ORAL HISTORY TRANSCRIPT

AARON COHEN
INTERVIEWED BY SUMMER CHICK BERGEN
HOUSTON, TEXAS – 25 SEPTEMBER 1998

BERGEN: This is an interview with Aaron Cohen on September 25, 1998, in Houston, Texas. Interviewer is Summer Chick Bergen, assisted by Carol Butler and Glen Swanson.

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

COHEN: My pleasure.

BERGEN: Let's start prior to your employment with NASA, when you worked at General Dynamics [Corporation], because that's when you first got involved in the space race, right?

COHEN: I worked at General Dynamics as an aerospace engineer. We were working primarily on the Atlas vehicle, and then, of course, the Centaur came along. My first real experience with NASA happened while I was at General Dynamics, and it was a very interesting experience. I didn't know what NASA was at the time. I didn't have any idea what NASA was. But we heard that NASA wanted to come talk to us about putting a capsule on top of the Atlas, because they wanted to put a vehicle in space.

Of course, if you know what the Atlas looks like, the Atlas was what they called a balloon structure. It had very thin skins and you actually stiffened the structure by putting gas in it. So it was not a very stout structure as you would think. So putting a capsule on it, we couldn't do it just by putting a capsule on it, so we had to put a band around a station. I remember the station; it was called Station 502, on the Atlas.

They said NASA was coming to review our design, and I said, "Who is this NASA?" So we worked all weekend, into the early mornings, and we had the design, and NASA came in to review our design. Of course, they finally wound up using the Atlas for the first launch vehicle for the first spacecraft. So that was my first experience with NASA, and I was still at General Dynamics.

Then I worked on guidance navigation and control and also on aerodynamic heating at General Dynamics, and then the Apollo proposal came out, the request for proposal for the Apollo Program. Of course, that really captured my imagination. I was working in General Dynamics at the time, and I helped work on the proposal for General Dynamics.

It turned out, of course, that General Dynamics did not win the proposal. In fact, that was an interesting story, too. People who know my wife know she's very alert and knows a lot of things, and I got a call from my wife. We had a war room set up for waiting for the award of the proposal to be announced, and we had all the big—I wasn't one of these people, but all the big executives were waiting for the announcement. I was down just in the room, my little office. Pretty soon my wife calls. She says, "I hear where North American Aviation won the contract."

I said, "How do you know?"

She said, "I was listening to the business report from New York, and they announced it." Of course, they announced it from the business news. [Laughter]

I went up and told the people on the ninth floor that we didn't win the contract, that North American Aviation won it. So I knew about it before they did.

So I was interested in working on the Apollo Program, and there were some NASA people that came around during our proposal period and I met them. I contacted them. I heard that they were going to hire some people, and I contacted them. I was fortunate enough to be hired as a very junior engineer at the Johnson Space Center in 1962. So I came as a very junior engineer working in the Apollo Program Office, Project Office at the time, as a junior engineer. But I did have the fortunate experience when I came there to be able to work in the guidance, navigation, and control area, and had the extreme great fortune of working with the MIT [Massachusetts Institute of Technology] Instrumentation Laboratory, which is now, of course, the Draper Laboratory.

So I worked with MIT Instrumentation Laboratory, and I was, you might say, the liaison between MIT and the Manned Spacecraft Center at the time, and the Apollo Project Office. There I had the very good fortune of working with some people that I still feel today are some of the great leaders in aerospace and really were the great leaders in getting us to the Moon—Dick [Richard] Battin, Norm Sears, Phil Felleman, Dave Hoag, who I still remain very, very good friends with and talk to frequently. Some of them are retired, but Dick Battin still teaches at MIT, he's still a professor at MIT, and Dick and I still talk to each other quite frequently, and both his wife and my wife, we get together and we even work together in various periods of time. He comes and visits my class and I come and visit his class. So that was a very fortunate experience for me.

Then time passed and the program was moving forward. We were starting to expand a little bit, and Joe [Joseph F.] Shea came in. Joe Shea and I became very close friends. Joe became one of my very first mentors at the Johnson Space Center. We used to play tennis every Saturday morning. We used to get out there, we didn't say a word to each other, we used to get out and play.

Then he took me out of that area and put me more into systems engineering, and gave the software, the Draper Lab, more to Chris [Christopher C.] Kraft [Jr.] and Bill [Howard W.] Tindall [Jr.] at the time. They were doing that, and I went off to doing more of the work of the total interfaces.

I remember a very, very interesting job he gave me, probably one of the biggest jobs I had at that time and I didn't recognize it. It was a job that Joe Shea gave me, was to resolve all of the interfaces on the Apollo Project. Now, that amounts to about 1,200 interfaces. Those were interfaces between the command module and the service module, between the command service module and service module and lunar module, and between all the guidance equipment and also between the booster and the launch complex. Interfaces, as I teach in my class today, are one of the hardest things to define, because you can't define an interface until you have something designed, and you can't design something until you have the interface designed. So it's very, very difficult to define interfaces. You'll find today, even when you build something, that's one of the toughest jobs to do.

So Joe Shea gave me that job, and the job was in a very limited time, because it had to be done. We were well along in the design, so I had a number of very large meetings with all the contractors and all the centers, Marshall Space Flight Center, at that time the Kennedy Space Center, and we all got together to resolve these interfaces, … North American Aviation, Grumman, all the contractors, to resolve these interfaces. I had a very good team of people. It wasn't a lot, but I had about twelve people, and we resolved the interfaces. We did get them resolved in that period of time, and I felt that was one of the biggest accomplishments I ever did, was, as a junior engineer, to really resolve all those interfaces.

I still have a note that I remember very clearly. Dave Hoag, from MIT, said, "We need to help Aaron Cohen resolve these interfaces. He's got a monumental task," and he sent me a copy of that note. I still have that, and I show it to my students, because here were the things, here were the checklists. So it was a very, very interesting experience.

I do remember one little anecdote. When Joe Shea took me around, he took me around to introduce me to all the people and tell them that I was going to resolve the interfaces. In fact, he took me to see Werner von Braun, and so he was telling Werner von Braun that Aaron Cohen was here and he was going to resolve all the interfaces. After this long discussion, Werner von Braun said, "What's an interface?" [Laughter] That was a funny story.

So I did that, and I was in the systems engineering organization. At that time I worked for Owen [E.] Maynard, who was head of systems engineering. I was Owen's deputy. I remember very distinctly—(this is a little out of sequence) but I remember very distinctly that Owen was leaving, and I remember George [M.] Low, who then became program manager, called me and told me I wasn't going to get the job. My heart just felt so heavy, I felt so sad, but he said he didn't think I was ready for it. I said, "George, I'll tell you what. I understand what you're saying. It hurts a lot, but I'm going to go out and do the best job I can," and I did. And it paid off, because George Low became a very, very—I thought the world of George Low. He became a very close friend of mine, and he really helped me a great deal. So it paid off.

But a couple of very significant things happened that I can remember very distinctly when I was deputy systems engineering, very significant things. One, after the Apollo 1 or 204 fire—I call it the 204 fire, but Apollo 1—I was selected by Frank Borman to be on the Borman team, and went out with Frank and several other people, Doug [Douglas R.] Broome and Jerry [W.] Craig and Frank, to be out at North American about four months to make decisions on the spot of the changes we wanted to make, to help understand the changes we wanted to make. So we were out there for about a four-month period, and that was a very, very significant point in my career.

