NASA JOHNSON SPACE CENTER ORAL HISTORY PROJECT
ORAL HISTORY TRANSCRIPT

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INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is January 10th, 2003. This oral history is being conducted with Joe Gavin in Amherst, Massachusetts, for the Johnson Space Center Oral History Project. Interviewer is Rebecca Wright.

We thank you for taking time today for this project. Mr. Gavin, you were with the Grumman Aircraft Engineering Corporation when the nation first began talking about sending men to the Moon. What were your duties with your company at that time, and how did your role transition from the chief missile and space engineer to the space program's director?

GAVIN: You have to go back a little bit before that, because I really had two careers. One was in naval aircraft, and then when the space age came along, there was a rather different interest and requirement.

During World War II, I was a reserve officer in the Bureau of Aeronautics and was the project officer on the Navy's first jet airplane, which was very interesting, being quite young at the time, but I think they figured that a bright new graduate might understand this new propulsion better.

At the end of the war, I went to Grumman and began as a design engineer, eventually got into preliminary design, and eventually became an engineer on their first jet fighter and [project engineer on] their second jet fighter.

Then the space age dawned on us, and the first effort that Grumman made was a canister for the Echo balloon. Then came the Orbiting Astronomical Observatory; it was at the point where I was called chief missile and space engineer. I did not run the project. The project was run by one of my colleagues, Walter Scott, but I supplied consulting and arranged for the experts that he needed to work on the job. That project really was the groundwork that made it credible when we bid on Mercury. [For] Mercury, I think we had a fairly decent proposal, but the Navy said we were too busy at Grumman with some of our airplanes.

I have to speak a little bit about those airplanes, because Grumman was one of the first of the aircraft groups that got into designing to the mission. In 1950, the Navy had a problem finding submarines, and a competition was held to design not just a flying machine, but a whole system for finding the submarines, including radar, magnetic anomaly detection, and sonobuoys. We won that competition, and that airplane was one that had a lot of my fingerprints on it. So [it was] the systems engineering that was developed, this was before people talked about systems engineering, but it was systems engineering, and that plus the background of the OAO [Orbiting Astronomical Observatory] provided a reasonable chance to bid on some of the space programs.

After the Mercury competition went by, we kept our preliminary design group working toward the future, and when the Apollo business came along, we first started out to bid as an independent contractor, and we had lined up TRW [Corporation] and Douglas [Aircraft Company, Inc.] to be on our team, but then the General Electric [Company] management got together with our senior management at the time, and out of that came an agreement that G.E. would be the bidder. They, of course, had been involved in Air Force satellites and had something really to contribute in the way of background.

So we entered the G.E. proposal effort, and we spent a lot of time commuting to Philadelphia [Pennsylvania] and moved people down there to carry that out. Needless to say, we were disappointed when North American [Aviation, Inc.] got the nod, but at that time, or almost immediately after that award was made, we were invited to come down to Langley Field [Langley Research Center, Hampton, Virginia] and talk about the idea of lunar-orbit rendezvous. That was one of the most significant things that came up. John [C.] Houbolt has been adequately recognized, I think, on that score, but he deserved it, because he really carried the ball on that.

We determined that within our own R&D [Research and Development] money we would run a study to try to validate the concept. Tom [Thomas J.] Kelly was the leader of that study, and that was a very important study, because it proved conclusively that lunar-orbit rendezvous was the way to go.

If you look back at the sequence of events, Houbolt submitted his study almost at the time that North American got the award for the command and service module, but it took until the following June, from November to June, for NASA to thrash out the question. I was never privy to those debates, but one hears through gossip that there were some pretty heated arguments about whether they should stick with the original [Wernher] von Braun approach, Earth-orbit rendezvous, or whether they should go to lunar-orbit rendezvous. That decision wasn't made until, I think it was June, the following June, and that led to a competition in the fall.

So we decided we were going to bid that, and we did, and it was an interesting competition, because it was unlike any other competition we'd seen, because it wasn't a call for a design; it was a series of twenty questions. "Answer these twenty questions, and we'll see if we think you know what you're doing." And you had to do this within a limited number of pages with a limited type size. Typically in proposals, when you answer questions, you try to show why you picked the solution you did, and that all other solutions are inferior. This was pretty hectic, because the time schedule was short, and I think the Labor Day weekend was ruined for a lot of people, but we did write a proposal that was good enough to be selected.

Then we were invited to send a team to Houston [Texas], and at that time they had rented an unfinished apartment complex up on one of the bayous, and the NASA team, and we sent down, I think, about maybe twenty people. So we lived there right through Thanksgiving. I'll never forget that we got to the pre-Christmas point, and we were in a negotiation and we didn't finish it. We had to go catch an airplane, and believe it or not, the two of us who stayed to wind up the conversation couldn't find the keys to the rent car at that point, and it took us another twenty minutes to chase them down. Of course, we were late getting to the airport, but the airplane was late, so we did get home for Christmas. Then, of course, we resumed right after the holidays and signed the contract in January, and we were off and running.

We thought they had bought our design. NASA hadn't really bought the design. They thought they'd bought an engineering service. Anyhow, this is the design we submitted. [Gavin shows model.] This is typical of what we did in preliminary design in those days. As soon as we had a few drawings, the model shop would make up a miniature quickie model with [wood and] paper clips.

But it has all of the components of the eventual design. It had a descent stage and a descent engine. It had an ascent engine on the ascent stage. It had two hatches, and it had good visibility. In fact, the initial concept was something like—I remember saying to the group that we need something that's more like a helicopter so you can see where you're going. The study that Tom Kelly had run hadn't really defined this area in great detail. It just considered a certain mass and certain characteristics.

So anyhow, we thought we had defined a design. NASA said, "No, we just effectively hired a bunch of engineers," and that led to almost two years of thrashing out the details. It was a learning process, because Grumman had an interesting culture which probably differed from almost everybody else. Roy [Leroy Randle] Grumman, who had been a naval aviator trainee in World War I, had one basic direction to all of us, and that was, "You bring the pilot back one way or another."

So anyhow, we knew, understood very clearly, that it was our responsibility to be satisfied that this thing was going to work. Sure, NASA would have a role, but we weren't going to do something just because NASA said, "Paint it pink," or whatever.

So it took about two years with several iterations of mockups to determine the configuration, and that was the period where we got rid of the seats. Our initial concept was, the pilot has to have a seat. Then we thought, well, gee, for twenty minutes you really don't need it. So we got rid of the seats.

Then [we] began to look at how heavy the transparent material would be for this basic arrangement and said, "Can't do it that way." So in effect, [we] turned the front end inside out and pulled the transparency right up to the astronaut's face, and that's what produced the little triangular windows that are characteristic of the ascent stage.

But it took us only two years to really settle down what the configuration was going to be, and in the course of this, there were also meetings about the schedule and the cost. The contract that we'd signed was, I think, a unique attempt by a procuring agency to incentivize the contractor. The contract was set up to emphasize mission success, the schedule, and the cost. There was a complex three-dimensional relationship that said you could trade off between these. It didn't take us too long, maybe two or three months, to recognize that there really wasn't any tradeoff. You couldn't afford to trade anything away from mission success.

The schedule was obviously critical because of the other parts of Apollo that were moving along more or less in step. So that came number two, second priority, and the costs came number three. Now this, of course, made a number of people unhappy. The contracts people weren't happy with this, and that was true inside the company as well as at NASA. But that's the way it was, and we had to face it.

At one point, we came to a, I guess you'd say, a dead end in talking about the contract costs, and finally went on, "Look, we're going to spend so much a month, and that's it."

The other thing that was happening at the same time had to do with the schedule, and that was that NASA was saying, "Well, you're not getting people on the job fast enough." Now, in looking back at it in hindsight, I don't think we could have put people sensibly on the job much faster than we did. Maybe today [when] we have computerized databanks, you could accelerate the number of people involved. But in those days, a group leader, when he had a new man, had to introduce him to the group, explain what was going on, point out where he could get information that would affect the task assigned to him. I think that in that paper that I gave you, I commented on the fact that the rate at which staff was added was about as good as you could do without just having people sit and wonder when it was going to be my turn to find out what I'm supposed to do.

So the early days were difficult because of the fact—well, because of the lunar-orbit rendezvous decision, we were starting about a year behind the command and service module. We still had to make the same end date, and it was a continual struggle.

Also during this period when the design was being decided and fixed, we had to arrange our major subcontractors, which involved running competitions, making selections, and NASA's view on this was very clear that Grumman was to make the selection, but NASA was to have a little finger in it on approval. And running competitions takes time. But we did, I think, quite well in picking the people we did.

In the case of the descent engine, we treated that rather specially because it was going to be the first throttleable rocket engine that [would] throttle over a wide range, say, from something like 10,000 pounds down to 2,000 pounds. This had never been done, so NASA said, "Well, get two people going on it, and we'll make a decision after they've worked on it for a year."

So we selected TRW and Rocketdyne [Division of North American Aviation, Inc.], two separate approaches. TRW had—what I think of as the shower-nozzle approach, a mechanical nozzle that effectively worked just like a shower nozzle. And Rocketdyne worked on a scheme for introducing helium into the propellant line so that not as much propellant got into the combustion chamber. Of course, what happened was a year later both of them looked like they could do it, so then we had the problem of how do you select one.

