ORAL HISTORY TRANSCRIPT

ALECK C. BOND
INTERVIEWED BY SUMMER CHICK BERGEN
HOUSTON, TEXAS – 25 AUGUST 1998

BERGEN: This is an interview with Aleck Bond on August 25, 1998, in the Signal [Corporation] offices in Houston, Texas. This is being done as part of the Johnson Space Center Oral History Project. The interviewer is Summer Chick Bergen, assisted by Carol Butler and Cary Radoff.

We thank you so much for coming and spending your time with us, doing this oral history. We really appreciate it.

BOND: Thank you very much. It's been a pleasure to be able to participate in this.

BERGEN: We want to start basically early in your career, in your aeronautical engineering career. You got your bachelor's degree and then World War II came along, and you decided to enlist, is that correct?

BOND: No, it didn't happen quite that way.

BERGEN: How did it happen?

BOND: I went to Georgia Tech. I was in the class of '44, actually, started in 1940, and because the war had started in '41, we went through an accelerated program, and I graduated in October of '43. During the time that I was at Tech and they were recruiting …, I had a desire to fly, and I had tried to get a commission in the—it was the Air Corps then, not the Air Force—the Air Corps. I tried the Air Corps, the Navy, the Marines. All of them turned me down because of poor eyesight, so I was unable to get a commission. I figured that, well, maybe as an aeronautical engineer I may be able to do more for the war effort by going to work in the aircraft plant, which I did.

In Marietta, Georgia, there was the bomber plant that was being completed to build [the] B-29 [Bomber]. It was a Boeing B-29 at that time. So I went to work for—it was called Bell Aircraft was under contract to Boeing to build the 29s, so I went to work in the wing design section of the plant, and I worked there for about a year and then got transferred to what we called Liaison Engineering. In Liaison Engineering, you worked directly on the manufacturing floor. This was an opportunity to learn about how airplanes were built from the ground up.

Actually, to me at that time, I had never seen an airplane bigger than a DC-3, which is fairly nonexistent today, and that was the biggest thing I'd ever seen. I think the only thing I'd ever flown in at that time was a Ford trimotor. That's an old, old airplane. Anyway, the opportunity to be in on the building of the B-29 was really quite something to a young engineer just getting out of school and really learning the ropes.

Working directly in Liaison Engineering on the manufacturing floor, we were kind of responsible for being able to take care of instant problems in order to make sure that we did not slow down the production line. As the airplanes were being built and they moved down the production line, you had to make sure that you kept the line moving, and there were, of course, many instances where there were urgent problems to be attended to by the engineers.

For instance, if there were any major assemblies that might have been damaged in the process of manufacturing, you had to give an instant fix. We had to interpret drawings on the engineering floor for the production, for the shop people, where they didn't understand maybe a change or something like that, or there might have to be a material substitution, things of that nature. Quality control problems also came to our attention. So it was very interesting and an opportunity to learn a whole lot.

I remember one particular Sunday that I was working. It was one of the wing assemblies that was being transported down to be mated with the fuselage, and it was dropped. There are stanchions that pick up the wing, that hold the wing up at hard points in the wing structure, and it was dropped on one of these stanchions, and they punched through in several places. So I was called and asked, "Well, what are we going to do about this?" The regular engineering department didn't work on Sundays, so I had to rush up and pull out the stress analysis books and go all through the design of the wing … and make an on-the-spot repair for those damaged places on that particular wing. I gave it to the shop, they proceeded to make the repairs that day, but the next morning I immediately went to the head stress people and said, "Hey, look, I had to make these repairs. Check me and see if I'm right." Fortunately, I had done the right thing. So, anyway, that was a big thing at that time.

BERGEN: That was a big responsibility for a new engineer.

BOND: It was. It was, a guy just out of school for a year and a quarter, something like that. I learned a lot. I learned a lot on how airplanes were built… Like I say, as long as I was at the bomber plant there, I was not going to be drafted, but as soon as the atomic bomb was dropped on Japan, then I became [free] game for being inducted. I got my induction notice, I guess sometime in September of '45, and I went into the service in November of '45.

At that time, of course, the veterans were coming back from overseas, were being released, and the group that I was inducted with were a whole bunch of engineers that were in like circumstances. One of the things that engineers do well is to print well. They put us in the induction center. This was in Fort McPherson, Georgia, right [outside] of Atlanta. We were interviewing the veterans that were being released and writing up their discharge papers and advising them on the kind of jobs that they could go into depending on the kind of experience they'd had in the services. We were retained there for several months doing all this work in the separation center, they called it.

Before the time that we were to be shipped off for basic to really learn what soldiering was all about, I was promoted to a buck sergeant position, and I went into basic training at Camp Lee, Virginia, as a buck sergeant. Of course, this did not impress the guys that were training us. In fact, I think it teed them off a little bit that here were a bunch of inductees coming in, and we've got several of them that are promoted to sergeant.

But anyhow, we went through basic at Camp Lee, Virginia. I understood that after basic I was going into the Quartermaster Corps, which was really a supply organization for the Army. I said, gee, that didn't seem to make sense, but a lot of things didn't make sense to the Army, you know, but I did write some letters and saw the Inspector General, and before I got shipped out of basic I got stationed at Wright Field with the Air Corps.

They figured an aeronautical engineer ought to know something about wind tunnels, and I got assigned to the Vertical Wind Tunnel at Wright-Patterson Air Force Base in Dayton, Ohio. The Vertical Wind Tunnel was used for doing spin testing on current aircraft at that time, but the tunnel had a major design defect. In a Vertical Wind Tunnel, the air flow goes from bottom to top. You inject the spin model into the air stream in the test section. This test section was open. In other words, there was a distance from where the jet exited from the floor of the tunnel, there was a collector bell at the top that collected the air as it came through, but the section was open so that the models could be injected into it.

But because of the turbulence of the air that was coming from the bottom of the tunnel, the top end had what we call a harmonic resonant vibration at certain air speeds. At certain air speeds there were certain harmonics that would come into play and the whole structure would just vibrate. If you let it continue for very long, the whole structure would collapse. So they were limited to very low air speed regimes [at which] they could run the tunnel.

So my commanding officer, who was a captain at the time, Bob Crawford, said, "Aleck, that's your job of making a fix here." I had worked a little bit in the Georgia Tech wind tunnel, the school's tunnel, and so I knew a little something about wind tunnels. I figured that the way to fix this was to put an enclosure around the test section to make sure that the air flow then was smoothly injected into the collector bell and continued its circuit.