I don't know how I really got on the team. I think Frank had something to do with it. I think George Low had something to do with it. Of course, my very, very good friend, and I think the person I really probably respected as much as anybody, and I still respect, is Chris Kraft. I mean, I think the world of Chris Kraft. Of course, I think Chris had something to do with it, too. But I was selected to be on that team. We did our job and were very successful.

BERGEN: What did that team do?

COHEN: What did the Frank Borman team do? After the fire, we went through a very systematic review of all the changes we wanted to make. Some were very big, like from the inward-opening hatch to an outward-opening hatch; wrap all the plumbing; changed the insulation on the wiring; changed the materials in the vehicle. I don't recall, but there were maybe a couple of hundred changes, specific changes, that were statements, "This is what we want to do."

Well, from turning a statement of what you want to do into a drawing so that somebody can build something, from that building something to putting in the vehicle is a big step. It's easy to make a statement. So we took those statements and turned those statements [into hardware], we worked with North American on a day-to-day, hour-to-hour basis to turn those statements into drawings, to interpret what was meant, and then to interpret the drawings into hardware and how you installed it in the vehicle. So that's what the team did. We were more or less the information to help North American implement the changes.

If there was interference or if they couldn't do it, we would make on-the-spot decisions to make those changes. In a normal way you would do that, you'd have to go back and forth, [with] phone calls. And we were out there right on the floor with the manufacturing people and the engineers, working with them on a very fast basis so we could do the implementation very quickly. That's what the Frank Borman team did.

BERGEN: What was the atmosphere like at the Downey plant?

COHEN: It was very upbeat. Everybody knew they had a sense of direction. Of course, Frank Borman was a fantastic leader, and being there helped instill confidence in them. There was a very good relationship and very good team spirit, and we got the job done. Of course, that's another highlight of my career, working with Frank Borman. Of course, his whole posture, just his confidence, his "can do" attitude, his willingness to work with people, to listen to people, really stimulated the whole thing. So that was just a wonderful experience of working with Frank and the people out there.

Also I had the great opportunity at that time to work with another great giant, George [W.] Jeffs [phonetic] from North American. George Jeffs and Ed Smith and George [B.] Merrick [phonetic]. George Jeffs and I still remain very good friends after all this period of time. He's another great person in the space program. So that was a very key milestone in my career.

There turned out that I remember while I was out at Downey at the North American plant (I think it was probably a little bit after the Frank Borman team had completed its work) George Low called me and said, "Aaron, we're having a problem with the atmosphere in the cabin. We have found that at 15 pounds per square inch, 100 percent oxygen, we cannot find materials that self-extinguish, and we've got to come up with some kind of atmosphere that will allow us to do that."

Max [Maxime A.] Faget's people at the time in engineering had been doing some testing, and found that a 60 percent oxygen atmosphere, 40 percent nitrogen atmosphere, we could find enough materials that were self-extinguishing.

So George asked me to pick a team and see if we could come up with an atmosphere starting off with something like a 60-40 percent oxygen-nitrogen, and see if we could implement that in the existing spacecraft. I picked a team, and the team was composed of a former astronaut, Dr. Joe [Joseph P.] Kerwin. In fact, interestingly enough, I was at a meeting with Joe Kerwin this morning and we were talking about 60-40. Every time we see each other, "Joe, do you remember 60-40?" Because that was, in my career, another famous implementation.

So, Joe Kerwin; there was an engineer from North American named Dave [David S.] Levine; and John Zaccaro. I remember the four of us worked as a team to implement the 60-40. We did it in a fashion that did not require any hardware modifications to the vehicle. Joe Kerwin, being a medical doctor, was convinced that 60-40 was okay from a physiological point of view. And the materials, Max Faget's people proved that it was okay from a materials point of view. Now our job was to see how we could implement it in the vehicle.

We found that we could pressurize the cabin with ground equipment at the Cape, so that didn't require any additional equipment. We could pressurize the cabin at 60-40, but we wanted to be at five pounds per square inch, 100 percent oxygen, when we got to orbit. So how did we do that? We found a port in the waste-management system that we could open, that was existing there, we could open it and that would vent the cabin down. We found an oxygen-replenishment system that we could turn on a little handle and calibrate the flow of oxygen into the cabin. By the time we got to orbit, we were down to five pounds per square inch, 100 percent oxygen. So we could implement it without any real significant changes to the existing hardware and solve the problem. Again, that, to me, was one of the very key points in my career in terms of the early days of the Apollo Program.

I then was made—I'm not sure I've got the sequence quite right—I was deputy chief of the Systems Engineering Division in the Apollo Project Office, and then the head of the Project Engineering Office left, so I was made chief of the Project Engineering Office. So I had two titles: deputy of Systems and chief of Project Engineering. They were at opposite ends of the hall, so I used to go between the offices of deputy of Systems Engineering and chief of Project Engineering. The job of Project Engineering was to get the vehicles ready to go to the Cape. We actually got the vehicles ready, checked out and everything. We had project engineers that actually worked on the vehicles, the command and service module—I'm talking now about the command and service module, not the lunar module—and get the vehicles ready to go to the Cape.

So that transpired, and then I happened to be out at Downey again one time, and I remember getting another call from George Low saying, "Aaron, we're thinking of a mission that we really would like to talk to you about. We need you to do a review of the hardware

(S/C 106). I really want to talk to you in person. It has to do with a change in the mission of Apollo 8."

So I went back to Houston, and he said, "I want you to get a group of people," and explained what the mission was, what the Apollo 8 mission was. "I want you to look at spacecraft 106, do a review of spacecraft 106, and see if spacecraft 106 can do a lunar orbital mission, if the command and service module can do a lunar orbital mission."

So I brought a team in from [North American]. We did it at the Johnson Space Center. I brought a team in from [North American]. Ed Smith was chief engineer. I brought Ed in and several people, and all the documentation, and we worked back with his people at Downey, and we looked at all the documentation, all the discrepancy reports that had been resolved, all the structural analysis reports, all the wiring, [and] anything we could find on the vehicle [that] had the credibility and the integrity to do the type of mission we were talking about.

So I remember writing the report, finishing the report, and my wife and I took it over to George Low's house about twelve o'clock at night because he was leaving at six o'clock in the morning on the Gulfstream to go to Washington, and this was a report that I presume he needed, that certified that the vehicle was ready to go fly.

I remember the memo. I kept a copy of the memo. I still have a copy of the memo. After the flight, I got Frank Borman to sign it. It's one of my prized possessions. It's a copy of that memo, and Frank says, "Aaron, you're right, it was a great vehicle." So that was another very key milestone.

One anecdote came up in that time period. I think I was chief of the Project Engineering Division at the time. The Rockwell people came to me and said, "Aaron, we've got a problem." Rockwell and our subsystem manager on the service propulsion system—you've got to recognize that the service propulsion system was the key system that got the command and service module into lunar orbit and it had to work to get out of lunar orbit, and there's one engine. Like somebody asked Frank Borman, they said, "What if this engine doesn't work when you want to get out of lunar orbit?" He says, "Well, you have a bad day," because you don't get out of lunar orbit.