NASA said, "Grumman, this is your job, but we want to be privy to it." So we set up a committee, and the leaders of the committee were myself on the one hand and Max [Maxime A.] Faget on the other. That's a name you will know, I'm sure. I think each of us had two assistants. We spent a day at each of the factories looking at the test results and talking to the people and so on. Then we got to the point of making a decision. Well, Max was very sensitive to the fact that NASA was not supposed to make the decision, so I made the decision, and he said, "Fine," and that was that.

Now, that story is not complete without telling you about what happened when we were going back to the motel after visiting Rocketdyne. We had picked a motel up in Beverly Hills [California] that was about equidistant between Rocketdyne and TRW. These were long days.

We got back to the hotel about nine in the evening. As we walked into the entrance, this man came running out, full flight, with a crying child in his arms. So we sort of watched that go by. We went into the lobby, and there was the security guard trying to calm down a woman at the top of the stairs, black-haired, terrycloth robe, with a pistol, raving that "That man has stolen my child!"

Well, the next morning we read about it in the papers. It was Marlon Brando retrieving his child after his estranged wife, Anna Kashfi, had spirited him away. I tell you, we got out of that lobby so fast, because this woman was distraught and screaming and waving this pistol. Anyhow, we disappeared. We may have [made] some good technical decisions on that trip, but none of us will ever forget that evening.

So anyhow, that's how TRW got picked for the descent engine.

The ascent engine was a little easier because it was supposed to be a derivative of the Agena engine, which had already been in space. I guess the reason I'm getting into this propulsion as much as I am is because I spent a lot of time dealing with the propulsion subcontractors. You know, when you look at an organization chart, you see a program director and then a bunch of lines and other people, but it really doesn't work that way. You tend to spend more time, in my case, where I thought it was more critical. But the members of the team—and we had a great team—all had specialties in their background that made them spend more time in different places, which you would never guess from the organization chart. So I spent time with the propulsion people. I spent a lot of time with RCA [Radio Corporation of America], which was a major contractor for communications and the rendezvous radar and so on.

You ask, well, what does a program director do. Well, it's like being the chief cook and bottle washer. You do whatever has to be done. You deal with the subcontractors one day, you deal with your own internal management the next day, because in a company there's always a competition for talent, and senior executives can become impatient about the schedule not being met or the cost overrunning. So it was a balancing act where the program director tries to keep the program on the right track despite what the internal management might think, and to some extent despite what NASA might think, because, after all, if [the product] doesn't work, it's our fault.

I should explain a little bit about our staff. We were fortunate in having a group of people, most of whom had worked together for ten or fifteen years. I think that's true in all but one or two of the key spots. One of the exceptions was Ozzie Williams, who came from Reaction Motors in New Jersey. He was in charge of the reaction control jets that controlled the lunar module and made it maneuver and so forth. But by and large, we had a cadre of people who had worked together, and this is true of the relationship between, say, engineering, contracts, and manufacturing. All of us had been involved in the manufacturing end of the business, because that's the way Grumman worked. Typically, in the Navy aircraft days, we had the engineers on one floor and the manufacturing was on the ground floor. We were not quite able to do that in the LM [Lunar Module] Program, because we eventually moved into a separate building because of the size that things grew to. But it was easy to get to the manufacturing floor, and the leaders in the manufacturing area were people that we had known for years.

Grumman was a peculiar company in some respects, in that nobody paid any attention to the organization chart. If somebody wanted to talk to somebody, [he] picked up the telephone and called, and it didn't make any difference who the person at the other end was. People felt free to do that. From a program director's point of view, that was great, because I'd get a call from almost anybody, saying, "Joe, do you realize what's going on down here?" or over there.

So with this kind of communication, [I] could really keep track of where the difficult points were and then be able, perhaps, to do something about helping them out.

The other thing that we evolved into about the time that the configuration was pinned down, was a daily morning stand-up meeting. We had a conference room which we outfitted with charts that showed progress in the program, and we'd gather somewhere between twelve and twenty key people there every morning and review where things stood, and that was the place where any group leader could say, "I need help," and he was wrong if he didn't say, "I need help," and he really did. It also prevented somebody from making a change without everybody being aware of it.

One of the things that you worry about—and this is true. We had this same problem in the aircraft business earlier. The change that gets made without everybody being aware of it causes problems, because the rest of the group doesn't adjust. Later on, this was dignified with the name of configuration control. We'd been doing this for years without realizing that it had a name. But anyhow, that meeting was daily for many years. Then finally, when we're in the operational phase, I think we did it three days a week, because the design was pretty well frozen.

Then, of course, there was the problem of dealing with the field sites. We set up a major site at White Sands [Test Facility, Las Cruces, New Mexico] to do propulsion testing. We, of course, had to set up a major operation at Cape Kennedy [Florida] to do the final assembly and checkout.

One of the things that caused us a lot of concern right from the beginning was the fact that it looked like we had a vehicle that could not be flight-tested. All of us were, I guess you'd say, graduates of the aircraft business. There you went out on the runway, and the pilot would taxi it fairly fast, and then he'd lift off a little bit and put it back down. If everything seemed to be all right, and after you'd checked it over, he'd make a first flight and not go very far, and maybe he'd not pull the landing gear up but just get up and fly it and get it back down.

Well, here we were with something that we couldn't figure out how to flight-test, and worse than that, we couldn't even figure out how to test the propulsion systems before the mission, because it became apparent that the storable propellants, while they had a lot of attributes that were good, were pretty good at [making] valves [stick], and it [was] not clear how you would purge the system adequately if you ran a test on the ground. Furthermore, all the conditions on the ground were completely different from the operating conditions, which were only in space, so they might not prove very much.

It took some time to face up to the fact that we just were going to have to launch the LM brand new for each mission without being flight-tested. Now, this was a real problem to face up to. At the same time, because we were worrying about reliability in general, and there were people who looked at the aircraft statistics of the time and said, "Hey, you're never going to get there," in the meantime, the failure [rate] on all of these equipments from experience is thus and so. In fact, one of the President's advisors, one of [John F.] Kennedy's advisors—I've forgotten his name, but I think he was his science advisor at the time—made a calculation that said it's going to take forty attempts to get a single successful landing. Well, obviously, that's no good.

So then we began to think about, well, how do we get reliability? That began to make us look at what we'd been doing in aircraft, which wasn't good enough. We kind of came up with the idea—and to this day I'm not sure who actually verbalized it the first time—was there should be no such thing as a random failure. In other words, if in running tests you find something that doesn't work, there has to be a reason for it, and if you're patient enough, you ought to be able to find out why it failed and then do something about it. Well, this was a new idea. This is a long time ago, if you look at today's calendar.

So what we did, basically, was to test everything at a breadboard circuit level or breadboard hydraulic level, if it involved fluids rather than electricity. Then we'd build a prototype and we'd test that, and we would then make what we thought would be the production version, and we would test that, and then we'd devise tests for each of the production elements that came along, so that by the time the equipment got put in the LM, we were pretty sure we had a reliable device.

We got into some very interesting arguments about, "Maybe you're overtesting. How do you know that by running that test you haven't strained something that will show up later?" To some of these questions there is no simple black or white answer. You have to go on experience, and you have to be clever about how you devise the tests. There are a lot of tests that we ran early where we looked at them later and said, "That wasn't good enough. We're going to redo that."

There was one major case that we debated with NASA for some time. Early on in the program, we had run a conventional structural drop test to demonstrate that the vehicle was structurally able to take the hardest landing that we had designed for. This is something that had been done in aircraft for years, and it was done fairly early in the program. Then we began to worry about the flexibility of the structure. We had built high-speed aircraft, where things were pretty rigid. Nothing is truly rigid, but if you shook [it] at one end, you'd [find]—the basic airplane has a natural frequency, and we were [comfortable] in a certain range. When you got to the LM, you could shake it at one end, and nothing would come out at the other end because it was so lightweight [and flexible].

So finally, we said to NASA, "We need to run a drop test with all of the electrical systems active to make sure that the flexibility doesn't upset the operation of the electronic systems." And after much debate and anguish, because that was going to be an additional cost and possibly a schedule problem, that drop test was authorized, and we did it. The electronics worked beautifully, and we found a tiny structural failure in one of the explosive bolts that were designed to separate the ascent stage from the descent stage. So the test was not a failure, and it did show that we weren't going to have the trouble we were looking for, and then we picked up this bonus in finding a minor weakness, which may never have shown up anyhow, because the drop tests were at a level of severity which far exceeded everything that was finally experienced.

How we got into this was simply that at the time we started, nobody had ever flown a rocket-propelled vertical takeoff and landing machine. So we were very conservative. We had thirty inches of stroke in the landing gear, and none of the landings on the Moon used more than about three inches. So the landing gear was basically overdesigned.

Of course, at the time we started, nobody knew what the surface of the Moon was like. We had an expert at Cornell [University, Ithaca, New York], Professor [Thomas] Gold, who said, "Look, there's ten meters of impalpable dust. It'll just go right down into it, and electrostatically it'll probably just cover everything up."

That gave us heartburn, and I remember we sat with some of the NASA people and said, "Well, now, really, do we believe this? What happens to the jet when it is coming down? Won't the dust all blow away?" Anyhow, we finally arbitrarily said, "We don't believe the ten meters," and went on from there, but we did retain some very conservative landing conditions.

We also worried about the sideways motion. You know, it's one thing to come down straight, but another thing is to sort of come gliding in. Then the question is, well, supposing you put the feet in little craters so that effectively you have no sliding at all? Or supposing you do slide, then you hit something like a curb?