So anyway, I was told—it's a very interesting observation at this point—there was [a World War II] operation called “Operation Paper Clip.” You may have run into that. The German scientists from Peenemunde were brought over to the U.S. under that particular code name, and I was told [by Captain Crawford], "Why don't you go see one of the scientists from Operation Paper Clip." So I said, well, ok. I didn't know who these people were but I called and made an appointment, and there was a Dr. Bernard Goethert, G-O-E-T-H-E-R-T, that I was told would consult with me and give me some advice on what to do.

So I walked into this very small room. It had a table and a couple of chairs, no books or anything like this. I guess he was of medium age about that time. He was probably in his—I imagine around his thirties, someplace along there. Dr. Goethert, he was very nice. We sat down, and he began to derive Bernoulli’s Theorem, which is a theorem of air flows and its characteristics… No books, no references, or anything. I mean, he just started going right through, just like a typical professor would. I guess we finally got to how to handle the problem. He said I was right, an enclosure was the right kind of thing to do, and he gave me a few other suggestions.

So I went off and did an analysis and came up with a preliminary design, but I never did get to see the fruits of my design because I was separated from the service just a few months after that. That was in, I guess, November of 1946. They had decided that they had enough guys in the service and they didn't need us anymore, so I was separated about that time. I always wondered did they go through with that fix or not. I think they did. I think they did, and it was the [right] thing to do.

Anyway, Bob Crawford, Captain Crawford, wanted me to stay at Wright-Pat at that time, and he offered me a civilian—what was called P-2 grade at that time—to stay, but I'd realized that, one, that there was a lot more to be learned in engineering before I got out and started working. I wanted to do aeronautical research. So I had decided that I would go back to graduate school. So I appreciated his offer, but I decided that I would go back to Georgia Tech.

I contacted a couple of my professors back there, Professor Don Dutton, who was the head of the department, and Allen Pope, Professor Pope, who was a wind tunnel authority back in those days. I contacted them and told them I wanted to come back to graduate school. We arranged for me to come back the first of 1947. So I went back to Atlanta and started graduate school, did my master's thesis and completed my course, I guess about the first part of February of '48. I had become engaged to an Atlanta girl, who is my wife now, Anastasia Marinos. We got married on the 29th of February, 1948, and then headed off to Langley Field, Virginia, where I had been interviewed for a job, and we got settled in Hampton, Virginia.

My major at Tech, I had majored, I guess, in both subjects of aerodynamics and also in structures, and, in fact, I did my thesis on fatigue of large aircraft structures. They had just started up a program [at Langley—NACA] to start testing a bunch of the old tired World War II airplanes that were being surplused. At first I thought, well, gee, this would be a program for me to get in on. I had been interested in airplane fatigue.

I got interviewed at the Structures Division, and somebody suggested, "Well, why don't you go over to PARD." That was the Pilotless Aircraft Research Division. Sometimes they called it "the dog food division." Pard was a dog food back in those days. I don't know if it still exists or not. But anyhow, I got interviewed by Dr. [Robert R.] Gilruth, and Dr. Gilruth was a division chief at that time, and I'll tell you, the man—I had learned, certainly, that he was a top aeronautical scientist. He was quite a visionary, and he had come up with the concept of doing aeronautical research in free flight using rocket-propelled models. I think just a couple of years before I'd gotten there, he had started up the Pilotless Aircraft Research Division and the [rocket-model] testing off of Wallops Island.

After being interviewed by that man, he was just such a likeable person and a wonderful human being, that I said, "This is the man I want to work for." So I accepted my job at PARD and I went to work for Paul Hill, who was … a branch head at that time. That was where I met Max [Maxime A.] Faget and Guy [Joseph G.] Thibodaux and a few of the others that had since come down to work on the Mercury Program. In fact, I guess I worked along with Max on a ramjet flight project, and I got assigned to a project to test one of the rocket motors that had been subcontracted to do—it was going to be one of the boosters that they used. It turned out, it wasn't a very good rocket at the time for that purpose. Anyway, I was given the job of testing it, which I did, and then gradually I was assigned to other projects.

But as we progressed—I think one of your questions had to do with did we get involved into manned space flight activity at that time. In the early 1950s, I don't recall that we even addressed the issue of man getting into space. We were beginning to see some studies on satellites in space for doing observations and that sort of thing, but nothing about men going into space in the mid-fifties.

We did begin to see the beginnings, and my boss, Paul Hill, was quite interested in space stations. Back in those days, there were some space station studies. One particular one that I remember fairly well was one started by Goodyear [Aircraft Corporation], and it was a circular design that would revolve and actually present a partial G [to the occupant]. We realized that in zero-G, man was going to be partially incapacitated, probably, so the idea that we had to have artificial G with a spinning wheel was a concept that was latched onto and thought would be a pretty good idea. I remember reading about those kinds of things. Paul

Hill was kind of interested in it. I think he participated to a certain extent, but I personally didn't get involved.

Then there was a big emphasis on trying to help the Air Force with their reentry ballistic missile program, and I was getting involved in doing aerodynamic heating studies with pilotless rocket models to be brought up to very high speed and thus study the effects of aerodynamic heating on different-shaped bodies, and I did a number of programs or projects that involved a study of nose cones and also wing-body combinations. One of the concerns was, as airplanes fly faster and faster, of course, the wings are going to be one of the major things that are going to be impacted, affected by the heating, and leading edges of wings had to be understood and designed. We wanted to understand the kind of heating that you had on various-shaped wings.

So I did some studies on those, and there was a race, I guess, within the group at pilotless aircraft to see who could get the record for the highest speed. At one time, I think one of the models that I flew went to Mach 10.3 or something like that, so I held the speed record for a little while, but it was finally exceeded by others that were able to get Mach speeds of 15 or 16 and that sort of thing on aerodynamic heatings of various-shaped bodies…

But that was a fun kind of a job here, that you were responsible [for, including] from coming up with the concept, designing the kind of shape that you felt needed to be studied, actually putting together the requirements, and having then the mechanical design people put together the instrumentation … that you prescribed and needed in order to be able to do your studies, and then you also designed the booster requirements and did all the analysis on the kinds of speed that you could achieve using different boosters… You essentially did [all of] this yourself. I mean, you were a one-person project, except you got the help of the [Flight] Model Design [Section] people. This was run by Caldwell [C.] Johnson, who had a major part in Mercury programs and subsequent programs in manned space flight. But you'd go to Caldwell, and he would have his people design the rocket model structurally for you, and then you would take it up to Wallops Island and arranged for your flight to occur at a certain time and date …, and you did the launch, and then you gathered the results, and you came back home and you analyzed the information and put out a technical report. That was the product that NACA did back in those days.

