'm seventy-two now, that's the only thing I know. [Laughter]

…So the program was canceled. I remember, because I had been interested in [NACA]…There were a lot of other people within [NACA] who were doing aerodynamics and derivatives and performance, so there had been a lot of discussion between what was the performance of the [CF105], and a lot of that discussion between [Avro] and people at NACA [National Advisory Committee for Aeronautics] at Langley. So there was a lot of connection between our engineering people and Langley. We had conducted…flight tests out of Chincoteague, which was a Langley facility, so they knew us and we knew them to some extent.

In addition to that…[many of our] contractors were [in the United States], for instance, Honeywell…Hughes, I used to go down to visit the Hughes people for our fire control system. We had Honeywell actually doing some of our hydraulics; too, so there was Honeywell for the flight control, Honeywell for the hydraulics. There were a lot of the subcontractors were in the United States, even when I was working at AVRO.

I remember that when this thing was canceled and they laid everybody off [the Avro management made an announcement on the PA system]…The people who were running the company were trying to embarrass the government for canceling the contract. It turns out there was a lot more to it, because apparently the [military] chief of staff was really the culprit behind it all…

I told Chamberlin, I said, "…Why don't you go investigate this NASA thing. I'm going to go skiing." [Laughter] So I went skiing for a month, and when I came back, Chamberlin had actually contacted them, which I thought he would, but I didn't realize it would go that quickly. So the first thing you know, we were having interviews.

WRIGHT: They came to see you?

CARLEY: Yes. Well, I mean, because other people had been there when I was gone, they had kind of made more connections than I had, which it was a mistake for me to go. [Laughter] Because I certainly lost my position in the queue…

I should discuss a little bit as background, and that is, when I was working for the telegraph company or doing communications work there with Bell equipment, I got interested in a subject which is called information theory, which is something that was put together by…[Shannon at] MIT…he did a tremendous job of kind of conceptually understanding modulation and what he used to call negative entropy and information theory. I thought it was pretty good, but it gave you a good intuitive feeling as to how efficient a particular modulation scheme would be. Like a simple on-off system is a very inefficient information transmission system. If you have a random modulation system, it's [perhaps]…theoretically the most efficient of all modulation systems. How you achieve it, that's another issue. Anyway, that was very useful…to understand [these]concepts…

Going back to the interviews. I guess I don't remember all that much about the interviews, except that I remember the person that interviewed me was Charlie [Charles J.] Donlan, and I don't know how I came across to Charlie, but I remember asking him one question. The concern always was, when you were in industry and looking at government, if you're working for a government you're not allowed to do anything. So my question was, "I want to work with hardware."

I had kind of an ideal situation at AVRO. I had my own lab, I had computers, I had people who could build hardware. If you wanted to play with the hardware yourself or you didn't like the way the electronics was, you could go build it. And that's the only way you learn. You don't learn by reading it; you learn by doing, and doing it more than once, too.

So my question was, well, you know, are we going to be allowed to ever have hardware, or is the hardware just going to be designed by contractors or something like that. There was assurance, yes…I [had grown] to respect…the concept that Langley had developed as far as learning how to use people to do research. They got technically involved with things. They built things. And they had a management scheme whereby the people who were actually working the work…might actually be earning more money than the supervisor…I think Bell Labs had a similar kind of a philosophy that it wasn't the manager that were doing all this [work], it's…[the people with the ideas] who are actually doing it.

So Langley had evolved, and I think the centers that were successful were…people would [assign from]…Langley and go start another center…Because they had the wind tunnels…they were able to be involved with…aeronautic design and since they'd done…[basic] work on air foils, and NACA wrote all the [aircraft] equations to motion and all of this terminology…It really set the [analytical] basis for [aircraft development]. And because they did such a good job of that, when you were talking in between companies you were all using the same reference material, so you were communicating.

Even though we were kind of reluctant to go work for a government, if you're going to work for a government, at least it was NACA. Donlan was fairly reassuring along that line. But that's about all I can remember, other than they were very nice people. Here we were young…

I guess the other assurance was…when we come up with…ideas, who's going to listen to us?…[Chamberlin] got…involved with the actual design, the structural design, the hydraulic design, Chamberlin was really—we had supervisors for each of these groups, but the guy who really dealt with them was Chamberlin. I spent a lot of time with Chamberlin. In fact, I used to, when I had really serious problems go over and visit him at his house and got to know his wife Ella. Very nice person.

So I was used to that kind of relationship. "I think I want to build a simulator and I want to do this thing." "Okay, go ahead and do it." [Laughter] So you had the basis. So when other people with the organization would come along and say, "Why are you doing that? What authority?" you didn't worry about that. I had the authority; it was going to be done. But I knew that when I went to…NACA, [things would not be the same]…

On the day of the rollout…[of the CF105], the Sputnik flew. So everybody was talking about Sputnik…

So the next step then was to arrive at the Space Task Group at Langley. But again, it all was made relatively easy. I…[received] a letter saying, be…[at this place at this time and] take this [letter] with you. Since I'd…[traveled to the] United States for [many contractor meetings this was not new]…Since I had a security clearance in Canada, I think there were some arrangements that we could transfer our security, at least for a short period of time, and then [the FBI] went back [and review the data]…I did get messages from some of my friends saying, "Oh, the Mounties were around asking questions." [Laughter]

So I don't know exactly how that was handled, but anyway, it was made very easy…at least from my point of view. The biggest problems were putting the stuff in the car and driving down and arriving at a strange place, going into a motel.