Well, we looked at some, I think it was, about 400 different landing conditions, where you came in nose down, downslope, or nose down upslope, trapped the landing gear, didn't trap the landing gear. We did build a quarter-scale model and actually threw it into a sandpit to validate the computer setup.

You have to realize in those days computers were different from what [is] going on today. We were using big IBM [International Business Machines Corporation] mainframes, and the engineer would formulate the problem, turn it over to the computer expert, who would program it, and if you were lucky, you would get the answer the next morning.

Anyhow, we ran all these computer runs about the landing condition, and finally said, "We think we've got it." Of course, we worried about the business of tipping over. We designed the ascent stage so that it would take over even from [a steep] angle [of response]. It could take off even if [it] weren't sitting straight up.

So there was a lot of conservatism in it. In hindsight, if we were doing it again, the landing gear would probably be about half as heavy as it turned out to be. But it wasn't until the astronauts had really flown the simulator a number of times, that we began to see how the landing would really take place. They found out from working in the simulator that the thing to do was to let the autopilot determine the rate of descent, and then fly the thing manually in the horizontal plane to pick the actual touchdown spot. Well, once that was set up as the way to do it, we breathed a sigh of relief, because it became apparent that we'd be nowhere near the design limits of the structure.

Of course, we did put the probes on the landing gear. That was sort of an afterthought because of the concern about the dust. We figured, "Maybe they'll make a landing where they don't actually see the surface."

Of course, until an actual landing was made, we never really knew where the dust went. Intuitively, you think it's a problem, because of watching a helicopter land on a field, where the dust goes up all around. But on the surface of the Moon, the dust just takes off horizontally, because there's no atmosphere to cause turbulence. So some of the things we worried about really didn't turn out to be nearly the problem they were thought to be in the beginning.

I've tended to talk a lot more about the technology than I have about the aspect of managing all of this, but the fact of the matter is, the technology was the dominant part of all of this. You weren't going to advance the program by meeting a schedule if the technology wasn't right. Of course, the problems that NASA had with us with respect to schedule and cost, we had the same problems with our subcontractors, because things did not always work exactly the way they were supposed to. I've sort of derived from that what I think is a basic truth, and that is, that if you're going to do something that's truly novel, you cannot possibly know either the schedule or the cost. I've recited this to a number of people, including congressmen, and they all nod their heads up and down, but then they don't like to agree to the fact that that's the way it is.

I don't know. From a management point of view, maybe the problem of keeping the NASA people satisfied that we're doing the right job and keeping our own senior management satisfied that we're doing the right job, maybe that's what I spent a lot of time doing. But I had lots of good help.

Initially, there were some aircraft people in the company who thought that "These guys on the lunar module are nuts." They wouldn't have anything to do with it. We got past that.

The chief technical officer in the company, [Ira] Grant Hedrick, was a solid rock of support. He's one of the finest engineers I've ever run across, and he made sure that we had the right people at the right specialty.

There were always surprises. Prior to Neil [A.] Armstrong's first mission, I think we were about sixty days from launch. The machine [was] at the Cape [Canaveral, Florida]. Everything had checked out reasonably well. A certain number of troubles had to be handled. We had a [problem] in the air conditioning and cabin life-support system. We [circulated] a glycol solution [to] keep the cabin at the right temperature. It would also take the heat out of the electronics equipment. See, all of the aircraft up to that time had used the air that they were flying through to cool the electronic equipment. Well, we didn't have any air that we were flying through, so we had to devise a scheme for getting the heat out of the electronics. This involved mounting [the boxes of electronics] on channels that had glycol fluid flowing through them, and this went through the air conditioning system. Eventually, the heat was transferred to some water, and the water was boiled overboard through a porous plate.

Anyhow, we had run this system in a ground test rig for, I think, about six years. It had worked beautifully. It had filters at various places and relief valves so that if the filter plugged, the flow would go on. Everything seemed to be routine.

We also provided access ports so that the fluid could be sampled at various times during the career of [each] lunar module. When we got ready for Apollo 11, I think about sixty days before flight, maybe it was a little longer, they discovered in the testing the fluid, they found some little crystals, sort of needlelike, that nobody had ever seen before. They were not only needlelike, they were small, but they were soft. They weren't hard crystals. The immediate reaction was, "We'll take that fluid out and transfuse a new batch in." Sure enough, more crystals.

Then we said, "Maybe there's something there that needs to be filtered better than anything we've done so far." So we got some very exotic filters that were outside the vehicle, and ran the fluid out through them, and it just generated more crystals. Pretty soon we had, I think, used almost all the bowls that the Grumman cafeteria had, to have samples of glycol sitting around where people could look at it. And the more we worked, the more crystals there were. There was no explanation.

So what we finally did was to run the ground test rig with a glycol solution that looked almost like orange juice. It was full of crystals. We ran it for two consecutive missions and showed that the crystals would not stop it from working. So Armstrong went to the Moon with crystals in his glycol.

Then in the meantime, we were conducting all of these tests, not just at Grumman, but places like Battelle [Memorial Institute, Columbus, Ohio, whenever] anybody seemed to be expert, and it turned out that all the tests we'd been running in the early years had been done with a rust-inhibitor additive that was real cheap stuff, bought from some outfit in New Jersey. Somewhere along the line, somebody had discovered that a purer version of the same material could be purchased from Eastman Chemical [Company], and so a change was made, and we used the pure stuff. As soon as we used the pure stuff, we had the crystals. So we reverted to the cheap stuff, and all the rest of the missions were straightforward.

Anyhow, this was a real conundrum, because you couldn't really put your finger on just exactly what was causing [the crystals], but it emphasized, again, the basic rule that if something works, be very careful if you try to change it, because maybe you'll get into something that you don't foresee.

We had another situation that was almost as bad, except that it happened earlier and we had more time to work on it, and that is that all of the plumbing and the tankage for the propellants was very lightweight. I tell the schoolkids that the tanks were proportionally thinner than an eggshell. We squeezed as much weight as we could out of them.

As a matter of fact, that led to an interesting situation. We had done a lot of work with tanks that were the right size, but were heavy, to work out the hydraulics. When we finally got the first flight-weight tank, we filled it and were amazed to find out that it had more capacity than we anticipated. So there was about a three-hour panic, and somebody said , "Ah! The tank stretched under the weight of the fluid." That picked us up twenty seconds of propulsion, which is about the margin that Neil Armstrong had when he landed. So that's one of the few things where something that was overlooked came out favorably. Usually, when you overlook something, it hurts you rather than helps you.

At the bottom of the propellant tanks, we had these big flanges where the plumbing mounted, and conventional seals and bolts, and we discovered in the testing out at White Sands, where we fired the whole descent stage or the whole ascent stage, in a vacuum for, I think we got two seconds was all the vacuum would last, but it would give us some idea of whether the system would work. And we had leaks. We had nitrogen tetroxide, which was the oxidizer, and a fifty-fifty mix of hydrazine and unsymmetrical dimethyl hydrazine, and neither of them are the things you want to handle very much. In fact, nitrogen tetroxide is a bad actor. If you have a little leak, pretty soon you have a bigger leak, because it [is] very corrosive.

So we had to redo all the seals on all of the connections, and this at one point involved bringing back some material from the Cape. I can't remember whether this was Apollo 9 or whether it was for the unmanned LM flight. We were looking for very small leaks, like one cubic centimeter per year, because the nitrogen tetroxide would just eat away. You [might] say, we're only going for maybe a five-day mission or something. Surely, that's not a big problem.

But you just can't be satisfied with something that may leak. We spent a lot of money curing leaks. We also spent a lot of effort trying to measure leaks, because the soap-bubble method is pretty good, but below a certain rate, [it] isn't very good. So we had mass spectrometers. It got very complicated. Maybe we overdid it, but we were not going to have any leaks.

So, you say these are technical things. Well, management gets involved in technical things. One of the practices that I made was that every day that I was in Bethpage [New York] and not visiting some subcontractor, I'd make it my practice after lunch, walking through the manufacturing area and talking to anybody that wanted to talk. But I knew all the foremen. Some of the older aircraft hands couldn't understand why we had to go through the paperwork and the smocks and the caps and the booties and the clean room, couldn't understand why we [did] that, and we had a certain amount of indoctrination to do, and we finally got that working. Of course, we had known some of these people long enough, or I had known some of them long enough, so they were very frank. "Why can't those stupid engineers do it right the first time?"

Of course, no design is ever really perfect in all dimensions right from scratch. I can recall an incident on an earlier aircraft design, where I was the project engineer, when the shop foreman came to me with a fitting in his hand and said, "You know, this is the third one we've made, and it still doesn't fit. When are you guys going to do it right?" And of course, he was right. We hadn't really done it right. But we had the kind of relationship between the manufacturing people and the engineers so that these things got handled without delay and progress did get made.

I think there are probably some anecdotes that might—well, we had all of these procedures that were followed very carefully, and we produced periodic design reviews. Then finally there was a pre-delivery massive review that went over everything in great detail, examined the pedigree, and those were done with joint teams. NASA would come with a number of people, and our group leaders would put them into the group, and they'd go through everything and come to a conclusion that it's okay except for maybe three or four items that would have to be picked up, and sometimes things weren't scheduled to be done until after delivery to the Cape, which was perfectly normal.

Those reviews were, by aircraft standards, a revelation. We really were outnumbered in the beginning, because people from NASA came not just from Johnson [Space Center, Houston, Texas], but from the Cape and anywhere elsewhere there was interest. [NASA] Headquarters [Washington, D.C.]. It was an exhaustive process, but in hindsight, it probably was one of the keys to success.