It was really a privilege to be able to work in an organization like that. There were very little restrictions. I mean, you could let your imagination just go just about wherever you wanted as long as you had a good justification for what you wanted to do. And we were paid money to do this. [Laughter]

So I was doing a series of projects on aerodynamic heating. One of the other projects I worked on was the Navajo missile, which was a Mach Number 3 cruise missile that would cruise long distances, and it was ramjet-powered, and I did some flight tests on that to study the performance of the configuration. That was one of the major programs I was involved with.

Then as we were getting into trying to come up with materials that would be able to withstand a ballistic reentry of ballistic missiles, I began to look at various kinds of materials that would be able to do the job. We studied different metals. Copper was at that time a [major contender]. [A] very, very highly polished copper surface was one of the concepts that the Air Force had come up with for being able to—and the fact that it would be highly polished would minimize the kind of frictional scrubbing action on the surface and lower the [aerodynamic] heating. So that was a concept that was being worked on.

Then we looked at other kinds of materials, what they called ablative materials. [With] the ablative material, actually there is a chemical reaction between the material and the [air flow] boundary layer; that [reaction] occurs in the boundary layer because these materials were organic kinds of materials. But the boundary layer, even though it was at fairly high temperatures, it still protected the basic material underneath from the very, very high temperatures that could be achieved by the air stream outside of the boundary layer.

That was the basic concept. I studied a number of materials. Teflon was one of the good ablators, but it had some other properties that were not quite as good as the kind of material that [we] ended up testing for [use] on the Mercury.

I'm kind of getting ahead of myself a little bit. I guess it was sometime in 1957—and I have brought along a copy of the report that I did on experimental ablation cooling, and this report gave the results of a number of materials that were studied in ground test facilities to study the physical properties of how ablatives worked in very hot air streams. Back in those days, the only way that you could get a very hot air stream was either through using an electric arc to heat the air, or one of the new innovations there at Langley was a pebble bed heater. It was a long column, cylindrical column, that was filled with zinc oxide pebbles. These pebbles were then heated to a very, very high temperature of [2,000] to 3000 degrees [°F], and once they were heated, then the air was flowed, at fairly high velocities, flowed through these ceramic pebble bed and picked up the heat and then exited as a regular wind tunnel. We used, again, specimens of different materials to study the effect of heating on them. That was one of the facilities that I used.

Another [test facility] was [at] the University of Chicago. I arranged to use [this facility which had formally been] an old streetcar barn where they had big electric generators to produce an arc jet that would produce a bigger jet than what we had at Langley and at fairly high temperatures—again, in the 2 to 3000-degree range—and I took several models up there and tested them.

So when the Mercury Project came up, and some of the results were in this report of those tests, but when the Mercury Project came up, I was called. Dr. Gilruth and Max Faget and two or three others had been doing some studies and making frequent trips to Washington to brief our people up there on the possibilities of doing a manned space program and the concept of a Mercury-type vehicle. So on one of their trips back, Max called me and he says, "Aleck, how would you like to work on Mercury and do a proof test of the Mercury heat shield?"

So I said, "I need to talk to you more about that. Just what does this involve?" I went over and we had a long talk. He told me that I would be asked to be a project engineer on the research vehicle that would be flown on the Atlas booster using the then-determined shape that Mercury was going to use, but the purpose would be to be more or less a proof test of a shield, and he asked what material would I prescribe for an ablative material to be used for the Mercury shield.

So I was pretty well convinced as a result of these tests and also the General Electric Company, who was, of course, involved with the Air Force in doing ballistic missile entry studies, had just recently flown a ballistic [nose cone] which was a fairly sharp nose cone, using resin, phenolic resin, glass and phenolic resin material, as a combination for the ballistic entry nose cone. In fact, I had even gone up to GE and got an opportunity to look at [the recovered test article] and examine it … and talk to their engineers.

So I was pretty well convinced that the ablative-glass-phenolic approach would work for Mercury. I had one doubt, and that was the difference [in reentry time] between a ballistic missile entry and that required for a manned vehicle was going to be a lot different. For the ballistic entry, you go up and then back down. The time that you spend during entry is relatively short. Therefore, the heat pulse that the material sees is short, and it doesn't take a long time for the heat pulse to travel through the material and heat up the core part of the material.

For the manned entry, because of the G limitations of man, the vehicle has to enter more or less at a grazing kind of an angle to the atmosphere and gradually come in so that you don't impose high Gs on the human occupant. So the concern was, well, now the heat pulse was going to be spread over a longer period of time, and that would give the heating an opportunity to progress into the basic core material, and the question was, would this affect the structural integrity of the shield. So that was really one of the main focuses of doing a proof test on this material on the kind of trajectory that you would use for the manned reentry.

Anyway, when I was first approached to come over and take that job, I told him, I said, "Well, I need a little time to think about this." I believe maybe he said, "Well, take twenty-four hours and come back and tell me tomorrow." Anyway, I went home with much concern, thinking about what all this was going to involve, not only taking on a big project here, but also the idea of maybe having to move from Langley. We were settled in, had built a home in the area, and we were pretty well settled in there, and the idea of maybe moving to some other place was a concern. So I went home and I talked to my wife, and we had a long discussion, and neither one of us wanted to move.

So I guess I came back with some reluctance and told Max, "I really am not sure that I want to do this." At the time, the Manned Spacecraft Center was purported to be established at Greenbelt, Maryland, where the Goddard Space Center is now. We weren't sure we wanted to go up there. Anyway, after I told him that, then Dr. Gilruth began to kind of put the pressure on me and tell me, "I need you," …, and "This is a wonderful opportunity," and all that kind of pressure began to work on me.

I said, "Well, I guess I'll have to go back and talk to Tassie (that's my wife's nickname) and convince her that this is the right thing for us to do." I went back and talked to her again that evening.

The next day I told Dr. Gilruth I guess I would accept the job. So I went to work on Mercury, and I was one of the first thirty-five to be named to join the Space Task Group. Taking on that job, you know, at first it was a tremendous kind of change in responsibility. I mean, it was much more responsibility than I had—I had risen to be a section head at PARD at that time, and I had eight or ten people working for me, and, of course, I was more or less my own boss and was able to do things on a more relaxed kind of basis. But getting in the

Space Task Group with a program that was going to have some milestones and schedules that had to be met was going to mean a lot more pressure … and, of course, a lot more people that I would be responsible for supervising and managing.