One of the other things that I kind of remember is that I think it was March or April and arriving in Hampton, Virginia. Hampton is right on the bay there, and we could go swimming. [Laughter] Which you certainly don't do in Canada at that time of year…

So I got assigned to Bob [Robert G.] Chilton's group…he did everything he could [to facilitate the transition]…He was very accommodating…Bob Chilton…had been at, must have been at a very young age, a B-17 bomber pilot…

I think at the time we joined the Space Task Group, it seems to me they had less than fifty people in it, and since there were thirty or twenty-eight or something of us, that was a fairly big jump. Then, of course, the thing just exploded in…[the following months]…Later on…I understood there had been three different…competing [concepts for the Mercury vehicle], one of which was Chuck [Charles W.] Mathews' [flat iron conventional landing configuration and another was the Faget couch configuration with parachute landing]…

They [had chosen the Faget approach]…had a contract with McDonnell to start work…on Mercury…I was brought into a meeting on the control system; [Bob was] having one [with] their…contractor…I think that Bob might have been hoping that I would say something, because I think he had a little bit of concern about some things about the control system. Honeywell had proposed what really…was…the first digital control system, but they didn't call it that. They called it a Boolean logic system. But all [its]…inputs were digital…[They used Boolean logic to determine the output signals]…

We did have…[problems with the sensors]…Converting…instruments from…analog device[s]…Instead of using ADD converters, which were pretty heavy and expensive and too large in those days, they actually put slip rings on the existing analog devices. As Bell Lab had learned earlier, there's something about contacts and migration of materials in contacts, so there [were]…problems [with] converting these conventional analog [devices] to give digital outputs…I was very wary of saying anything in an unknown environment. I didn't feel comfortable enough about really having an understanding of what it was all about.

But as the time went by, one of the things that struck me fairly early was, is that the meetings between NACA and the contractor were far too infrequent. If you have people working on things, inevitably people look at things in slightly differently. If there isn't regular communication, you're going to go off in different directions. I guess I had learned very early that it's very essential that you stay with something and you pay attention. You don't have wishful thinking hoping that everything's going to come out all right. You have to know it's going to come out right, and the only way you know it is by examining all the possible reasons why it might not, including management ones. Especially management ones, or especially lack of communication.

…We established…more frequent meetings with contractors and…more elaborate agendas. Although I'm not sure how Bob Chilton would look back on that, but as I remember it, I think that was one of our first sort of changing a little bit of the direction for going with more meetings with the contractors. The contractors obviously didn't like that. [Laughter]

WRIGHT: I guess not.

CARLEY: "Why don't you go away and let us do our work?" Which is okay. But on the other hand, if you really have good communication and you have technically competent people on both sides, usually the communication gets to the place where it's encouraged…If there's mutual respect…It's only when people's egos get involved…"I've decided this," that's when this desire to "Leave us alone and let us do our work," that's the first sign of trouble, when people start talking like that. I think that's one of the things that I learned…

[The Space Task Group became] NASA [and] was growing…quickly…There was a desire to get [early]…tests…Faget had laid out a kind of a whole series of tests of the Mercury capsule…Big Joe and Little Joe…These things had to be built and launched…[There were discussions on how much] monitoring…

A lot of things were going on at the same time. The organization was growing and bringing new people on board. [There] were…early test results [that had to be analyzed and understood and adjustments made to the Mercury design]…All parts of the organization were growing all the time, so people's responsibilities were changing…

[The] Little Joe [was]…a rocket with [the payload]…shaped like a Mercury capsule…[It did not have an active control system]…They never knew what Q it was going to hit, what speed it was going to hit or what attitude it was going to be at, because it was [passively controlled]…Of course, any data is better than none, so they're getting…data out of it. They're getting…a feel for [the performance].

But Faget had the idea that maybe we ought to have a control system on this Little Joe, so he conceived of an idea of a big tube with engine starter cartridges inside…Which would [create] hot gas in this tube…[with lines] going out [to]…jets on the…tips of the fins…

That meant amongst doing all the other flight control…I was traveling out to the West Coast every week, at least once a week…Following my usual philosophy, we [created]…a simulation [of the system]…[and] a test rig…[Rockwell] put really a good analytical [person], who [understood] thermodynamics, who wrote the equations for the conditions within this tank when we were firing cartridges. Then we had the usual equations and motions…[The engine starter cartridges were sealed with tar. The] jet nozzles would get covered with this tar, and it would change all the characteristics…

Then another thing we learned is that we weren't getting…[the total impulse] we wanted…[The simulation determined that injecting water at the right time would give the total energy we needed. Water was added based on the measured] temperature…But what I liked about it…was that we did an analog analysis and then we ran it on the test rig, and we'd get results that were pretty darn close…I think we actually had five or six firings…So we demonstrated it would work. I don't think it ever flew…

WRIGHT: Before you get into the details of all those changes, let's change our tapes out…

CARLEY: Well, I think that was the dominant thing, so there was so much change. Then amongst all that there was a discussion [of where the manned space activity would be located]. When I first joined the Space Task Group…I had a badge, which said "Space Task Group." Then it [was changed to] "Goddard [Space Center]…" Of course, fairly quickly after that, there were alternate proposals that came along…

People were building up [the] capability of NASA and all the facilities…and at the same time bring Mercury along. Well, while we were building Mercury, there was also an Advanced Planning Group going on that were looking at things like Mars missions and other…missions…

There was…a discussion…that we [have] two [lunar] proposals, one, which would be circumlunar, and the second one, which would…actually [be] a [landing]…

Then there was an effort to look around in the industry in the United States and try to find people who had working programs that could run…trajectories…Even if you have a theoretically spherical Earth…there's certain [digital] computational problems when you [try to get orbits to close]…

So there was a lot of basic reviewing of fundamental constants and trajectories and reviewing all kinds of guidance schemes and envisioning what they could be, because in addition to doing Mercury, we were always looking at the [lunar] project…Early concepts were…to have an on-board capability…One way to do Apollo would be control from the ground and the other way is to have some kind of on-board capability…[I supported the development of an on-]board capability [in addition to ground support].

…I didn't really believe that pilots should be involved in the short-term dynamics [of the control system]. They should really be more of a kind of a management structure. Pilots don't feel like that…It really is, I think, it's very destructive, because it gets the emphasis of what people are really good for. Even a tennis player can't hit a ball in the same place twice in a row. I guess that was one of the philosophies I learned in designing a control system. You always design a control system so that…he's going to make a mistake once out of three times [under pressure]. That's what you've got to think of. If it's a highly repetitive…it ought to be automated. I mean, control systems can do a better job than an individual.

If you want a really good example of that, can you imagine us ever making computer chips manually? [Laughter] Can you imagine how reliable it'd be? …All of that is fully automated so you can control every aspect of it, it's amazing how reliable those devices are, but it's because we got the man out of the loop. [There were people who argued against automated elevators in buildings].

