ORAL HISTORY TRANSCRIPT

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INTERVIEWED BY ROY NEAL
HUNTSVILLE, ALABAMA – 1 MAY 1998
AND
INTERVIEWED BY REBECCA WRIGHT
HOUSTON, TEXAS – 25 FEBRUARY 1999

[NOTE: This transcript is a combination of two oral history interviews conducted with Chuck Mathews for the Johnson Space Center Oral History Project. The first was conducted by Roy Neal at the Marshall Space Flight Center in Huntsville, Alabama, on 1 May 1998. The second was conducted by Rebecca Wright in Houston, Texas, on 25 February 1999. The transcripts have been combined per Mr. Mathew's request and include his other clarifying remarks.]

NEAL: All right, come back with me now in time. You and I are going to go all the way back to 1958. And you were with NACA, the [National] Advisory Committee for Aeronautics, when it turned into NASA, the National Aeronautics and Space Administration. Tell me about that transition, from your point of view.

MATHEWS: Well, I'd actually have to go back to 1957, because at that time I became personally interested in whether men could return from space safely. I knew that somebody was going to orbit a satellite very soon, and that the ICBM [Intercontinental Ballistic Missile] capabilities were such that a man could probably be put up there. And as I said, as soon as they decided that a man should go up, the question was: Could you bring them back safely? So I drew on some work that a fellow by the name of Harvey [H. Julian] Allen out at the Ames Lab [Ames Research Center, Mountain View, California] did that established that very blunt shapes were the shapes to be used for reentry. But up to that time, it was applied to missiles, warheads that entered very steeply; and a man, in order to keep the accelerations down and so forth, would come in at a very shallow angle, just brushing the atmosphere and reentering much more slowly. That would produce a much greater heat input; the heating rate itself would be lower, but the heating would be higher.

So I took these formulas that Harvey Allen had developed and calculated reentries with the shallow reentry angles that were for man and found out that, yes, it did seem practical; that the g's developed were about 8 g, which with a properly positioned man could [withstand]. And the heating rates and total heat input [were] such that you could protect a very blunt body with something like a beryllium heatshield, something that could very easily absorb the heat. I didn't do anything with that. That was back in February of 1957.

But, NACA was working on a new high-speed program. They'd already, of course, flown the Bell X-1 airplane at supersonic speeds, and they were just about to fly the X-15 airplane that would go up to about 5 times the speed of sound. They began investigating the possibilities of flying an airplane that would go up to about 12 times the speed of sound, and this they called "Round Three"; X-1 being Round One, X-15 being Round Two. This would be the Round Three situation. Both Ames and Langley [Langley Research Center, Hampton, Virginia] were working on this type of thing; and Ames, where Harvey Allen was, decided it should be somewhat of a blunt shape. They more or less used a cone sliced in two as the type of body that they were looking at. The Langley people decided they were going to look at a highly swept delta wing with a flat bottom emphasizing the fact that a large lift-to-drag ratio would be a good thing to do. And the reason they did that was because they were envisioning trajectories [in] which the craft would come in and then skip out of the atmosphere any number of times. A low drag was very important for that particular maneuver.

But, they intended to go to the Ames Laboratory—the Langley people intended to go to the Ames Laboratory—and try to settle this situation. And before they went, they decided to have a meeting to really discuss the pros and cons of the two approaches. And they always would invite somebody from the Flight Division, which I was a member of, over—even though we were not really supposed to be dealing with space; we were supposed to be dealing with airplanes. But, we were the only ones that had manned flight experience, so we always would be invited. And, I was invited over to this meeting. Near the end of the meeting, it was discussed, and I think this was just after Sputnik flew; it was either just before or just after, but a discussion took place about a return from orbit. And I spoke up and told them that once you're in orbit, lift-to-drag, high lift and low drag, didn't mean a thing because, in orbital flight, the flight is capable of sustaining itself and drag is minimum anyway, because you're really outside the atmosphere; and that what you have to worry about is getting the man back safely. And there you wanted the most drag possible. So they somewhat (shall I say?) ignored that input. But they did go out to Ames; and I believe Harvey Allen presented a very similar argument.

But meanwhile, I really hadn't developed what kind of a body or what kind of a configuration [I was] talking about. And I thought it over and ultimately came up with the idea that we should have a winged vehicle that came in at very high angles of attack, almost broadside—very similar to the Shuttle, but even higher angles of attack, because at that time heat protection was more difficult. [I] went over and talked to my boss about this, and he agreed it was [a] fairly good idea. But when the Langley people came back, Max [Maxime A.] Faget was a part of the committee that went to Ames, and when he came back, he had the idea of the Mercury capsule shape, which is really a conical section of a sphere, as [it was] originally envisioned. If you envision cutting a conical section [out of] a sphere, this is the type of shape you would get. There was a considerable argument about which of these approaches would be best. And then the man [who] was in charge of the Round Three [John Becker] still wanted to maintain his approach. So in early January of 1958, the laboratory hierarchy decided they'd have a design competition. And so my particular group from the Flight Division [FRD] worked on this delta-wing, high-angle-attack approach. Max and his people from Pilotless Aircraft Research Division [PARD] worked on his approach [the capsule], and Johnny Becker, the fellow

[who] was really pushing the Round Three, worked on that approach. And, regardless of what we did, the wing configurations came out weighing two to three times as much as the capsule. And it became apparent that we didn't have the launch vehicles, at that time, that [were] capable of lifting a winged vehicle. So, [Dr. Robert R.] Bob Gilruth, who was charged with making the decision on that, opted for the so-called capsule; and the winged vehicle sort of dropped by the wayside at that particular time.

Johnny Becker wanted to continue the argument and came over to see me about it. But, I told him, "No," I thought the decision was right. So I really kind of dropped out of the space activities for a short period of time, just telling Gilruth if some true activities did develop in space—and in particular in manned spaceflight—I would appreciate being brought into that particular activity. But I had some important airplane-type projects, so I went back working on those particular projects. And after, I guess, a period of about a month—a month-and-a-half, the Deputy Center Director [Floyd Thompson] came up to me and said, "Chuck, we've got some people [who] are working on the capsule, and they've been working for quite a few months now. But we're worried about not really having anybody that has manned experience, and we'd like you to head up that group and end up with a specification for a manned space vehicle."

So I went over and reviewed the work of this group, and it was surprising how much they had done. They had come up with a contoured couch to protect the human, for example. They had come up with refinements of the shape. They had initiated tests up in Johnsville [Pennsylvania] on the centrifuge to demonstrate that man, indeed, could withstand the g-forces if he was positioned properly in this contoured couch. There were a few things. I never really was happy with the structure that was involved; not the front heatshield part of it but the afterbody, which still would obtain considerable heat. But as a matter of fact, that really wasn't resolved until after the McDonnell Corporation took on the job and came up with the structure that ultimately was used in Mercury some, oh, I guess 9 months later.

But in any event, we did write a spec; prepare a spec. The people had all the material; they hadn't put it in spec form. I got them to put it in spec form. I went up and briefed the people in Washington—and, by the way, Gilruth and Faget had moved up to Washington, briefly, during that time period to fight the battle of what NACA should do in the space effort.

NEAL: Was this before or after the Russians launched [Yuri] Gagarin?

MATHEWS: Before.

NEAL: Before?

MATHEWS: Yes.

NEAL: All of this is *pre*-Gagarin?

MATHEWS: Yes. That's right.

NEAL: That, to a degree at least, prompted action, didn't it?

MATHEWS: That's right. This activity was in the spring of '58. And [Sputnik had been launched roughly 7 months before]—

NEAL: This was leading up, then, to the formation of NASA [National Aeronautics and Space Administration].

MATHEWS: Yes, it was leading up to the formation of NASA. And as I say, Gilruth and Faget and one other person—Charlie [Charles H.] Zimmerman—were key people in the fight not only for NASA, but in the fight for a manned spaceflight program. Even within the NACA group, there [were] questions about how important a manned spaceflight program might be. Many were much more interested in a pure science type of program.

But, in any event, I went up and briefed them with this spec. And as a result, Max was able to show this spec to the President's Scientific Advisory Committee—the PSAC committee. The Air Force, who was our chief competitor, didn't have a spec at that time. And, although they had been doing work on it in advance of the work that we had done, they hadn't bothered to really end up having a true detailed specification. And that was a significant [point] in getting a manned spaceflight program and having it put in NASA.

The reason why NASA, I guess, was formed in the first place as a civil agency is that's what Eisenhower wanted. He did not want to have a military activity in space other than for military purposes itself. He felt that if there were space activities for other purposes, that should be headed by a civil agency. Well, after it was pretty well decided that there would be a NASA and that NACA would be the nucleus for developing a NASA, Gilruth, [Zimmerman,] and Faget came back. We went to work on preparing a work statement for the purpose of letting contracts for the so-called Mercury spacecraft. That was also about the time the spacecraft was named "Mercury," by the way.

NEAL: By this time, was the Space Task Group yet in place?

MATHEWS: That was the start of the Space Task Group. At this point in time, when we started working on a work statement, the Space Task Group was formed. Gilruth was assigned the job of heading it up. Faget was given an assignment that really was the technical head of [an engineering] group, which [had] yet to be formed, called the Flight Systems Division. And, Charlie Zimmerman came back from Washington; he was an older man, and he was assigned to head up a group called the Contracts and Administration Division. I continued working on the work statement and other preparations for putting out a request for proposal from the industry. But, I wondered what my role in this would be.

After about 2 weeks, Gilruth came up to me and said that I would be in charge of an Operations Division; and I regarded this as quite [an] input, because we really hadn't worked very much on how this vehicle was to be operated. We worked on the vehicle. We *had* worked on the worldwide network of tracking and data receiving stations that would occur, but we really hadn't worked on how to operate the spacecraft. At that time I went over, Chris [Christopher C.] Kraft [Jr.] was in the same [Flight] Division that I was in and had done work for me in the past at various times. And, I felt he was the guy to really start working on certain aspects of the operations. So I went over and got him and gradually brought in a few more Flight Division people. Max did the same thing with the Pilotless Aircraft Research Division, augmenting his group.

The first thing I asked Chris to do was to look at launching, because we realized this would be rather dangerous. These launch vehicles at that time were quite unreliable; there were failures all over the place. And yet we were talking about putting a man on board. So he began working with the Huntsville people, here; it was not a NASA group at that time. It was called the Army Ballistic Missile Agency [ABMA], but it was the same [Wernher] von Braun group that ultimately came into NASA later on. And working with the people in the Ballistic Missiles Division and General Dynamics, or Convair, on the Atlas, which it appeared that we would use—we might use the Redstone indefinitely [at that time]; [but] we'd *have* to use the Atlas to achieve orbital flight.

Coming out of that came the various abort detection systems that are used to sense whether a missile was operating properly. We knew that something would have to be done. As a matter of fact, Gilruth came in some time before that—I'm going back a little bit—and said, "We need some means of escape from the booster." This is really before NASA was formed. And Max in his inimitable way came up with the escape rocket that was part of the Mercury system. And, that was incorporated as a part of the specification, this escape rocket; but no one had figured out exactly what to do about [it]. Would the astronaut make the sole decision of getting off the booster, sort of like a pilot using an ejection seat? But the feeling was, there wouldn't be time; so that there would have to be an automatic abort system in which various critical measurements were sensed, and that would automatically cause the escape rocket to fire and pull the capsule away from the booster.

So that's the first thing that was done by that group. Then I had another group. The powers-that-be decided that the Flight Division at Lewis—there was a Flight Division also at Lewis [Research Center, Cleveland, Ohio]—would come under my wing; and at first it was somewhat undecided exactly what role they would have. But I began feeling that probably they would have something to do with the launch preparation of the spacecraft. And that was pretty well firmed up because in November of '58, Max came to me and said, "Look, we are already in weight trouble with this capsule, and if we could use an ablation heatshield instead of a beryllium heatshield, it would be much lighter."

Now, we had discussed that when we were writing the spec some time earlier but decided to go with the beryllium shield because we knew it would work. We were worried about the ablation shield because, with this long heating that would occur in this shallow reentry, we were afraid that the plastic material might melt and slough off, and the heat protection would be lost. But anyway, I agreed with him that if we could convince the management of this that we would rewrite the spec to put in this ablative type of heatshield. Well instead of agreeing with this, they said we should carry on a parallel program of both the beryllium heatshield and the ablative heatshield. And by the way, the ballistic flights that occurred on the Redstone [including] the ones that [Alan B.] Shepard [Jr.] and [Virgil I. "Gus"] Grissom flew [used] a beryllium heatshield, where all the rest of them used the ablative heatshield.

But anyway, they came also back and said, "Well, you're going to have to demonstrate that ablation will really work." So Max suggested that we fly a spacecraft—not a production spacecraft but a test spacecraft—on an Atlas very early. And because we didn't really know what we were doing—this was, as I say, in the November/December time period—we set the flight date for the next 4th of July; just 6 months [later]—

NEAL: A boilerplate model?

MATHEWS: What did you say?

NEAL: A boilerplate model?

MATHEWS: A boilerplate model, right. But it wasn't that simple because it had to be stabilized; it had to have an attitude control; it had to, of course, have instrumentation so we knew how the heat protection was performing; it had to have a parachute system; and so forth. And, by the way, the [tests] of the parachute system, that's one place where the Flight Division had gotten into the act very early. A fellow by the name of Jerry [Jerome B.] Hammack, from the Flight Division, began dropping boilerplate spacecraft of the Max Faget type and landing them using a parachute system, and that was done well before NASA was formed.

But [in] any event, it worked out that because Langley was familiar with the parachute recovery system—rescue beacons and the other parts of the recovery system—they would do the afterbody of this boilerplate spacecraft; but the Cleveland Flight Group [Lewis] would do the forebody with the ablative heatshield, the various temperature [measurements], and the attitude stabilization and so forth. It was a program called Big Joe.

So, a man by the name of Scott [H.] Simpkinson headed up the group that would do this Big Joe spacecraft. And the man [who] was assigned to me as my Deputy—Chris Kraft was one of my Deputies—a fellow by the name of George Merritt Preston ["Pres"], was my other Deputy. He would be the one that would take this down to the Cape and, incidentally, the people would go with him and probably stay there and form the group that would check out subsequent spacecraft. So we started to work and all sorts of things happened; for example, when Scott went out to General Dynamics to check on the Atlas, they told us, "Well, we would have to have some kind of a blockhouse system to verify that the spacecraft was operating properly," something we hadn't even thought about. So I ended up saying, "Well, gee, I know a fellow that's working for RCA down at the Cape. He's a very good instrument engineer. We'll put him to work designing these blockhouse consoles." So [those were] the first blockhouse consoles that were ever designed for the manned spaceflight program.