After a few weeks of that, I began to get the hang of it and things sort of fell into place, but it was still quite a significant change in the kind of things that I had been doing and how they were being done. So we set out to design what was called—the test vehicle was called "Big Joe," and that was in comparison to a group of smaller test articles that were going to be used on Mercury that were called "Little Joe." Now, Little Joe came about because of the four rocket motors that were used in the booster configuration for Little Joe. Little Joe is a term in playing dice, that is a four in dice. When you throw a four, you call it a Little Joe. Anyway, Max, I believe, came up with that name, said, "We're going to call the other vehicle Little Joe. The big one we're going to call Big Joe."

There were, I guess, some, I don't know, forty to fifty or sixty specialists and engineers that were pulled into the Space Task Group that became involved on Big Joe. I remember doing the initial thermal analysis, and I did it on a slide rule. We didn't have the computers that we have today. We had Friedan calculators and slide rules. All my analytical work was done on a ten- to twelve-inch slide rule, and I did my initial computations, and we had several ladies that did our analytical computations. I would turn over the computation to them, and they would check me out, you know, to be sure that I hadn't made any mistakes or errors along the way.

We came up with—well, what we were trying to do is judge what thickness of the shield we ought to use. I came up with—that a one-inch thick shield would suffice, but said, you know, I'm not taking any chances. This is a research vehicle. We doubled it. I doubled it, [to] two inches. Went up to General Electric [Company], and we had a contract for them to actually manufacture the shield, and we had the shape that we wanted to use on Mercury, and so they manufactured it.

We went into the design of the rest of the vehicle. NACA was the mother organization, and it became NASA in October of 1958, when General [Dwight D.] Eisenhower signed the Space Act, and I guess it was sometime in early November that we started to design the Big Joe. The design of the Big Joe involved a control system, a cooling system, a parachute recovery system, and all that good stuff that we had to have, as well as the manufacture of the heat shield and the body that it was going to be put on and also the instrumentation that we would require. We had fairly extensive thermal instrumentation and also instrumentation for analyzing the orientation of the vehicle…

This all was done in a period of less than nine months, which in today's time is just unimaginable, to be able to sit down from concept to completion of an article in that short a period of time. Today it would take two or three years at least. Anyway, we did all the ground testing and everything that we needed to do, took it to the Cape in, I guess, late August, first part of September, and I think we flew it on September the 9th, if I'm not mistaken, of 1959.

An interesting thing happened a couple of days before we were going to launch. Again, this was something that could not occur today. Scotty [Scott H.] Simpkinson—Scotty was a good friend. He has since passed away. He was my instrumentation specialist on this vehicle, and he and I had worked closely together. He was from Cleveland, and we had worked closely together on some other projects in the past. We were given a hangar, Hangar S at the time, to do the Mercury Project out of. Like I say, a couple of days before launch, in the checkout we detected that the barostats, the devices or the instrumentation that were going to be measuring the [static pressure of the] atmosphere as we reentered and [which] then would give signals to deploy the chutes, that there was some appearance of malfunction in the barostats that were installed.

So he and I and Max and, I think, one of the other guys that was working with us—and we literally took the barostats out of the vehicle, brought them back to Hangar S, and tested them. We found the malfunctioning parts, put them back in, put the new parts back in, calibrated, took the barostats back out to the vehicle—this was the night before launch—installed them. We didn't have any QC [Quality Control] people hanging over us or telling us what we could or couldn't do, installed them, flew the vehicle the next morning. We had a very successful flight.

The Atlas booster that we were using at that time, that we used for that launch, actually had a little hangup. The first stage did not stage. In other words, it did not come off. There was no malfunction as far as—but it continued to hang on [failed to separate from 2nd stage], which meant that it carried somewhat more weight than it would have had, so we didn't reach the kind of velocity that we wanted to achieve. I think we were about a couple of thousand feet per second below the designated speed. The vehicle continued on the trajectory. We had a somewhat slightly different trajectory than the prescribed trajectory that we were trying to fly on Mercury, but we were able to achieve heating rates that were pretty comparable to the kind of rates that we were going to see on Mercury. The shield was recovered. At first we thought it was lost for a while, but the Navy was able to find it within—gee, I guess it must have been two or three hours after it impacted—and picked it up. The shield was intact. Everything was fine. We brought it back to the Cape, examined it very thoroughly.

BOND: We got all the instrumentation data … and analyzed it, and the shield showed fairly minimal ablation and heat soak-back. Thus, we were able to take this data and give it to the McDonnell people and tell them that they could now take the shield down to the one-inch thickness that was finally used for the production shield that was used on the Mc production models. So it was a success.

We had a back-up flight article ready to go with another ablation shield. We didn't have to use that because we were quite satisfied with the results. There was also an alternate design that had been carried on just in case the ablation approach didn't work, and this was the heat sink approach. With the heat sink, beryllium, which is a metal, was chosen because of its very fantastic properties. It's a light metal and it has a heat capacity, an ability to soak up heat and contain it, much greater than most any other metals. It surpassed aluminum and copper… It was able to really soak up and hold heat, about three times the capacity of these other metals.

So we went to the beryllium company people and asked them if they could manufacture a shield. At that time, that material was a classifed material that was, I think, for the war effort, and the Atomic Energy Commission Agency had to be asked if they could release the use of that material for the Mercury Program, and we finally got the okay. The beryllium people said, "Well, we've never manufactured anything as large as that." That was going to be somewhat of a pioneering effort. Anyway, they did make a shield large enough for the Mercury, and we had two shields manufactured for two vehicles for testing in case the ablation approach didn't work.

I should say the beryllium had a couple of drawbacks. One, it was very, very expensive, a very expensive process of getting that much beryllium to make this large shield. It was also a material that was toxic in the manufacture. If a person breathed the particles of it, they could have a disease called berylliosis, which was an infection in the lung that actually could become cancerous. So they had to use precautions in its manufacture. It also had another big drawback that, fortunately, we did not have to prove out.

With the fact that it was going to be containing all these thousands and thousands of BTUs [British Thermal Units] of pent-up heat in the shield, the shield would have been at maybe 1500, 2000 degrees [when] it impacted the water. Well, the concern was, well, you take this very hot piece of metal and suddenly subject it to cool ocean water, what is going to happen? You're going to have an explosion. So that was the other major concern.

Fortunately, we didn't have to face that problem or try to work it. The ablation approach proved to be the approach to go. That was the story on the heat shield.

BERGEN: Did you have any heat sink proponents that you had to convince?

BOND: Well, I think we had a few doubting Thomases about the ablation approach, and that was why we had to go the heat sink approach, but it would have been interesting to have flown a beryllium shield just to see just what kind of a reaction you would have, and I think it probably would have been very exciting.