We had run some tests, and the test facility had found that we got a very large not explosion, but a very large pressure spike when they fired the engine without the barrel being wetted previously if you fired them both at the same time. There were two of them. Well, the mission rules called for if you were going to make a mid-course correction, called for both banks to be fired at the same time, because you wanted to be sure you truly made that mid-course correction, or they really wanted to be sure that if you didn't have to make a mid-course correction, if you went into lunar orbit, you fired both of them at the same time.

Well, they came to me and said, "Aaron, if we fire both of them at the same time when they're not wetted, you can really blow the engine up." So I had to call George Low and tell him that. He was at the Cape with Frank Borman. It was about three or four days before launch. And George Low was one of the nicest people you ever wanted to meet. He was very mild-mannered. He was not very happy with me. I think that's the first time I ever remember, he said, "Why are you telling me this at this late date?" That's what managers always say. I said it millions of times. It's the first time when you tell people something.

I said, "George, we have a solution." So we worked out a solution that if you fired one bank at a time and wetted it, then you didn't have a problem. So what we could do, without a mid-course correction, you can use one bank, fire a few feet per second out a plane, and a little later you could fire—not destroy the trajectory. That would wet that bank. Then a little later you'd fire the other bank and put you back in plane. So you'd have both banks wetted, so when you went to go into lunar orbit you could fire both banks and not worry about the problems. That's how you solved it.

Now, the issue, was you had to change the mission rules. Anyway, that was a solution to the problem. So I got the solution out. It was very heated at the time to get the solution out. So I do remember that very distinctly.

Those are some of the highlights. Then I became manager, in [1969], of the command and service module, and my history says after the announcement hadn't come out, I became command and service module right at the time of Apollo 13. So I was the manager. That was my first mission. I'm not even sure the announcement was out yet. It was in '69, I think. Apollo [13] happened in '70.

I do remember very distinctly going through all the investigations with James McDivitt, who was the program manager at the time. I remember going to Washington with Jim and going through the reviews.

Of course, where we really got very well chewed out by our congressional committees for Apollo 13 is—I don't know if you recall, one of the major problems was that we tried to boil off the liquid oxygen. After the Apollo fire, we changed the pressurization system. We changed the voltage from 28 volts to 65 volts, and that wasn't a problem had we not left the power on for that length of time, but we didn't change the relay that activated the thermostat. What happened is that when we left it on for that long a period of time, the relay essentially welded the contacts closed and the thermostat wouldn't open, so what happened is when you boiled it off, we got to 1,000 degrees Fahrenheit and damaged the insulation on the heater. Of course, when they tried to turn on the fan and the heater, it blew up, is what happened to Apollo 13.

The real issue there, the way we got very well chewed out was that there we didn't document clearly the fact that we changed from 28 volts to 65 volts. Of course, that made us understand that we needed to recognize, we needed to understand our hardware very, very well. Of course, that was a lesson we learned very clearly. That wasn't the only reason why it happened, but that was where we really got chewed out, you might say, by congressional committees.

BERGEN: Did you reflect any differently on Apollo 8 after the incident that happened in Apollo 13?

COHEN: No, not really. I was worried about Apollo 8, but I guess sometimes when you don't know things, you don't know. You mean did I reflect on it? No, not really, because I was worried about other things on Apollo. I was worried about, first of all, the navigation system, even though I was familiar with it. Here you're 240,000 miles away. Could you aim and point and ignite and thrust and hit a target 240,000 miles away? Of course, now it's obvious you could, but at that time it wasn't quite so obvious. The other thing was the heat shield. Was the heat shield really going to withstand the reentry heat coming in at 36,000 feet per second from the moon?

So I was more worried about those things. I know Frank Borman was probably worrying whether the hatch was going to stay tight, because there were reasons why we had an inward-opening hatch. An outward-opening hatch does have a mode that if it failed, wouldn't be too good in space.

But, no, I didn't really reflect too much differently on it. I feel Apollo 8 was the greatest mission I ever participated in, even though I was a younger engineer. I participated in Apollo 11 and, of course, the Shuttle STS-1 was really my vehicle, but I really feel that Apollo 8 was really the greatest mission we ever—it was a bold decision and it really is the first time humans left the gravitational field of Earth. To me, emotionally, that was very, very significant. Extremely significant.

BERGEN: Do you have any special memories from during that time of the mission?

COHEN: Of course, everybody reflects on the fact that when you make the burn and go into lunar orbit, you don't know if it's successful because you're behind the moon and had to wait till they come back around and tell you it was a good burn. When you get out of lunar orbit, it's the same thing. So those two moments have to be very, very emotional and a relief. Of course, I was still worried about the parachutes coming out and all that, because that always bothered me a little bit, that the bags were going to come out, we were going to be upright. Were the parachutes going to come out, that always sort of worried me.

But each phase of the mission had its own problem: getting rid of the launch escape tower during launch and separation. There were a lot of things that happened. When you look back on it, you say, my gosh, how did everything happen to well? I mean, it was pretty fantastic.

So that is a quick trip through my Apollo career. Of course, I was the command and service module project manager for Apollo 13, 14, 15, 16, and 17, and I was manager for that. Of course, each one of those missions had a little few instances here and there. I think on Apollo 14 we had a hard time docking. Each one had its own character. I don't remember them all right now, but I'm sure at the time—of course, as my wife always said, I got the vehicle up and I was worried about the next vehicle. Once it was launched and on its way to the moon, I was worried about how we were going to get the next vehicle ready to get to the Cape. That was really my job and to stay within budget.

Then I was called in one day by Chris Kraft, who told me I was going to become manager of the Space Shuttle Orbiter. Of course, that was a very big thrill. I mean, I didn't know what I was getting into but, as my wife said, that day was probably the greatest day I had in the Shuttle Program, that first day. [Laughter] But she was being facetious.

The Shuttle Program and the Orbiter Project provided a complete new set of problems in that being manager of Orbiter, the vehicle was much, much more complicated than the

Apollo vehicle. I mean, there's no comparison to the complexity of the Shuttle vehicle. The mission probably is not as sophisticated or probably as hard or as difficult, but certainly the vehicle is very complicated. What it is, it's a launch vehicle, it's a spacecraft, and, of course, it's an airplane, so it's three functions. It really was a challenge.

Chris Kraft gave me the distinct opportunity of taking a concept that were viewgraphs and paper and turning it into hardware, and turning that hardware into an operational vehicle. I was able to do that. I was able to do that through a lot of hard work, but through a lot of great support from a lot of people, a lot of people at Johnson Space Center, a lot of contractors, a lot of people at headquarters, and, of course, working with the Marshall Space Flight people and the Kennedy people. So I was very fortunate.

Some of the very interesting things there, well, if I go through in sequence, some of the interesting things there, first, I guess you'd have to say, is the budget. That was probably one of the biggest differences between the Shuttle Program and the Apollo Program. The Apollo Program, I think I went to Washington maybe one or two times on the budget. On the Shuttle Program, I was in Washington almost every week on the budget.

I remember distinctly, we got the authority to proceed in August of 1972. That's when they gave us the ATP, authority to proceed. Rockwell was the lowest bidder. The first year, I remember very clearly, in that year dollars was 140 million dollars. I remember that number. Right out of the bat, they cut it to 70 million and they said you couldn't slip-schedule. So I took a team of people out there. I had had some experience in going through program reviews during the Apollo Program, which I didn't mention. Even though we did have the luxury of a little more leniency in dollars, we still did program reviews in dollars. We did do a lot of reviews in dollars under George Low, Joe Shea, Tom [J. Thomas] Markley. So we did do a lot of dollar reviews.