But perhaps even worse, we made arrangements with the Vanguard people that were launching the original [U.S.] satellite to make use of their hangar—Hangar S—down at the Cape to check out this boilerplate spacecraft. So, sure enough, in June we brought the spacecraft down. We brought it down in bits and pieces. We were, in fact, behind schedule; it was almost a miracle that you could do that much in the time period. But anyway, I began receiving some complaints from a representative that Max had down at the Cape that things weren't really being supported very well down there. But I would ask Pres, my Deputy in charge of this, and he'd say, "No, things are going okay." But, the spacecraft ran into problems of [an] electronic nature called RFI [Radio Frequency Interference] and EMI [Electromagnetic Interference]—I won't go into the details of that; you probably know what that is—but, in combination with the reports of not being supported very well and the problems with the spacecraft, Gilruth decided to go down and look himself. I had been down to the Cape [only] one time before—and I'll go into that—but we went down there and the Hangar S people had roped off about a 30-foot square area where we were to check out the spacecraft, and they [had given] us one office. The hangar had offices on either side, but they gave us one office meant for about 4 people; and we had about 8 or 10 engineers crawling all over each other in that office. And, Gilruth was pretty darn unhappy. So he sent the Cleveland people back to Cleveland to solve their technical problems with the spacecraft and immediately approached the powers-that-be within NASA to get the support down there straightened out. And, indeed, it was. We were given, really for the time, ample office space. I guess there was nothing in the hangar, so we had plenty of space. Why they roped off a 30-foot space, I don't know. But anyway, that's the way the manned space program started its flight program.

NEAL: It seems to me, Chuck, that where we've been talking along the lines of Mercury, you had that spacecraft designed just about to that point where it is ready to fly.

MATHEWS: Right. Let me go back a little bit, though, and talk about the first trip that we made down to the Cape. Bob Gilruth, myself, Merritt Preston, [and] several other people decided we would go down to the Cape. This was again in November of 1958. General Yates wanted to meet us and find out, you know, what we were really up to.

NEAL: That's Donald Gordon Yates, the commander of the Eastern Test Range.

MATHEWS: He was the commander of the Atlantic Missile Range [AMR], right. And we, of course, wanted to find out, you know, what support he could give us. We went down there, and we ended up staying in a very unusual place called the Tradewinds Hotel. I don't know whether you've ever heard of it—

NEAL: It's where the Navy stayed, too.

MATHEWS: That's right. And the ABMA people also stayed [there]. Well, the man that managed that motel was *tremendously* enthusiastic about manned space. There wasn't *anything* he wouldn't do for us; and on those first visits down there, he would have crab claw hors d'oeuvres and all sorts of things just to make sure that we would stay at his place whenever the opportunity came about for us to go down there. Anyway, the first day we were down there, we just [toured] the Cape and the Patrick Air Force Base facilities to find out what was down there. And then we found out that General Yates had been called away and would not be available; so Bob Gilruth, who had other things to do, said, well, he would like me to stay down there and wait for General Yates to come back, which was a few days hence. But he would have to go back and do some things in Washington and so forth. The next morning, I got up very early and went up to the Cape to see an Atlas launch. And the Atlas fired up, and before it got a foot off the pad just fell right over and burst into flames. A group of us were outside the blockhouse, which provided the best view, and there was [an] area between a kind of a facility building and the blockhouse, a covered area that you could run to in case something happened. And, I watched this happen and was wondering, "Now what on Earth would happen if a man was on top of that?" And, I noticed everybody running toward this [building], but I stayed and watched. But then the shock wave hit, and so I turned and ran into that area. It turned out with the distances, it was just about a second or two, but it seemed like an awfully long time. So that was our first experience with an Atlas.

A day or two later, one of General Yates's Deputies, a Colonel Cooper, called and said he would like to talk to me in advance of my talking to General Yates, and he came forward and discussed several things. One was a worldwide satellite system, low-altitude satellite system, of multiple satellites that would form the basis for communication back to a central control area. The other thing he talked about was creating an armada of small oceangoing vessels that would be used to recover the manned spacecraft. Now, the Atlantic Missile Range did have a small recovery fleet already, [with] which they recovered things immediately offshore. But I was really worried about both of these propositions because I could not see us putting in place a satellite complex of this type—a communications satellite complex of that type—in the time period we were talking about, that we'd have to use some other means. And I couldn't see creating an AMR Navy, so to speak. So I was really worried when I went the next day to see General Yates. Cooper described his communications satellite configuration, and I guess Yates could tell that I was not very enthusiastic about it. But then when Cooper began describing this navy, Yates blew his top, and said, "What have we got the U.S. Navy for? We don't have to develop the big capability. We can ask the Navy to do it."

So, I was really impressed with General Yates. And meanwhile, of course, the other thing disappeared, too. But it did bring up the question, "Well, how are we going to get all this [data] back to some place, some central place? And what are we going to do about it if things go wrong, or if—even if things go right, how do we know what's going on, as we say, in real time?" So I put Chris Kraft on that activity, and he worked at [it]. I kept after him saying, you know, "When are you going to come up with something?" And so forth. Ultimately, he did come up with a plan—a very *basic*, rudimentary type plan—but he also suggested that he would like to be the Flight Director at the centralized facility. This was before Walt [Walter C.] Williams came on board, by the way, considerably before. And I said, recognizing Chris as the type of person that would be very good at that, "Yes, as we develop this, if it turns out to be as you describe it, then you will be the Flight Director."

The other thing that happened in that regard was, we got word from Headquarters that the Canadian government would be willing to provide us with engineering capability. The Canadian government had developed a *very* fine, two-place fighter airplane called the CF-105; better than anything the United States had at the time, and already demonstrated to twice the speed of sound. But the Canadian government had decided that after many overruns and so forth that they couldn't really afford to build this fighter. So even though they had, I think it was, like 10 flight test airplanes flying and demonstrating this tremendous performance, they scrapped the program. The government said to A. V. Roe [AVRO], the company that was building the airplane, "No, not only are you not going to do this but we are going to actually break the airplanes up." And a couple of men, realizing that, you know, there would be thousands of engineers out of work, began looking at where these people could get work. And one of the places was in the space program. And many of these people had flight-test experience, or at least had, you know, good, solid, industrial airplane experience—both from England and from [Canada]—[so] the powers-that-be at NASA deemed that was a very good thing to do. So, Gilruth and I and two other people—Charlie [Charles J.] Donlan, who was Gilruth's Deputy at the time, and a fellow by the name of Paul [E.] Purser—went up to Canada on a cold February day in 1959 and spent, I guess it was 2 days interviewing people, just one right after another, one right after another. And out of thousands—we didn't interview thousands; we interviewed, perhaps, several hundred —but out of the thousands of engineers that expressed an interest, we picked 40—30 or 40, I can't remember the exact number, and brought them down. I had picked a certain number to be assigned to the Operations Group; and they really had operations experience. These were primary flight-test engineers. And, Chris used these people largely to develop the concept of the Control Center that was to be implemented down at the Cape. And—

NEAL: Down at the Cape, the old Mercury Control?

MATHEWS: Mercury Control.

NEAL: So, in other words, that was the predecessor of all the more modern control rooms.

MATHEWS: That's correct.

NEAL: With such things as flight dynamics—

MATHEWS: Well, we had all those. We had flight dynamics officers and it was much more rudimentary, of course. But we had all those things. In fact, Chris had set up this Control Center organization; and some very key people—there was a fellow by the name of Tec, Tecwyn Roberts, who was a Welshman, who in a way, as far as Flight Control was [concerned], was Chris's right-hand man. Very low profile. He did operate in the Control Center, but he was much more low profile. But he, in many respects, was the brains behind a lot of the things that went on. And then there was a Scotchman who ultimately went back; I think he went back to Great Britain. But he did an awful lot of work on the design of the Control Center itself. But there [were] a considerable number of people, like John [D.] Hodge was another Flight Director, and so forth. And these were either Canadians or Englishmen or Welshmen and Scotchmen and what-have-you. So that was the international impact on the Mercury Program.

NEAL: And right about that time, when all of these things were beginning to go into place, is that when Walt Williams came aboard with his X-15 experience?

MATHEWS: Just about. I mentioned the fact that we had this problem down at the Cape; and we were coming up to a—we didn't launch in July, but we launched in September, just a few months later. And, at the time of the launch, Gilruth came up to me and indicated that Williams would be coming on board. I really didn't understand *why* he was coming on board, but I suspect it was related to the problems we had had down at the Cape. But, in any event, the flight went off; and it was sort of a comedy of errors that proved to be very successful. The Atlas failed [to] stage, for one thing. And the base manager for General Dynamics would stand on the top of their hangar and call out staging, which he did. But it was called out on the basis of the booster engines shutting down and him being able to see it, which they did. But instead of jettisoning the booster engines, they didn't jettison. So the Atlas was carrying extra weight along on this flight. But it shows how crude some things were. People depended very much on MacNabb's callout, even though they had telemetry and so forth.

NEAL: That's B. G. MacNabb you're talking about.

MATHEWS: Yes. Never before had anybody really thought of a need to do much in what we call real time; that is, call out things as they really happen from telemetry records and from other types of data that are received. Because, they were flying things in which there wasn't much that could be done anyway; and you could look at whatever happened later on. So there was no great real-time capability. But anyway, because the booster engines did not stage, unknown to us, the vehicle didn't get up to total velocity and it also went to propellant depletion; instead of having a normal cutoff signal, it just ran out of fuel. And because it didn't have a cutoff signal, the retrorockets that would back the Atlas away from the spacecraft didn't fire, because it was fired on the basis of that cutoff signal. So, although the separation system for the spacecraft took place—the spacecraft was separated, [but] the Atlas with this residual propulsion—just dumping residual [propellant] out—kept nudging the spacecraft. And the spacecraft kept fighting it, trying to control its attitude and so forth, because it was activated, and ran out of attitude control fuel. So the spacecraft finally got away from the launch vehicle and started a slow tumble. This is all in retrospect, by the way; we didn't know this in real time. [It] started a slow tumble. But it just happened as it entered the atmosphere [it was] going heatshield forward. A great fortune.

And meanwhile, Chris was not down there. There [were] no Flight Control people involved in this. We did have recovery people involved; we did have connections between the downrange recovery people that operated out of places like Antigua and Puerto Rico, and so forth—had communications with them—but we never heard from them. We were aware that something was wrong. We didn't know if the booster had over-sped and the spacecraft had got into orbit, or under-sped. So Big Joe, in retrospect, came in properly; but we didn't know that.

And we waited around, probably Bob Gilruth waited around for at least a half an hour. And because there had been so many failures, General Yates was always very insistent on having a press conference in a very timely fashion after any flight. They always took place down at Patrick Air Force Base, which is about 30 miles, I guess, removed from the Cape. And Bob said, "Well, I guess General Yates wants me down there, so I'll go on down. But you wait around and see what might have happened." We didn't have the slightest idea what had happened to the spacecraft.

What had really happened was, some observers on one destroyer downrange in the vicinity of Puerto Rico had seen the flash of the spacecraft reentry going over its bow, and then, when [one positioned] further [downrange], going over its stern, approaching from the stern. So the recovery force deployed from Puerto Rico, very shortly had the spacecraft in sight, and moved their destroyers at flank speed, you know, to recover the spacecraft. But they didn't bother to tell anybody about it. So I waited another half an hour, I guess, and very reluctantly got in the car and drove down to Patrick. And as I was entering the building, the man in charge of the recovery fleet—Destroyer Flotilla Five, [Admiral Harry Smith] I guess it was, or Destroyer Flotilla Four—was coming out of the press conference and said, "Chuck, someone's told me there's a phone call." And so he took the phone call and turned to me and said, "The spacecraft's been recovered." So it was a very successful thing; but half the Press had already left; so some of the newspapers came out and said the thing was a failure; others came out and said it was a success. It turned out to be very much of a success, because the Atlas didn't quite make the speed and it produced a trajectory that was similar to an abort trajectory, where the Atlas would not make it to orbit. It was more critical from a heating standpoint than if we'd reentered from orbit. And we found out the conical afterbody section on the spacecraft was almost plastic; it had already hardened, but it had given evidence of flowing. And we had to make a complete redesign of the production spacecraft in that particular area. Fortunately, that's where the parachutes were; but the parachutes came through it fine.

But anyway, Walt Williams came on board. He was there for the Big Joe operation; he took no part in it, but came on board. [He] had a difficult transition in the sense that the X-15 was just about flying. So he had to spend probably more than half his time initially back at Edwards, where the X-15 was flying, and a lesser time at Langley Field, where the Space Task Group was located. And the time that he spent back there was not very much time anyway, because he had been made the single point-of-contact with General Yates and he was in the process of negotiating that agreement and other aspects of an agreement of support from the Atlantic Missile Range. So, the various activities went along. The first production spacecraft were delivered. One of the production spacecraft—I forget whether it was the first or the second one—was sent down here to Huntsville. I think we were ordered to send it down to Huntsville for some structural tests; they had some facilities down here. But I think the real reason was that it was for them to take a look at it. Well, we already knew the spacecraft quality was very poor as they were being delivered out of the White Room at McDonnell Douglas. And it was reinforced by feedback from the people down here in Huntsville, that the spacecraft quality was very poor.

NEAL: In what respect?

MATHEWS: Almost generally poor. It wasn't the structure or anything like that. It was tied in with things like the wiring and so forth, aggravated by the fact that you had to get inside the spacecraft to check it out; and it was very small, and people would kick things or step on things and break wires, and so forth. It was a real problem. But anyway, it was decided that the spacecraft would be delivered in spite of their poor quality and that a group of McDonnell and NASA people at the Cape would rework the spacecraft. I objected to that. I felt that McDonnell ought to be required to deliver flight-worthy spacecraft. It was their responsibility, and the minute [it] went down to the Cape, they could get rid of some of the responsibility. And furthermore, they could get partial payment on the spacecraft once they delivered it. So there was a fair argument developing about this, I have to say; and that was at the time of the MA-1 flight, which [was] a year after Big Joe. And, the MA-1 flight was again scheduled to take place on the [August] time period of 1960. And, even in those days we had a flight readiness review activity.

Well, I had gone down to the Cape several months prior to launch—I actually had my family go down [there] and stay in a hotel so I could be down [there]—because of the fact that I wanted to make sure that the Cape people, this time, were really on the ball and doing things right and so forth. And I had not had a lot of contact with Walt. Whenever I was back at Langley Field, he would come in to talk to me and sometimes ask me for data.

NEAL: Mercury-Atlas 1.

MATHEWS: Right.

NEAL: Oh sure.

MATHEWS: This was to fly a production spacecraft; not necessarily with all the systems on board, but the structural thing and some of the production systems as well. And I had gone down to the Cape, I guess, about in June some time and really just stayed down there to make sure this spacecraft would be truly flight-ready. Just about every day it would rain. And the afterbody of the Mercury spacecraft had shingles made out of a high-temperature material called René-41 and then some super insulation inside of that, which is sort of like a fiberglass insulation but much better. And because of the shingled nature of this, this prevented the heat from producing any breakage structurally because the shingles could slip, they also provided for leaks. So when it rained—and we had no White Room aboard the gantry at that time; it was just open—the shingles would leak and the super insulation—a kind of cottony material—would get wet; and we'd go in there and take all the shingles off and put dryers on, dry it out; and the next day, one of us would do the same thing [again]. One day, one of the fellows decided that the thing to do would be to put a plastic *shroud* around the spacecraft, and connect this shroud to a cable that when the spacecraft lifted off, the cable—really a line, a lanyard line—would pull the shroud away.