BERGEN: Were there any other concerns you had when you did your ablation testing, about maybe the shield not being the first to enter the atmosphere, enter in the right direction, or things of that sort? Were there any concerns that you dealt with?

BOND: Well, the aerodynamics of the vehicle and the control system assure that you would reenter the atmosphere with the shield facing forward and at the right [entry] angle. The Big Joe, the center of gravity was slightly moved off the center line just a few inches, and this actually made the vehicle reenter in a helical kind of a motion. The center of heating was not exactly in the center of the shield, it was slightly offset because of that reentry motion that it had. But that didn't affect the shield. The heating was fairly uniform, as theory [predicted], going around [the] radius of the shield.

We measured the heating on the after-body and were not surprised at the levels of heating. We were able to accommodate it in the design, in the Mercury after-body. But it did provide very, very valuable information to the designers so that they could go back and make sure their designs met all the requirements.

BERGEN: I read that you put a letter in the capsule before you— [launched]

BOND: Yes, we did. All those that worked on Big Joe signed a letter to Dr. Gilruth to the effect of, "This is your first message from space," and, of course, we took a picture of [the group giving the letter to Dr. Gilruth when] we brought the vehicle back [to the hangar]. We gave him the letter, and there is a picture of Dr. Gilruth holding the letter and reading it with several of us gathered around.

BERGEN: That's an interesting story. I like that story. Would you like to take a little break right now?

BOND: Yes, I'll take a little break. [Tape recorder turned off.]

BERGEN: So we finished up with the successful completion of the Big Joe project. What did you go into next?

BOND: Well, my next assignment was being the project engineer for the first production model of the Mercury capsule, and it was designated MA-1, Mercury Atlas One. Its purpose, of course, was to test several of the entry systems and also the heat shield at conditions of entry, you know, the Mercury conditions. McDonnell shipped us the capsule. In fact, that was back when we were still at Langley, they shipped the vehicle to Langley. We did some work on it. I remember some of our instrumentation people put some additional instrumentation in it.

Anyway, it was to be flown at the Cape [Canaveral, Florida] [sometime] in 1960. I'm not sure of the exact date. That was the summer that I took my family to the Cape, because I was gone quite a bit of the time, and in the summer months when the kids were out of school, I was able to take them along with me, and they spent the summer of 1960 down at Cape Canaveral with me.

We flew the vehicle. That brings back some sad memories, really. In all of our mission activity at that time, we had always sat down to establish mission rules and under what kind of conditions we would fly … and we had prescribed that there would be certain kind of weather conditions that we would not fly under… I remember being in the blockhouse that day as we were counting down. It was in the morning hours. We were going to launch, and a very, very heavy front began to come in over the Cape, and it was raining quite heavily. I was in the blockhouse, and the way you take a look out of the blockhouse, there was a periscope that goes out of the blockhouse and allows you to take a look down at the vehicle to see what's going on. An hour or so before launch, I went to the periscope and took a look out, and it was really just terrible weather, overcast, raining, wind blowing… I said, "Well, we'll never launch in a case like this." We said in our mission rules that those were not acceptable launch conditions.

I talked to our flight director, and he said, "Well, don't worry about it. We'll proceed on with the count." We continued into the count, and things began to get worse. I kept telling the flight director, who was Walter Williams, "Walt, we can't launch in conditions like this. We won't get the photography that we need, and if something happens, you know, we just won't be able to see visually what happened."

He says, "Oh, don't worry about it. We're going to go ahead and proceed with this launch." He says, "We've got a schedule to meet," and all that kind of stuff.

I kept resisting, and at T minus ten or something like that, I said, "No, it's impossible. Let's don't do it," etc.

Well, even though I was project engineer, I did not have the authority to countermand the launch director. He went on and launched it anyway. The vehicle rose up. I don't know what the altitude was. It was about fifty-some seconds into the launch, and everything went haywire. Of course, the wind was blowing and howling and forth. There was a heavy cloud cover. We didn't get any kind of pictures.

So we immediately had to start a salvage operation. It wasn't too far offshore that all of the wreckage and debris fell. So we started a salvage operation to recover everything, and we did. We brought everything back to Hangar S and began to actually rebuild the vehicle, the booster and the test article. If you remember seeing the pictures of the Pan Am 747 [that exploded off New York last year], you will see the reconstruction of an article. That was the kind of reconstruction that we went through with the MA-1.

In looking at the salvaged material and also the instrumentation, we began to conclude that there was some kind of a structural problem at the nose end of the Atlas booster, and a very intensive analytical [structural] analysis was undertaken by not only the Space Task Group people. We had our own loads and stress people who began to dig into the details of the structural problem, but also people at McDonnell. The Air Force contractor at that time was Aerospace Company, and they had their specialists. We had a number of meetings back and forth on what the cause of the problem. Well, the upshot was that, you know, there was finger-pointing in both directions. The booster people were pointing at the McDonnell adapter that held the vehicle and attached to the top end of the booster. We were, in turn, pointing at the head end of the Atlas, that it had a structural problem in the front end.

I don't know if you know about the Atlas booster. It's essentially designed as a balloon. As long as there is pressure in the Atlas, it is structurally sound. It needs [internal] pressure in order to maintain its shape and its rigidity and also be able to take the structural loads. Not that that had anything to do with the problem. Up in the nose end, where the adapter attached, it was reasoned that there was some structural weakness up in that area, and I think we ended up making some modifications to the adapter as well as some heavy strengthening or reinforcing material was put on the Atlas.

I think everybody was satisfied with the fix, and we went on with MA-2. MA-2 was flown successfully. In looking back at that incident, I think maybe we could actually consider that [it] was fortuitous that the Atlas failed at that particular point in time in the program, because it allowed us to make the fix, strengthen the Atlas, and do what we needed to do in order to be able then to progress orderly into the Mercury Program without maybe down the road some time, the problem happening on the manned flight. So we were more or less lucky to have found the problem early.

So we went on with MA-2, then the subsequent test articles to prove out. I think there were—I don't recall exactly how many unmanned shots before we finally went to John [H.] Glenn's [Jr.] first manned orbital launch.

BERGEN: You were working on Project Mercury, and we were hoping to have the first man in space, but Yuri Gagarin beat us to it. How did the people you worked with take that?