We had some cases where some of these procedures were misinterpreted, to everybody's disadvantage. For example, one time I discovered that the people who assembled the parts kits for some assembly were packaging ordinary washers in a special package to protect them. And the question was, protect them from what? But they'd been so thoroughly indoctrinated that everything had to be done with kid gloves, that they had packaged these washers, ordinary washers, in, shall we say, a non-cost-effective manner, and it took some adjustment to get things done right.

But we adhered pretty carefully to these procedures. There is a slide I've used in talking to various groups where I show them an almost completed ascent stage, but the purpose of the slide is not to look at the ascent stage, but to look at the pile of paper on the table adjacent to it. There was lots of paper, and it forced the discipline that had not existed in the aircraft business up to that time. I would say later on, because of becoming president, I got back into worrying about aircraft. We adopted a lot of the practices learned on the LM back into the aircraft business and managed to cut down the number of flight tests before delivery. In other words, you build a better vehicle with discipline, and then you don't have to flight-test it so many times to work out the bugs before you deliver it. In fact, we were accused of taking over the aircraft company in those days

Another anecdote where we departed from the tried-and-true procedures: the idea was that when you build up the vehicle, you went through all these testing procedures, and if there was a problem and something had to be replaced, you had to go back and start all over again. Well, in one of the LMs—and I have forgotten now whether it was—it may have been Apollo 12. It was at the Cape. It was approaching mission day, and one of the electric motors in the air conditioning system just stopped running. George [M.] Low was visiting when we got this news, and he said, "Joe, what are we going to do? Strictly speaking, we ought to go back and redo that whole system after we replace the motor, but I hate to do that. I'd like to make the schedule."

So we talked about it for a few minutes, and I finally said, "Here's a possible solution. We'll get Hamilton Standard [Division, United Aircraft Corporation]," who built the equipment, "to send their best technician to replace the motor, and I'll get Grumman's best technician to watch him do it." See, the problem was, you could only get two people into the cockpit, and that meant you couldn't get a quality-control guy, you couldn't get a NASA person in there.

I said, "I've known this chap for over fifteen years, and he's the best mechanic I have ever seen do anything." And that was no exaggeration.

And he said, "Well, let's do that."

So I called up a chap named Nelson Vosbergh, and I'll never forget this. I had encountered him first when I was a very junior engineer at Grumman. I think it was the second year I was there. He was clearly the best nuts-and-bolts mechanic I have ever seen, and he could do a lot of other things. So by this time, when this problem occurred on the LM, he [was] now a foreman. I think his group was making some of the skins for the LM, but nothing to do with the air conditioning system.

I called him up, and I said, "Nelson, how would you like a trip to Florida?"

And he said, "You're pulling my leg."

I said, "No. Here's what I want you to do."

So he said, "Gosh. Yeah, I could do that."

So we got him indoctrinated on what to look for, and we got the expert from Hamilton Standard and the two of them at the Cape, and they went in and they changed the motor. A routine check said everything works, and on the basis of that, we launched the mission. And [Nelson will] never forget that, and I won't ever forget it, because it was one of the few times that we really breached the procedural testing sequence that we had set up.

Now, we were behind schedule, as I've said earlier, almost all the time, and that's what led to [Frank] Borman, [William A.] Anders, and [James A.] Lovell going around the Moon, because they wanted to do something because apparently they thought the command and service module was ready, but the LM wasn't. I sat in on a conference at Houston where that mission was discussed and where, in effect, I was in the position of having to say, "Well, it's practical, but face the fact, no LM is going to be ready for that date that you're trying to make."

Anyhow, there were things that happened that were not anticipated. Another one that comes to mind has to do with the potable water. There had to be a tank of drinking water for the crew. Again, it's a short mission, five days. Really, you don't expect to have too much trouble. The medical people said, "Look, if we put a little iodine in it, there'll be no problem whatsoever." So we did. But in running tests on the tank, it became apparent that the iodine disappeared somewhere. It wasn't exactly clear, but at the end of five days, there was a lot less than what we started with, and the medical people began to worry about it. "Where did it go to?" Of course, we began to wonder, "Where did it go to?"

So we ran a number of tests, and no matter what we did, the iodine tended to decrease. So the medical people said, "Well, you know, the rate isn't so bad. We'll just jack up the amount of iodine that we put in in the first place."

So then the question is, "How much is too much?"

I remember one day we had a meeting at Grumman where the NASA people and ourselves sat around a table, and we sampled little glasses of water with iodine in them, and some of them were very strong. The medical people said, "All of these are acceptable from a health point of view." But let me tell you, the strongest was so metallic in taste, that everybody said, "Absolutely not," and we picked an intermediate level. It was more than you would like to have, but it was still—the astronaut who was there—and I can't remember which one it was—said, "Yeah, we can put up with that." It was probably Deke [Donald K.] Slayton. I'm not sure about that.

Anyhow, that took time. We had one of these tanks [under] test in Houston. Dorothy and I went down there because our local Houston representative had a new house and had a housewarming. So my number two guy, Ralph [H.] Tripp, and I took our wives down there. On the way back, they said, "Hey, take this water tank back with you to Bethpage." So we bought a separate seat in the airplane for this box with the water tank.

About an hour out of Houston, the pilot said, "You know, there's a big snowstorm in New York [New York]. We're going to have to land in Philadelphia." And we did land in Philadelphia. It was snowing. They said, "We will get buses to take you to New York," and that took some time to arrange. Finally, the four of us plus this tank get on a bus along with a lot of other people. There were several buses involved, and also we had picked up a woman who was going to join her NASA husband up in the Hartford [Connecticut] area. She had never seen a snowstorm, I think, and she had one of the heaviest suitcases I've encountered. Anyhow, we accumulated her.

To make a long story short, this is before the days that a bus driver could communicate with the outer world. He made his way up the turnpike to New York, and after many real adventures, I think we got on that bus about three o'clock in the afternoon, and I think it was about two in the morning that we pulled into the—I can't remember now whether it was—

MRS. GAVIN: East.

GAVIN: Was it the east side? The east side airline terminal. As a matter of fact, he finally got stuck about fifty or sixty feet short, and we all disembarked and went into the terminal where there were some people washing the floor and nothing else was happening. We get on the phone and discover that "Don't come to Kennedy. There are people that are sleeping in the airplanes. It's all tied up."

So I went out on the street and looked down across the cross street, and in the distance I could see a thing that said motel. So we picked everything up, trudged down there. Of course, in a snowstorm in New York, when you step off the curb, you step into ice water. We got to the motel. They had rooms, but no food. We hadn't had anything since breakfast. To make a long story short, we stayed overnight, called the plant the next morning. They said, "You're stuck. Nothing is moving." The storm had caught everybody by surprise. So we went to Macy's, bought galoshes. Then we arranged for dinner at Mama Leone's and we got tickets to *Man of La Mancha*, had a free day. The two wives said, "There's no other way these two guys would have spent a free day in New York." [Laughter]

The plant sent in a station wagon the next morning. Took them all morning to get in and all [afternoon] to get us out, but we did get the water tank back to where it was supposed to go, eventually.

There were a number of odd adventures like that, which really were distractions from the main thing.

Now, we did work people very hard. We were continually under criticism from NASA for the amount of overtime that we ran. Grumman had always believed that it was better to have some overtime than to overstaff and then have to let people go. But in the LM program, we essentially wound up running two twelve-hour shifts a day with some overlap. So there still were a couple of unoccupied hours so that the maintenance crew could take care of machinery and housekeeping and so forth. But we worked a lot of eighty- and ninety-hour weeks.

This was particularly hard on group leaders. The group leaders at Grumman had a lot of autonomy as to what they could do with their people. In other words, they [could] give them a day off, if the group leader felt it was necessary, and that worked quite well, but the group leaders were the people that the management had to worry about, and we occasionally would have to send somebody home and say, "Look, just don't come back for two days," or whatever, because people would come even when they were sick, and that's no good.

In hindsight, I don't think I would have done it any differently, because just adding people doesn't always make things move faster, and if you have a group of people who know what they're doing, it's a little better to work overtime than to just add some people.

Of course, the night shift felt they were orphans, because most of the engineers went home. I remember one period when we were having trouble—this was well before the delivery of the first lunar module—where I think I spent about six weeks going to work at one in the afternoon and leaving at one in the morning, which gave me a chance to see what the day crew was doing, but also spend some time with the night crew. You can say, what the heck did Gavin do that made any difference in night crew? Well, substantially, the only advantage was that it made the night crew feel that they were appreciated, and I got to know a number of supervisors that I wouldn't have run across, because the night supervisors was a different group from the day supervisors. And I learned a lot of things because they felt free to talk.

It was very illuminating, because it was going through these test buildup procedures which the methodology had to be precise, and to a lot of people that was a burden, to do it just the [right] way—in fact, I've wondered in recent years whether surgeons are as well proceduralized as we were, because we did it by the book. We wrote a program and we did it. So we did work a lot of overtime, and most of us—I don't think we had any deaths directly attributed to it.

The other thing I should mention is that the—a lot of things I should mention, but the astronauts were in the plant, I think at least once a month, and they got to know a lot of the group leaders, particularly in the—well, in all the areas. And I have to give them credit. The Apollo astronauts were all different, and I've always been amazed at how different they were, and yet they were talented, capable, and their visits to the plant made people feel that "We're not just building something for some mysterious customer; we're building it for these people." And that was very useful.