Well, Walt came down for the flight readiness review, and when he heard about this he blew his stack and said, "Geez, I don't want to have this plastic thing aboard the spacecraft." And we talked to him, at length, about the fact [of] how much it rained and convinced him that it could be very reliably pulled off. So, he ultimately gave in. The day [before] the flight was a relatively sunny day and then, in the evening—or late afternoon—it became overcast; and then it started raining a little bit, a little kind of drizzly rain. And the rain then over time—I think that the launch was like at 4:00; it was in the early morning—over time, the rain increased a little bit and a little bit more; but it never really became too serious. And Walt and I would check with the Air Force about what their feeling was, and their feeling was that, well, this is a ballistic missile and we can launch ballistic missiles under *any* weather conditions. So we kept going; probably both Walt and I having a little bit of "go" fever.

But at the time of the launch, almost within seconds of the launch, a gale hit us. You couldn't see—they tell me from the blockhouse, you couldn't see the launch. And, it was really a very bad storm; a tremendous rain. The launch took place. The launch [vehicle] went up to about 30,000 ft and something happened. It was obvious that the launch vehicle had disintegrated; and it was augmented by the Range Safety Officer blowing it anyway, to make sure no big pieces fell anyplace, and most of it fell off shore. Well, Bob Gilruth was not only disconcerted by the fact that the launch took place under the conditions it did, but he also, I think, felt the program was probably over at that time, at least for a day or so. No one had fed anything back to him or anything like that, but he came up to me and said, "Chuck, I'm going to go over and see my dad and mother," who lived on the other side of Florida, "and if you find out anything, here's my phone number—here's their phone number, and give me a call." And he left.

So, what I'm going to tell you now is in the [background] of that failure. I had previously said that we had this debate going about whether we should make McDonnell deliver flight-ready spacecraft or not. And Gilruth had told me that if I could convince Walter [F.] Burke, who was the Vice President in charge of the Mercury Program for McDonnell, that this was the thing to do, [then] that's what we would do. I talked to Burke—he's a very good friend of mine, we did a lot of things together during the Gemini Program—but couldn't convince him. And I don't exactly blame him because it wasn't at all in McDonnell's interest, either financially or any way, to not get those spacecraft out the door, okay? So I lost that argument; and I maintained it very strongly to such a point that Gilruth later called me in and said, "Chuck, you're no longer going to be involved with the checkout down here." He obviously felt, you know, if I was taking a position I couldn't really be very conscientious in what they were planning to do, which was sort of to rebuild the spacecraft down at the Cape—which was done by the way. So Walt then put a fellow [who] later became my Deputy on the Gemini Program, Kenny [Kenneth S.] Kleinknecht, in charge of the Mercury work, although Preston still maintained his position as the guy down [there]; permanently down there. And I was left with just the Flight Control operations.

[Those] operations proceeded very efficiently and expeditiously. I think one of the main things that was important about it was the fact that we developed a very major simulation program; that is, we not only had the astronauts flying their simulators, but we had all the flight controllers in their positions, monitoring the flight of the spacecraft. And we not only simulated normal flights but all sorts of malfunctions. Now, the malfunctions that [occur] never are the ones that you've simulated. But it injects a certain discipline and professionalism; and I am sure Chris takes great pride in the fact that right from the word go that flight control operation was regarded as a very professional operation by anybody that observed it, including the news media. And, it was largely due to this simulation effort that we initiated.

NEAL: During that period of time, also, were you not concerned somewhat with Apollo and lunar orbiting, with rendezvous, that sort of thing? Were you not beginning to consider those various items? You were working part of that, too, weren't you?

MATHEWS: Yeah. Actually, after Grissom's flight, I didn't really follow the day-to-day operations of Mercury at all—because I thought the Flight Control operations had already demonstrated that they could be done very satisfactorily—and [I] started working just as an individual with some people on advanced programs. Now there were two programs: one was headed up by a fellow by the name of Jim [James A.] Chamberlin, who was a Canadian [who] came down here with the [AVRO] group; in fact, he was the lead guy. And, by the way, going back for just a minute: he was put in—Charlie Zimmerman, who I mentioned before, was head of the Contracts and Administration activities. Being an older man, he decided he would go back and work research at the Langley Research Center. And, Gilruth replaced him with Jim Chamberlin; not on the administration aspects—they got another fellow by the name of Wes [Wesley L.] Hjornevik to come down from Washington and take on the administration—but he took on the contracts activity and he also took on the engineering because—what happened, really, was that Max and his group, with a background of "researchy"-type of activities, had produced a situation at McDonnell where there [were] all kind of changes being put in all the time. And, Gilruth thought that a guy with the industrial experience, that Jim Chamberlin had, would be better at getting a more disciplined activity.

So Max then moved over to working on advanced programs like Apollo, and Chamberlin took on the Mercury development activity. And, out of the Mercury development activity, there were a number of problems that came up initially—just with the spacecraft, as I sort of implied already. And, Jim Chamberlin and a number of McDonnell people came forward and said, "You know, in the next spacecraft, we should really be doing it this way rather than the way on Mercury" and so [forth]. One was an obvious one: We really should have two men instead of one man, because one man's got his hands full. He can hardly do anything besides just monitor the spacecraft. But there were other things, like a [spacecraft] where most of the equipment was outside the cockpit rather than in. I mentioned the fact that checking the spacecraft out—the Mercury spacecraft—they had to get inside and stepping on things and kicking things and breaking wires, and so the new spacecraft was designed with most of the equipment outside the pressurized cockpit; so the cockpit was relatively clean. It had only the displays and the controls that the astronauts absolutely needed. All the support equipment, environmental control, communications equipment; [these were] all outside.

NEAL: So you then had really begun working on the second-generation spacecraft—

MATHEWS: He had begun working on it; right.

NEAL: Right.

MATHEWS: And I started working with him on it, largely to bring in the operational inputs again. And at that time I spent most of the time on that spacecraft, which turned out to be the Gemini spacecraft. Later on, as things developed more completely, the Gemini spacecraft went well beyond that because, as you know—as everybody knows—the President decided to go to the Moon; and as we determined what things might be required to do that—like flying for a week or two, or rendezvous, or extravehicular activity, and so forth—these things were injected into the Gemini spacecraft.

NEAL: According to my notes, too, they even had something called parasail, which they wanted to work with—

MATHEWS: Right.

NEAL: —and introducing the Titan rocket rather than the Atlas.

MATHEWS: Yeah, that's correct. And I'll get to that eventually in just a minute.

NEAL: Okay.

MATHEWS: But anyway, Gilruth was very strong in this, that there should be an intermediate spacecraft between Mercury and Apollo that could demonstrate that the things that were being specified for Apollo, in terms of mission specifications and other things, could really be accomplished. And as a result, the Gemini spacecraft was subsequently approved. And as a matter of fact, Gemini and the Apollo command module, the contracts for those two were let within a month of each other—one for a short-base program to get information in a hurry, and the other for a much longer program, which ultimately would land on the Moon.

NEAL: And somewhere in there, somebody had to decide how do you pronounce the name. Was it Gemini [*long e*] or Gemini [*long i*].

MATHEWS: Right.

NEAL: Which way would it be?

MATHEWS: Well, I gave a talk just recently down at the Cape. They're putting in a Gemini memorial down there, [and] they had a banquet. They raised that same question. And I told the audience that I'd like to have them vote. So I told them that I would like everyone [who] favored Gemini [*long e*] to say "Aye," and after that I asked the audience that everyone [who] favored Gemini [*long i*] to say "Aye," and of course there were ayes on both sides. And I said, "The ayes have it." And, it's really pronounced Gemini [*long i*], okay?

NEAL: [*laughs*] Well—

MATHEWS: Frequently, I call it Gemini [*long e*], but—

NEAL: I noticed you've been calling it Gemini [*long e*] throughout this interview.

MATHEWS: Right.

NEAL: So do I. Walt Williams told me to call it that.

MATHEWS: Right.

NEAL: He said, "That's the way it's going to be."

MATHEWS: Okay.

NEAL: He made an instant decision. I don't think it'll ever get fully resolved, do you?

MATHEWS: No, I don't.

NEAL: I'm sorry I interrupted, but I—

MATHEWS: Oh, that's okay.

NEAL: —had to go with that, so that for future reference, future generations will probably call it Gemini [*long e*] or Gemini [*long i*].

MATHEWS: Right. [*laughs*]

NEAL: All right, sir. Go ahead.

MATHEWS: Okay. About the time I was working on that, I was paying very little attention to Apollo, but President [John F.] Kennedy had already made his pronouncement, and people were working on that. And the only thing that I did was, as I say—was to look at what people were coming up with for mission requirements for Apollo, and making sure that they were going to be put into Gemini. And Jim Chamberlin was a *marvelous* conceptual engineer; he knew *exactly* how to get these things properly incorporated in the spacecraft.

But in [September] of '61, I was very surprised to find out that—the decision had already been made that we were going to move to Houston and set up a major Center down there. [Later] I was very surprised to find out that Gilruth had assigned me to another division other than the Operations Division, called the Spacecraft Technology Division. And, in a way, it was a blow to me because Chris was raised to a Directorate position in the new Center organization and Max Faget was raised to a Directorate position, and a Division Chief was one level below that, and I was wondering, you know, what was going on, sometimes even wondering whether I should *go* to Houston or not.

But, Gilruth came in to Max and [me] after that had happened and said, "I'd like you to write [an] article for the Institute of Aeronautics and Astronautics about how to land on the Moon." So, we set to work on writing that article; and Max was quite strong on—the standard mission at that time was called "the direct [ascent]." You came off the Earth in a direct translunar run to the Moon, and you made a direct landing on the Moon. And Max had really embraced that idea to the extent of trying to get his people to design a spacecraft that could do everything; that is, go to the Moon, land on the Moon, return, and reenter. And his emphasis had been on that. But there had been this activity, within Langley actually, that was promoting the idea of a thing called lunar orbit rendezvous. And they started promoting that, I think, way back in the early sixties some time or other. But it kind of dangled in place. Max had one group sort of working on that, but [he] hadn't embraced it.

So anyway, we were to write this article and, because there was no real decision made as to what [the mode] really should be, the article was not a good article, a very poor article. I've read it since, and it just treats lunar landing in a very general way. But Gilruth told me one day that he would like me to go up to Headquarters. The Langley people were up there giving a discussion of this lunar orbit rendezvous. And Gilruth, at that time, was starting to be somewhat positive about it, as was [D.] Brainerd Holmes, the guy [who] was running the manned spaceflight program at the time, but not, you know, to the point that a decision would be made or anything like that. Gilruth asked me what I thought of it, and I said, you know, I thought it was probably something really worth fairly strenuous study. And he said, "Well, Max has got this group studying it. But he's got another group that's studying the direct [ascent] type of approach to the thing. And part of [your] organization will be involved with working with both these groups to see what you ultimately think about this."

So I had these groups—Spacecraft Technology Division did a lot of other things—but these are the groups that I really focused on, and they had moved down to Houston in December. I started going down there in January and following this activity and talking to both these groups.

And the group that was working on the direct [ascent] suddenly started also looking at lunar landers of the lunar module type. And, it was pretty apparent to me that at least all my people were … integrated in coming to the conclusion that the lunar module and lunar orbit rendezvous [were] the way to go. So, I'll give you a little side story.

Subsequently, I don't know sometime in 1962—early 1962—I went back up to Washington where there was a further discussion of lunar orbit rendezvous. Meanwhile a fellow by the name of Joe [Joseph F.] Shea, [who] ultimately took on the Apollo spacecraft activity, was up there as one of Brainerd's Deputies; and he was really charged with making this particular decision. But Joe was away on a trip—I don't know; maybe it was a familiarization trip, I'm not quite sure why—but, anyway, the President's Scientific Advisory Committee had informed Holmes that they wanted to go down to Huntsville here. Huntsville was espousing Earth orbit rendezvous. The two are not exactly compatible with each other. Things are done for slightly different reasons. But one place they are compatible is the fact that [they both] could utilize a much smaller booster, if you use rendezvous either in Earth orbit or lunar orbit. The direct [ascent] thing took a very big booster called a Nova; [with rendezvous] the Saturn was big enough, but that was a much smaller vehicle and, of course, it was the one that was ultimately used.

The discussion up in Washington following this information was pretty serious because PSAC told us they didn't want anybody to attend this meeting but the Huntsville people. And Brainerd said, "Well, we can't do that. We'll at least want to know what's going on. So, Chuck, will you go down there and attend that meeting?" Now, I want to inject that [there're] a number of stories that [came] out about the lunar orbit rendezvous decision that are not compatible with what I'm going to tell you; but this is what actually happened. I called the fellow that was working on the lunar orbit rendezvous thing down at Houston and had him come up, too: a fellow by the name of Owen [E.] Maynard. And we sat in the meeting here in Huntsville with the PSAC people. One of the Huntsville people made this presentation about Earth orbit rendezvous. Incidentally, the [Houston] people had come to Huntsville about a week before that, and I was with them, to present lunar orbit rendezvous to them. And, there wasn't a strong argument that ensued, but it was just a "Thank you very much" and then we went back, you know, kind of thing.

So anyway, I went down and Owen Maynard showed up. And, the first day of the meeting took place, all about [Earth] orbit rendezvous. We didn't say a word. Then they had the usual dinner and cocktail hour. And Owen and I got into some discussions with some of these people about Earth orbit rendezvous and lunar orbit rendezvous, not thinking too much about what we were going to get into. So, anyway, the next day, again in the morning there was a discussion about Earth orbit rendezvous. And then in the afternoon, after lunch, the head of—well, he was [the] Deputy—I can't remember his name; Wiesner was the head of PSAC at the time, but he didn't show up at this—

NEAL: That's Jerome Wiesner.

MATHEWS: Who?

NEAL: Jerome Wiesner?

MATHEWS: Yeah. Wiesner was the head of it, but he didn't show up at this meeting. It was his Deputy, and he ultimately became head of it, but I can't remember his name right now.

NEAL: As we left off, we were talking here about lunar orbit, Earth orbit, Huntsville vs. Houston—

MATHEWS: Right.

NEAL: —and the whole thing was a big Magilla.

MATHEWS: Right.

NEAL: How did it work out?

MATHEWS: I had mentioned the fact that we had discussed lunar orbit rendezvous. We had fun with all these people at a cocktail hour between the 2 days of meetings, and then in the afternoon of the second day, the fellow that was chairing the meeting said, "I understand that there's somebody here [who] knows something about lunar orbit rendezvous. And I wonder if Mr. Mathews would get up and tell us about it." So I [got] up and, of course, these PSAC people were very brilliant people and great debaters and everything else, and they were literally cutting me to ribbons, I'm telling you. [Every time] I'd bring up something, there was about five different reasons why the idea was no good, you know. And I was very miserable. But, there was kind of a pause, and Wernher got up and said, "I've been thinking about lunar orbit rendezvous, and lunar orbit rendezvous is the way to go."

NEAL: This is Wernher von Braun?

MATHEWS: Wernher von Braun. Now that story has been told other ways, like Joe Shea being involved with it. And I think he was subsequently involved with—not with the PSAC meeting but with a discussion with the Huntsville people. But that's really what happened at that particular time.

NEAL: PSAC is what? The Presidential Science Advisory—?

MATHEWS: Committee.

NEAL: Committee. Just for interest.