The Shuttle Program was much, much more significant. Here we go from 140 to 70 million dollars, no schedule slip. So we went out there and we did a program review, and the first thing we started to do was take things out of the program or delaying things we could delay. There were certain phases of the program: there was DDT&E, design, development, test & evaluation; there was production; and then there was test and evaluation; and in between that there was increment two and increment three. Increment three was more production vehicles, building the production vehicles. So I kept taking things out of increment one and putting it in increment two. Finally, somebody leaned over to me, "I pity the poor person who's going to be the project manager on increment two." [Laughter] That's the way I was getting the cost down.

But we were able to get the cost down actually to a significant value which allowed us to get going. Now, a lot of people say we made some shortchanges there. I don't really think we did. I don't think we really made, from my point of view, any shortcuts in terms of safety, reliability. Little differences, we did have schedule as a variable, so we could slip-schedule it a little bit at a certain point in time. That changed a little later, but earlier we could slip-schedule. So we were able to create a pretty good program and get going with a restrained budget.

Of course, then the next big issue that hit, after we got over that budget hurdle, was the weight started to grow. Of course, pretty soon we found that, my gosh, we had the main engines, the SSMEs [Space Shuttle Main Engines] and the boosters, the solid rocket boosters, and the vehicles too heavy.

So then we had to go into a very detailed weight-reduction program where we started reducing things. For example, at one time the Shuttle had four hydraulic systems. Well, we reduced from four to three hydraulic systems. It had four auxiliary power units. We went from four to three. Henry [O.] Pohl, in Engineering, was just a fantastic help to me in doing that. Henry was the man I relied on in getting that done. So we were able to reduce the weight of the vehicle significantly, and people like Tom Moser and Bill [William C.] Schneider in the Structures group, and Norm Levine were very, very important in helping get the structure reduced. So I had very, very outstanding people at the Johnson Space Center in the Engineering Directorate that really worked very hard—Ralph [S.] Sawyer, I could go on and on. Ken Cox. Go on and on, of how we reduced the hardware and reduced the cost and the weight of the vehicle. Of course, Rockwell was very prominent during that, George Jeffs, Ed Smith. So we were able to do that, reduce the weight, or at least get it to a point where we operate the vehicle.

Then, of course, the next big famous problem was the thermal protection system. The tiles kept coming off. As my wife says, "Aaron's hair is gray for every tile it took to put on the vehicle." When you look back on it, it was such a simple problem, but we were amazed that we didn't solve it. And we didn't solve it until after we had most of the tiles on, on the first vehicle. Of course, I guess at that point in time that was the bleakest day. I really didn't know how we were going to get out of that problem. I really didn't know how we were going to solve the tile problem.

And it's such a simple problem, the strength of where you bonded the tile to the room-temperature vulcanizing [RTV] to the strain isolator pad, was weaker than the basic tile. Stress risers were set up. When people started looking at it, like Bill Schneider and Tom Moser, it was such an easy thing to understand, why didn't we understand it before.

Of course, then we went to various techniques to get the strength of the tile, like sonic tests and pull tests, and we were able to, you might say, crutch our way through it until several people at the Johnson Space Center and North American came up with the idea—I probably am going to give credit to people that I know deserve it and probably leave out some people that also deserve it, but a person like Bob [Robert L.] Dotts at the Johnson Space Center came up with the idea of densifying the lower quarter-inch of the tile with liquid glass, which essentially then made that like a solid base that you could essentially glue the tile to the vehicle to [eliminate] the stress riser.

Of course, that was the solution to it, and that's what we do today. We densify the tile and that's how the problem was solved. That really bailed us out, really solved a major, major problem. It bailed us out of a major, significant problem. At the time, though, I really didn't see how it was going to work, but that's what we do now, we densify the tiles, we put the tiles on, and then we came up with blankets. I think the thermal protection system today is probably one of the better systems. It's still a little fragile, but if you look at the alternatives, the tiles were really a tough system.

Of course, I remember distinctly Kenny [Kenneth S.] Kleinknecht, who's a very good friend of mine, and Bob Overmyer [phonetic], who passed away just recently, an astronaut who was down at the Cape, and they were putting tiles on and taking tiles off. Every morning we would have a status report, "How many tiles did you take off and how many tiles did you put on?" And it was just a touch-and-go process, "Are we going to get all the tiles on?"

My son, who was very humorous at my going-away party, my wife and my three children talked, and my youngest son said, "I used to hear my dad talk about this. He said they finally realized that you were going to have to put more tiles on than you took off or you're never going to get from here to there." [Laughter] "So they finally realized that. For a while I didn't think they were going to realize that. I think they thought they could take more tiles off than they put on and solve the problem, but they were going to have to put more tiles on than they took off." And, of course, we did and it was successful.

Of course, then we had a number of successful flights. Each flight had a couple of its own issues, the first liftoff where we had a software problem and we couldn't get the software working—I don't remember all the details, but I'm sure they're documented—to various failures.

I then was the program manager of the Orbiter Project for four flights, I believe, and then I became director, for a short time, of Engineering. Then we changed Engineering to

Research and Engineering. We combined science and engineering, and I became director of Research and Engineering. Of course, that was a new experience for me, dealing with the scientists and dealing with engineers, both of them, and it was really a pleasure, working with people like Joe Kerwin and Mike Duke and those people at the Johnson Space Center and the engineering people that I had in the organization.

Of course, then we still kept up to date with the Shuttle launches, but my job was more concerned about advanced technology, development of the Space Station and getting going on that type of activity. Tom Moser was director of Engineering, and he was more concerned with the day-to-day activity of the Engineering organization.

Of course, then the *Challenger* accident occurred and that was a very trying experience in terms of what we did there. Again, like any accident, any of a number of things could have happened and you wouldn't have had the accident. Of course, we did have the accident. When you look back in history, you wonder why we didn't fix the seal earlier. And I can't answer that question. The seal, if I look at it today as a professor, the seal is a very poor design. I mean, you don't design an O-ring seal that tends to open when you pressurize it. An O-ring seal has to be locked in place. Whether there's cold temperature or hot temperature, that is not a good design. Cold temperature certainly takes away the margin. But even under hot temperature, it was not a good design. So it was an unfortunate situation.

After the accident, we did a very similar thing that we did, at least in my feeling, after the 204 fire, Apollo 1, we went through all the systems and looked at all the systems and looked at what changes we had to make. It turns out, I felt that I was very wrong. I felt that it took us about—and I think my numbers are correct—it took us about twenty-one months from the time of Apollo 7 till we flew Apollo 8, and it was something like thirty-two months between the *Challenger* accident and we flew our return to flight. So it took us longer to get the Shuttle back to flight than it did Apollo.

I try to think why, and I guess the reason why is that we knew so much more about the Shuttle, because we had flown so many more times that we knew some issues that bothered us, so we fixed many more things than we did on the Apollo vehicle. Like you were asking me the question before, we knew more. We knew more, and as you know more, you fix more. I'm not saying it's wrong, but I think that was really the issue. We fixed not just the seal, but we fixed a lot of things on the vehicle, on the Orbiter, a lot of things in the tank, a lot of things in the engine, and we fixed a lot of things which essentially cost us a little bit more time, but I think it was the right thing to do. I don't question it at all.