Now, here we are arguing the other side of equation, that is, we ought to have on-board capability and we want to have man involved. [Laughter] So I think the real issue…is how do you have man involved and yet not get him involved in the things he shouldn't be involved in? NASA's never solved that problem. I don't know if anybody has…

I guess the next step was, is that it became clear that there was going to be something after Mercury, something more challenging, something in which we could, since the Russians had already beat us into orbit, that we could get ahead of them…

It became…clear that we obviously had to harness the expertise of the industry to [meet] all these challenges…We… ought to do a survey of what is the current…[state of the art of navigation, guidance and control] components. Who's making what…what kind of [development] time schedule [do] they [have]…What kind of facilities we're going to have available to us to put together some advanced systems.

Bob Chilton and I put together a whole series of meetings…You rapidly discover that…each of these groups…[are] proposing their solution and it's obviously the only right one…Huntsville proposing that all gyros should be air-bearing gyros…Then [contractors]… MIT…[and others] who had…rotating case gyros [and magnet suspension bearing gyros and accelerometers]…So that was kind of interesting, but, anyway, we kind of got an idea of what was available…There were a lot of ideas…

Then there were proposals of starting to write requirements. What is it that we want this thing to do? When you start to write requirements, one of the first things you have to work on, you have to look at what's the environment it's going to work in. So you've got to define the atmosphere. You've got to arrive at some definition of, [celestial coordinates, and earth gravitational characteristics etc.]…Since guidance and control depends on all these reference systems, you've got to define to the contractor what these reference systems are… To get all the trajectories to be run within a common reference system, so that you could compare results…So that took quite a bit of work, amongst all the other things that are going on. [I specified in the Apollo RFP that the vehicle had to have three-axis attitude control and three-axis translation control. To make on orbit maneuvering, rendezvoused and docking practical]…

[When the decision was made that a Manned Spacecraft Center would be created in Houston the question then became] what kind of facility should we have? What kind of buildings?

…I should go back a little bit. We had…people working at our AVRO flight test program who had seen the method by which we monitored the flight of the AVRO aircraft, these people came down and were working for NASA and became part of the nucleus of the NASA flight control organization. I mean the operations part. So they were already started with the concept, that, yes, obviously you're going to have people looking at [vehicle data]…when it's happening, and yet that really wasn't a part of the original Mercury Program.

…You can't look at something if you don't have any data, so you've got to have [instrumentation on the vehicle to provide] data. You've got to create a network so you can gather this data. If you're orbiting around the Earth, your line of flight is fairly limited, so you've got to have a lot of [tracking] sites…Len Packham['s]…background had been communications…He was a fairly integral guy in establishing [the tracking network]…

We need to improve our capability of monitoring Mercury. What kind of an organization—first idea was, well, you ought to have engineers, and so we would train our young people within the control system group, within Chilton's group, to be [monitors]…Maybe we should have a separate operations group…[Many] organizational changes…

[After the Kennedy speech the Apollo program was committed]. Gilruth and Donlan decided that…Faget['s] [group]…spend full time on…Apollo…"

Chilton was very strongly behind [an] on-board capability. He…[had graduated at] MIT…he knew a fair amount about Instrumentation Lab. We spent a fair amount of time visiting with MIT to see what their ideas and what there programs were…

Phase One [studies RFP were prepared]…Gilruth and Donlan arranged… contractor's… presentations…I remember…Gilruth's question to their management…What was the most significant challenge [of the Apollo mission]. Naturally every one of them answering, "Oh, it's a management challenge."

Yet on the other hand, I think [Bob Gilruth's] idea was that probably the biggest challenge was launching this thing from the moon, when all we'd ever done was launch from the Cape with all kinds of ground monitoring and all kinds of testing. Here we're going to do this all remotely. So I thought that was a tremendous question. That was the difference between someone understanding the problem…

That, at least, started the program off on the right orientation, that this is a technical challenge…This thing's got to work and it is a difficult problem. I enjoyed that part of NASA, because I had a great deal of confidence in management being competent and technically addressing issues, and then at the same time being reasonably fair. So I thought that made it a lot easier, because when you're proposing things, there [were] a lot of different conflicting ideas…around. As long as you know that the people at the top are going to somehow or other not let people get too arrogant…It gives you the freedom [to]…do your work. So I was very encouraged about that.

WRIGHT: You mentioned so many things were going on at one time. Did you feel much of a transition from Mercury to Gemini, or did it flow?

CARLEY: …For Phase One…I was appointed the guidance and control…[representative. Previously I had written a] memo…on-board conceptual idea of guidance…system for Apollo for the next generation…I defined…Guidance [as targeting equations]…Navigation [as position determination]…and…Control [as three-axis attitude and translation control]…

A lot of people use these words all interchangeably…That's more or less the same kind of thinking as…object-oriented computer programming.

There was another function that we used early in the avionics business…was this issue of form, fit, and function. You want to be able to design things so that they have the same form, fit, and function, so that you can replace them with another box that may have improved technology, but it performs the same form, fit, and function. That's a good concept in a way. It allows you to break problems up and address them and allows you to have a continuous improvement program, because as long as it meets the same form, fit, and function, you can do something inside it that's different than you did before…

So we are pursuing this, and Bob [Chilton] was obviously doing the same kind of thing, except that he was, first of all, the section head, and then the branch head. So he was doing it, plus all these organizational responsibilities.

…[For the] Phase One…[RFP, we had] to write requirements…[We wrote what] you want the contractor to respond on. So that you have to start focusing your thoughts on…[defining] the environment, you have to define what the concepts are you want. You've got to define what kind of on-board capability that you want, what kind of on-the-ground capabilities. So these…ideas were gradually maturing.

…I was also the representative on the Phase Two contract, which is getting little closer to [a vehicle design concept]…that will go to the moon…[Mission concepts of] Earth orbit [versus] lunar orbit [were resolved to direct launch and lunar orbit]…

All the time the organization is growing, so those decisions [that] were made originally by the smaller group, now there [are new people coming on board]… [Some] are experienced people, and they expect[ed] to make a lot of serious decisions…I put together [an Apollo GN&C concept and management] proposal…I went over it with Chilton, and Chilton arranged for us to present it to Gilruth…We had this little presentation, just a small group of people…I proposed…"Well, we ought to have an industrial contractor. [This should be an Associate Prime Contractor, separate]… from the prime contractor…

Anyway, the end result was that Chilton somewhat disagreed with my philosophy. He had an idea that this was such a big program and it was so technically challenging that we really ought to rely [on expert help to manage industrial contractors]…

But anyway, the alternate proposal…[presented by] Chilton, was that…we ought to have MIT…manage this. Naturally my proposal didn't win. [Laughter] But that's all right. I had nothing against MIT. My idea was, though, that university professors and people who work in universities, and even instrumentations labs…don't have the same attitude as design people. They can be very good overseers, because…they understand the science…They can be aware of some places were there could be a difficulty and it should be examined, where a contractor might kind of gloss it over. I would have rather have seen them as an advisory level, with a pretty strong carte blanche to go wherever they want and investigate whatever they want, but to kind of be more technical. Whereas the other view that [Dr. Charles Stark Draper]…wanted them much more in line. [The Instrumentation Lab] wanted something similar…where MIT…design[ed] and the industry would[manufacture]…

It was a fait accompli…There's nothing wrong with having a pretty good resource like MIT working on the program.