MATHEWS: And it was in existence for a very long period of time; well before the Mercury—well before NASA. And then, oh, up to at least to the seventies, I'd say; maybe even longer. But, in any event, there was one other aspect of that story which I wasn't involved with. But, during a visit that Kennedy made to the Marshall Center, the PSAC people again brought this question of Earth orbit rendezvous up, and von Braun told them, "No, NASA has already decided it's going to be lunar orbit rendezvous." And, of course, that pretty well ended [it]. There were some other formalities, you know; reports to be written and things like that—

NEAL: The essence of this, as I gather it, Chuck, is: that during this period of time, you were looking ahead and then the real essence of what *you* were doing at that time was to figure out what did you need to know before you could ever possibly move on into Apollo, and what it would entail. And that seems to me to be squarely centered on Gemini, which was the interim program, was it not?

MATHEWS: Yes.

NEAL: Well, how in the world did you get to be head of the Gemini Program?

MATHEWS: All right. Well, as I say I was working in the Spacecraft Technology Division, and we had fought the battle of lunar orbit rendezvous and the lunar module. And, just like on Mercury or any of these programs, we'd sent out a request for proposal with a specification and got proposals in and had, in fact, selected Grumman as the developer of the lunar module. This occurred just before Christmas in 1962. And that pretty well ended my involvement with the Apollo Program. The groups that I [was] working with went on to other things, even doing some work on the Space Station and things like that.

NEAL: It's remarkable to me that Space Station was being seriously discussed and considered all the way back in time, into the early sixties. It took a long time to come to fruition.

MATHEWS: That's correct.

NEAL: Meanwhile, I'm sorry I interrupted.

MATHEWS: No, that's fine.

NEAL: It was just a thought.

MATHEWS: Well, in February, I had somewhat followed other aspects of Apollo, but not that carefully, because I was working pretty much just on this lunar mode and the lunar module preliminary design. But, a new man had come in as a Deputy for Bob Gilruth—really the Engineering Deputy—a fellow by the name of Jim [James C.] Elms. Charlie Donlan, who was the [Engineering] Deputy … in the time of the [Mercury] spacecraft, chose not to come to Houston and became the Deputy Director for the Langley Center shortly thereafter.

But in any event, the new Deputy and Bob Gilruth—Gilruth's new Deputy and Bob hit it off very well and immediately became very trustful of each other. I had had really no meetings with Elms at all. But he called me over to his office, and I think it was right near the end of February of 1963, and asked me various questions about Apollo: what I thought [its problems were], and this, that, and the other thing. I couldn't imagine why he was really asking me these questions. And, it turned out it had nothing to do with what he was really wanting to know. What he was really wanting to know was: Was I qualified to become the Gemini Program Manager? As I mentioned, Jim Chamberlin had ended up with some difficulties in that job. Part of it was political; part of it was some personal problems he had at home; and part of it was, as I say, a difficulty in communicating. But be that as it may, the powers-that-be had decided that Jim would no longer continue in that job.

So, some time in March—probably early March—Gilruth called me and said, "I want you to take over the Gemini Program." Well, I hadn't even worried about the Gemini Program for about 18 months or so; meanwhile it had gone along great guns [but] had gotten into considerable difficulties, financially and technically. So, I really wasn't too happy about it in a way, because I didn't really know what I was getting into. In fact, Gemini was located in [an] old GSA building in the middle of Houston. At that time there was no Center, and the people in Houston were located all over the place—[about 20] different locations, as a matter of fact.

NEAL: They were still working out at the cow pasture, just starting to cut the ground.

MATHEWS: Yes.

NEAL: Build the foundations.

MATHEWS: But, I think in a way we did our best work at that particular time. Anyway, it was the only [program] that was really downtown and in a rather poor district. So I went down there and spent about 3 hours trying to find it; I couldn't even find where it was located. Finally [I] found it; it was operating on the 4th floor of this GSA building. Only one other floor was occupied; the rest of it was rat-infested, I guess you'd say. But they sent me over there with the idea that I had to determine, within 2 weeks, whether to continue the program or recommend to disband it. One of the reasons behind this was the fact that—I didn't get to talk about Dyna-Soar; but that's another manned spaceflight program that the Air Force put into being, really based on this Round Three concept that I mentioned a long time ago. It was a winged vehicle; and the program was being developed by the Boeing Company. And the Secretary of Defense—

NEAL: [Robert S.] McNamara?

MATHEWS: McNamara, right. Every once in a while, these things slip me these days.

NEAL: Yup. It was McNamara.

MATHEWS: But anyway, McNamara had decided that the Dyna-Soar Program was going to be way too expensive for the Department of Defense; and he wasn't even sure that there should be a manned program in the Department of Defense. And sort of as throwing a bone to them, when he decided he was going to cancel it, he gave them the option of picking up some aspect of the Gemini Program—either running [a] program in parallel with it, but using Gemini spacecraft or Apollo, or what-have-you. There apparently [were] quite strong arguments between McNamara and [James E.] Webb [NASA Administrator] about Gemini and the interference that the Air Force might make with it. But because the program was in problems anyway, I was sent over to decide, you know, whether to continue it or let it go and possibly transfer it to the Air Force. When I got over to the office and worked on it for the first week, it was a tremendous opportunity because I had a chance to—I didn't have a chance to correct the [myriad of] technical problems—not that I could—I had a chance to really evaluate where the program stood and I had a chance, as a new guy on the block, to change things and make new cost estimates and new schedule estimates and so forth. Which I did.

One of the first things I did was to come up—they'd had a very optimistic schedule that kept slipping. I came up with a schedule that slipped it even further but sustained a very high launch rate once the program got flying; and I based that on having McDonnell, who was also the contractor, deliver flight-ready spacecraft. And I developed a plan of how this would be done, which we implemented. It involved—I knew the Cape people would not be satisfied, no matter how good McDonnell did, with it unless they had some hands-on aspect. So I arranged for two things. I arranged for a fellow by the name of John [F.] Yardley, who was [McDonnell's] Base Manager at the Cape, to be transferred back to St. Louis to become—we already had an Engineering Manager. So we had to create a new name, and he was called … Technical Manager. And this was sort of over Walter Burke's dead body, but he really didn't have any choice in the matter. As the new guy on the block, I was calling the shots this time and so forth. So, John transferred back.

But over and above that, for the first two spacecraft that would go through the White Room checkout at McDonnell, I had a team of Cape government people go up, with [a] government [man] being—the man being—John Williams being the test conductor, and checking out the spacecraft at McDonnell, along with the McDonnell people. So, that was the plan and typical of some of the changes I made. We bumped the program cost up considerably, too. I think it—the numbers may not be right, but I think we had the program cost coming out about $1.15B. And, at one time, it did oscillate above that to [$1.2B] later in the program; but actually it ended up lower than that. We actually gave money back. It's one of the few programs that that ever happened to. But, it was because I had this opportunity to get the right margins in the program.

The next thing that was done: I made a trip to most of the subcontractors, but particularly to the people involved with the Titan launch vehicle. The Air Force had responsibility for two things in the program: one was the Gemini-Titan, which was a modified version of the Titan;

and they had also a responsibility for the target vehicle, the Gemini-Agena, which is again a highly modified version of an upper stage that Lockheed built for the Air Force.

Well, in traveling around to these various places, [I found] there was a strong argument between the man that was head of the Ballistic Missile Division and Chamberlin. I don't think it was head-to-head, because I don't think they did that much—if any—talking together; but there was a pogo problem with the Titan, a longitudinal vibration of fairly large magnitude. And the contractor that was involved, the engine contractor, Aerojet, and the vehicle contractor, Martin, had gotten to work on this and brought the pogo down quite a bit; I think down to half a g or something like that. But Chamberlin decided he wanted it to be two-tenths of a g, and this produced this big argument because the military people saw no need, really, for anything *they* were doing with the Titan to eliminate pogo oscillations down to that level; and it was just because there was a man in there that there was a requirement to do that—if there was truly a requirement to do it. But anyway, in traveling around I found that the Aerojet and the Martin people not only had been hard at work on this; but, in spite of the fact that the Air Force had told them not to work on it, they had worked on it and could guarantee that it would get down to that [.2g] level. So in a way, it was an artificial problem; I mean, the problem was already solved. And so, in a way it was unfortunate that Chamberlin fell prey to that situation. But it was not true in a lot of other situations. I immediately got into a, you know, very good relationship with all the contractor people and the military people [who] were involved with the Titan. And, the Titan proceeded fairly smoothly on. [In] the spacecraft systems, there were all sorts of problems that continued for a long time.

But, by going out and working directly with these subcontractors and … McDonnell gave us permission to do this, which is very unusual; [because] a company doesn't usually want you to monkey around with their subcontractors. But they gave us permission that we could go visit them. There were strict rules on what we could direct them to do without going through McDonnell. But, it was an ability not only to find out what the problem was but also to give the subcontractors a feeling, you know, of the importance of what they were doing and so forth. So gradually, those problems over the subsequent year-and-a-half before launch were pretty much resolved…

[ORAL HISTORY 2]

WRIGHT: Today is February 25, 1999. We are speaking with Mr. Charles Mathews as part of the Johnson Space Center Oral History Program. We are here at the Signal corporate office in Houston, and I'm Rebecca Wright. Helping today are also Tim Farrell and Summer [Chick] Bergen.

Thank you so much for taking time out of your schedule to meet with us. We'd like to begin today and ask you to tell us about the problems that existed in the Gemini Program when you took over.

MATHEWS: Well, there were problems in many areas, but the spacecraft was the element that had the biggest problems and the most effect on the schedule. In fact, the schedule that we revised at the time I came on board was really dictated by the spacecraft, vis-a-vis the launch vehicle or the target vehicle. The spacecraft, it was only logical that it would have the most problems because it had by far the most advanced systems with the least experience behind them, and that was necessitated by the requirements of the Gemini Program for long-duration flight and rendezvous and things like that.

It works out that most of the rendezvous equipment, such as [the] inertial platform and the computer and the rendezvous radar, had their share of problems, but were more easily solved because the contractors involved had similar experiences with airborne equipment, with the military, but things like the fuel cells, no one had any experience with them. It was a brand-new development. The process by which they operated had been demonstrated and validated, but that isn't saying that you know really how to build a working fuel cell that will last a reasonable length of time. So that was kind of expected to be a dominant problem in the program, I think, from the word go.

But another area where there were significant problems was with the little thrusters, the little devices, little jet devices that provided for the control of the attitude of the spacecraft and for precision translation for the spacecraft. That's always a requirement for any spacecraft to do rendezvous operations as well. However, that was somewhat of a surprise, because the feeling was that spacecraft [thrusters] of that type had been tested.

They were so-called ablative thrusters. The ablative principle had been established not only for general heat protection, but had been actually established for rocket devices. But it turned out that the ability of these devices to work satisfactorily was determined with larger nozzle configurations and with a steady-state mode of operation, whereas in the rendezvous operation, you need a very precision pulse type of operation.

So it was with great surprise that these things would burn through very rapidly, and here was a program dedicated to conducting rendezvous that couldn't provide maneuvering thrusters that would work. And so again, just like the fuel cell, it was kind of back to a research program for a while, as compared to a development program. Those were the two things that were [big] problems.

The fuel cells' problems were the type of things like material problems, contamination problems, water scavenging problems, the kind of things that would be very natural. With the ablative thrusters, we tried to nickel and dime it at first, but then decided we had to do something rather fundamental, and we ultimately got them to work.

Another area that I thought would be a major problem was the ejection seats. Again, there was experience with ejection seats in aircraft, even so-called zero length ejections, that is, an ejection of a pilot right on the runway. But these particular seats had to really have a fairly long rocket-propelled trajectory to get out of the blast of a very big launch vehicle in the event of its failure on the pad, and so had to clear that, and then, in addition, not provide too high a G on the [astronaut] and so forth. But it worked out that, really, it was done rather successfully, and I always admired the subjects that tested that, not on a rocket, but on a sled-type device that was propelled at various speeds, and these people had to eject actually during the development program and demonstrate that it would work.

But there was another system that wasn't exactly tied to the spacecraft, called the paraglider, and it was to provide a land-landing capability. When I came on board, I was never very sympathetic with the paraglider in the first place, because I didn't really see the land-landing requirement as anything that was needed in the immediate future, certainly not for Apollo. In fact, there was no contemplation that Apollo would land on land.

That program did create problems, and although it was ultimately demonstrated, I had, by that time, chosen to delete it from the program and go to just a parachute system, as was done [in] the Mercury Program, and then also in the Apollo Program. The reason that the paraglider was of interest, of course, was the land-landing capability, but it seemed to me that having a flexible device to provide that just entailed too much risk. Its deletion from the program had the advantage that it took some content out of the program and helped simplify it, and it also helped the cost problem, too. So in my mind, it's always a good idea to have things that you can remove from a program from time to time. It makes you more honest. It meets your schedules and meets your costs and things like that, you see.

So the spacecraft moved along in an undesirably slow way, I would say, but the problems were solved one by one, and as they were solved, we also injected really new management techniques to prevent certain things from happening. For example, we organized an extremely rigorous change control system, where no change could be made—this applied to the launch vehicle as well as the spacecraft—no change could be made until it was not only reviewed by the contractors, but was brought up to the program office and could only happen if I personally signed [off] for it. And so there was an organization that reviewed each change, and there was a weekly meeting where the changes were reviewed before they happened, and they were either passed by the board or not, as the case may be. This has always been a problem with engineers, because they like to keep improving things, you know, and it's very important to kind of discourage that, but only to the degree in which you also solve your problems.

Another thing we did as time went on is we established a very effective program control organization, involving considerations of costs, schedules, and the technical progress. When I came on board, I found out that, really, the people that had that responsibility knew what the status was in terms of costs, schedules, and technical progress, but they weren't really projecting implications.

For example, there were cases in the McDonnell [Aircraft Corporation] spacecraft build where more work was being created than was being completed, so you were actually making negative progress at various times, and so you had to really very elegantly determine exactly what was going on in those particular cases. What we ended up with was a control room that was updated on a daily basis, and we made the control rooms between the contractor's, like McDonnell, and our control room, and. ultimately, the headquarters [do] the same thing, and that was applied to Apollo, by the way. So that they had exactly the same information displayed in exactly the same way and so forth. Again, anytime somebody wanted to change the manner in which the information was displayed, they had to change it with respect to all these various places.

There are other things that happened. For example, when you start out in a program, many times you haven't made the analysis that really determines what certain requirements are. For example, the time delay of a pyrotechnic device or the drift rate of an inertial platform, for example. And so the engineer takes this and says, "Well, I'll just specify the state of the art." Well, when an engineer specifies the state of the art, it's well beyond the state of the art, believe me.

So it's very important to review these specifications, and we found in the two cases I related, that as we became knowledgeable about the requirements, in terms of analysis and so forth, that we could relax these specifications, whereas people previously had been beating their brains out, trying to make these things pass an unrealistic specification. Those are the kinds of things that we did.

I guess the biggest thing was the incentive contracts that we established. Again, these are things you have to be careful with, because if you establish them too early in the program, particularly in a highly experimental program like this, and the contractor gets behind the eight ball and can't do the job, it actually produces a negative effect. In other words, he starts losing money, based on schedule losses and cost overruns and things like that. So the name of the game is to have the contractor really make money. So you initiate this at the time where he feels, "Boy, I've got a handle on this and I can really make the contract go and I will make a little bit more profit by doing this," and you'd be surprised what a change that produces in the contractor. A lot of people wouldn't believe this, but it's a fact.