NASA had some question about whether the rendezvous radar was really going to be ready in time, and they began to look at an alternate method of doing the navigation for rendezvous, and Rusty [Russell L.] Schweickart was the astronaut who really followed that.

The rendezvous, I think we had at least four different ways to accomplish it. If something didn't work, you'd drop back to number two method, and then number three. Then finally, you'd get a lot of cooperation from the command module and be able to do it. But we evaluated a blinking-light scheme, a flasher on the LM, and finally it was one of those decisions where it could have gone either way, but there was a slight [preference], in our view, and I think in NASA's view, too, that we should continue with the rendezvous radar, and we did. But a lot of effort involving the astronauts went into looking into this other scheme to see if it would work.

I think I'd like to switch now to talking about the fire in the command module at the Cape, because that happened at a particular time. George [E.] Mueller had called a meeting of all the contractors' senior people in Washington to review where things stood. There was something going on at the White House that day. I can't remember whether it was the—had something to do with the peaceful use of outer space or what it was. But in any event, all the people at that meeting got invited over to a reception at the White House. So about five o'clock or six o'clock, I've forgotten when, five o'clock, we were over there, and this was a big deal. I had never been in the White House before. We met Lyndon [B.] Johnson, who is bigger in real life than he shows up in the photographs. We met Hubert [H.] Humphrey, who is smaller in real life than shows up in the photographs. And it was kind of nice, after a day of being in the position of having to say that we're still behind schedule.

But I was keeping my eye on the weather, because I wanted to get back to New York that evening. So I left that reception probably twenty minutes early and went to the airport and, of course, found the weather was so bad that I wasn't going to get back that way. So I went to the train station and took the train back to New York. When I got in the taxicab to go back to the airport to retrieve my car, the taxi driver said, "Hey, have you heard about the fire at the Cape?" So that was the first I had heard.

Apparently, within fifteen minutes after I had left, there was a call to the White House that informed them about the fire at the Cape. Of course, that was a terrible tragedy, and I have always admired Sam [Samuel C.] Phillips for having stood up and faced the press on that. He did exactly what had to be done. Today, I'm not sure the program could have continued under today's situation, but then it could because we were in the midst of this superpower contest.

Of course, this changed what we were doing at Grumman, and we were told immediately to slow down our expenditures. Now, when you have a program running, there are some things that are fixed and some things that are variable costs. You've already rented the big IBM computers, so you can't really do much with that immediately. As far as people are concerned, you can divert some of them to other projects in the company and cut down overtime. So we did that to try to minimize the monthly expenditures.

But then, of course, we had to go through and examine everything in the LM to see whether it indeed was fireproof. I think we all would have to admit that the oxygen atmosphere was something that we had not fully appreciated. So we built a steel box that had the dimensions of the LM ascent stage [cabin], we outfitted it with all of the equipment that had been proposed or developed, and then we deliberately tried to set it on fire. Then we went through, and from that beginning, fireproofed things. The circuit breakers were committed for the program, and they burned. So we put little Nomex bags around the circuit breakers, and then the back of the control panels, all of the terminals were painted with this—can't remember the name of the stuff, but it looked like brown mud. Eventually, we could show that even if you started a fire, it would not propagate, and that took some time, but we did it.

Of course, the question on a lot of people's minds is, would there have been an ignition source anyhow? Well, by fireproofing it and limiting the amount of Velcro—the astronauts were great on Velcro, but it burned very nicely, so that was limited. Then we kept track, very carefully, of all of the things that would burn that were in the cockpit.

But having mentioned the circuit breakers, I should also tell you about the toggle switches. Toggle switches are standard AN miniature toggle switches. It's a little pin that you push back and forth. They'd been [used in] aircraft for, I think, maybe a dozen years. But one of our young engineers said, "You know, I really don't know what happens inside that switch. I think I'll have some of them cut open to see what's in there." And he did, and I think he had a dozen sectioned, and about a third of them had a little loose pellet of solder.

Now, in 1-G or in normal aircraft flight, that probably doesn't cause a problem, but in zero-G, it could float anywhere and give you a wrong setting for the switch. So there we were. It was too late in the program to develop new switches, so we devised a test to tell which of these switches had the little pellets in them. We threw away about a third of the switches, but we knew the switches we [kept] did not have the pellets in them.

Now this is a case, I think a wonderful case, of how an inquisitive mind did the right thing, led to the right thing. Nobody could have told that individual that this was something that should be done. It's something that he said, "You know, I am responsible. I'd better find out. I'd better understand everything about everything." It really was a remarkable accomplishment. That's the kind of failure which, if we had had it—I went back and looked at a couple of the aircraft failures that I remembered, and I'm not so sure but what we didn't have that symptom earlier on, because [we] would never find it. [Sometimes] you would never find the cause.

So anyhow, the standdown after the fire at the Cape, I think cost the program almost a year. We were able to get some things back going sooner than that, and, of course, it increased the cost considerably, and we had to revise some of our testing procedures, because we had to test control panels, for example, after they'd been fireproofed. These were different. They reacted differently from the earlier ones.

WRIGHT: After the fire, George Low instituted a configuration control or control board meetings every week for a while so that you all could sit down and talk. Could you talk about those and how well they worked?

GAVIN: Well, the thing that went on there—see, the design was fairly mature at that point, and the problem of making a change and sending it to Houston for approval, then getting the approval back took time. So he said, "We'll set up a board that meets—," I think it was once a month, "and the group leaders at NASA and Grumman will review the change in advance and come to an agreement or disagreement, depending." And then this control board meeting will include George Low, Deke Slayton, and I've forgotten. I'd have to look back to see who else was there. We'd sit there for one day and go through all these things, and either "yes" or "no."

The way it was worked is that there'd be somebody from North American attending the meeting at Grumman, and then one of us would go with the group that night to Los Angeles [California] and sit on the same kind of meeting at what was then—I think by that time it was [North American] Rockwell [Corporation]. In this way, first of all, the proposed changes got handled very quickly, and secondly, no changes slipped through without getting reviewed very carefully. That was a very effective scheme, and it put a time limit on the debate between the group leaders, you see. They had to be ready no later than a week before this meeting to either go to bat or not. I would say it was a very effective way of doing it.

I can think of many cases. I remember one. We had an ascent stage in final assembly. I can't remember exactly where this happened in the scheme of things. It was [well] before any of the missions. The window shattered without anybody touching it. Now, the windows were made at Corning. They were monolithic glass, three-eighths of an inch thick, very carefully made. I remember we had one of these configuration control meetings, and Bob [Robert R.] Gilruth was there. He said, "All right. What are you guys going to do about that?"

So we were kind of hung. There were some people who felt we should go to a thicker laminated glass and take the weight penalty, but we were so critical on weight that we didn't want to do that. So we went back to Corning and reviewed the whole process, and finally wound up having a much more rigorous acceptance test procedure; and secondly, finding out a lot about glass that I don't think any of us realized.

Glass doesn't look porous, but it really is. It is particularly subject to moisture getting into those tiny pores. So what we did finally, to make a long story short, was that we coated the glass with a plastic material which, if you touched it, would show a mark, and it kept the glass dry. It was one of the last things done before the launch at the Cape, was to tear off those protective covers. And we never had a future problem. But it was one of those things that could have gone either way, and the fact that it works, says we must've been inspired in making the choice. I remember Bob Gilruth, who was known in some quarters as the "Great Stone Face," he was very serious, always very serious, and demanding, and he should've been because of his position. I remember him looking at me at the conclusion of that meeting and saying to myself, "Boy, we'd better make this work or else there's going to be real trouble."

I don't think he ever got full credit for what he did, and I really feel that he should have gotten far more credit. I know in a lot of people's minds Wernher von Braun got all the publicity, and nobody ever heard of Bob Gilruth, and there you are.

Now I've brought up von Braun. He came to visit us once, only once. At that time his brother was the federal German government's unofficial delegate—he wasn't an accredited delegate—to the United Nations, lived in New York City. So von Braun came to visit him. I took a station wagon and went in to pick him up, and wound up having breakfast with the von Brauns. I learned, very interesting, that those two men called their father once a day every morning.

Anyhow, we got von Braun out to the [Grumman plant]. I had met him several times before, but never to have much of a conversation, and I took him on a tour of the engineering and manufacturing areas. That man had real charisma, and I can best describe it by saying that he would stop and talk to, say, a lathe operator and ask him about what he was doing and where that part was going to go and so forth. He'd spend maybe two minutes doing that and then leave. The lathe operator would be standing there with a numb expression, "What a wonderful guy. What a wonderful guy." Some people have that talent and some people don't, but he had it.

But then we had another [visitor] who came for several days, Eberhardt Rees, and he never got credit for what he did, because he was the engineer that made the boosters happen. He was von Braun's number two. You could almost see that von Braun was the front man, and Eberhardt was the quiet guy who was back there making it work. He and I hit it off very well, and he spent two or three days visiting Grumman and, I'm told, filed a report, said, "Hey, they're doing it all right." Then I think he was assigned for several months out at Rockwell. He never got credit for what he did really. Fine engineer.

WRIGHT: Speaking of North American Rockwell, could you talk to us a few minutes about that? You've talked about working with the subcontractors, but you weren't contracted to North American, but yet you certainly had to work in parallel with them and what they were doing.