…Things that did come out of that [were]…positive…There was…more… recognition on the management side [of]…electronics [and software. At that time airframe manufacturers management were more oriented towards]…structures, aerodynamics, [and] propulsion… Whereas my attitude was…[that] avionics [software and electronics] is…[the future]. Structures and propulsion and aerodynamicists don't understand electronics, so they shouldn't be in charge. That's not a very popular position with large airframe companies or people who have had the other background.

I was amazed, actually, at the tolerance of Gilruth and Donlan. I think, to some degree, there was some recognition, and obviously MIT thought it was a good idea to have [a] kind of a super subcontractor status separate from the prime contractor. So I think that part of it was pursued a little bit. I thought that good part did come out of it.

…About that time I…thought that there was going to be a huge transition from Mercury to Apollo, [particularly in the areas of GNC with ground and airborne capability using software and electronics. It just couldn't happen. We needed something in between]…

We were looking at how to do horizontal landings…instead of… landing in the ocean…We would really like to have the spacecraft…[to] have controlled reentry, so we [would] know exactly were it's coming down, and preferably you'd like it to [come down on land]… Something better than going and searching for it in the ocean…

[During the early Mercury studies I was working with an] aerodynamicist…[who was interested in a] lenticular shape [vehicle]. Which would come in perpendicular, so you had to right ballistic factor on reentering, and, on the other hand, you could convert it, so it would have some aerodynamic characteristics…[to provide a horizontal landing capability. This had similar characteristics to the flat iron concept for Mercury that Chuck Mathews had proposed]. We couldn't implement them on Mercury…The next vehicle, we were hoping, might.

…I decided…[to] write a proposal…Chilton had worked on…a way of controlling the spacecraft by putting a ring around the thing and pumping mercury in it, which would change the center of gravity and which would allow you to have some control…This brought up the idea of having control of the center of gravity of a Mercury-shaped vehicle. If you do that, then you can create a force perpendicular to the trajectories, so you can control it…

We ought to have something, which we'll call Lifting Mercury. The idea of lifting mercury is you offset the CG [Center of Gravity] so that it has a lift component. Now we can put the control system in there and maneuvering this. Well, actually it might be better called a component [of] drag. Anyway, it has a force perpendicular to the trajectory so you can control it…I proposed a program…that has a much more [capable] control system or guidance navigation control [(comparable to that required for Apollo) and] has the concept of ground monitoring built into it from the start, like we did back at AVRO and what we probably should do in the future.

…[The] lunar [mission is] going to have to have rendezvous capability and maneuvering large engines in orbit. We need to demonstrate all this…[before] Apollo. Not only…the means of designing the hardware, but from bringing [on the] operational support group of people, getting them so they get their feet wet and learn how to do all these things. So I proposed this Lifting Mercury, and Bob Chilton agreed and we both worked together on this program. [The program would demonstrate onboard assent guidance, orbital control, rendezvous and controlled reentry. It proposed an enlarged booster (perhaps a Centaur stage), and a rendezvous target].

I remember one day Bob Chilton says, "Well, we're getting fairly close to making the presentation to Gilruth…" So one of the steps was, "You'd better go see Ray [Raymond] Zavasky."

I went over and to see Ray Zavasky. [He] looked at my estimates of how much it's going to cost to get this Atlas vehicle modified and put this extra propulsion on it and do all the things we have to do…[The] vehicle is going to be heavier than Mercury, obviously. So we had to modify the launch vehicle.

…He would look at my estimates and double them. [Laughter] He was putting more realistic estimates in. So anyway, we ended up with a number that he had put in, I forget exactly what the number was. If I had started out with…200 million, by the time he was finished, it was more like 600 million. Probably rightly so…I was being a little too optimistic on things that he'd had a lot more experience with…We went through that stage and we eventually get to the place where…we're going to make a presentation.

In the meantime, when I had started out at NASA, I had never spoken in front of people. I couldn't talk to [large groups of] people. I remember the first time that I had this Little Joe program, I came back from the Little Joe program and Faget [arranged a group] of people…to listen to me explain what happened. I just fumbled around, because I just didn't have the confidence to talk to [groups of] people.

Obviously…that's one thing that you shed pretty quickly. You learn to hold a meeting; you learn to talk [to groups of people]. So I was getting use to this. Even though I was getting rejected, at least I was getting use to making presentations and learning how to put presentations together so that they come across the right way to people.

Incidentally, there is another thing, that when we were working on the Gemini Program, since I was working on the control system…I was…starting to work on the fire control system. The fire control system for the CF-100 was a joint effort between the United States and Canada, in which the United States was going to [create a] alternate group of fire control people…other than Hughes. This group was…headed by Dr. [Robert C.] Seamans. Obviously, you probably know how Dr. Seamans fits into the space program. But anyway, that was kind of an interesting thing, because I used to go over to Waltham and the Waltham watch factory and [discuss the integration of]…the fire control program, which was an outgrowth of something, which I think, was called the Warrior program…

I make my presentation, and then people review it. Chamberlin is the main critic. He said, "This is impractical," or something or another. However, since I'd go over the Chamberlin's [house] to have dinner with him, I guess, somehow or another, mentally I was already convinced that management wasn't going to let me do anything like this. But that really wasn't the purpose; my purpose was to get a program between Mercury and [Apollo]… he decision was made…that, "Yes, we probably do need a program…Chamberlin, why don't you [pursue this]…"

Chamberlin starts to work on a program and starts to talk to McDonnell-Douglas and they start calling it Mercury Mark VII. Obviously, Mark VII is not a very good name. But anyway, it starts to become something. It starts to be serious discussions with McDonnell… I don't know…all the other inputs, I'm sure Gilruth had some other ideas [as well as] Donlan and all the rest.