I think the date was in October 1986, I believe, I became Center director of the Johnson Space Center, and I guess I had the job of trying to bring the Center back to having confidence in itself. My feeling was things I had learned, the way I did that, the way I approached the problem, and this is where I really used my previous history, I looked at what George Low taught me and I looked at what Chris Kraft taught me, and those were the two people that I looked at. I looked at what they taught me to see how I could best bring it back.

Of course, my real issue was, it was going back to the fundamentals. I liken it to when a football team has a losing streak. What they do is they go back to the fundamental. I thought we just needed to go back to the fundamentals. We needed to look at what we had, look at the vehicle, look at the people, and see how we could build confidence.

Of course, I had a fantastic deputy director under P. J. [Paul] Weitz. P.J. was just an outstanding person. P.J.'s a lot more stable. I get excited; P.J. was always very calm. I could always go rant and rave to P.J. and he'd calm me down, and we could go on and make decisions. But P.J. was a great guy. I couldn't have done it without P.J.

I believe we were able to get the vehicle not only in good shape, but get the people in good shape, and were able to fly. We had great crews, great flight controllers, Gene [Eugene F.] Kranz and his team, and great crew members. We were able to get the people flying. Of course, Dick [Richard H.] Truly was the administrator at that time, and Bob Crippen and

Dick Kohrs and Arnie Aldrich were all there as a team. Of course, I've got to say a lot about General Forrest McCartney [phonetic] at the Kennedy Space Center, was a very big help, and so was J.R. Thompson at Marshall. Then, of course, Roy [S.] Estess of Stennis.

So that team got us going again, and I think we were very successful in what we did and the vehicle we had and the management team that we brought about. I think if I had to go back, the people I relied on most was the teaching I learned from George Low and Chris Kraft. I know P.J. and I—you know, it's always very interesting, you make things so good that you can't do it. We found a—I don't know if P.J. gave it to me or I gave it to P.J., it was a ship on a very ominous sea and it said, "Ships in the harbor are safe, but that's not what ships are built for." And we kept that in front of us, because we could make this so safe that we could never fly again, but our job was to fly. We wanted to make it safe. We couldn't do everything everybody wanted, but we wanted them to share in what we were doing. So, I think based on that we instilled a confidence in the people to fly again. Of course, we had a number of good flights.

Then in between that time, I remember getting a call from Dick Truly. He was the administrator of NASA. I don't recall just the time he called, but he said that George Bush was going to make a speech, the twentieth anniversary of the lunar landing, and we needed to be prepared to do something in order to prepare to send humans to the moon this time, this time to stay, and on to Mars. He said, "I want you to assemble a team, and it's got to be sort of quiet. I want you to assemble a team and come up with some frameworks of how to do this."

So we did. I can't recall who was on the team. I know Mike Duke was one of them, and I think Mark Craig was the other. I don't recall who was on the team. I'll have to go back and research that. But we had a team. Somebody got me a room in the back rooms of the Johnson Space Center, and we did some studies. Then we started a ninety-day study. I think it wound up being 120 days. But it was a study that we did. I was in Washington.

We did that study, and there were some problems with the study. The costs were too great and I don't think the timing was quite right. I think it was a good start. We did have a lot of good people working on it. We had Johnson Space Center, we had Marshall, we had Kennedy, we had Ames. We had all the centers working on it. We had JPL [Jet Propulsion Laboratory]. We had everybody working on it, and I think it was a good report. It was a little bit too much money, maybe too grandiose, but I still think it set the stage for some good thought processes, but it wasn't tremendously well accepted, and I accept that, but I still am proud of the study.

Then in time I was asked to come to Washington to be Dick Truly's acting deputy administrator. Then Truly left and [Daniel S.] Goldin came, and I stayed with Goldin as acting deputy administrator for a year. We had some very interesting experiences there as deputy administrator. Washington was different for me, being involved in Washington. I really was more of a Center director, I think, than a manager in Washington, but I did learn a lot. I was able to, I think, help both Dick Truly and Dan Goldin through some testimonies and through some budgetary issues and through some technical issues, and I really enjoyed that experience. It was very, very interesting, and I liked it very much.

Then I came back to the Johnson Space Center for a year, and then my wife and I decided I really would like to do something different. I really had always wanted to teach. It's something I'd always wanted to do. I decided to retire, and Texas A&M offered me a professorship to teach mechanical engineering design and systems engineering, and that's what I do. So I retired and now I teach at Texas A&M, two courses. I teach senior mechanical engineering design and I teach systems engineering, two things I hope I learned while I was here. I've got to be honest with you, teaching is a lot harder than I thought it was going to be. I mean, teaching seniors, they don't let you off the hook so easy. So I enjoy it, but I'm working a lot harder than I thought I was going to work.

That's a very quick overview of my career. Maybe now you could ask me some questions. I've talked for a while. Maybe you can ask me some questions.

BERGEN: I would like to take you back to your earlier career. You did a great deal of work in guidance and navigation and control. Tell us about some of the challenges of that, in the early stage of the Space Program.

COHEN: The challenge of guidance, navigation, and control, there were a lot of challenges. I think guidance, navigation, and control—and if you look at what we did then, what we did in Apollo, what we did on Shuttle, and what they're doing today on spacecraft, is probably one of the hardest or most difficult systems engineering problems. Why do I say that? Because, first of all, it involves flight mechanics, orbital mechanics; it involves structure; it involves electronics; it involves software; it involves almost every system or subsystem or discipline you can imagine put together in being able to navigate, guide, and control a vehicle.

The challenges in the Apollo Program, of course, were ones where you really needed to do inertial guidance. Inertial guidance, in contrast to radio guidance, everything is on board. It's self-contained. During the Apollo Program, did we really have the computer capacity? Today your PC that you use is much, much more powerful than the computer we used on Apollo. I mean, if it wasn't for people at the Draper or MIT Labs, [like] Hal [J. H.] Laning and Dick Battin, that came up with the computer—you know, the computer on Apollo was a wire-rope memory. It was about 36,000 words of hard-core memory and it was wire rope. It was 1,000 words of erasable memory. You had to have the system designed. It was wired in with a rope. The zeroes and ones were wired in, so you had to decide what you wanted six months before you used it. So the computer was really the big question.

The next big question were the algorithms, and by algorithms I mean the actual mathematical analysis of what you wanted to do in terms of navigation and guidance. You were going to use a Star Tracker, where astronauts actually measured the angles between stars, just as the old sailors used to do, only we were doing it in three-dimensional form, and were putting that information into an onboard computer and calculating your position.

So when you put all that together and look at that technology and look at the software that you needed, the formulation you needed, you needed the computer, you wondered how was that all going to work. I mean, how were we going to be able to put that together and make it work? Then you said, my gosh, now once I do that, I've got to figure out what to do with that information. So now you've got to fire engines to put you on the right course. Well, the reeaction control engine or the service propulsion engine? And put you on the right course.