BOND: Well, we had our own concerns and problems. We had to be realistic. We were not quite ready, and you never try to push ahead and gamble on things without being sure. That was one of the things about the Space Task Group and all of its leaders and all: we were going to be extremely conservative in whatever we did. In spite of the fact that the Russians had already put up a man in space, we were not going to hurry things along, just to be second, anyway. You wanted to be sure, if you were going to be second, that you did it right. So there [was] not any undue pressures or unusual kind of pressures to, "Hey, we've got to get up and do this thing faster." Of course, [Alan B.] Shepard's [Jr.] flight gave us a niche into being able to fly a man in space with that vehicle, and then we progressively continued our work, building up to John Glenn's flight.

BERGEN: What did you think after Shepard's flight when President [John F.] Kennedy announced that we would go to the moon before the end of the decade?

BOND: Well, I know we were all sort of, kind of—it was mind-boggling to think about, hey, we were just beginning to scratch the surface on how do you fly a man in space here, and we weren't even sure then were we going to be able to do it, and all of a sudden President Kennedy says, "Hey, you're going to the moon."

I think there were a lot of people who had done more detailed studying on the go-to-the-moon kind of technology and were we ready to be able to undertake something like that, and there were certainly a lot of people that were back at Langley that had done those kinds of studies and were pretty confident from a technological standpoint that there were not any real breakthroughs necessary. So I guess we sort of took it on as, "Well, I guess we can progress step by step and see how it can be done," which we did do.

There was a lot of help along the way, not only within the Space Task Group organization and then when we became the Manned Spacecraft Center. We got a lot of help, of course, from our sister centers. In fact, I think there was an overemphasis by the other centers to suddenly get into the space business because they found it, I guess, very exotic and dramatic at that time, and it was the "in" thing to do, and maybe there might have been a little overemphasis in some of the other centers to get some of their people into that, and maybe the aeronautical business kind of suffered for a while. I think NASA owned up to that later, yes, they had let some things go that were still very pertinent and necessary in the aeronautical side of the business, particularly the design and development of smaller aircraft and some of the faster aircraft for commercial purposes.

BERGEN: About this time plans were being made to move to Houston.

BOND: Right.

BERGEN: So how did you feel about moving from Virginia to Houston?

BOND: Well, I'll tell you. We watched events. I think it was announced that we were coming to Houston sometime in—what was it, August of '61? Then we heard about Hurricane Carla and that it had hit Houston, and I guess our thoughts were, well, maybe they're going to change their mind about going to a place like that, because, you know, hurricanes can have a dampening effect on your enthusiasm to go someplace. Anyway, September the 12th, I guess, 11th or 12th, was when Carla hit, and we had been told just before that that we were coming to this area.

I guess in November of the year, I think we sent down a few advanced people that had come down and got settled here, but the rest of us were offered orientation flights, and mine and my wife's were, I think, around early January. We came down here in January to look at the area and look for homes and building sites and all that kind of stuff. As I recall, that January was one of the coldest Januaries that I think Houston had experienced in many, many years. Temperatures got down into, gee, into the teens. Here was the energy capital of the nation, essentially, and it was embarrassed that they were in short supply on natural gas. Many schools had to close down because they could not heat the schools. Many businesses shut down because of energy problems. The only place where I think I felt real warm was the motel that we were staying at, and I forget the name of it. It was up near Hobby Airport. It seemed every place else we went—we had a couple of real estate agents taking us around to show us homes and all, and every home we went into, the families that were showing them were gathered around in their den and their fireplace because that was the only heat, from a fireplace fire. The natural gas supply was reduced so that they couldn't heat their homes well, and it was really a cold, cold time.

But then getting back down to looking at the area down here in the Seabrook [Texas] area, our real estate agent took us along the waterfront here in Seabrook along Toddville Road and some places like that, and all you could see was bare slabs of homes that had been swept away by the hurricane. That was kind of disturbing, to see, hey, we were going into an area that had this kind of thing happen to it. We ended up buying a piece of property where I'm presently living, in Timber Cove, and still live there.

We went back to Virginia, shaking our heads, wondering what are we really getting into going to a place like that. But we made the move. I transferred down officially, I think it was in February, the first part of February of ['62], and then my wife and children, two girls, came down in July of ['62], and we got settled kind of quickly. Actually, we were fortunate in that a lot of our friends and their children and all settled in the same places, and it really was not as much of a transition for our kids as we thought it might be. It was more of a transition to ourselves.

Personally, I had trouble with coming from Virginia, where the vegetation was nice and lush and plentiful and green, coming to—the Clear Lake area at that time was just a vast prairie with no trees on it, hardly. That was one of the reasons we settled in Timber Cove; it had a few trees. It took me about a year to finally adapt to wanting to stay in Texas. But we've got our roots here now, and we're here to stay.

BERGEN: So you moved down to Texas. What were you doing at that time then?

BOND: Well, when I moved down to the Manned Spacecraft Center, I was assigned to head up the Space Evaluation and Development Division, SEDD. It was a conglomerate of quite a number of engineering disciplines. We had tracking and communication, avionics, structures and mechanics, propulsion and power. All of that was combined in SEDD at that particular time, and I was appointed as chief of the division. I had an excellent assistant division chief, Joe [Joseph N.] Kotanchik, who was an excellent structures man, had come from the Structure Division at Langley, and a number of other very good branch heads and all that all became—most all of them became division chiefs.

When we began to expand the Engineering and Development Directorate, which was headed up by Max Faget, we then separated SEDD into several divisions, creating Propulsion and Power and Crew Systems, Structures and Mechanics, Tracking and Communications. Then we formed another division, Space Environment Simulation Laboratories [SESL] Division. But in ['62], that was my job. Actually, it was housed for a while … [in the Rich Building on Telephone Road], and then we moved … [to the Air Force base at Ellington]. The Rich Building, that was an old warehouse that we converted into a series of laboratories and offices. In fact, when President Kennedy came down, we actually toured him through our labs and all, and we had the lunar module mock-up in that building at that time that was shown. [SEDD was initially housed in 1962 in the Rich Building and later—sometime in late 1963 I elected to move the division to Ellington because of the high density crowding as we increased staffing. Ellington offered the opportunity to move into quarters where we could have considerably more office space for our personnel.]

That was before the center was built, of course, and when we first came down we were in, I think, about thirteen different locations around the City of Houston, different offices. We couldn't find enough building space in one building to house every activity. Anyway, Telephone Road [and subsequently Ellington] was my office space and lab space up until the time that we had the center laboratories completed in the 1964-65 time period. I moved on to the center in 1964 when Building One was completed. Max pulled me up to the directorate office and asked me to help kind of manage the activities of E&D, more or less looking over those divisions that were broken off from SEDD.

BERGEN: So if we go back to when you were in charge of the SEDD, you kind of helped put all that together, didn't you, help bring the people in and establish all those different sections?