Anyway, the thing expanded from my concept of it to a concept, which included everything that I had, all the rendezvous and all the guidance controlled reentry. I proposed that we ought to have launch guidance on board, on-orbit guidance and navigation on board, controlled reentry, and rendezvous demonstration…[This] would be a significant step [up] from Mercury and yet not [having the risks of a vehicle]…required to go all the way to the moon, although we could design this vehicle so it could go to the moon…

WRIGHT: Yes. We're ready if you would like to start.

CARLEY: …That was the…start of the Gemini. As I said, it got created into a better concept, I guess, in terms of the fact that the decision was made that it should be variation that was closer to what it would be for the Apollo. The idea then of having—which we didn't really need it in my version—the Gemini really didn't need any extravehicular astronaut activity, but obviously in developing suits…it would be desirable to do that on some intermediate program prior to Apollo. So that was added.

So in order to do that, you had to change the design of the vehicle so you could get in and out of it. So that evolved into some doors. Anyway, those sorts of ideas started to proceed. The idea was, well, we're going to have a controlled reentry, how do we bring it down on land?

So, in the meantime, Langley had been working on paragliders and so the idea was that this would have some paraglider capability. I was never very happy with that, I didn't think it was very—but on the other hand, I didn't want to say anything against it, because I wanted to make sure that we kept in the controlled reentry. As long as there was a paraglider there and a possibility of landing, then we had a further justification for a controlled reentry, if there was any question about that. So, anyway, it kind of expanded.

In a sense it was now becoming an official program, then more people have got to get involved and there's got to be a bunch of discussions. Then again, I'm not working for Chamberlin, who now has this; I'm working for Faget. So all my discussions with Chamberlin on the Gemini Program for a while were over the dinner table or after dinner or something, at his house, on, "We've got to do this. It's got to have—"

…He was gradually setting up an organization, bringing on people to do that. At some point or other, when it got well enough along, Gilruth felt confident enough in it, then they start to involve headquarters and gradually get all the things that have to go on in order for it to get funding, once you start involving headquarters, then you've got to somehow get headquarters talk to somebody who's got some money, and I guess that involves Congress.

So in the meantime, I'm not involved with that aspect of it at all. I'm still just trying to look at this other part, this hardware, and is there any way that we can have some kind of an associate avionics contractor, knowing pretty well that chances aren't too good, but we might as well fight for what we can.

Someplace in between there, or after that, there was some kind of arrangement made between Chamberlin and Faget, and then I moved over to Gemini. It still wasn't called Gemini at that time, but I forget the name of what it was. I think we'd stopped calling it the Mark VII and something else, some other name. Anyway, we were in the process of looking for a name…

Other people were coming on board. I had been having discussions with Chamberlin on what kind of an organization we should have to run Gemini technically to have the contract, and that we should set up some panels. I forget what we called them. Anyway, we had a guidance and control panel, we had a reentry panel. We had some groups of people who would bring in all the contractors associated with that part of the thing, and all the elements within NASA that were responsible for it and try to get everybody talking on the same agenda, informing people and making some decisions where they had to be made. So we were working on that organization and how we should set up…

Before we got those kinds of formalities going, [one of] the first thing that I did officially on the program that became Gemini, when I was transferred, was that because we were going to have doors so the astronauts could get out, and because—well, one of the big things that I was concerned about was that we didn't want to have to put up with the terrible vibration environment that existed in Mercury. One of the major causes of the vibration environment on Mercury was the tower, because it was a tremendous turbulence producer. So whenever you looked at vibration spectrum for launch vehicles, you could separate out those caused by the engine and those caused by the tower. We certainly didn't want those caused by the tower…It's hard enough as it was to get electronics to meet the vibration requirements without making them any more severe than they needed to be.

So if we're not going to have a tower, we're going to have doors, how are we going to get the astronauts out in case of an emergency? …For all the aircraft people, it's obvious, you…put in ejection seats. Okay. The next step is, if you got ejection seats, how is a guy going to know when he should use it? So, well, now, what are we going to do? All right, so I get assigned to go look. Chamberlin says, "You go look at that…" He says, "I arranged to give you all this Titan data…" The Air Force had done some studies on an advanced Titan, and they'd done a bunch of studies on failure modes and the first indication of failure modes.

So I went through all this material…So anyway, my principles were in control [systems were] that you should always have [an indication] that causes a problem, then you have another verification completely independent. When those two things happen, then you do it. Certainly something as serious as ejection seat, you've got to have the indication of the problem, you've got to have that verification of the problem, and you've got to have the understanding or some other thing to know there's no other solution except this one. These have to be not just something you sort of generally feel; you've got to make very definitive decisions on these quickly.

So anyway, the object was, what kind of indications can we look for? All right, we'll use tank pressure, because we had some indication from the analysis that [the Glenn L.] Martin [Company] had done on the design of the vehicle that the pressures should change by a certain amount before the thing would actually explode. I think we had some things on the hydraulics, which I've forgotten them now. But anyway, I picked out two or three of these variables and we said, "These are the ones we'll monitor," and then we figured out how much time you had, time of your first indications to the time of catastrophic action, and then I made up some kind of a concept of this.

So then at this time the organization is starting to expand a bit more, so people know we're working on this and are [unclear] this. "Oh, you'd better simulate this." This isn't me proposing the simulation for the first time…But anyway, there was a group of people who were associated with operations. Then Chamberlin was also trying to get support of the operations people, so we somehow or other would give them something to do and get them involved. Anyway, they took up this thing, "Oh, we've got to have this simulation." So they started to do these. So we had this simulation to see if the guys could actually make these decisions off these conditions…So anyway, that was the first step.

The second step then was, is that proceeding on this issue of trying to understand reliability of components and the environment, prior to this time the Air Force had done a very large program to try to improve the avionics reliability and they had instigated this idea of testing every component…[Do] tests on every component and [certify] every component to assure that things that went into the vehicles were reliable…

The second step was to review all this data and decide which parts of it we were going to apply to the Gemini Program. Well, what requirements were we going to require in terms…testing…? So since I'd already been involved in vibration, I'd been involved with vibration on the airplane, on the AVRO aircraft, and since this little issue with transistor devices sort of accelerated, the reason we'd gone to transistors was because we didn't like tubes, and environments were an issue to everything we did electronically.