So those are the kinds of things we did, and, as I say, the spacecraft struggled along. It was really always the long pole in the tent, and as a matter of fact, on some of those things like fuel cells and ablative thrusters, we never completely got out of the woods with them. There were problems on the flights which we managed to work around, but there were problems.

The launch vehicle had the advantage of being based on a development program of the Air Force, and the Gemini Titan was actually a modification of the Air Force Titan, and that was a big advantage, but there were problems. The biggest problem was the fact that the launch vehicle—initially, the military Titan launch vehicle showed a very severe pogo oscillation, which is a longitudinal jumping up and down, you might say, during the flight, which could shake an astronaut up pretty good if it got too high. Jim [James A.] Chamberlin had established a requirement for this, and got into a debating match with the military Titan people, and it was one of the reasons he had to leave the program.

When I made my first visit to the launch vehicle contractors, the engine contractor and the vehicle contractor, and various support contractors that the Air Force had, particularly the Aerospace Corporation, it worked out that they had had problems in getting this pogo reduced, which they were trying to do for NASA, but on my first visit, they had accomplished an analysis that not only showed that it could be reduced to the desired level, but it predicted all the other events that had occurred in the past. So I really didn't worry about the launch vehicle that much, even though they had not demonstrated this.

As a matter of fact, in the early summer of '63, they tried to demonstrate the ultimate pogo fix, I'd guess you'd say, and what happened is the vehicle failed for other reasons and so they weren't able to demonstrate it, and that was a result that occurred when there had been two failures in a row. So the Titan program manager for the Air Force, who, by the way, had only an indirect relationship to the Gemini Titan, decided he wasn't going to demonstrate these pogo fixes.

I realized this was going to be a situation that would be solved at high levels, [so] I just tossed it up [to] high level. I knew it was going to be fixed, [but] there was a great to-do over many months, right at the top levels of NASA and the Air Force, about this problem and other purported problems with the Titan, but about the time they settled this, the Titan was demonstrated with the pogo fix.

There was another problem, also, that was regarded with some importance, that the engine people indicated they could probably fix if we needed it, and we did have them work on it. It was called a combustion instability problem in the second-stage engine of the Titan, but they'd never really had the problem. The tests had indicated the incipient problem was there. The engine contractor did take steps to fix that, under the pressure of NASA, but the fact of the matter is, the first six Titans we flew on Gemini didn't have the fix, and we did fly six without it and six with it.

Anyway, the Gemini Titan rolled along pretty well. It had its difficulties, in that, well, first off, a very good decision was made to actually not conduct the assembly operations out in Colorado, where the military Titan II was assembled, but to do it at the Martin [Company] plant in Baltimore, Maryland. So the Geminis were assembled in Baltimore, and this was to avoid any conflict. We had our own people, geographically well divorced, to accomplish this. The tanks themselves were built in Colorado and shipped there, but the complete assembly of the vehicle was done in Baltimore, and it had that advantage, but it had the disadvantage of really having somewhat of a green crew.

So particularly when they got into testing, they did run into some difficulties that slowed it down, and when they got down to the Cape [Canaveral, Florida] with the first Gemini Titan, the GLV-1 [Gemini Launch Vehicle-1], again they had to go through a learning curve there because it was a different crew than the military crew. So the Gemini Launch Vehicle-1 [Gemini I] flight did occur later than we had hoped on the schedule that I had projected, but still much earlier than was going to be available for the second flight because of spacecraft limitations.

When I made out that schedule, [Robert R.] Gilruth [Director, Manned Spacecraft Center—now Lyndon B. Johnson Space Center] insisted I put in this Gemini Launch Vehicle-1 [Gemini I] into the program, which made the program have two unmanned flights instead of just one unmanned flight. And I wondered why he really wanted to do it. I think, in retrospect, I'd say he figured, one, it would keep the pressure on the launch vehicle, because the spacecraft that was flown would be proper structurally, but you wouldn't have all the systems on board, and there was no requirement to have the systems on board. So it kept the pressure on the launch vehicle, it allowed for an early demonstration of the launch vehicle. It also allowed a demonstration of the structural compatibility between the launch vehicle and the spacecraft, which was a problem in the Mercury Program. In addition, it allowed the public and the Congress and so forth to see an early launch. Okay. So I think he was really very wise.

The target vehicle, the Gemini Agena, was also a program operated by the Air Force, but a different group and a different contractor. The main contractor for the launch vehicle was the Martin Corporation and, for the Agena, was the Lockheed Corporation. The Agena target vehicle wasn't going to be used till later in the program, so we didn't worry about it too much in the early phases while we worried about getting the spacecraft along and so forth, and it turned out that probably was a mistake, because it came back to haunt us later on. We didn't pay enough attention. I'll go into that a little more in a moment.

But anyway, the first GLV-1, the first Gemini Titan launch [in April of 1964], was almost, I'd say, a complete success. I can't remember really anything that was wrong with that flight, and of course, this produced, in a way, in my mind, a kind of a detrimental effect, because all of a sudden the Air Force and the associated contractor people, who certainly regarded themselves as kind of an elite crew, and [looked] down at the Gemini spacecraft people kind of struggling along, and this had me somewhat worried.

And then in the fall of [1964], when they had erected the second launch vehicle on the pad, an event occurred that I thought might help simplify the situation in terms of getting the spacecraft and launch vehicle on a more compatible schedule, in that it [the GLV-1] was struck by lightning. The launch vehicle people very effectively and rapidly exchanged out all the electronic equipment, all the equipment, and there was no real concern about the mechanical equipment, but there was concern about the wiring in the [launch vehicle], and I was concerned about it, but I also was affected by saying, gee, if we really make these people change out the wiring—really what they would have to have done was put [GLV-3] on the pad and then rework [GLV-3]—then we would sort of have a compatible schedule between the spacecraft and the launch vehicle.

But the Aerospace Corporation, the Air Force technical advisors, had worked out a testing procedure involving impedance measurements that they felt very strongly would, if the test was made and satisfactory results obtained, they would be able to indicate that the wiring was satisfactory. But I guess maybe because of this desire to get this schedule compatibility, I was insisting that they take the launch vehicle down.

So it went above my head to Bob Gilruth, and Gilruth decided that he would go along with the Air Force on this one. He explained it to me, he said, "You know, they're very strong in their opinions, they have a pretty good analysis, and although we're not sure of it, I'm not going to buck them at this stage." But I was very concerned. I thought now, boy, I might very well be pulling another Chamberlin here of losing my control and stature with respect to these people. But it didn't exactly happen.

The activity proceeded toward a launch in December of 1964, the GLV-2, and now I'm talking about the flight program here, and what happened in the first launch attempt was that the launch vehicle failed to lift off. And it was a problem that the Air Force knew about but really had not really encountered in terms of anything that had produced a problem. Frequently, you'll get into these things where you know something is a problem, but you've not encountered it, so you go along.

This problem, which involved an asymmetric load on the nozzles of the launch vehicle, produced an overload and broke apart a so-called Moog valve, a hydraulic valve. So this is the one time the launch vehicle was the bad party. In fact, though, they turned to and, in extremely short order, diagnosed the problem, redesigned the valve, got the people to remanufacture the valve right over the Christmas season, and we were ready to launch again in January.

But those sort of two events really produced a strong melding of this team, that is, the Air Force and NASA and Martin and McDonnell, and all the people involved really behaved as a team. You really couldn't tell one organization from the other, so that had a very desirable effect.

But anyway, we launched the Gemini…II [mission] in late January, and it was just a ballistic flight intended to produce the worst [reentry] heating conditions that we would ever encounter. It was actually a late abort, very late in the flight of the launch vehicle. Simulated that, and that would produce the worst heating condition on the spacecraft, and it was all very successful. There was a slight heating problem detected. The Gemini [spacecraft] came in at an angle of attack in order to adjust the landing point, and because of the heating problem, we actually had to make a minor adjustment as to what angle attack it actually operated. But in general, the flight was extremely good.

So we're into the flight program. The next flight [in March of 1965] was a flight to test out the man-machine relationships. The Gemini…II was an unmanned flight. The [Gemini III] flight involved two astronauts—an experienced astronaut, Gus [Virgil I.] Grissom, who later, of course, died in the Apollo [1] fire, and John [W.] Young, who was a rookie astronaut. I don't know whether it was true at this particular time, but there was a time in the not too far distant past in which he had flown more flights on more [spacecraft] types than any other astronaut. Anyway, he was a rookie pilot on that flight, did a good job.

Again, the spacecraft worked with really no outstanding problem, so we were really ready to go. Nobody had gotten too excited yet, in terms of the public or anyone else. But it was very satisfactory. The landing point was quite a ways off. I think, because Grissom had a little trouble really following the displays. He was to follow computer-generated displays, but for some reason or other, wasn't able to do it. So I forget, the landing was off maybe 100 miles even, or something like that, but produced no real problems.

So we were ready to go into the next flight, feeling very good about the spacecraft and right after that GLV, the Gemini II flight. No, I've got that wrong. It would be the Gemini III flight, with Grissom and John Young. Gilruth approached me and said, "Do you think we could do an extravehicular activity [EVA] on Gemini IV?"

We had discussed and had provisions for extravehicular activity. In fact, we had discussed having a stand-up EVA on one of the early flights, either IV or V, and so forth, but Gilruth described an activity where the pilots [astronauts] would get out and actually float in space, in a weightless environment, and use a little maneuvering pneumatic gun for control. And although I had some reservations about certain aspects of this, I said, “Yes, I think we can do it,” but it was going to be a fairly hurried-up thing.

By the way, the Russians had conducted an EVA prior to that. I think one time in the early [taping—Roy Neal’s interview] I said two to three weeks; I meant two to three months. But it was before the Gemini III flight. We didn't know too much about it, but that probably was, to some degree, an incentive to conducting an early EVA. So we went ahead with it. The chest pack that was used for emergency oxygen supply and so forth, in case [the oxygen hose in the] tether broke, was an item of concern, in that it was not through its qualification test, but we hurried that up and got pretty well satisfied with it.

Once the EVA was announced—it was discussed whether it should be maintained quiet or not, but the powers that be ultimately decided, well, it'll leak out anyway, better announce it. Well, there was a tremendous interest in it, almost an unbelievable interest in it. For instance, there were more press requirements in terms of people wanting to attend either the launch or the mission than ever before, and about as many as was on Apollo 11, believe it or not. It was just hard to believe how much interest in what I thought was kind of a single aspect of the Gemini Program.

But anyway, we made the flight [in June of 1965], and we got up and we tried to station-keep with the launch vehicle, and that was not successful, and nobody knows exactly why it wasn't. It was either, I think, because they waited till they were too far away to station-keep—some people say we didn't understand orbital mechanics. We understood orbital mechanics, but if you're close enough, you don't need orbital mechanics. Or it may be a case where Jim McDivitt didn't use his thrusters aggressively enough to close the distance.

But anyway, that part wasn't successful, but when we got to doing the EVA, it was very successful. They had a little trouble opening the hatch and they had trouble getting back in the spacecraft, scooching down enough and closing the hatch, because of the pressure, suited configuration and the tether in the way and this type of thing. But the EVA itself was just a resounding success, and it was broadcast on the ground, part of it in real time. That is, people heard it actually going on, and the astronauts were extremely enthusiastic about the activity. We could tell they were pumping a lot of adrenaline. Of course, for the first time, these things were broadcast into Europe.

So after the mission was over—again, there was a computer failure, so we didn't make an accurate recovery again, an accurate landing point, but after the mission was over, everybody seemed to almost go crazy, in my mind. The President came down here to Houston to see the astronauts return from the mission. They had a monumental parade and activity up in Chicago. They decided it was going to be in Chicago. And I can remember all the bridges over the route from the airport were just loaded full of people. Every single one of them the whole town was loaded full of people.

Then a few days later, the astronauts and myself and our families were invited up to the White House to get medals, and then we went to the joint session of Congress, a State Department dinner, and then we went back to the White House and the President had us stay overnight, all the families. I think that's the only time that's ever happened, by the way. And [we] got up early in the morning, took Air Force One over to Paris to the air show, and I was just awed at the same way, in Paris, there were just people all over the place.

The President didn't go, but the Vice President did, and the crowds were so massive that the Secret Service lost the Vice President for a period of time. So that was quite a—there was nothing in the Gemini Program that was anything ever like it, even though, in my mind, some of the accomplishments were more complex and difficult and more important to the program. Not that EVA wasn't important. So anyway, that was Gemini IV.

Gemini V was to go on a one-week mission. Actually, I think I said seven days [in the Roy Neal taping], but it was eight days, and actually accomplished eight days, but it wasn't easy. We launched into orbit [in August of 1965] just fine and the crew was to launch a target called a Rendezvous Evaluation Pod, to practice rendezvous, but shortly after they got it launched, they suddenly saw that the pressure in the oxygen supply to the fuel cells was dropping down very rapidly, and nobody understood immediately why.

As a matter of fact, Chris [Christopher C.] Kraft [Jr.] was making arrangements, really, to bring the spacecraft back in, and he was here in Houston, I was down at the Cape, as I was during all launches. I would come back in a jet airplane to Houston as soon as I could. Anyway, John [F.] Yardley, a very brilliant technical man from McDonnell, who, by the way, ultimately headed up the Shuttle Program for NASA, was available in the VIP room, and I went out of the operations room at the old Mercury Control Center down at the Cape and contacted Yardley with a question about how long could we operate on batteries. We made some quick calculations of how we could, by powering way down, which we felt we could do, we could operate for quite a long length of time.

So I'm contacting Chris, and he also had his people work the problem and concluded the same thing, so they said, well, we would go on. But the important thing that Yardley did, he said, "Well, look, we've got—"—and this is one of the beauties of the Gemini system. Each system was almost self-contained by itself. They would have their own power supplies, for example. They would have their own controls. They were physically integrated.

So all these systems had an operating system at the McDonnell plant. So we called and asked them to start running, at these very low supply pressures, these very low oxygen supply pressures, and the indications were, the fuel cells kept operating. You couldn't operate them at as high a power, but they kept operating.

This data was brought to Chris' attention. He agreed that we could fly for quite a long time, and they ultimately started powering back up. The pressures remained low, but in these McDonnell tests, we found we could run these fuel cells at pretty low supply pressures, something that had never been tried before. And so we kept right on chugging along. We were afraid of it, so we powered up for a while to run some experiments, some simulated rendezvouses, and some other experiments that required a little bit more power than we would like, but we then powered back down.

What we did was run all the experiments sort of in one day that required quite a bit of power, and then the rest of the time in orbit, we spent doing experiments that didn't require much power, like visual experiments and camera experiments and things like that, and the guys drifted along for the whole eight days. Again, there was a programming error, human error, ground human error, wrong input into the computer, and again, we didn't accomplish a [satisfactorily] accurate landing, but we showed that people could fly for eight days.

As a matter of fact, the medical people really were very concerned about these long-duration flights, even the Gemini IV four-day mission, because that was four times as long as [L. Gordon] Cooper [Jr.] had flown on Mercury [MA-9], and they saw these various types of deleterious physiological trends, loss of calcium, calcium loss, and cardiovascular deconditioning and the various other things.