GAVIN: Well, we did. We put one of our tried-and-true out at Rockwell as a representative. They had a representative at our plant. One of the things that was worried about early on was the potential for a static electrical charge on docking. Well, the Gemini Program pretty well disposed of that problem. But then, of course, we got into the details of the docking device itself, as to where the action would be, and that was a joint undertaking, to make sure that everything was compatible. There were some tests run, and I think it's true to say that it always worked very well except for [Alan B.] Shepard's mission, where he had to fiddle around, two or three tries, to get docked.

But we had a good relationship there. I had known some of the North American people right from the beginning. In fact, I had met some of them back in the aircraft business. So we spoke more or less the same language, and really, I think it worked quite well. I don't recall there ever being a time when NASA had to step in and knock heads or anything like that. No, it worked well. They had some very good people on the job. I know they changed leadership in the middle, so to speak. Dale [D.] Myers became one of my lifelong friends. In fact, we know Dale and Marge, have known them for years.

So despite the lack of contractual requirement, there was really no problem.

WRIGHT: The balancing act that you had to do every day between the pressures of NASA, your management, internal affairs, did you have to ever suggest some type of reorganization within your troops to help meet the deadlines or schedules or costing?

GAVIN: Well, I acquired additional help over a period of time, compared with the original organization. But Grumman was a strange place. The organization chart really didn't mean that much, and where if you had a problem, you knew who to call up and you'd get help.

Yes, I took a lot of criticism from the inside as well as the outside over a period of time, because, frankly, we never caught up on the schedule, and we were always running a little bit over cost. But in the end, when you thrashed out all of the directed changes, we had about, I think, no more than a 10 or 15 percent overrun, and, of course, it worked every time. And to be blunt about it, we didn't endanger any astronauts and we didn't kill any, so we must have done something right.

I should tell you a little bit about what we went through during the launch period. NASA had a three-day-before-launch meeting at the Cape where the senior people from the various involved contractors came and said we're either ready or not ready. I was the one from Grumman that sat in on that. Of course, we had a crew of people at the Cape. I had a really topnotch manager there, George Skurla, who, unfortunately, is dead now, and we took a lot of guff from the Cape initially, because the very first article we sent down there really was not as good as we could do, and we had to fix things up there when they should have been fixed at Bethpage. But we got better at it.

So I never had a problem with saying we were ready to go, and that was always said with respect to the procedures yet on the schedule. In other words, "We're ready at this point. If [we] finish the procedure successfully, we're fine."

There was at the Cape a separate room for monitoring the condition of the lunar module, and that's where I would be. I never saw a live launch, because I was always there watching the people from NASA monitor the vehicle. Then after it was safely in Earth orbit, a few of us would get in an airplane and go to Houston.

At Houston, everybody is familiar with the Mission Control room. It's all been on TV. Most people don't know that across the aisle there was a smaller room where about four or five people from Grumman and four or five people from Rockwell were the first level of technical backup. Then in another building, for Grumman there was another twenty or so people who would be the second level of backup. Then there was an open line back to Bethpage, and there'd be another two dozen people as the final detail backup. So if a question came out of Mission Control, you could have an answer within seconds, almost.

So most of those people were in place, and I and a few others would go over to—Tom Kelly, for example, when he was there for the first two missions, before we sent him off to school, would be in the room across the aisle from Mission Control. But I got the assignment of sitting in the VIP [Very Important Person] area behind the glass panels, but I discovered that with bird glasses I could read the monitors, at least in the first row, which were the important ones.

To go back, the first mission was the unmanned LM, and it didn't terminate quite the way it was supposed to, because the firing of the—let's see. It's got to be the ascent engine, I guess. Anyhow, the software that governed the firing of that engine cut off prematurely, so we had to change that. So we learned one thing. But the rest of the mission was a success, and the decision was made that a second unmanned flight would not have to be made.

There was a panic before that flight, that mission, because it was discovered that there was a transistor in the autopilot, which was unique to the unmanned flight, that was in backwards. What we finally did just days before the flight was to run some tests that showed that it didn't make any difference, it would work. And I remember making that final phone call from the airport on my way to the Cape, saying that it's all set.

In the case of the Earth orbital mission, that was relatively more straightforward, although if you look back at it, the training period for that crew was really not as adequate as some of the training periods for the later missions. Of course, Rusty Schweickart became very, very ill, which almost queered the mission. But everything worked, so [we] were then on to the next mission and so on.

Then in the course of working our way [through] Apollo 13—because that was really a trial. Everything had gone well at the Cape. We had gotten to Mission Control. We'd heard them fire to get into translunar trajectory, and we said, "Well, we'll go get some supper." I can't remember what time it was. It was early evening. We went to the hotel and had just started to eat when the waitress came in and said, "You guys better get back over there." By that time—all of Houston had these little boxes that would allow them to listen to Mission Control, so they knew what was—they heard the "Houston, we have a problem."

So we left dinner, went back over there. Then, of course, we faced the agonizing decision of what to do, what sequence to power down so [we] didn't get into an irreversible situation, and some real concern about the consumables in the LM lasting long enough to make it happen. Well, it did work, and I think I got two hours of sleep in that whole mission, because I stuck it out at Mission Control.

And that's the only time—when they were finally down on the water, that place just burst into cheering, and that's the only time I was invited to come into Mission Control. George Low waved at me through the window and said, "Come on in," and so I did. The atmosphere was—well, you could feel it, it was so buoyant and so relieved. We really didn't realize how sick Freddy [Fred W.] Haise was until he got back on the carrier.

WRIGHT: Had there been any testing, or had you gone through any simulations with the LM that would have given you the thought that it could've done what it did?

GAVIN: Well, I've heard that question a number of times, and I have to tell you that I was not aware that anybody had looked at that kind of a mission. In talking to some of the people who were involved in what was known as the Apollo Mission Planning Task Force, which, incidentally, is something that Grumman promoted, when we got going on the project, we found that the mission wasn't very well defined, so we got NASA to set up a committee, and we had a couple of people from Grumman and, I think, counterparts from Rockwell, and I don't know who else was involved. People from NASA, obviously. They sat down and defined a standard mission and then some deviations from the standard mission. Then, of course, during the training [for] Mission Control they had all kinds of emergencies put in.

Somebody told me one time that there was something that was looked at that somewhat resembled the Apollo 13 recovery, but I don't think it was carried through to the same extent. Looking back at it, it was sheer circumstance that that crisis occurred when it did. If it'd been earlier or later, it would have been a catastrophe. It was a nerve-racking experience.

I have to go back and tell you about Neil Armstrong's landing, because, you see, he got down there, and he was downrange beyond where it was planned because we didn't understand the Moon that well. He had a lot of boulders [in sight], and he had to extend the flight, and we could see the time remaining on the fuel getting shorter and shorter. Finally, he got it down, and that was a little bit of a crisis, because we didn't know where he was, and I'm not sure he knew where he was. He just knew he wasn't going to land on those boulders.

But to most of us, some people have asked me, well wasn't that the most critical thing? And I've said, no, the critical thing was the takeoff [from the Moon], because you had a limited time, you had to punch the button, and everything had to work. The ascent engine had to ignite. The explosive bolts had to explode. The guillotine had to cut the connections, and then it had to fly up. And this is something we never saw happen until the last mission. So it was all, well, hearsay. It's something we never could test for, because the conditions couldn't be duplicated on Earth.

Anyhow, after Armstrong and [Edwin E. "Buzz"] Aldrin got back into orbit and got connected to the command and service module, the people in the VIP group—well, two things happened there. One, very heavily accented German-accent voice said, [mimics German accent] "Vera, vy is it that it takes 10,000 people to make a launch at the Cape and only two people on the Moon?" [Laughs]

Anyhow, it was such a relief that they were actually back up there, that a bunch of us who had been there for quite a long time decided to go out and get some coffee and a sandwich, and one of the people that was with me at the time was Representative Olin [E.] Teague, who is a guy I got to know quite well, a really wonderful individual. He made it his business to get acquainted with us very early, before we even had the final contract. He kept track of us periodically over the years. As we went out to get coffee, he put his arm around my shoulder, and he said, "Joe, you've been telling me for years how it was going to work, and I have to admit to you that in my heart I didn't think you could do it."

Now, Olin, he was a war hero. I think he got badly wounded at—I don't know whether it was Salerno, somewhere in the Italian campaign, and he resisted having amputation, and he had this foot that really wasn't much of a foot, for years. He also came from a district that didn't have any money from NASA.

What are you trying to tell me?

MRS. GAVIN: The state.

GAVIN: Yes, well, he was from Austin, Texas, somewhere up around Austin. I remember him telling me one time, he said, "You know, I go home and sit on my porch, and my neighbors come up to me and say, 'Olin, why are you shooting all that money out into space?'" He [said], "Now, sit down and I'll tell you. None of that money gets shot into space; it's being spent in Texas, it's being spent in Louisiana, it's being spent in Florida, and in other parts of the country. Furthermore, this is something we have to do."

Anyhow, I got to know him reasonably well, and after the Apollo was over, and after the, I think after the Shuttle award was made, he was diabetic, and he got put in the Walter Reed Hospital. I happened to be in Washington [D.C.] one day and made it my business to go visit him. I'm glad he wasn't my patient. He must've driven them wild up there, because he arranged for one of his staff assistants to bring him corned beef sandwiches and a bottle of beer. You know, he just wasn't going to play by the rules. But really a very impressive guy. I think it was support like that that kept [Apollo] going. You look at some of the ways that Congress has jerked other programs up and down.

That brings up the subject of Jim [James E.] Webb. I got to know him only after Apollo was over. In fact, I got involved with the Department of Energy as a consultant on a panel that reviewed fusion energy research, and they asked me, "How do you run a big program?"