…We established criteria. We also established the kind of vibration testing that had to be done on each component. When we established the idea of random vibration, in addition to sinusoidal vibration, which was the first time it had been written in the specs as such, so, anyway, we made some of these [decisions].

Also there was an issue about Gemini as compared to Mercury. Well, not so much between them, but as compared to Apollo was, is that we were going to put all our equipment on the outside of the vehicle [pressure chamber] for two reasons…One, trying to manufacture a vehicle with all the stuff inside the pressure [vessel was difficult]…Was terrible, because you could only get a couple of people inside the capsule to work on it etc. So you obviously want stuff on the outside, not on the inside.

Two, you've got hazards like fire and other kind of hazards associated. The disadvantage, obviously, is that the environment out there is tougher. But is it really that much tougher than what it is at an airplane at high altitude? So the argument is, no, not really. If we can fly an airplane at 60,000 feet, what's the difference between that and—well, there is a difference, but how much? So anyway, the next step for me was writing and establishing these environmental things. Then in the meantime we were starting to choose hardware.

The next step, from my point of view…we've got to do is…to make darn sure—there's going to be a lot of criticism about the Gemini Program, like there are in all programs, so we've got to make sure that we've got something that's liable to stick together. One of the ways you do that is you make sure you have a…strong team. And in addition, a reason for having a…strong team is that when you've got problems, you've got some resources to fix it. So we don't want just some small little guy who can make some good inertial components, we want something substantial. So obviously if we're going to have a computer, we're going to have a computer on Gemini, it ought to be made by somebody who [unclear]…

"We want something that's pretty good…" We also want something that there's some test behind it. Somebody's got to be on a schedule that we started after Apollo. We've got to finish it before Apollo. That's a pretty tight schedule. [Laughter] You've got to go from the concept of Gemini, of having a Gemini, decided two years after Apollo is decided, and have [the entire] flight test done before any of the first flight tests of Apollo. So we don't have a lot of time, but on the other hand, we've got to have a reasonably capable vehicle, so we can't just fill it with some old stuff…

We decided we wanted to have an inertial platform. Again, we get into this argument that I had had during the whole Phase One and Phase Two of Apollo was, is that I felt that the inertial system should be a four-axis inertial system more like an airplane, whereas MIT's point of view was that, well, we don't need all these things. You can get along with three-gimbal platform, so besides that, three-gimbal platform on a Polaris, and if they use a Polaris on Apollo…I was determined we weren't going to do that. We were going to have something that was more like an airplane and that the pilot didn't have to worry about what attitude he was when he was making maneuvers because of the platform. He could just fly it like he does an airplane.

So anyway, strangely enough, during the CF-105 program, in addition to having this advanced fire-control system under development, part of that was the development of a stable platform, a four-gimbal stable platform. So there was already some background in knowing about these advanced platforms…

None of these things are done kind of sequentially; they're all done together…We get to the place where…we still have this idea you ought to have an associate avionics contractor, but it's still clear that if McDonnell-Douglas is going to be the contractor, they're going to want to run everything. So what do we do?

…In the meantime, we're looking at proposals and McDonnell's coming in with their proposals, what the avionics should look like. We've got all the stuff that we've been working on from Apollo, and then we've got our own prejudices. [Laughter] …This is the decision as made from my point of view…You decide, well…we could have all the logic in the computer as sort of a third try after a Titan. So they've already done a certain amount on the Titan, we're going to do some improvements to it, but we've got all that background behind it. Then on the memory side of it, they've been working on something with Goddard for a memory for one of their satellites. We could take that memory part and now we put the two together and we have a pretty good system.

In one of the McDonnell proposals that come in, they were going to have the display from the computer was going to be a binary display. So you have all these series of lights and you try to read a binary display. So we said, no, we're not going to do that…But anyway, you got a digital display, which was a big step forward…

Then we decided…[on] this platform. We've already [had] the background. In addition to that, in the meantime, that platform was scheduled for another program…[the Centaur Program]. Eventually, Marshall took over that [(Centaur)] system and they looked at the guidance system, and that guidance system obviously didn't have air-bearing gyros in it, so it obviously had to be evaluated. [Laughter]

So they made a great long list of all the things that were wrong with this platform, which I thought was marvelous, because here you could choose a platform. We already had confidence it was a good platform. Here we had the most severe critic you could think of, listing in detail everything possible thing that could be wrong. If you fix all those things, you've got a wonderful device. [Laughter] So that's what we did. We take that platform, we've got all this background behind it…if we can't make that thing work [on time], then we'll never make anything work…

Then the concept of [a radar system,] horizon scanner, [fuel cells, reaction control system, etc.] came up…But there's a whole bunch of these things. Anyway, we went through this, but the idea of having somebody with a history…[and] that had the background of the Mercury control system, had extensive background of aircraft flight control systems, and also were pretty respectable and had some political clout as a company. Then IBM, which up until that time hadn't been in the space program, hadn't got any role. MIT had a role, but not IBM. So we were giving them an opportunity. [Laughter] So it was pretty clear that we were going to be able to get pretty substantial support out of IBM…

The next step was…[to have an] integrating avionics contractor. Now, McDonnell really objected. "We've got our own work." …At least the first draft for a while, we got it to run that way and then we had [to] integrate the platform and the computer together…But on the other hand, it was still along this original idea of trying to have…people working on the avionics who understood avionics…Compared to airframe people [controlling] scheduling and…resources and…[trying to understand avionics and software] problems…It's different than it is for airframes, and to have a tin-bender make decisions about it, to me, was…dangerous. Actually, within the original Apollo Program, the…people who were brought on to run Apollo at the very top actually came out of the electronics industry…

So anyway, in the meantime we were proceeding to choose contractors, get them on [board]…

So that was pretty tough, because we had a very tight schedule, tighter than Apollo. Yet we were trying to do essentially most of what Apollo was doing. But one advantage that Gemini had was, is that everybody wanted to work on Apollo and everybody wanted a piece of something they could say they did. So you had a huge organization with every conceivable person wanting to work on it.