Again, the program office didn't really worry about it too much. We figured they probably could do it, but in each one of these cases it took a lot of convincing of the medical people, and the eight-day mission showed that an Apollo flight indeed could be made. A nominal Apollo mission would be about six or seven days, if you wanted [a reasonable lunar surface stay, you might go beyond that.]

I might mention one other thing, and that is, I'm talking mostly about the main things that happened during the missions, because it would be almost impossible to talk about all the little things that were done. For example, in the whole program, there was something like sixty different types of experiments conducted. They were in the medical area, the technical area, the science area, and then there were special experiments that the military wanted conducted, and we had some problems with those, because some of them were classified, and it was very difficult to keep the press from wanting to know what was going on in certain cases, but we did manage to not create too much difficulty.

But anyway, I should mention one other thing we—I'll go back a minute and mention one other thing we did. We conducted a lot of activities off line, so to speak. The things that weren't absolutely required for the major mission objectives were assigned to various elements of the Center here to conduct. [EVA] Pressure suits were one thing. People in the Environmental Control Group—I forget exactly what it was called—worked the [EVA] pressure suits and the chest packs and the EVA thing, because we didn't have to conduct the EVA [on any particular mission] and it could be inserted at any particular time in the program.

Similarly, it was true of these experiments, and there was a separate experiment program office, and that particular program office handled these roughly sixty-odd experiments. Well, with the exception of the Air Force experiments. There was an Air Force group assigned here that handled the Air Force experiments.

Another off-line activity was a tape input system for the computer, and that proved to be very important because it allowed us to try all sorts of rendezvouses on the same mission. We could program the computer, try one rendezvous, reprogram the computer and so forth. I guess that capability came in about—well, I'm not quite sure. Maybe Gemini IX, but it might even have been earlier than that.

But in any event, that off-line activity was very important because all the other [development] activities did not place any [major] requirements on the Center. We had about a hundred-man office that really handled almost everything in Gemini except for these off-line activities, because the whole Center was so tied up with Apollo. So the hundred-man group worked with the contractors and the Air Force to develop these various things and then ultimately fly these things.

Gemini was also unusual. It was not too much different from Mercury, though, in that only one Center, this Center [Johnson Space Center], conducted the whole program. All the vehicles [and] spacecraft were managed out of this one hundred-man office. Initially, the Agena was given to Marshall [Space Flight Center—Huntsville, Alabama], largely on the basis of, well, gee, they're supposed to be the people doing the rocket propulsion type of activity. But again, they were so tied up with Apollo that they didn't pay too much attention to it, and so it was taken back and reassigned here. Incidentally, that wasn't very helpful, either, because of the schedule relationship that I spoke about earlier, we didn't pay an adequate amount of attention to it either when we got it, and that came back to haunt us, as I said before.

I might mention a little bit about the Gemini Agena. The Agena was a standard Air Force upper-stage, a vehicle that was used to help get things into orbit as the last stage in a stack or to maneuver in orbit, and it was a vehicle that had some considerable development time behind it and considerable operational experience behind it.

But we wanted to do a number of different things with it than were done with the standard Agenas, so it produced modifications to it. We had to have more restarts than was originally available on the [standard] Agena. The auxiliary propulsion, secondary propulsion system was also redesigned because of considerations of operating with a docked spacecraft, and then there were other design modifications, like it had its own display system that the astronauts could look at, out through their spacecraft window, to see how the Agena was doing and so forth.

So there were a considerable number of changes, and because we weren't paying adequate attention, many more changes probably than was really required, because, as I say, once you get engineers working on a problem and they've got time, they're going to make it better and so forth. So there were problems.

And then the Air Force itself provided very poor communications in to us for a long while, particularly the people dealing with the standard Agena, and it created problems. For example, they changed the entire wiring harness on the Agena without telling the people working on the Gemini, so all of a sudden we were assembling stuff and nothing works. So it has to go through a complete redesign. So the Agena ended up to be a real headache for us, probably mostly our fault, though.

So let me go back to the flight activities for just a minute. The [Gemini VI] mission was to be the first rendezvous, and it was to use the Agena as the target vehicle, and the Agena was to be launched on an Atlas, and the Atlas would be launched first, place the Agena into orbit, and then the Titan would launch the Gemini spacecraft and it would perform a rendezvous. Of course, the Agena had a radar transponder so that radar can be used and so forth.

The first attempt to launch the Atlas—and this was in the fall of '65—produced a failure. Initially, there was some thought that it might have been the Atlas, but from telemetry and so forth, it was pretty obvious that it was the Agena that had somehow or other produced an explosion, and so we had a real problem. We had a spacecraft sitting on the pad, waiting to be launched and to rendezvous with a target that no longer existed.

So we were very discouraged, but Walter [F.] Burke and John Yardley approached myself and a fellow by the name of [G.] Merritt Preston, who was in charge of activities for the spacecraft down at the Cape, and suggested that we launch two Gemini spacecraft. The only problem was, we only had one launch pad. Most everyone thought that couldn't be done, including myself, that we could turn something [the launch pad] around.

But it so happened that the spacecraft that was to follow the Gemini VI was Gemini VII, which could fly for fourteen days, so the feeling was that if we could turn the launch pad around and launch two spacecraft within some reasonable period less than, well, I'd say well less than fourteen days, that we could possibly do this.

As a matter of fact, there were a number of studies. The headquarters had done a little—frequently, they would make some side studies, and they had done studies of what they called the salvo launch, which is somewhat similar to that, and then the Martin people down at the Cape had looked at a very rapid pad replacement. Initially, they had told us they really couldn't accomplish this, but they had run a study and they dusted off that study and concluded they could.

But anyway, at least at the outset, the whole idea was very negatively received, except these McDonnell guys persisted and they talked to everybody under the sun, and gradually everyone came around that they couldn't think of any reason it couldn't be done, so we went ahead and did it. But it wasn't that easy.

The existing launch vehicle was taken down and stored, because it wasn't capable of lifting the heavier, long-duration mission spacecraft, and so a new launch vehicle was erected. Gemini VII was placed on top of the new launch vehicle and launched [in December of 1965], and we wanted to get the next vehicle off in terms of about a week, and we could have taken longer but we wanted a pretty short target time. And we did get it ready to be launched in about a week. I think it was eight days.

The attempt to launch it was a failure again. Again, the Titan shut down. What happened was the abort sensing system sensed something was wrong and shut the launch vehicle down before it had lifted off. The difficult part was that a liftoff signal had been sent to the spacecraft, so the spacecraft clock started, so as far as the spacecraft is concerned and as far as the displays in the spacecraft are concerned, it said it was flying.

In retrospect, you know, everything happened so fast that people really didn't think too much about it right at the time, but it was really surprising that the astronauts didn't punch out with their ejection seats. I think part of the reason was, they didn't like the ejection seats any more than I liked them. [Laughter] But anyway, they decided the launch vehicle had not lifted off and they did not eject, and then the erector was erected around the spacecraft and they were taken out.

So we then contacted the Martin people and they had immediately determined, from telemetry, that the umbilicals that went up to the tail of the Titan had been improperly safety-wired and had fallen out, and that's what produced the abort signal. So this seemed very easy, that we just make sure that things were done properly the next time, and we would take off within a day or so.

But just about the time that we solved that problem, an Aerojet engine contractor came in and said the engine's start cartridge started firing, and the engines were coming up to speed, but one of the engines did not come up to speed in a regular fashion. So I said, "Well, I don't think we can launch unless we explain that."

So I can't remember—we worried about this for a day or two without really understanding what was happening, and then an Aerojet engineer came up and indicated that he had found the problem, that a dust cover was left on a port in the start cartridge, and this had produced the problem.

This was a major lesson to us that happened one more time later in the program, and that is, there was a request from the Air Force to dissemble the start cartridges from the launch vehicle for all the Titans, so that they could be inspected. There was some indication that the grains in these start cartridges, the propellant grains, might be cracking, and so the Martin Company, who was the launch vehicle contractor, undertook this inspection, but using the engine contractor's procedures, and the words in the procedures was to place dust covers on all open ports, and then in the reassembly, the statement was made, "Remove all dust covers." It didn't say how many, but because of the unfamiliarity and the fact that the procedures didn't indicate the number, one dust cover was, in fact, left on. So it shows you that your procedures should be very rigorous, but the people that are also the most familiar with the job should be doing the job and not depending on somebody else.

But anyway, it all ended happily, because I guess about four days later, we were able to launch. I think it was eleven days after we first started turning the pad around, that we launched and in time to conduct a very successful rendezvous. The crew had no trouble rendezvousing or station-keeping. Each spacecraft walked around each other and looked at them, and there were some surprises, like there was debris hanging out of the adapter area of the spacecraft that nobody had expected to be there and so forth.

But the other thing that was very positive—the rendezvous was certainly very positive—what happened was, Wally [Walter M.] Schirra [Jr.] very conscientiously trained to do a very accurate reentry and follow the needles very properly, and he did a very accurate reentry, not as accurate as some of them later, but one that was plenty good enough. I think it was like ten miles or something like that. So that was very successful.

The 76 mission [the Gemini VII/Gemini VI-A missions were often called the 76 mission] was very useful to the Gemini VII people [astronauts], too, because it kind of kept their interest up, you know. They were going to be visited and all that sort of thing, and then when the problems happened, they were right with it and so forth. So it probably enhanced their ability to fly for fourteen days. They did have very considerable difficulty in staying comfortable. A special suit was designed for them, but the suit proved to be way too warm, and it was decided that one or the other could remove their suit if the other kept his on, but it really didn't work to the satisfaction of the crew, and ultimately they were allowed to take both suits off.

Then the fuel cell system did give trouble in terms of water scavenging, in other words, too much water piling up and tending to flood the fuel cells, so they had to be nursed along, but operated throughout the entire mission. And there were some thruster failures. There were thruster failures on Gemini VI, the eight-day mission, Gemini V, the eight-day mission, too, but neither really had an important mission effect. Then again, the crew made a very accurate landing. This was just before Christmas, I remember, and it made for a very pleasant Christmas for everybody, I must say.

But we didn't stay out of trouble for long, because on Gemini VIII, the Agena [whose explosion problem had been corrected] was launched by the Atlas very satisfactorily and was performing very satisfactorily while it waited for the rendezvous. The rendezvous launch occurred essentially on time. These had to be very accurately timed launches to conduct rendezvous, and, indeed, a rendezvous was accomplished with no trouble, but this time it gave us also the opportunity to dock. The Agena had a docking collar that allowed the spacecraft to nudge in and lock into the Agena because at some time or other in the program, maybe on Gemini IX, we would fire off the Agena in the docked configuration. This was quite important for Apollo, because they had to do that in terms of the lunar module and the [command and] service module. They had to dock and then they had to demonstrate that structurally, the firing of a big stage in a docked configuration, that the structural analysis was all right and the structural dynamics were all right, and this sort of thing.

So anyway, we got up and docked [in March of 1966]. Most of the people, once they got into orbit and everything was working satisfactorily, took off either for Washington [DC] or for Houston. The Washington people took off because there was a Goddard dinner that same night and George [E.] Mueller wanted to attend that dinner, and the McDonnell people went off to Houston, but I decided to stay around for the docking. After the docking, my people were suggesting that we take off for Houston, and for some reason, I decided not to. I don't know exactly why. I guess I wanted—the crew had gone out of range of stations and were not indicating what their status was.

But when they came back out, they had had a very severe problem. I say that with a smile on my face, but it wasn't a very smiling situation at that particular time, because shortly after they had docked, they had detected a tendency for the dock configuration to roll, and they were able to stop it or slow it down, and it didn't tend to roll too fast, but they immediately suspected there was something wrong with the Agena, because the spacecraft had always performed okay from a standpoint of [not encountering] uncontrolled roll rates or anything like that.

So they decided to undock, and then because of the lower inertia of the spacecraft alone, the spacecraft really spun up pretty badly, and the crew had a hard time really isolating the problem, because with this roll it's sort of like on some of these amusement park rides, if you move your head the wrong way, you get vertigo, you know, and so they were having to be very careful about their head movements. But apparently, very calmly, they were able to establish that it was a stuck-on thruster, and they shut off the whole Orbit Attitude [and] Maneuvering System [OAMS], the system that was used to orient the spacecraft in orbit, and also to do precision translations.

But to get the roll stopped, they would have had to turn it back on and they knew they couldn't do that, so there was, fortunately, a redundant system aboard the spacecraft called the Reentry Control System [RCS], and they used the Reentry Control System to stop the roll, and that's about the time we heard about it.

Fortunately, I was at the Cape so that Chris could call me and ask me if there was anything about the configuration that I knew about that would preclude early reentry. He was somewhat worried about the fact that we had this rather large [Astronaut] Maneuvering Unit [AMU] stowed in the adapter section, and when we jettisoned the adapter, would that give us any problem, things like that, and I was able to assure him that no, that shouldn't be a problem.

So the only problem was when and how to get them down. They'd used about half of their reentry maneuvering fuel, so the idea was to get them down in a hurry. We turned the problem completely over to the flight controllers from that point on, and they determined where to land, and it was not in a normal landing area, but it was in a designated area for emergencies in the Western Pacific, and in a less than an orbit, they retrofired and landed in the Pacific.

It was kind of interesting, because at this Goddard dinner, I was to get the Aerospace Engineer of the Year Award in 1966, and the Vice President was up talking when this was happening, and they didn't want him to finish until they were able to say that the thing was under control, so they slipped him a couple of notes, indicating he should keep on talking, and of course, Hubert [H.] Humphrey was very good at it, he had no trouble talking, so he did, and was able to tell the audience that everything was okay, and they went on to give me my award. Jim [James C.] Elms, at one time, he was Bob Gilruth's deputy down here, accepted the award for me. And that was the end of the Gemini VIII mission.

So we go on to Gemini IX. We hadn't tried an EVA since that very impressive experiment that we did on Gemini IV. Let me make sure I get these numbers right. Yes, Gemini IV. But somehow or other, we talked ourselves into committing to a very major EVA exercise. This involved having Gene [Eugene A.] Cernan exit the spacecraft and move down into the spacecraft adapter area, which is at the open end at the rear of the spacecraft, where this [A]MU, this [Astronaut] Maneuvering Unit, was stowed—it was one that Chris was worried about on Gemini VIII—and don it. That was a device in which the astronaut would then fly free. There was a big argument in advance of the mission as to whether he should be tethered or whether he shouldn't be tethered. Ultimately, the idea was to at least use the tether to get back into the adapter area and so forth. In fact, I guess you had to use the tether.

But when Gemini IX was launched [in June of 1966], again, it conducted a perfect rendezvous. It was a different type of rendezvous than we had used previously. The first rendezvous were rendezvous after four orbits of flight. This was after three orbits of flight. It speeded things up a little bit. But when we got there, we found that there was a problem in that the shroud covering the target docking adapter—incidentally, this was not the Agena on this particular case. It was a backup to the Agena [Agena Target Docking Adapter (ATDA)].