Then some way you had to control the vehicle about a center of gravity for control. So you had all those to put together and do it in a very small computer, and have all the instruments, such as inertial measurement unit, which you [use to] determine your attitude and measure your acceleration on, and all the analog-to-digital converters and digital-to-analog converters. How was that all going to work? Did you have all the right constants? Did you have all the right characteristics of the lunar surface?

So it was tough to do. The question is, were we going to be able to make it happen? And, of course, was it all going to be able to fit into the computer? So Apollo had its own set of things.

Then when you get to the Shuttle, the Shuttle brought its own set of changes. The computer was still a little bit in question. We used the old 4 pi [phonetic] computer from IBM, which we still use. Again, that's very, very obsolete. But then Shuttle was a much more complicated problem because we needed to have what we call a fail-operational fail-safe system. So you had to have four computers essentially working together in synchronization, talking to each other 550 times a second, and then if it made a mistake, that one was [voted] out and you had another computer take over. So that redundancy management system had never been done before, and we were worried were we going to be able to make this work.

It turns out, we made it work on the first approach and landing test. The first approach and landing test I remember very distinctly. I was at Palmdale, sitting in the control room right next to Edwards Air Force Base, and at that time I smoked a pipe. I was famous for my pipe. We separated off the 747. We blew the pyros off the 747, and you got a big X across the screen. The lead computer had failed. I think I bit my pipe in two. I thought, oh, my God. Of course, the next computer came on, just as we planned it. The next computer came in and we landed. What happened is, we had a bad solder joint, so when you blew the pyro, it knocked that computer off. But the point being, we had a fault-tolerance system, fail-operational, fail-safe system. So the question was whether we were going to be able to make that work or not.

So those were the big issues in the guidance system. In Apollo, was could we get everything in the computer. In the Orbiter, the Shuttle Program, could we handle the redundancy management system. Those were the two big issues. There were a lot of day-to-day issues, but if you really try to boil it down, is was could we get everything in the computer for Apollo, would it work. And for Shuttle, would the fault-tolerant or redundancy management system work. So those were very trying times, and we weren't sure we were going to be able to make it.

We had some awful good people working on the problem during the Apollo Program, people like Bill Tindall and people at MIT. Then during the Shuttle Program, people like John [W.] Aaron and those kind of people that were really key in making this thing work.

BERGEN: Thank you. Next I'd like to go to your job [working with] interface control documents. How did you make all those different contractors and all the different centers work together to agree on how these interfaces would work?

COHEN: That's a really interesting question. I had some very good people. Let's see if I can recall them all. One was Sid Jones, one was Jesse [F.] Goree. I had some really cream-of-the-crop people that were working for me. I did know a lot of people.

The way I started, we got a list of all the interface documents, just a list of what we had of all the interface documents between each elements. The elements were, to start with the command module, the interface between the command module and the guidance system, because the guidance system was provided by AC Spark Plug at the time. They actually provided the computer, the Raytheon computer, the Kollsman optics, and their own inertial measurement unit. So we had to get all those interfaces defined, because they went into the command module and also into the lunar module. So this time I was involved with the lunar module.

Then we had to figure out the interfaces between the command module and the service module, between the command service module and the lunar module, how that all fit into the interfaces into the Saturn launch vehicle, and how the Saturn launch vehicle actually interfaced with the launch complex. So we defined all those interfaces, and I recall—it's just from memory, but it was over 1,000. I think it was between 1,200 and 1,500 individual interface control documents.

We had a big meeting. I don't know if you've ever been to the Cape, but we used the big control room at the Cape, the firing room, and we had people all in there. We had people all in there by disciplines—lunar module, command service module. They were all sitting together. I briefed them on what we wanted to go do. We met at the Cape and we had rooms set aside for them. We said, "We are not going home until we at least identify all the interfaces and have all the actions assigned what it takes to solve them. If it takes a week or two weeks, we're going to stay here."

So we put them all in rooms, we had rooms for them, and I think we stayed there about a week. They came up with a list of all those things. That didn't solve it; that just was the identification of what had to be solved. That didn't solve anything, it just was the identification.

Then we set out a plan and a schedule of how we were going to solve those problems. We had leaders from each group, and we would send it to them, and then we would make visits to them periodically to see how they were doing, what stumbling blocks they had. Then very much like we did on the Frank Borman team, we would make on-the-spot decisions.

Now, I had to go back with those on-the-spot decisions and review that at the Change Control Board, because I was making decisions that affected other people's hardware. But we made on-the-spot decisions. Of course, that solved a lot of the problems. That didn't solve all of them; that solved a lot of them. Then we took that fallout and then we did it again.

Over about a six- to eight-month period, we were able to solve them all to a point where they could identify them and turn those definitions into hardware. So that's basically how you do it.

As I teach my students, there is no simple mathematical equation that will allow you to solve an interface. I mean, there is no way to do it. You've just got to work with it and do each one bit by bit, and if you look how it's done in industry or government today on a design, that's how it's done. So that's how we did it. It was a mammoth job.

But I remember the big firing room. Have you ever been to the firing room? Do you know what I'm talking about?

BERGEN: No.

COHEN: Well, we just had them all lined up and put in various seats, and that's how we worked. I didn't know if we were going to be able to do it or not, but we pulled it off.

BERGEN: You managed to work with all those contractors, too.

COHEN: All the contractors and all the other centers.

BERGEN: Were there any contractors that stand out in your mind from working with them, as being either exceptionally good at this or not?

COHEN: They all were good. Of course, I think that we relied very heavily on North American Aviation, because they at that time were really our lead contractor. So we relied very heavily on North American, and I have to say they probably were the driving force that helped us. But Grumman was good. They all were good. If I had to single out, the best one would be North American.

BERGEN: Tell us more about your interaction with MIT and the work that they did, because sometimes you don't hear as much about the work that they did.

COHEN: I personally feel—and if I'd write a book, I'd say it—I personally feel that MIT, the Instrumentation Lab, really were the ones who led us to the moon. I think if we would not have had Dick Battin's formulation of all the guidance algorithms, had they not been able to put that in the computer, I really feel that we would not have made it. That's my own personal feeling. I feel that that part of it was so germane to what we were doing, it was so much an unknown of how to do it, how to do the guidance that got you from the Earth to the moon, how you did the mid-course corrections, understanding where you were. Now, we did start to rely a little bit on ground information, but still, how you did the navigation.

Let me explain. There are three things that you need to do: navigation, guidance, and control. Navigation is finding out where you are, so you need to figure out where you are. That's where you basically use the sextant and the stars, and you did use some ground data. Ground data came up and told you where you were and you used that.

Once you find out where you are, you have a reference system and the computer tells you where you want to go. The guidance then takes that and tells you what to do to get to where you want to go. That is another set of algorithms, and that uses what we call effectors. It can be engines on Apollo. On Shuttle it's both engines and the aerosurfaces. What we call effectors.

Then control is how you maintain the stability of those systems around its center of gravity while you're doing all this.