I said, "Why don't we get a meeting with Jim Webb." I took the leader from Princeton [University, Princeton, New Jersey], Mel [Melvin B.] Gottlieb, down to lunch with Jim Webb, and we talked about how you run a big program, because Princeton was just then building or about to build the TFTR, the Tokamak Fusion Test Reactor, which, incidentally, years later was a great success.

But Webb told us about how he had handled Congress in the Apollo business. He said, "I knew most of the committee chairmen. The first time I went over there, I told them this. I said, 'This is what my people say it probably will cost, but I've been around long enough to know that it could cost three times this much, and I'm probably going to have to come back every year and ask for more money, and you'd better be prepared for it.'" And he got away with it. Now, is there any department head who could get away with that today? I don't think so. But he was the right man at the right time, and he made it work.

It was one of these things that—well, in my paper, I think I note this briefly, but it made a major difference. We had cases in the aircraft business where the Navy would cancel a program, counting on the congressional committee to restore it so that the Navy didn't get blamed for adding the money. This kind of maneuvering between the Congress and the procuring agency can make it very difficult to have a continuity, and continuity is what gives you consistent results, I think.

WRIGHT: How much was the continuity altered from the LM for Apollo 9 to the one that was for

Apollo 17?

GAVIN: The changes made were not that radical. Basically, we had to have more oxygen, more batteries to provide longer stay time. The only thing we worried about was the landing on the Moon. We had worried about how close the descent rocket engine nozzle would come to the lunar surface, and from some of the other earlier landings, we've got photos that showed that. When we went to the heavier LM for the last three missions, we thought, well, it would help if we could get more efficiency out of the rocket engine. So the thought was, we'll extend the skirt on that nozzle, which will improve the efficiency. Then the concern was, well, what if it gets too close to the surface during the landing? We agonized about this for a while, and finally—I don't know who came up with the idea—devised a test that we could run right at Bethpage that would show what happened.

What [we] did was, [we] had a big tank of hot carbon dioxide that was exhausted through an engine nozzle, and they had a jack that pushed a steel plate right up toward it. What happened was, the nozzle folded back just like a collapsible drinking cup. The nozzle was made of columbium, a heat-resistant element, and very thin, very thin. In fact, we had to protect it once it was installed so it wouldn't get dinged.

But once we had run that test, the concern about the nozzle coming too close disappeared, and we got the higher performance out of the engine. The last three missions, of course, carried the lunar rover as well as the extra life-support supply. Those were pretty routine by comparison to the first couple of missions.

WRIGHT: Was the rover included as part of the original discussions, or at what point was the rover introduced?

GAVIN: The rover came along after we were started. It was never considered for the initial lunar modules. In fact, the initial contract didn't cover the lunar modules at the end, so there was a chance to introduce it. As a matter of fact, we were competitors for the lunar rover, and that was a case of—that was one of the more irritating things that happened in the whole program. We produced a really first-class rover. We sent it out to—there was a place out near Phoenix [Arizona], out in the cinderbeds, where they set up a demonstration, and we went out there and demonstrated it. In the meantime, even as that demonstration was going on, [NASA] awarded the job to Boeing [Company]. So there was a certain amount of, I think, politics involved in that. Can't prove it.

But anyhow, we learned a lot about lunar rovers so that the idea of carrying one was not strange. I still think we had the better rover, but that's another subject.

I'm trying to think. There are so many anecdotes that come to mind, where things that were supposed to happen didn't happen that way. Well, I can tell you one that involved me personally. I had gone to the Cape, and I can't recall which launch this was. It was maybe Apollo 12. I'm not sure of that. But anyhow, our local manager there, George Skurla, said, "Joe, you've never been up on the gantry. This is something we should do." So everything else was running in routine fashion, so we took a half an hour off, and we went out to the site. To get up on the gantry, you had to take your regular badge, pass it in, and get a special badge, which allowed you up. We got up there just about sunset, and it was quite impressive.

This gust of wind came along and ripped off my special badge, and it went out to sea, and there I was without a badge. It took us about an hour to work our way back out, because obviously, "Where'd this guy come from?" These people there had never seen me before. So obviously, there was some question. George finally had to, I think, pledge the crown jewels to get me out. But it was interesting.

We had a crew of people at the Cape. The way we shipped the LM to the Cape was sort of interesting. We had these big climate-controlled boxes, one for the ascent stage, one for the descent stage, and they went into the big Super Guppy, and it took off from the Bethpage runway and went down to the Cape. Of course, we had a crew at the Cape, which I think eventually was somewhere around 500 people. Most of the managers had been with Grumman a while, but a lot of them had come from the Chrysler [Corporation] operation that had been there for one of the intermediate-range ballistic missiles.

Then they'd retest everything, reassemble everything, and get it ready to go, and that was a several-month job to do that. Typically, we'd have a telephone conference once a day with George and his troops, usually about five o'clock back into Bethpage where we would review his progress, give him a chance—

MRS. GAVIN: Skurla.

GAVIN: George Skurla, yes. Give him a chance to complain about what additional support he needed, or whatever. He was a veteran at Grumman. He'd been there during the war. He'd been in the Structural Design Department. At the time that we got him for the LM, he'd spent some time in flight test. So he had a good background, knew a lot of people, and was very good at getting people to work for him, which was an outstanding talent.

WRIGHT: You were originally contracted to build fifteen LMs.

GAVIN: Not originally.

WRIGHT: Not originally?

GAVIN: The original really covered only the—well, I've forgotten now whether it was three or six, but then there was an add-on beyond that, and that was all before the extended stay time came into consideration. So there was a major contract amendment for the additional. And it's true, we did not build them all, because the [program] was terminated after Apollo 17, and we had a couple in partial stages of assembly and a bunch of parts. I think all of that [material] has wound up in some museum somewhere. I know that the Smithsonian [Institution, Washington, D.C.] was our thermal test LM.

We had quite a number of test articles, because we were continually inventing new tests to try to get around this business of no flight test. Of course, some of the tests were run in the big chamber at Houston. Some of the things got added very late in the program.

Beneath the reaction control jets that provide for maneuvering, there's a little chute that protected the descent stage from the heat of the jet. Those were added very late in the game, after some tests of those reaction control jets at Houston. So there were some things that were done quite late.

I remember that we began to worry at one point about the effects of hot and cold temperatures on the—oh, that's another story. I have to talk to you about the landing gear. It didn't hit home for a few weeks that we only had to make one landing. It wasn't like an airplane.

One landing. So immediately we stopped thinking of conventional landing shock absorbers, and somebody said, "Well, we can take a column of aluminum honeycomb and just compress it, and it won't rebound."

So we went off in that direction, and of course, it worked fine when we tested it. But then late in the game, they began to worry about the effects of temperature. So we wound up putting additional insulation around the struts. Did we have to do that? I don't know. I just don't know. But it was the conservative thing to do, and we were using this Mylar foil that was so light that you could do all four struts for very little weight, so we did it. We were very conservative in a lot of ways, and there were probably some ways that we were not smart enough to be conservative, where we were just lucky.

Now that I [have] mentioned the aluminized Mylar, one of the last things we did was design the exterior surface of the LM. The LM was built sort of inside out. See, in an airplane, in a high-speed airplane or almost any airplane, [we] build an aerodynamic shape that's structurally sound. Then [we] pack stuff into it. In the case of the LM, there was no atmosphere to contend with, so it gave [us] another degree of freedom. So after thinking about it, we said, well, [we'll] sort of build it from the inside out. [First] make the pressure vessel. [Then] try to get as much equipment outside of it as [we] can, and then there's the concern about two things: micrometeoroids, and also the thermal balance. So the skin has got to handle those two things.

So what we did eventually was, around the pressure vessel we built a thing called the birdcage, which was the support for the external skin, and the idea was that if you had a skin that would cause the micrometeoroid to slow down or perhaps even shatter, the likelihood that it would penetrate the pressure vessel would be much less. Couldn't guarantee anything, but it looked like—of course, in those days, we really didn't know what the frequency of micrometeoroids would be.

So anyhow, we built this device that would hold the skin about two inches off the pressure vessel. Then we began to worry about the skin, because in addition, it had to maintain a thermal balance because we wanted to minimize the power needed to keep the crew in a reasonable atmosphere, because the power had to come from the batteries, and the batteries are heavy, and we wanted to minimize the weight of the [batteries], so we spent a lot of time analyzing and testing the external skin.

In the meantime, it [was] beginning to dawn on us that there are a lot of square yards of external skin, and so, therefore, there's [motivation] to keep it light. And this came late enough in the game so it was a real struggle. We got there really by doing a lot of testing and using a lot of ingenuity as to what parts to make certain combinations of Mylar and metal, and what parts to make shiny and what parts to make black. Of course, all this got tested in the vacuum chamber, and, again, it worked.

[Another] thing about the LM that was kind of interesting is, we never got anything back to look at. Now, in the aircraft business, [we] frequently would have a chance to go over a machine after a long flight or even go over a machine that had crashed, and [we] can find out something about it and feel better about the next one. In the case of the LM, we never got anything back, and that brings me to the batteries.

The art of batteries is a mystery, and it was probably more of a mystery back in those days. But we obviously wanted very high-performance batteries. By "high performance" I mean lots of energy per pound of battery.