At the time of the 76 mission, I had asked McDonnell to come up with an alternate to the Agena, which they did do, and it was a much simpler version. It had less capability, no maneuvering capability and so forth. But we did use the same shroud that covered it during launch as for the Agena, but again, we hit the same problem. We had the McDonnell people assemble that shroud, whereas Lockheed had always done it for the Agena. There were some electrical wires that were used to provide separation of the shroud [by igniting] of charges to separate it, but they were actually strung in a manner that caught the shroud halfway deployed. This is a thing called the "angry alligator." So the thing was hung up by these wires, half open, and so docking was not a practical possibility in this case.

They did run some additional rendezvous, using these tape inputs that I talked about before, which had the very positive effect of saying, you know, there's a substantial number of rendezvous we can run successfully, there are different types, but no docking. Nevertheless—

WRIGHT: Before you start on that next thought, we need to change our tape out, and if we could just take a quick break right here, and go from there.

MATHEWS: There was considerable discussion as to whether anything could be done about this hung-up shroud, like nudging it with the spacecraft or having Cernan do something in terms of either just plain going out or flying over with the [Astronaut] Maneuvering Unit. But I think nobody really felt, you know, that it was very practical. It was too dangerous to try anything like that. But it was intended to go ahead and conduct the EVA and ply the [Astronaut] Maneuvering Unit.

Cernan opened the hatch, installed various camera and mirror devices, as I recall. He did have some limited hand-holds. Well, they were really, I think—what do you call the patching device that you [used to tie] shoes? You know. You would know what I'm talking about. It doesn't make any difference. Anyway, he did have means of sort of attaching himself to the spacecraft as he went down to the adapter. Funny, I can't remember that. But anyway, they did not really provide enough adhesion. Velcro is what I'm talking about.

And he kept wanting to drift off and so forth, having a very difficult time, getting very hot and tired and so forth, but he finally did get back to the adapter and into the adapter and started activating the [A]MU. In the process, his overheatedness produced a fogging of his visor, so he stopped being able to see. At one time he actually went and sat in the [A]MU, and he was in a sufficiently restrained and comfortable position at that time, so the visor began to clear up, but the minute he went back to work, the visor would fog up again. So he said, "Gee, you know, if I'm out there flying around and my visor fogs up, how am I going to be able to see to do anything?"

So they called the EVA off, and he went back in the spacecraft, but he did spend a very considerable time outside. I guess in a way we did not appreciate the difficulty that he really experienced, at least enough to really start dealing with that particular problem.

But anyway, I'd say that mission was somewhat the low point, I guess, in my estimation of the program. Again, they did a satisfactory reentry and did a lot of other experiments, but in the main, a lot of the things we were intending to accomplish weren't really accomplished.

So on Gemini X, we decided, well, we'd go back to square one as far as EVA is concerned, and use the same equipment that Ed [Edward H.] White [II] had used in his Gemini IV experience, make it a little more complicated, but not too much so. What we did, after the Gemini VIII experience, even though some of the flight controllers were dealing with how to get the astronauts landed, others were dealing with what to do with the Agena, because Agena had experimental patches on the outside for purposes of determining the effect of the space environment on certain surfaces and looking at micrometeorite impacts and this type of thing. It was with the idea that we could some day go back and get these things, because we knew we could rendezvous.

So they ended up maneuvering the Agena very satisfactorily and parking it in a relatively high-altitude orbit so it wouldn't decay too rapidly, and leaving it there. So on [Gemini] X, we decided that will be the EVA exercise, we'll rendezvous with that Agena, and we'll get those patches back, those experiments.

The rendezvous that we envisioned at that particular time was probably the most complicated rendezvous we did in the whole program, in that it was a double rendezvous. We launched the Agena connected with Gemini X [in July of 1966], and then Gemini X went up and rendezvoused with it. Then they [the astronauts] used the Agena propulsion to change the orbit into relatively high-altitude orbits that allowed it to catch up with this old Agena that we left in space [from Gemini VIII], and it all worked extremely well. There was a number of concerns that we were able to eliminate prior to the launch and that we wanted to make sure that a retrofire would get us back down from these higher orbits and things like that, but we found we could, in fact, maneuver on down if we had to.

So over an extended period of time, these various maneuvers were conducted, and, of course, the old Gemini VIII [Agena] was without power, so it had to be a rendezvous [where] there was no transponder. So the rendezvous was really conducted primarily by ground control, this second rendezvous, but was done quite successfully. The crew was within station-keeping distance.

Then the problem was to get in quite close to this Agena, which was uncontrolled, but fortunately not rolling at a high rate, it was rolling at a very slow rate, and getting sufficiently close so that with a—I guess it was about a fifteen-foot tether, Michael Collins could go out and snatch these experiments off the Agena, while John Young's station kept. So Mike—well, first you'd say he initially—I can't exactly remember when this occurred, but he did perform a stand-up EVA, an EVA where he just opened the hatch and stood up there and kind of got used to being outside and in a pressurized suit, and John Young in an unpressurized spacecraft.

But then when it came to getting the experiments, he got out and made a grab at the experiments and sort of bounced off [or] drifted off, but he had this little gun with him, so he just maneuvered back to the spacecraft and reconnoitered. None of this involved very high [heart] rates on his part, though. He was a very cool customer, even though he seems like kind of a nervous individual when you see him. But then in the next try he went and got them and came back.

That was the main element of that particular mission, again, [they] had a very accurate reentry, and a lot of other experiments were accomplished…[on the mission.] But it, again, showed that all sorts of rendezvous sequences and ideas and so forth, if they're properly planned out, can be conducted very successfully. This was giving the Apollo people a lot of confidence about lunar-orbit rendezvous, of course.

Okay. On Gemini XI, we thought, well, we'll get a little more sophisticated with the EVA, so we launched Gemini XI [in September]. The other thing that was interesting about Gemini XI was, we used a one-orbit rendezvous. This is rather difficult to achieve because you almost have to know what you're doing, almost like a shot out of the gun. There's no chance to really make mistakes. You have to give them the parameters to adjust [the] orbit to the rendezvous orbit almost as soon as separation takes place from the launch vehicle. Of course, this was given from the ground. A lot of people indicated that they didn't think it could be done, but I don't know how many times, whether you could do it two out of three or ten out of ten or what, but the first time we tried it, we did it. So that was, again, a helpful thing.

The other thing that was done—let's see, was it before the EVA? I believe it was. We went to a very high altitude, considerably higher than Gemini X, where we had to use the high altitude for a catch-up. It was around, I think, about 1,000 miles, probably something a little less than that. Of course, that would be broken by a long shot by the Apollo missions, but that was the record for the day.

By the way, going back for just a minute, Gemini VIII created a duration record. Of course, it didn't last very long because—I mean Gemini [V] created a duration record. The mission, the Russians had not flown that long. So we had started to do things that the Russians hadn't accomplished—certainly by the time we were through with the rendezvous and the fourteen-day mission. We had done a more sophisticated EVA. We had done these rendezvous that they had not done, accomplished much longer flights.

And there was reason for that. When I went over to that Paris Air Show that I mentioned after Gemini IV, we saw the long-duration Russian spacecraft, and it was just loaded full of gaseous oxygen bottles. You know, there were just bottles…everyplace they could put them, they had a bottle because they had to use gaseous oxygen. We used cryogenic oxygen. In talking to Roy Neal [interview on 1 May 1998], I mentioned that this oxygen was stored in a special cryogenic state, where the fluid was supposed to be kept at the triple point of the fluid. Fluids have a triple point where they're neither gas nor a liquid; they're sort of a fog, I guess you'd say. And they maintain a high [density] under those conditions, but they're homogeneous so they're easy to extract.

When that failure occurred [on Gemini V], the loss of pressure in the oxygen cryogenic tank, it was because a heater failed. I didn't mention that. But that didn't allow us to keep the liquid at the triple point, and so that's why the pressure dropped. What happened, I guess, is that fluid flashes into liquid and gas with blobs of liquid bouncing around in a weightless state and so forth. Anyway, I gave Roy Neal a technical presentation that wasn't totally technically adequate, but that is what the story is on that.

So, anyway, going back to Gemini XI now, we went to the high-altitude mission and accomplished that very successfully again, waiting with bated breath, to perform the EVA. And sure enough, when Dick [Richard F.] Gordon [Jr.] got out, he got into all sorts of problems. Again, we had somewhat better hand-holds. He was supposed to go forward and perform some tasks at the nose of the spacecraft, and then he was supposed to go back to the adapter area. Even before he got out, he began getting quite tired and had difficulty positioning cameras and mirrors and so forth. When he got out to the nose of the spacecraft, he was not sufficiently supported by any means, and so he had a difficult time just keeping his position, particularly with the restraint of the pressure suit. Every time he'd make a move, his body would move in another direction, you know, and he had what he called a floating-up tendency that he described.

He did manage to connect a tether. We carried a thirty-meter tether on the Agena target vehicle, and he did attach one end of the tether to the spacecraft before he came back, but he had all sorts of trouble. He suddenly drifted back to the adapter unintentionally, and finally, you know, his exhaustion and everything else caused Pete [Charles C.] Conrad [Jr.] to say to him, "Get back in. We don't want to carry this any further."

It did give us the opportunity, though, to conduct a tether experiment, which was to take and have these spacecraft separated while connected to this tether and then rotate the spacecraft to have this system stabilize, keep the tether tight. You know, there was some difficulty in doing that. They did manage to do it and even managed to spin it up at progressively higher rates, which demonstrated not only the principle of station-keeping with a tether, but also the principle of artificial G. They never got enough rotational rate to feel any G, but they did conduct some experiments that showed that the G field was there. A small G field was there.

Then they again conducted a very satisfactory reentry. I think this was the one where they got within about half a mile or something like that. But anyway, all these spacecraft from here on out were coming very close to the aiming point.

But people became very unhappy with us in terms of our EVA performance. The deputy administrator, Bob [Robert C.] Seamans [Jr.], a very good friend of mine, I'd known him ever since about 1950 or something like that, he was on a committee with me in the fifties, but he was very unhappy with us. He had already set up a group right after the Cernan thing to kind of look at what was wrong with the particular mission objectives that we weren't achieving, and it was just a very small group. I was on it, by the way.

My friend Jim Elms was the chairman. He, by that time, was Deputy Associate Administrator for Manned Space Flight, a position I subsequently held. He had left the deputy's job down here to go back to industry, and then had some health problems and ended up deciding to come back. So he ended up in Washington [DC]… Well, anyway, he chaired this particular group.

They began looking at EVA, and I think it wasn't so much…the things they came up with, but the fact that they motivated us, you know, we'd better solve this problem or we're going to have our management on our neck. Not that they weren't already.

So what to do. Well, in the first place, we designed for Gemini XII some very carefully thought out experiments to be done that represented the kind of tasks that you would expect people [EVA astronauts] to really do, and then we began looking at the kinds of restraints and so forth that really seemed to be involved in helping [the astronauts] conduct these particular kinds of tasks.

But the most important thing was that we decided that Buzz [Edwin E.] Aldrin [Jr.] would train on these tasks and evaluate restraints in a water tank, in a swimming pool. People had suggested this before. The astronauts were, I'd say, reluctant to try it, for whatever reason. I think they instinctively thought, well, it really wouldn't be that good a simulation of weightlessness. And it turned out it's not so much a simulation of weightlessness, because you're certainly not weightless, you're buoyant and floating around like you do [in space], but you're not weightless, and furthermore the viscosity of the water makes operations difficult. They have to be done very slowly [and under proper restraint] or, as you know, in a swimming pool, you start moving in another direction and so forth.

So Cernan tried it out after his experience, and he said it had some merit, so Buzz Aldrin was trained in this particular activity, and he did this very willingly. So Gemini XII [launched in November 1966], as far as main operations were concerned, was largely devoted to EVA. There was a lot of other things that were done to clean up the experiment program. In fact, it was lengthened to a four-day mission so that a lot of things that were left open could be completed. But it was an EVA mission, and we actually had them involved in three EVAs, one an EVA that was a stand-up EVA to acclimatize him. He was out there, just standing there, observing and so forth, for a long length of time, a couple of hours, I think.

Then…he tackled the second EVA, which was the tricky one, but when he did that he had proper restraints, he had waist tethers and hand-holds, and in the adapter he had some shoes that were kind of harness type of shoes that he could fit into. He went out and did the EVA, and nothing to it, kind of thing. It really turned out to be fairly easy, which gave us a lot of comfort, because we had a lot of EVAs to look at and to accomplish. In Apollo, of course, that was in one-sixth G, [but] it was EVA. Then the Wet Workshop, which I guess I have time to get to, talk about in a minute, [as initially planned,] involved a lot of EVA activities.

[James A.] Lovell [Jr.], who was the commanding astronaut [Gemini XII], was kind of disappointed that we didn't do a lot of other things, I think, in the end [he] agreed with us that it was probably best that we focus on this [EVA], and it caused Gemini to end on a very positive note with just about everything that we set out to do concluded and feeling very good about it.

But as we started to phase down, several decisions had to be made… [The Gemini Program Office] was truly an elite group, I'd have to say, and would we keep the group together and what would they do, and if we didn't, what was the best use of these people? It really didn't take any of us very long to say, well, yeah, this is an elite group, but these other programs need the experience, and the best way to transfer experience is not by sending memos back and forth or instructions or procedures, but to transfer the people.

So we all agreed, but we also worried about the fact that we had to keep people on the ball, so to speak, right to the end of the program. So, in conjunction with a group that Gilruth set up [we] very carefully schemed out, the transfer methods and so forth, one of the things that we did was to take on additional jobs, too, as the program phased out, so we didn't have these people coming down too fast or with time on their hands. They might still be working on Gemini, but they could be working on something else.

There was really two things. The Air Force had always been interested in using Gemini in some way, shape, or form. I think I indicated that in the first taping [Roy Neal interview]. But they became very interested in it in terms of a transfer vehicle for what they called the Manned Orbiting Laboratory [MOL], which was their new program. Dyna-Soar [Dynamic Soaring] had fallen by the wayside and so forth. They were planning to use Gemini as the vehicle to do that, so-called Blue Gemini. So we helped them in developing the modifications, [so they] could operate from a higher orbit, the reentry and so forth, and also furnished them all the [used] spacecraft that they could have McDonnell refurbish to be used in the early phase of their experiment program. So that was one thing we did.

The other thing we did was—as I told you at lunch, we'll repeat it for the taping here—we got involved with a program called the Wet Workshop. A man from [Marshall Space Flight Center] had suggested that because of the EVA problems, it would be a good idea to take the upper stage of a Saturn V that had been used in a launch activity and was [in orbit, but] spent, to purge it and to reconstitute its environment into a living oxygen-nitrogen environment, and have the astronauts go in there, where they're not really worried about floating free, and you could check effects of different amounts of suit pressurization and all that sort of thing. And, furthermore, there would be simple structures in there that didn't interfere with the tank function for its launch purposes, that could be used sort of as a jungle gym to try various actions.

And everybody thought this was a pretty good idea, and they gave Gemini the job of coming up with what they called an airlock module. An airlock is a device, an intermediate device, it's used in diving, or was used in diving, too, in which you transferred from one environment to another environment in an intermediate device. For example, you transfer into this device, which is under pressure, and then you allow the pressure in space to reduce, and then move out. In fact, the [Soviet] astronaut [Alexei A.] Leonov used that airlock principle in his first EVA.