So to put all those algorithms together, the MIT Instrumentation Lab had to do all that and work out all those algorithms. The leader was Dick Battin, and he had fantastic people like Norm Sears and Phil Fellerman and people like that, that really helped him do that. To me, it was just a phenomenal thing. I think on Apollo, you know, like somebody said, it's easy to get to the moon when it's a bright moon; you just look up there and point. When the moon is shining, when you go when you have a full moon, you're bound to hit it. But it's not quite that simple. You're 240,000 miles away. You want to be able to rendezvous with the moon. You want to break into lunar orbit. You want to then deploy the lunar module, let it come down, and come down in a fashion where it can actually land on the lunar surface at a particular point. You want to be able to lift the lunar module off the surface of the lunar surface, rendezvous with the command module, all that software, and then fire the engine and get out of lunar orbit with the command and service module on the way back to Earth, rendezvous with the Earth, come in, and not come in too steep so you burn up, or too shallow so you skip out. You want to hit that corridor just right from 240,000 miles away. Then you want to be able to maneuver the command module down with guidance algorithms that will allow you not to exceed its heating loads and its G loads, and land in a given place in the ocean, and allow the parachutes to come down and recover.

So those are really very complicated things. So I say that that was all done by Draper Lab, or MIT at the time, the Instrumentation Lab. So I give them an awful lot of credit. And that had to all fit within their hardware. The major hardware there was the computer, was the inertial measurement unit, which is a combination of accelerometers which measure your increase and decrease and acceleration. From a very simple math equation, if the computer integrates acceleration, you get velocity. If you integrate velocity, you get position. So by doing that, you're able to determine where you are. We call it a state vector. You're able to determine where you are.

Then the gyros maintain your attitude, so you have an attitude. Of course, that inertial measurement unit is aligned with the optics to a given reference system, so you know where your reference measurement system is, so you're measuring everything.

So, to make all that work in inertial guidance in that day and time was pretty fantastic. Of course, now inertial guidance, we use it a lot. We have global positioning systems now. Of course, it's a little bit more sophisticated and it's improved a lot. Of course, the computer's improved fantastic.

So that was my experience with MIT. Of course, we were starting off from scratch. Once you do something, it's interesting how much easier it is to do it, but when you don't know what to do, when you're starting off—in fact, I used to say—I'll digress a moment. Another anecdote. For a while on the Shuttle Program, I used to say that all that was going to happen was a bunch of viewgraphs. I said, "I want to see some hardware." I used to say, "Where are the chips?" So you need to get out of the viewgraph mode and start building something. Of course, to start off from scratch, it is hard to do.

To just elaborate on that, I teach a course in mechanical engineering design at Texas A&M, and I give my students something very, very complicated, that they know nothing about, they know absolutely nothing about, and they get very, very frustrated, but I tell them very clearly, I say, "If you study the program, if you diagnose the problem, you tear it apart, you build it back up, you'll know how to do it. When you get a job, your boss, he or she, is not going to know any more about it than you do, and you're going to have to figure out what to go do." And that's what we had to do with the guidance system. I think MIT did that.

BERGEN: It was an amazing feat.

COHEN: It really was. It really was. Of course, a lot of people at Johnson Space Center helped a lot, people like Chris Kraft and Bill Tindall were a very big help to them during the Apollo Program. I think I was, too.

BERGEN: Then later you were in charge of the command and service module during the latter part of the Apollo Program. How did you feel, as you were approaching Apollo 17 and the Apollo Program was coming to an end?

COHEN: That's an interesting question. You see, I knew that I was going to be manager of the Orbiter, so I was so preoccupied with getting into the Orbiter, I really didn't have, shall I say, any real strong feelings. Of course I would have liked to see Apollo continue, but I was so busy trying to figure out how I was going to do something from 140 millions to 70 million dollars, that I really didn't worry about that. Maybe I was wrong. To be honest with you, when I think back on it, I really did not think about that very, very much. That's just an honest feeling. We did some good missions, I liked the missions, it was great, it was a wonderful experience, and I can't thank people enough, being involved in it. On the other hand, I was so busy trying to figure out how to get the Orbiter built, even at that period of time, that it didn't really bother me a lot. That's an honest feeling.

BERGEN: Did you get involved in Skylab at all?

COHEN: The only thing I did in Skylab, yes, a little bit. The only thing I did in Skylab, Kenny Kleinknecht was the manager of the Skylab Program, and he gave me the responsibility to build the command modules for it. So I was the interface. I was responsible. He could have taken over the command module and built it, but it was a very good decision on his part because there was only one interface with North American at the time. So I built the command module. I was responsible for the command modules at North American for the Skylab Program. I didn't get involved in the tradeoffs between wet workshop and dry workshop. I didn't get involved in that. I really got involved in the command module. When they had a problem with the solar panels and all that, I really did not get involved in that. I was involved with getting the command module ready and getting the Shuttle built.

BERGEN: Looking at the Shuttle, if you look at the Russians, they've used the Soyuz spacecraft since the 1960s. Why do you feel the American focus shifted to reusable spacecraft?

COHEN: Well, I really think that the ultimate answer for reducing the cost of a pound of payload to orbit is by you doing a reusable vehicle. Now, whether the Shuttle really proved that or not, I think that's a little bit in question. You know, some people say, "Well, it was because you took shortcuts early in the program," i.e., the 140 to 70 million dollar type of problem I was talking about. I'm not sure I agree with that, but it's hard to argue. The Shuttle did not prove the economy we thought it could.

Now, I've got to also be honest with you, digressing a little bit from your question, also be honest with you. We changed the ground rules on the Shuttle as we went through the phase. I'll tell you what I mean by that, and people don't really recognize it. We said the Shuttle was going to be a very standard vehicle. You were going to put a vehicle in it, you were going to build a vehicle, you were going to have very standard interfaces. You were going to get a payload, you were going to bolt the payload in. All the payloads that came to you were going to be very standard. All the electrical wiring was going to be very standard. All the mission profiles were going to be very standard. It was really going to be a cargo where you took something up, dumped it, and went back.

Well, that changed. We changed it. I remember being called up to Washington and I was told by somebody you interviewed—Dale Myers. He said, "Aaron, this is going to be very simple. We're going to use off-the-shelf hardware. We're going to just take something up. We're going to have very simple interfaces. That's what I want you to do."

Well, I felt that's what they meant, but as you go through time, you then say, "Well, this payload doesn't want this. This payload wants this." So you start trying to accommodate people. And I'm not saying it's wrong, but as soon as you start accommodating people, the cost grows, the weight grows, the complexity grows. So I feel that, in my mind—people may disagree with me—I feel, in my mind, that's the reason why the Shuttle did not come off as cost-efficient as people thought it should, because we made everything for everybody. Is that wrong? No, it's not wrong, but it does not come out to be as economical a vehicle.

I still feel, though, that a reusable launch vehicle is the right thing to do, and I do think that what we did, in doing it, is the right thing, because I think whatever the Shuttle is or the next generation is going to be either—I don't know about a single stage to orbit, but I do think a reusable launch vehicle is still going to be the right thing to do. It may be something a little bit different than the Shuttle, but I feel we're on the right track to reduce the cost of payload to orbit. But I do think we've got to make it more of a standard launch vehicle than one that accommodates everybody, if you really want to make it less expensive. I really think that's the problem.

So I think we're on the right track. I think it's the right thing to do. I think basically it's going to be the most economical way to go. Actually, I think it's going to be the most reliable way to go.

BERGEN: You made the statement that the Shuttle was made to try to accommodate everybody, do something for everybody. Something you haven't mentioned in discussing the Shuttle is the Air Force. Did you interact with the Air Force?