On the other hand, if you have a small organization where there's a limited number of people, you can communicate and you've got a tight schedule and you've got some concern that maybe somebody might want to cancel the project, you have a much better chance of succeeding. Once a contractor works on a contract that he feels the government can[not] cancel…[or] once anybody gets some guaranteed position, then sometimes they can take advantage of that…

The rest of it was just…hard work, of…having lots of meetings and going through all the steps of trying to get Gemini to [be successful]. The advantage we had was, is we were on Gemini from the start and we stayed on Gemini till the last flight. In between there, though, the [un]fortunate circumstance of a change of management between Chamberlin and Chuck Mathews. When Chuck Mathews came on board, he didn't know any of us. There's a certain amount of transition. In the meantime, they bring in new…people…So I ended up having a new boss…

You probably know about all the instances that occurred in Gemini. You know, [they] had this…extravehicular activity planned for the first flight, which was not done with all the finesse that it should have been. Then we had the spin-up and unscheduled recovery.

But in general, I think Gemini was a pretty successful program. We met all the objectives of the Gemini Program by about the sixth launch and then from then on it could be more oriented toward astronaut training and sort of extending of things. So there was a lot of other things in between there, like we set up rendezvous simulations.

There's a little bit of an interesting thing that might be said about that. During our rendezvous simulations, the old idea of simulations for airplanes, you build a big map of something or other, you put a camera on it and then you display this camera on a sphere and then you put the guidance, so that it looks like the whole world to [the crew].

But Chilton started some work within the control system group on computer-generated displays, which is really some of the earliest work on virtual [reality]. [For] Gemini, our target was an Agena. An Agena is just like a cylinder, so mathematically it's relatively easy. So it was not a bad thing to start trying to make virtual displays. [We also had conventional simulations]….

We finally got…our last flights over and we wrote the reports and then the issue is, what do you do next? [Laughter] So I think Mathews decided that maybe I should go work…for Joe Shea [who was the Apollo Manager] and take some of the Gemini experience over to Apollo. So that's what I did for a while…I had…ideas about developing…[an arrangement for managing] multiple sources of data…radar-tracking data, you've got the on-board capability of generated data, and you want to have someplace…where you put all this data together. All the on-board capabilities, the ground capabilities. Have meetings, get this whole thing put together…you need to have some kind of an organizational structure to do that.

So I wrote a great long memo about all the ways you couldn't do it and why you couldn't do it and what the right way was and who some of the potential candidates were… While I was preparing…it, we really weren't worried about who was going to run it. It was just how should we get organized and how should this system work. This was going to be a horrendous activity to pull all this stuff together, all the tracking, all that stuff, both the contractor and the government side and any other aspects.

Then we had the fire. There was a big turmoil between there. Joe Shea left and Low came in…I don't really have much to say [about] that time period…In between all this, Apollo had been going on, I mean, after Phase Two. There [were] a number of management changes and structural changes and there were some configuration arguments.

At one point or another, I made a suggestion that they really ought to use an advanced version of the Gemini platform and put two of them in [the LEM], and apparently some people didn't think I should be making those kinds of suggestions. [Laughter] But I didn't worry about it at the time, but I found out later that they actually came up to me and said, "Well, I guess it was all right for you to make that suggestion." [Laughter] I guess they were kind of telling me politely…There was another issue that had gone on, and that was that there was some difficulty with the guidance and control system being on schedule.

So there was, again, amongst all the management changes, [Robert C.] Cliff Duncan was brought in to…manage the Apollo guidance and navigation control, all the avionics. I don't know exactly what it his title is, but he was going to run it all.

So I had a lot of discussions with Cliff Duncan, and I reiterated my discussions about the structure of the organization of the program ought to be oriented with a contractor being a prime-integrating contractor for all the avionics. Then that should have preferably been an associate contractor with the prime contractor, but if it isn't an associate, they ought to have some distinction so that they could make their decisions. Now, obviously it has to work in the vehicle and so there has to be a decent interface. They can't be fighting with one another. It's got to fit in the vehicle. So I had had discussions with Cliff Duncan about how the Apollo avionics should be reorganized, and Cliff Duncan implemented a fair number of them.

So anyway, by the time the Gemini was over, Apollo was starting to come into shape, even though the fire that really doesn't affect the avionics part of it. Cliff Duncan…decided to [help] start the Electronics Research Center [ERC]…

Jim Elms, who had worked on the Apollo…was…out of Wright-Patterson [Air Force Base, Dayton, Ohio], was going to head…up [ERC]. This was after…Kennedy had sort of got it going. So Cliff Duncan had gone there. Joe Shea had left. [George M.] Low had come on board. Low had a little different way of which he was going to manage things. So I gave him my memo on how…[ground and vehicle operations] should be organized…. But then I said… "Cliff Duncan [has invited] me to go up to up to ERC with him, so I think I'll go…" He said, "Okay." So that was it.

[Low chose Bill Tindall to implement the memo objectives]…Bill…did a tremendous job…

WRIGHT: I would imagine with all that talent and all those challenges, you must have had some disagreements.

CARLEY: Oh, there were lots of them…But if you really think the guy is a pretty good guy, it's a little easier…

Cliff Duncan…was…interested in what kind of an organization should you have…In any organization you really ought to have a planning group… In…research, you ought to have some kind of advanced scheme of where you're going, how do you get organized. So anyway, they established a planning group.

I was…to…be head of the Planning Division…Elms had brought…[on] people… oriented [to the Wright-Patterson organization]. I was…used to the Langley style of NASA[/NACA]. The Lewis [Research] Center [Cleveland, Ohio, Ames Research Center, Moffett Field, California, and]…Edwards Air Force Base…were started by people from Langley, or at least Langley had a tremendous influence on it, but ERC wasn't…It was set up somewhat separately…I went there and we tried to set up some planning. But we did do some things…

One thing I remember out of that, which was amongst all the things that were going on, that one of the things we were trying to promote out of the Electronic Research Center was more use of satellites, just trying to figure out what kind of things could you do with satellites. So there was a lot of discussion about using them for monitoring ships, maritime kind of satellites, [air traffic control, rescue, geographic surveys,] and various other kinds.