So we were to [procure] this airlock. It also contained tankage by which the spent stage would be purged and also the environmental reconstituted. And McDonnell accepted this contract, accepted this activity as a part of the [Gemini] contract—we were actually saving money at that particular time—and undertook this and [McDonnell] was doing a very good job on it.

We went up to the McDonnell plant up St. Louis, at the request of George Mueller, …our boss up in Washington, who asked to talk to McDonnell about this Wet Workshop. We didn't let our bosses, any more than McDonnell let us talk to their subcontractors, [without] them being in [present], we wouldn't let George Mueller make a visit to McDonnell without our being present, you know, for obvious reasons, and he understood it, too. But anyway, he wanted to go out there, so we went up there.

The so-called Wet Workshop program and the airlock program in particular was reviewed, and then George started talking in what I call his "Best of All Worlds" speech. He talked about, "In the best of all worlds, we can maybe fly this for thirty days or maybe even sixty days." Now, you would have to [put] an environmental control system and everything else in there [using an EVA], you know. Remember this was a spent stage. That was in the context of a spent stage, the so-called Wet Workshop.

There was a program in which this was a part, called Apollo Applications, in which there were individual flights using Apollo hardware, command modules and Saturn IB launch vehicles and so forth, in which various technical and scientific experiments would be performed, and there was a large number of these. Well, he talked about, “In the best of all worlds, we could actually put these in the Wet Workshop [using EVA] and carry these experiments out there.”

Well, I'll be frank with you. I couldn't believe I was hearing what he said, you know. It seemed so far-fetched to me. But we went along and let it [go by]—well, he later got talking to the Marshall people, and the Marshall people kind of accepted the idea, and, indeed, they started designing a system that with EVA and so forth you could do this, you know. Well, the Wet Workshop, actually many generations hence, or evolutions hence, became the Skylab, but at that time it was just what I described. I really thought, well, people would change it, which they did, you know.

Anyway, there was a concern about this in Houston here, because this was the first time Marshall ever was involved with a spacecraft. The Wet Workshop was a spacecraft and now it was going to be quite a big, fancy, elegant spacecraft. Marshall had only been doing launch vehicles up to then. So there was a concern here and a real concern about the practicality of this. The two things were put together.

Anyway, as the program phased down, we started transferring these people, and just as we had judged, it served a very useful purpose. Some very key activities in Apollo ultimately could be credited to some of the Gemini experience. And, of course, we trained a lot of astronauts. In fact, every Apollo lunar flight except one had at least one Gemini astronaut on board. That particular one, Apollo 14, happened to have a Mercury astronaut on board, Alan [B.] Shepard [Jr.]. By the way, he was the only Mercury astronaut that flew on Apollo.

The majority of the astronauts in the Apollo Program were Gemini-trained, but the training of the flight controllers was [also very] important, because this had become a very sophisticated operation involving gathering global data and then converting it into a display of information and having backup people ancillary to the Control Center also taking this data, specialists in each system taking this data, solving problems or looking for problems, as the case may be.

So there was a training of the flight controllers in this very sophisticated type of operation, and the flight control activities [in] Mercury, Gemini, and Apollo, because of the training that was done, always was a very professional-looking operation, and it's because it had these kinds of experiences and this kind of training. As I've said before, you know, the problems that occur are not the ones you anticipate, particularly, but you've trained on enough problems so that you can take a very professional approach to what's going on.

But I think one other thing, this is a little bit of a diversion, but I firmly believe that that was the very beginning of the Internet activities. See, it was a global activity involving things being sent back [close to] instantaneously and displayed in kind of an optimum way so people could use them, and that was really the first time, to my knowledge, that that had happened, that people had been able to do that on a global basis, at least. So it's, I think, a very good ancestor of the Internet activities. I've talked to the IBM people, for example, and they say the same thing, or even volunteer it, that this is true.

Well, anyway, we began shifting these people and then, of course, the flight controllers began moving into Apollo, and we not only trained the detailed flight controllers, but we trained the whole corps of Flight Directors. Chris [Kraft] kind of backed off. The Gene [Eugene F.] Kranzes and Glynn [S.] Lunneys and Cliff [Clifford E.] Charlesworths of the world, you know, took over the flight control, and it was because you had to. There was a shift problem. You couldn't keep these guys up day and night, so you had to have people, extra people.

But anyway, I guess, as for myself, as it wound down, I decided probably I didn't want to transfer into another program, particularly an ongoing program. Bob Gilruth wanted me to become the Deputy Director of the Apollo Program at that time, bring that experience in. I don't blame him a bit, you know. But I felt two things. One is, I was aware that nobody, including headquarters, was looking at any program beyond Apollo. And down at Houston, I again can see the logic in this, that Apollo was such a complicated, difficult thing to achieve, that Gilruth and [George M.] Low just didn't feel comfortable with diverting anything off this now that Gemini was over. And Jim [James E.] Webb up at headquarters felt the same way. He wouldn't let the people up there—there was no advanced program activity to amount to anything at that particular time period.

I approached Bob down here to take on an advanced program with some of the Gemini people, but he really didn't want to do it. So in the end, we disagreed on how I would be assigned, and looking at this Apollo Applications Program [AAP] involving the Wet Workshop, I said, "That's the closest thing to an advanced program that I can find, so I'll go work on that and try to make it do right." But that's another story. [Chuckles]

The main point I would be making is that it is very important to transfer the people, and what I did was not necessarily the best thing for the [Apollo] program. I really felt, you know, I had had enough stress in the [nearly four]-year period. I was running [Gemini], too, and my family, although they reaped benefits of it in terms of going to parades and stuff like that, it was very stressful on them. I know at the end of Apollo, George Low—not even at the end of

Apollo, when the Apollo landing was successfully conducted, he was very eager to get back to [being] the [Center] Deputy Director. He bugged me on that. I was up in Washington at the time, and he bugged me on that all the time to try to get him out of [Apollo]. I'm sure it was the same feeling on his part, you know, a tremendous strain trying to be program manager of these programs with so much at stake and so much risk involved.

[ORAL HISTORY 1]

NEAL: …Which brings us, I guess then, Chuck, to a final question. You've described for us in detail the Gemini missions. What was the overall importance, as you see it—looking back now—of Gemini in the scheme of manned spaceflight?

MATHEWS: Well, I think as a number of people have said before—Glynn [S.] Lunney wrote a little article for the Alumni News talking about how important Gemini was and saying that he felt that Gemini had really made it possible to conduct the Apollo mission on the time scale associated with the presidential directive to conduct it in the sixties—before the decade of the sixties was out. I feel the same way; and I give great credit to Chamberlin and Gilruth for this. The fact that the critical mission functions of Apollo were identified—such as flying for 14 days; such as docking two vehicles together and lighting off a large propulsive stage associated with them; such as, of course, rendezvous—all these things were done in Gemini, and done in such a way that it gave great confidence that they could be accomplished in a mission such as Apollo. If it had not been done in Gemini, there's no question there'd have to have been additional flights in Apollo in order to verify that these things could be done. And I think, therefore, Glynn Lunney is absolutely right, that they would not have made it in the decade of the sixties if Gemini had not existed.

The reason I give Gilruth a lot of credit is he recognized that there was a tremendous step between Mercury and Apollo, that there were a tremendous number of unknowns, [and that] there were a tremendous number of systems that had to be created in order for [it] to be successful. And he, more than anybody else, pushed to get a Gemini Program in there, even though some of the bright people in Apollo said, "Oh, we don't need a Gemini Program, you know." The other one [who] deserves great credit is Jim Chamberlin, who, in his conceptual work, put the proper systems in Gemini. Now, they weren't necessarily one-to-one relationships [with Apollo]. Some of them were. [In] the cryogenic tanks, for instance, there were problems with stress corrosion of the titanium tanks. Titanium was a material whose properties were not too well documented. When we solved that problem in Gemini, we solved it for Apollo. The same is true with the ablative thrusters that were used in Gemini. We had all kinds of problems with ablative thrusters initially. A solution ultimately presented itself that could be immediately applied to Apollo, because they literally were getting those thrusters from the same [contractor] as we were getting them from, but at a later date.

NEAL: As you think back across those years, from that very crude beginning to the end of Gemini and on through Apollo and into Space Shuttle, what are your thoughts?

MATHEWS: Well, I really think a tremendous amount was accomplished in that particular period, that particular decade. I'm a little concerned with the fact that the program ended up to be somewhat dead-ended; that is, there wasn't an easy follow-up to it. And there was quite a struggle in terms of coming up with what the next thing would be. So I think there are both negative and positive aspects. We moved the program very fast, went way beyond the normal curve of progress in accomplishing Apollo, for example; but, we had a situation where it was more difficult to be creative in the next programs. I think, as it stands right now, the program probably should have had a Space Station somewhat earlier than it did. But that was one of the problems, because there was really not a good spacecraft to provide logistic support for a Space Station at that particular time. So the Shuttle, or something like that, had to come along in order to get the Space Station in there. So in a way, our learning in terms of having people up there for very long periods of time and having plenty of time to understand, *look*, and feel about space and what it all means, has to await the Space Station. And it will be very interesting to see how that all comes out.

NEAL: Chuck, tell us about, if you will, what happened in the aftermath of the Apollo 1 tragedy.

MATHEWS: Right. I moved up to Headquarters in January of '67. I'd been going up there for some time, and was supposed to be working on what became the Skylab Program—it was called Apollo Applications in those days, but that's sort of beside the point. When I got up there, it was on a Tuesday—in terms of physically moving into our house—and the fire occurred on Friday. I was home hanging pictures or something; when I heard about it on the radio, it was in the evening. I went down to Headquarters, [but] there was nobody there. It was the day of the—I guess it was the Wright Brothers' dinner, I'm not quite sure. But everybody had gone to this dinner. And, of course, the word of this came out and there was much confusion, much discussion, and so forth about this. But, as I understand it, Jim Webb made the decision that we had *to* proceed with all possible speed in getting an investigation going or it was likely to be taken out of NASA's hands. Anyway, I wasn't privy to that; it's just something I've heard. But the next morning I got up fairly early, I guess. I arrived in my office, which [was] adjacent to George Mueller's office—no, down one floor from George Mueller's office—I'm confused there.

But I went up to George Mueller's office and found [him] and Bob Seamans there. We, naturally, did not know too much about the exact details of the fire at that time; but we pressed immediately on how to get an investigation going and who to head up that investigation. And I guess this—I think this probably had also been discussed that evening before. But anyway, it was decided the guy who was my original boss in the Flight Division at Langley, Floyd [L.

"Tommy"] Thompson, who was head of the Langley Laboratory at the time, would run that investigation; that I would take a NASA airplane to fly down and pick him up, and whoever he wanted to bring with him; and that we would go down to the Cape. Which we did.

We went about trying to decide who would participate in the investigation activities; and Thompson was very strong in his desire that a good many of these people would be non-NASA people—people from the Bureau of Mines, other people that had safety concerns about them. And in just a few days, these people were organized and down at the Cape. I was sent down there largely to be representative of the manned spaceflight organization, to make sure that, in as far as possible, the program was not impacted in a major way or any more than it *had* to be by this particular event. Thompson was not only aware of, you know, the need for that but he was [also] very much aware of the need to bring these people on board, [get] them to understand the situation, not let them think he was railroading them in any way; and, frankly, it made me very impatient. I got together—John Yardley was one of the other people [who] was assigned to the investigation. He was not involved in the Apollo Program at all otherwise. But he came and was involved in the investigation.

What we did was to set up a schedule of activities that could be used in terms of conducting this investigation and making sure we had all the things that were appropriate to [an] investigation involved there. At that particular point, I did leave the investigation per se. I was responsible for the Skylab activity; I felt I had to actually go back. But, a couple of important things did occur. One was that the Deputy Administrator, Bob Seamans, was—well, he resigned. But he still retained a very strong friendship with Jim Webb the whole length of time between the time that he resigned and the various activities that he was involved with subsequently to the fire activity. But the other thing that I think is important—at least from my point of view—was that Joe [Joseph F.] Shea, who ran the Apollo Program—and a very brilliant man [who] did extremely good works for Raytheon subsequently—was very unnerved by this event. He was a systems engineer, not too used to the kind of environment, say, a flight test organization involves; and he found it very difficult to deal with this particular problem. As a result, he more or less had to be relieved of his particular duties. The decision was made by Gilruth in Houston that I would become the Apollo Spacecraft Program Manager. And when I got word of that—I would like to go into this in more detail, but I can't at the moment—I did not think it was a good thing to do.

There had been somewhat of a rivalry between Gemini and Apollo in Houston. There was also a feeling, on my part, that we were not doing enough work in determining what should follow Apollo. So, one of the reasons I left Houston and moved to Headquarters was because I really felt I wanted to work on advanced programs. I had proposed to Gilruth that I do that in Houston, but he, at that time, wanted me to become Deputy Manager of the Apollo Spacecraft Program. But anyway, I did move to Washington. I was in—this is all happening in the Washington scene. But anyway, he and George [M.] Low, who was his Deputy, came up to Washington with the idea that I would take on this job. And it wasn't the only reason. I had just moved up to Washington, and my family was reluctant to move back. But, my feeling was that George Low was the very best man to take on this job, because he had been the Deputy Director of the Center, [was] well respected, and didn't have any rivalry problems. And furthermore, he had contacts where he could motivate and muster the entire resources of the Center.

So, they came up to Washington, and I guess Gilruth had told Jim Webb what his intentions were. But I had already talked to George Mueller and told him that I thought George Low should do the thing. So when they came up, I had George Mueller in a position to say, "No, it's not going to be Chuck Mathews. It's going to be George Low." And, George [Low] is the kind of person who always likes to have control of his destiny. In fact, that's one of his strongest capabilities, and a very desirable capability. So this is one of the first times he was caught completely short. He had no idea he would—it was actually a demotion for him. And he and I and Bob Gilruth ended up in a Georgetown hotel that night; and he said, "Chuck, I've got the worst headache I've ever had in my life. Let's go take a walk." But anyway, that's how George

Low ended up being the Apollo manager. And, one other thing about that: See, in a way, Bob Seamans was the fall guy for that. Joe Shea, I don't think you could say, was because there was—he had had problems. Bob Seamans was the fall guy. But, in a way, George [Mueller] had created some very natural problems in that he'd always approached Jim Webb as being—he was very optimistic about how Apollo was going, you know, pre the fire. He's that kind of guy; he's a very optimistic type of person. And it's very natural, particularly with industry people, I've found, that they're reluctant to tell you about their problems. They think they're going to get their problems solved before you find out about [them], you know.

Well, this bothered Webb I'm sure; but George Mueller had very strong contacts and very strong friendships in the Congress—for example, Tiger [Olin] Teague and so forth. They really liked him. And Jim Webb was wise enough to know that he should not really ruffle any congressional feathers. So he decided [that], no, it would not be George Mueller that would be moved out, it would be Bob Seamans. And, later on, he put another man into the Deputy position under George Mueller who he hoped would provide better information flow. But it really didn't work out. So in the end, he put me into that position; and he told me that he really wanted George to stay in that program and leave, as he put it, "with all flags flying"; and that I was to help George on the program. So I became George's Deputy at that particular time [February 1968]. I guess [that is] a little bit rough, in terms of skirting a lot of things. But because of time, I'll let it go at that.

NEAL: Very good, Chuck. Thank you.

MATHEWS: Okay.

[End of Interview]