There had been some batteries built by EaglePicher [Inc.] for satellites. So we went there, and their first batch of batteries showed performance that looked like you had shot a shotgun at the wall. No consistency at all. So we went through the whole process and came to the conclusion that their real problem was that there was one step in the process where they were exposed to the humid atmosphere of the everyday world, and once that was fixed, then [the batteries] began to be consistent. And this was a pretty special battery in that it was designed to be used right down to the dregs, so you couldn't recharge it.

So then the question was, how do you know that the batteries you're going to put in the vehicle are good if you can't test them? So we worked up a sampling procedure where they'd build two dozen batteries, and we would select six on the basis that all of the others in that batch tested okay, and that's the way we qualified the batteries for flight.

The other thing about batteries is that they have to be vented, because the reaction causes the cells to swell a little bit. So there has to be a vent to relieve the pressure. In zero gravity, which way do you aim the vent? I think it's true that we modified the vent on the batteries for every mission, or almost every mission, and yet the batteries worked every time. It was very disturbing to feel that [we] didn't really know whether [we] had the right design for the battery or not. All that [we] knew was that they worked the last time. And should [we] tamper with it any further?

But they were silver zinc batteries. Sodium hydroxide was the fluid involved. They were works of art. They were in magnesium cases and very beautifully built. But to somebody who is more of a bolts-and-nuts engineer, as I am, it left [me] with a little feeling of never being 100 percent sure. [We]'d do the best you could. But when [we] tighten up on a bolt with a torque wrench, [we] know exactly what you have. With the battery, we never knew exactly what we had. We just had what we thought was enough, and we certainly had done a lot of testing.

MRS. GAVIN: The same—

GAVIN: The engine? Which engine?

MRS. GAVIN: It was the same. Couldn't test it.

GAVIN: I mentioned that earlier, that the whole propulsion system could not be tested before a mission. We did a lot of testing on systems that were hydraulically similar.

We had trouble with the ascent engine in that after we were into the program, I think the people from Marshall [Space Flight Center, Huntsville, Alabama] came up with the bomb testing for combustion stability, and that was a new requirement, and it turned out that the adaptation of the Agena engine, which had always worked, became very difficult, because [it] began to show combustion instability.

This went on, and I made numerous trips up to Bell [Aerosystems Company, Division of Bell Aerospace Corporation] at Niagara [Falls, New York], and they finally got a configuration that seemed to work. And then, by golly, they lost configuration control, and the instability came back. At that point, NASA said, "Look, we've been developing a backup injector at Rocketdyne. I think we'll go with it." So Bell built the engine except for the injector. They went out to Rocketdyne, [who] put their injector in, and that's the engine we used.

I remember meeting with the Bell people to tell them that that's what was going to be done. It was a terrible disappointment, but the fact of the matter is, they had lost configuration control, and what that said was there was some little geometry in the injector that was very sensitive, and they'd lost it. So we did the right thing, but it was painful all around.

My father died just two days before I was to go to the Cape for the [Apollo 17] launch, and the net result was that I witnessed that mission from the Bethpage facility and didn't go to Houston until the crew was back.

You know, I've left out something. On every mission, after the crew was back, there would be a debriefing, and a couple of us would go to Houston for the day to listen in to the crew's debriefing. I remember the—let's see. Which was this? I can't remember which mission, but on one mission the doctors had jacked up the—was it potassium content of the food? Apparently, it showed that the astronauts lost potassium because of the weightlessness, so they'd jacked up the amount of potassium in the food, and that was a disaster. I remember John [W.] Young, in the debriefing, say, "If those medical people ever do that again, I'm going to shoot 'em." Apparently, it led to being strongly physicked throughout the mission, and it was very unhappy.

As far as afterwards is concerned, needless to say, there are a lot of people that had to look elsewhere for employment. A number of lunar module people went into the school systems, both at the high school and college level. We had a very careful system for separating people, which took time, but which was considered by all to be fair.

Of course, all during this time, the company was going through the F-14 development and its associated crises, of which there were some. I can't remember the exact sequence, but our charismatic president, Lew [Llewllyn J.] Evans, died, and I became president. Then I was faced with catching up what had been happening for ten years in naval aviation and for getting the F-14 into production, and that was a learning experience.

The F-14, looking back at it, was a remarkable development. The principal designer, Mike Pelehach, just died this year. But he created an airplane [where] the first squadron went to service in 1975, and today it's still the top fighter in the world, certainly one of the two top fighters in the world. It was a complex machine, and getting it into production was quite a chore, but some of the people that had worked on getting the LM built became principals in the final stage of manufacturer and the flight test of that machine.

I remember one, Paul Butler, went from being an organizer for the LM checkout at Bethpage to doing a similar thing with the airplanes at the Peconic River Facility [Calverton, New York]. As I said earlier, we did manage to cut down the number of flight tests prior to delivery, and that was an uphill struggle.

So as far as the Shuttle is concerned, yes, we decided to bid on that. We had a good team, and we had Martin [Marietta Corporation] Denver [Colorado] on our team, and that was a nice relationship. We fitted together well, we talked the same language, and I think we had an outstanding proposal. I can't verify what happened in the final selection, but the gossip has it that Mr. [Richard M.] Nixon put it in California, and that's all I should say about it.

Earlier on, before the final competition, we and Boeing had joined up to define the basic characteristics of the Shuttle, namely, the Shuttle, the big tank, and the boosters, because previously there had been some designs that had two almost identical devices. The booster would fly back and land. This was all based on looking at a cost-frequency curve, where if you had a very high frequency, you'd want a more costly machine, and vice versa. Now, part of the problems with the Shuttle over the years since has been that the frequency required has always been overestimated to some degree, and consequently, the cost per launch has always been higher than desired, and that's a whole 'nother story.

The consolation prize was to build the wings for the Shuttle. We did that, and that was an exercise in internal management, because [we] didn't build them one right after another. [We] built one, and then the number of people involved drop off, and then a year later [we would] build another. So [we]'d have these humps in the manpower requirements. But I think we did it adequately, and our relationships with Rockwell were always pretty good.

I retired in '85, and I consulted for the company until 1990, and then I cut the cord, and I think it was '93—I believe it was '93 or '94 that Northrop [Corporation] bought the company. So I was happy I wasn't there for that.

WRIGHT: [We] certainly appreciate all the time that you've given the project this morning, and I have to ask, after all those years that you lived your job almost every moment, and then even borrowed some into the next day, was there ever a time that you thought that maybe Grumman shouldn't have moved into the space business and stayed into the aircraft business?

GAVIN: No. I can tell you an anecdote connected with that. I've been giving talks to sixth graders in one of the local schools about going to the Moon, and in one case I leaned very heavily on how hard it was to get real reliability, something you'd bet your life on. In the question period afterwards, one of the little girls said, "Mr. Gavin, why would anybody want a job like the one you had?"

Of course, the teachers looked at me and said, "Okay. Perform." [Laughs]

I said, "Well, you must understand that there's a certain satisfaction in living and working at the cutting edge of new technology. And while this isn't for everybody, for those of us who are true enthusiasts, it is the place to be." You know, I could've easily stayed on the airplane side of the company, because there were some very interesting things going on there, but I never regretted getting into the space business. It taught us some things we would not have discovered as quickly otherwise.

I think the major thing we learned was how to make something reliable, and I think this is a lesson that perhaps people haven't paid much attention to. It's very costly, and it takes a determined group of people to make it happen, but it can be done, and it's something that I suspect is not fully appreciated.

Of course, I keep up with the trade magazines, and I know what's going on with various new developments and so on, but it's tough. It's tough, and it is costly, and it takes patience, and it takes continuity. I think one thing that Apollo had was a commitment to do something in a certain length of time, and somehow it happened that way. Whether you could ever do it in today's world, I'm not sure. In today's world, there are so many other factors that are going on that, thinking back to it, none of us ever worried about litigation or anything of that sort. We just went and did what we thought was right. Today, I have no idea what the conditions would be surrounding the effort.

We were fortunate in having a wonderful group of people and also, as I've said in my paper, NASA had collected a wonderful group of people. I knew Joe [Joseph F.] Shea quite [well]. I knew George Low quite [well], Owen [G.] Morris, Max Faget, all of those people. Didn't always agree with them, but between us, we usually came to a pretty good decision.

Fortunately, I had some excellent support most of the time from the senior management of my company. Our executive vice president, Bill [William T.] Schwendler, who originally, when the Grumman Company was founded, was chief engineer and [had] been chief engineer through the wartime, backed me up in a number of cases where any waffling would have made it very difficult. So I was fortunate to have a great team, and some of us still keep in contact. Looking at the Christmas card list, there are still a number of people there that I like to keep in touch with.

Well, another indication of the sign of the times. When we first went to Houston to negotiate the details—this is what the fall of '61, I think—there was only one hotel that would take the whole team because two of our lead engineers were black. Of course, it's all different today. But that was important in those days.

WRIGHT: Well, it certainly has been a time of interest, in retrospect. People still study the Apollo days for lessons. Again, we thank you for spending so much time with us this morning so that we could hear what you've experienced as well.

GAVIN: Well, it was a great experience. At the time I didn't appreciate the whole thing. I've read Tom Kelly's book [*Moon Lander: How We Developed the Apollo Lunar Module*] and some of the other books, but the thing that has impressed me is that you get so involved, you see the things that you're involved with, but there are a lot of other things going on that you only hear about later. It was a great collaborative effort, I think. So it was a great experience. Furthermore, I wouldn't have gotten elected to the National Academy of Engineering otherwise.

WRIGHT: I think we'll stop at that point, and thanks again.

[End of interview]