But one of the biggest issues that we were always working on was this issue of airline safety, collision avoidance systems, air turbulence detectors, that sort of thing, and air traffic control systems, all the software that goes on on board on the ground and things like that…

Then there were proposals that the Air Force wanted to have some advanced satellites, so the contractors who were going to bid on that would come and visit us. So I remember distinctly one particular one I guess I can talk about it…It was TRW, and they were talking about an advanced satellite for the Air Force and they wanted to know what requirements should be…They were looking at things like whether it should just be two-axis [position (longitude and latitude)] or what kind of capabilities it should have…The basic problem with air traffic control is you don't know altitude, we can separate airplanes' longitude and latitude, but it's hard to separate them altitude-wise, especially if you're dependent on altimeters. So you really need some way that we could more specifically identify where airplanes were.

So my proposal…was, by all means this has to be a three-dimensional surveillance system. It has to give us three-axis data, not just two. So they took that to heart and put it in their [proposals]. I think that program eventually became GPS [Global Positioning System]. So I don't know how many other inputs they had, but I thought at the time that if we had that kind of a capability, then you could redesign the whole air traffic control systems because you would have decent kind of data. It's amazing how much time has gone by and we still are only now just gradually integrating that stuff into the current aircraft…

When I was at ERC there [were continuing] discussion…[on what should be the priorities] after Apollo…Shouldn't we have a space station? What should we have? I'd become a very strong proponent that a space station [should not be the first priority]…It should really aim for this low-cost transportation [to low earth] orbit [system].

…Other people were already working on new ideas…There [were] people who were starting work on a reusable Earth's orbit transportation system. I guess probably some of the top people decided that probably [it should] be done before the space station, although there were other people who thought the space station should come first…[Management decided to create] requirement documents, even before Phase One. The idea was…we can't really tell the contractor what to do, but we can certainly make some suggestions. So I remember there was a great lot of discussions about the title of these documents…as I remember, it came out, "Desired Systems Characteristics…" [Laughter] [At this time we also established a set of advanced technology initiatives to fund technology development.]

…I guess I forgot… During…[the] Gemini development, I think when Shea came on to the Apollo Program, his idea when he looked at Gemini was, well, Gemini really ought to be testing Apollo hardware…A reply had to be made to this.

So Mathews asked me to put together a presentation…of the Gemini guidance and control system, where we were, what our hardware [schedules and] deliveries were, what our hardware status was, why it was, what it was, [etc.]…At that time…Seamans was head of NASA…He…was going to make the decision. So I [went] to Washington [with Chuck] and made…[the] presentation, this is what Gemini is. I think it was three hours in the morning and three hours in the afternoon. I had this big pile of slides. I'd go through every system on Gemini, why it was what it was, what its status was, how many pieces we had ordered. You know, everything from conceptual to exactly where we were on the hardware…Since Seamans had been on the Warrior program on the CF-105, I threw in a few little things about rendezvous…[compared to aircraft interception. Using Apollo hardware would considerably delay the Gemini program.]

Anyway, apparently it helped, at least, in getting the pressure off Gemini, that we could carry on doing what we were doing…It was one of these little things you do in between all the other things…

In the meantime, Seamans, I guess, had come to the conclusion that the next launch vehicle for the military and NASA, instead of them each going off and developing their own vehicles, we ought to try and develop a vehicle for both. I don't know if it was quite that cold, clear-cut, but there seemed to be a kind of a leaning in that direction. So there was a first evaluation of how to set up a…program or something, some kind of a reentry reusable…low-earth orbit transportation, hopefully low cost, or primarily intended to be low cost.

So how do you get started on that? Now, the head of [Manned Space Flight at headquarters]…written some papers on [concepts] and…there [were others]…The French had done some work, the British had done some work, there was something called a trimaran…that had three identical vehicles and you drop two of them off [during launch] and the last one went [on to orbit]. So there was a bunch of ideas about how to do this, how to get the thrust capability and the payload capability out…of available technology, how to make it reusable…horizontal landing, how all the parts of it could be reusable.

So anyway, there was this writing of this "Desired System Characteristics…" Somebody asked me to work on that. There was [a] team set up…These teams consisted of both Air Force people and NASA people. I was on the team to write all the guidance and control and avionics [requirements]. I had more ambitions than that as far as what I wanted to aim at.

In the meantime, while I was working at ERC, one of the things I was asked to do was to go evaluate the supersonic transport control system, because at that time [a US] supersonic transport was being developed and they were having some trouble…Was the [vehicle stability]…and was it a workable control system. So I spent a couple of weeks going over all their [design at in Seattle]…In the meantime, learning what their new technology was for airplanes. They had proposed…very elaborate quadruple hydraulic system for the SST, which…was (instead of doing anything electronically)…all done mechanically…Take all the four inputs and add them all up and put them onto the power actuators. But the concept was a great concept.

Also when I was at ERC, we had set up panels to work with the other NASA centers…Anyway…this…leads up to writing the requirements for Shuttle, and the main thing that I feel that I made some contribution to there, is that I wrote…several requirements for avionics [and] for all systems on the vehicle…All systems should be designed to be fail-[operational]. All avionics and preferably all the other systems, and all the hydraulics, had to be fail-op/fail-op. Which everybody thinks, well, what does that mean? …Well, what that means is that if you have a single failure, it does not change the operating characteristics of the system…One of the problems you have when you had a backup system is it has different characteristics than the primary system. So you have to design all your missions' [procedures] so it can be done with a backup system. So now you have to design the primary system and the backup [procedures and train for them]. You've got two different ways of doing and it's a big mess. If you have more than two different systems, if you have three different systems, it gets even more horrendous. So the best thing to do is have identical systems so that you only have one [set of procedures], and then design your system so that if you do have a single failure, it doesn't change your operational capabilities. So that's the idea of fail-[operational Fail-op/Fail-op is required to maintain consistent procedures even if two single point failures occur. If a single failure occurs early in the flight on option is available to complete the mission as planned]…That's the way the avionics system [was originally] designed in Shuttle. [I arranged with the military representative on Shuttle at HQ to have a delta design evaluation included in the program. At the time the straight wing orbiter concept was preferred by JSC.]

WRIGHT: It sounds wonderful. You were able to get that done.

CARLEY: …[I would like to clarify that this discussion today was not a prepared presentation but was just some thoughts that came to mind. The issues discussed were neither the most important issues nor the most representative, and are a very small part of the activities that actually occurred during the period.]

WRIGHT: Unfortunately for us, we're out of time to hear more, but we certainly thank you for sharing.

[End of Interview]