BOND: Well, of course, we were in a tremendous recruiting campaign at that time, and we were recruiting specialists and engineers from all over the country. Of course, we pulled some from the other labs of NASA. Yes, that was one of the big, big jobs at that time, was to build up the staffing and bring in technically capable people in order to be able to run the Mercury, the Gemini, and then also get into the Apollo Program.

BERGEN: You were heavily involved in developing test facilities, correct?

BOND: That was one of my big jobs. We started that when I was head of SEDD. Before we left Langley even, in the Space Task Group, we began to put together our thoughts on what is a new center going to require in the way of test facilities. We began to conceptualize several kinds of test facilities that we were going to need, and they began to be formulated and put together. Some of the major facilities that we were thinking about that really were brought to fruition down here were the Structure and Mechanics Laboratory that we have today, the Arc Jet Test Facility, which was used for doing a lot of the testing later on materials for the Shuttle, and also we did a lot of tests on our Apollo materials also.

Vibration and Acoustics Test Facility, which is a facility that was built to test the full-sized articles on the Apollo vehicle, the lunar module …, and also the command and service modules all together is one big piece, to test them in the vibration and acoustic environments, vibration first. It's a two-sided [two towers] building. One side of the building will impose the acoustic kinds of environment that you have from the motors … and the external aerodynamic forces and noises. On the other side, the mechanical vibrations that occur as a result of the forces of the rocket motors and also the wind stream. So that was another major facility.

The Thermochemical Test Area was conceived as a test area that we could do testing of small rocket motors, the reaction motors. Larger motors, of course, were tested at Huntsville, at the Marshall Space Flight Center's facility there, and also at Michoud [Lousiana]. We were also given the responsibility of conceiving the test facilities out at White Sands for a time, and then it was decided that that ought to be a separate entity, and E&D was finally divorced from having to consider it. There were some others brought in to do that job.

But the other major test facility was the Space Environment Simulation Laboratory that was conceived to be able to test the complete Apollo command and service module in the conditions of the deep space environment, not only the vacuum, not only the hard vacuum, but also the thermal environments, the very, very cold coldness of space and the solar radiation that you see in space, which is very intense in the deep space environment. That was one of the more or less major challenges. It became a major challenge to me, I guess, once we took that on.

Going back a little bit, up to that time in 1961, to my knowledge, there was very little done in making and building large-space vacuum facilities. I think McDonnell and GE had a couple of medium-sized chambers. I believe they were thirty-nine foot in diameter, and they would go to a medium vacuum. But outside of those two facilities, there weren't any that would simulate really the hard conditions that you see in deep space. So, like I said, the only laboratory facilities that could achieve deep vacuums were little belljar facilities of only two or three feet in diameter, maybe. So when we undertook to build a chamber that was going to be 120 feet tall and 65 feet in diameter to be able to give these kinds of conditions, it was really one tremendous step, much beyond what the technology could support at that time.

Anyway, we were assured by people we talked with—we talked to the people in Tullahoma [Tennessee], who had been studying the building of such a chamber, and, in fact, I pulled one of their people, Jim McLane, who actually had worked on the Tullahoma Project, and brought Jim over and eventually made him chief of that division. But in talking with the specialists in the vacuum business, the kinds of pumping that were available and things like that, they assured us that we could achieve those kinds of conditions.

It was a major challenge to be able to develop the refrigeration systems that were needed to give us the coldness of space. The coldness of space is somewhere around 3 or 4 degrees Kelvin, which is almost down to absolute zero, and to be able to achieve those kinds of conditions, you needed to have helium refrigerators that could cool down the insides of this chamber to those kinds of conditions.

The other major part of the problem was the solar simulation. Solar simulation at that time was somewhat in its infancy. There were two techniques that were being looked at. One was using carbon rods in an electric arc to produce a light that would match the spectral conditions of the sun, and another was a xenon lamp that would also match it, but not quite as well as the carbon arc. We ended up with going to RCA [Radio Corporation of America], who at that time was developing the carbon arc, and having them do the carbon arc solar simulators for the Space Environment Simulation Lab.

We started the design, I guess sometime in mid-'61 with the Bechtel Corporation, who is a designer of large industrial-type facilities, and the period of design went on for two or three years and then went into the construction process. We thought we were ready to go about the spring of 1965, when we did our initial pump-down on the big chamber, and, lo and behold, we experienced a structural problem. The front end of the chamber around the forty-foot door, opening door, actually gave in [deflected] and it leaked, and we had a major structural problem on our hands. It took a year to solve that problem.

We had tried to convince the [U.S. Army] Corps of Engineers, who were the actual constructors for the center, to allow us to go into a model test program before we actually did the initial design [of the chamber]. They said, "Aw, we don't need that. We can do the design on this. Bechtel knows how to do it," … Well, Joe Kotanchik, who was, like I say, really an experienced structural man, he says, "You know, we really ought to go into models." After the structural failure, we built models and tested them, proved the design fix, and went back and fixed the chamber. It took, like I said, about a year to get the chamber operational. So in early 1966 we actually had the shake-down and successful pump-down of the facility. But that was a major undertaking, a major challenge.

The other part of that was, in order to assure the safety of the people that would be inside that chamber in those kinds of conditions, we had to be able to demonstrate that we were going to be able to have these people come out safely, and it had to be a manned operation. We had to be able to repressurize the chamber into conditions where rescuers could enter in, save whoever was involved in the test, and make sure we brought them down to a breathable atmosphere and safe atmosphere in a minimum of time. Those were some major pioneering engineering activities that had to be addressed. So, yes, that was one of my major challenges.

BERGEN: Who were some of the experts you worked with to solve some of these problems?

BOND: [H.] Kurt Strass was another colleague that I had worked with at Langley and the PARD. He was our first division chief of the SESL. Another guy that was really one of my mainstays in vacuum technology was a fellow by the name of Rich Piotrowsky. He was one of the few people that came to us that had any real background in vacuum technology.

We had several other guys that were real good. Now you're taxing my memory here. I think I mentioned Jim McLane. Jim I brought in from Tullahoma, and he actually worked with me on staff, working with several of the facility problems that we had, and then later, when Kurt Strauss transferred to headquarters, I put Jim in as the division chief of the SESL.

BERGEN: So after you got these facilities constructed, did you manage the operation?

BOND: Yes, that was the job to do then. Not only were we responsible for laying out the requirements and making sure that we oversaw the designs and development, then we had to staff them and operate them. At first I thought we were going to be able to staff these facilities with civil service people, and come to find out that headquarters and the Congress thought otherwise, and they said, no, we have to open this up to support contractor people.

We actually had a proposal for staffing E&D facilities. Again, that was something that I managed and ran, was this proposal activity, and we had several of the major proposers that—we had quite a competition, and it was going to be a big job. The Northrup Company was selected, but there was another company. Brown & Root-Northrup had put together a team effort to come in and do the contractor support, and it worked out very well. We had an excellent relationship with those people.

BOND: They brought in some experienced and knowledgeable people in those kinds of operations and really they did a very, very good job.

BERGEN: The test facilities were designed, basically, to ensure that all the equipment and systems were going to work in the space environment. Can you think of any specific examples of things where the testing facilities really found some major things that needed to be changed or fixed?

BOND: Yes, I think all along the way. The one thing I wanted to mention before I answer your question. All the while when we were designing and getting ready to build these facilities, I remember a trip that I had taken with Dr. Gilruth at one time, and we were talking about what we were doing …, and how things were coming along. This was a way of giving him a briefing, more or less, on an airplane, when we were traveling some place. He said, "Aleck, in all this work that we're doing on building facilities for the center, I'd just like to make sure that we don't have any white elephants."

And, man, that really got to me. I said, "Dr. Gilruth, that's one of my main concerns, too." Because we were putting a lot of money into these facilities. At that time, we were talking about hundreds of thousands of dollars, and millions of dollars, and that was quite a concern, you know, that we wanted to make sure that everything we were building there was a good justification for, and that there was a real need for. The kind of things that we were looking at, the kind of facilities were more or less unique, one-of-a-kind facilities that did not exist anyplace else, and that we were going to be able to push the technology capability envelope beyond the current state of the art of what had existed then. In other words, we were doing more or less pioneering effort in extending the capabilities in each one of these facility areas because of the need to be able to go to those kinds of conditions that were envisioned for the Apollo Program…

That really made an impression on me whenever he said that, and I said, "I have thought about this many times and I have many sleepless nights over thinking about, well, is this really necessary, is that necessary…"

Well, as it turned out, every facility that we built had major impact and usefulness on the Apollo program and on Shuttle. The Vibration and Acoustic Test Facility [VATF], I think, was able to point up some necessary areas where maybe the structure might have to be beefed up a little bit to keep down any resonances or problems of that kind.

The SESL laboratory, we came across any number of problems in the command module that needed to be improved, or the design … changed. They were invaluable in doing suit testing of the suited astronauts. This was an experience that cannot be gotten anyplace else, particularly for those astronauts that had to go onto the lunar surface. It was just the only place where they could get that kind of exposure in the environment, and there were many, I guess, structural tests were done in the Structures Laboratory that proved the need for some structural strengthening.

Thermochemical Test Area [TTA]. We did all kinds of tests on the reaction control motors. You know, we had the problem on Apollo 13, with the oxygen tank on the fuel cells. Had it not been for the capability of the Thermochemical Test Area, where we had a separate lab, that was designed specifically to be able to test the fuel cells. We had that kind of capability. The other thing that the Thermochemical Test Area was used for was being able to test the initiators that were used in all of the pyrotechnic circuits. We had to certify the pyrotechnics and develop pyrotechnics that went on the various vehicles, and this was all because we had those kinds of capabilities.

As far as any major kind of design deficiency …, I can't put my finger on any one kind of thing, but there were a multiplicity of things along the way, in the development process, that you find out, that you've got to correct or change …, and go on from there.

But the other thing that these facilities were used for were for certification. We had to be able to certify a design for the conditions it had been designed for, and we had to impose those kinds of conditions on it, and go through the rigorous process of step-by-step investigating and analyzing how the articles reacted to those environments. That was the means of certifying and proving to our managers—that was one of the things we had, early on, Dr. Gilruth had decided. He said, "I do not want to have our people, our engineers, sit in their offices and only look at paper. I want them to get their hands dirty, understand the hardware, bring it here and test it, and make sure that the hardware meets the design requirements that have been sent out to the contractor to develop. And only then will we certify that the hardware is ready to go and fly, fly a man."

I might mention one other thing. Early in 1961 President Kennedy set up [a] study [of] the large launch vehicles that the country should embark on building, and this was headed up by a Dr. Nicholas [E.] Golovin, and [Dr. Gilruth] assigned me to go to Washington. In fact, I was there for about six months, working on this committee as the Space Task Group representative, and making inputs on what kind of things that Apollo was concerned about, and the large launch vehicles that we were possibly going to be looking at for doing Apollo.

One of the areas that had been designated was that we have something on, [what] was called "man-rating," and I was asked to write a couple of pages on what is man-rating all about. I didn't know anything about man-rating at that time. I came back and I talked to a few people who had been involved in the manned airplane flight programs like Walt Williams and Chuck Matthews, a few others, and I finally began to put together some concepts and thoughts on what man-rating was all about, and I did write a couple of pages that went into the Golovin Report that went to President Kennedy on what man-rating was all about, and also then had other parts about what the Apollo Program was going to be facing.

Then when we were facing Mercury, we said, "Well, what should man-rating concepts be for Mercury?" And we laid it out in pretty simple concept. Man-rating really ought to—that was one of the things that came about with test facilities. We ought to be able to examine the hardware and test it and make sure that it meets and is capable of meeting the design requirements that were set out for it, and that we have enough testing to be able to give this assurance and do the certification. That was one of the parts of man-rating. The other parts were that we needed to make sure that we had, for all critical systems, that we had a redundant backup system for it. Those concepts have been used, continue to be used today.

One other thing that was encompassed in man-rating, that you should use no technology that you were unsure of, because technology is being developed every day, and there are new concepts, new ideas, and new materials being developed all the time. Designers often think, "Well, gee, I can use this new material over here and I can lighten the weight of the vehicle by X percent." But you have to be real careful whenever you look at information like that and say, well, now, this is well and good, but can that material do the job in all instances of the what-ifs of the systems engineering kind of approach that you've got to take materials through. You're not only looking at can it do the job that it's designed for, but what all the other nuances of the kind of environment it's going to see and whether it has some kind of other complication along the way. So that's so much for man-rating.

Those were the simple kind of concepts that we put together and observed, of course, through Mercury and Gemini, and then passed on to the Apollo. Excuse me now, I think I've strayed from your question. Would you ask it again? [Laughter]

BERGEN: To be perfectly honest, I don't remember what my question was. [Laughter] But you've talked for a couple of hours now, and we thank you for your time, but before I go in to asking you more questions, I didn't know if you might want to … end this, maybe schedule for another interview, that we could pick up and take you on from this point at a later time.

BOND: Whatever you like.

[End of interview]