COHEN: Yes, I did. I did interface with the Air Force a lot. We did do a lot of compromises for the Air Force—the cross range, the volume of the payload bay. So I did interface with the Air Force a lot. Of course, they had a lot of requirements which I think were good requirements, and I'm not sorry we did what we did for them and worked with them, because I think it did help us. I think it did make a more useful vehicle, but it did make a more expensive vehicle.

Just to give you an example, too. You talked about the avionics system, or the guidance system. When we started off, we were going to have four systems, fault-tolerant, and we said very clearly at the beginning, if we lost the one on the pad prior to liftoff, we'd go with three. That's what we said at the very beginning. Well, that's not what we do today. In fact, we added another one. We've got a fifth system. We've got a backup system.

So we changed the ground rules, which made the thing more expensive. People don't remember, but that's what we said very clearly, we were going to have four systems, and if one failed, we were going to go with three. We'd never do that. It probably was a wrong statement to begin with, but that's what we said. And that's one reason why it's not as cheap as it was. I'm not arguing that that was a right or wrong decision; I'm just saying that's why it's probably more expensive.

BERGEN: Talking about Shuttle, for Shuttle a new management structure was established: the lead center management. How do you feel about that management philosophy, and how did that work for you?

COHEN: There's a lot of argument with that. I personally think that it worked very well for me. I personally think it worked very well. There are people that will disagree with me, and very good friends of mine that will disagree with me. But I think the lead center was the right thing to do. I think it was the most economical way to go. I think it was the most reliable way to go. And we made it work.

Now comes the *Challenger* accident. Was that the right thing to do? People question whether that was the right thing to do. You know, it's always interesting to second-guess, but I personally feel that the lead center was good. I think it was good for me. I think I didn't have a problem with it while I was working on it. I think the leadership in Washington at the time, under John Yardley—by the way, I didn't mention John Yardley, and he deserves an awful lot of credit for the Shuttle Program. John was a wonderful leader, a wonderful man, and I can't say enough about John.

I will say this about John. Let me digress while I'm thinking about it. I used to go to Washington, and John Yardley used to just chew me up. I mean, I used to feel like a piece of Swiss cheese when I got through with him. But one thing about him, once he chewed you up, chewed you up about the problem, it was his problem; it wasn't your problem. I mean, he didn't even know what your face looked like; he was just upset. So I forgot who it was from the Cape, came up, was after me, and he got up and said, "John, I don't want to win. I just want to go the distance." So he just wanted to go the distance with John. John Yardley was a very great leader that we had in the program.

BERGEN: Talking about *Challenger*, from your perspective, what do you feel the differences from the effect on NASA in the public, between Apollo and the *Challenger*?

COHEN: Well, you know, that's a very good question. I've thought about that a lot, and I'm not sure I have a good answer. I've thought about it a lot. I don't really know. I guess the only thing I can really say—and I'm not sure it's right, but you've asked me the question. I think people during the Apollo Program, when Apollo 1 happened, I don't think they knew enough about the program. It was still a mystique.

During the *Challenger* accident, we had already flown so many times, it was not a mystique anymore. It was expected to be good. So I don't really know if that's right or not. But it's just like the point I made to you, I thought that we were going to be able to solve return to flight on the Shuttle much quicker than we did on Apollo, because the *Challenger* was really one problem. It was the seal. On Apollo, we never did find the ignition source. We really never did find out what the problem was. So I said, well, here's something we never really understood, here's something we understand, so I made the assessment in my mind that we were going to be able to return to flight very quickly. It took us longer. I really think that's symptomatic of the question you asked, because I think the feeling was much more serious during the Shuttle Program than it was during the Apollo Program.

Of course, it may be, too, that the investigation was more open to everybody on the news than it was during the Apollo Program. I remember sitting in my den one night watching the investigation of the Shuttle Program, and having previously, on CNN, watched the fact that a Blackhawk helicopter had crashed and killed ten people or so, and I was moaning and groaning. My wife came in, said, "What are you moaning and growing about?"

I said, "Here a Blackhawk helicopter crashed and killed all these people, and they don't have their hearing aired on CNN."

And she said, "Well, let me ask you a question. Where did you go for lunch after the first flight on the Shuttle?"

I said, "I went to the White House for lunch with the President."

She said, "Where do you think the Blackhawk helicopter people went after they had their first flight?"

I said, "They probably didn't go to the White House."

She said, "That's the difference!" [Laughter] So she sort of put me in my place. So that's the difference, I guess.

BERGEN: That's a tough question.

COHEN: It is a tough question. I really don't know the answer. I think a lot just depends on the time, because the time and the things people see, it was tough. But I don't know if I'm right or not. That's my own take on it. I thought about it, but I never did come up with a good answer.

BERGEN: What were some of the things that you tried to implement when you [became Center Director of] JSC after the *Challenger* accident, to try to boost morale?

COHEN: Well, what I tried to do is I tried to get back to the basics. I tried to have reviews that would let people air their concerns. I tried to make sure that people could voice their concerns. I tried to do that, but I also made it very clear that we could not do everything for everybody, and that we were going to look at things and then make an open decision of what we're going to do and why we're going to do it, and, whether they liked it or not, at least give them a decision point.

For example, there was a big issue about the big 17-inch line that fed the liquid oxygen from the tank to the engine. That was a very sensitive issue. We had had one failure at the test facility where it failed shut. It was held open by the aerodynamic configuration, the hydrodynamic configuration. If that shut while the fluid was flowing, it would essentially blow up an engine, blow up an SSME [Space Shuttle Maine Engine].

The crew felt very, very strongly that it should be more of a positive latch that kept that open and then deactivate that latch when you finish flowing, rather than holding it open just by so-called rigging of the hydrodynamic shape. I felt very strongly that better is the enemy of the good, and that we'd make a change, you don't know what you're going to get into, and we really had a problem. The crew felt very, very strongly about it, and so I, in an open forum, changed my mind and said this is what we were going to go do. And it was the right thing to do. I was wrong, and it was the right thing to do. So I do think that getting back and making open decisions, let people voice their opinions.

At my going-away party, Dave Leetsma said very clearly, "I've been trying to get a software change in since the *Challenger* accident, and Aaron Cohen wouldn't ever let me get in. Now that he leaves, maybe I can get it in." So, you know, you don't let everybody do everything, but you tell them why you're not going to do it. And that's what I tried to bring back to them. I made it very clear that we couldn't do everything that everybody wanted us to do if we wanted to fly, like the statement I made before. So that's what I tried to do, and I tried to use the lessons that I learned from George Low and Chris Kraft.

BERGEN: While you were director of JSC, you worked on public opinion, I guess, by trying to get more positive view of NASA out into the public with the Space Center Houston and things of that nature. How do you feel NASA is doing with that right now?

COHEN: I think they're doing good. I think it's very important, and I think they're doing very good. I think some of the things I did were very good. I was very interested in working with the uses of space to solve problems on Earth. I felt that was what had to be done to really make a big mark.

I'll go back to a very fundamental thing. Why did we do the Space Program in the first place? We did it because of the Cold War. We did not do it for science. We did not do it for technology. When [John F.] Kennedy said, "We're going to send men to the Moon and return them safely before the end of the decade," he didn't say that for anything but the Cold War.

Now the Cold War is gone, so I felt very strongly that NASA had to establish a need. What is the need, so the American public could clearly see the need? The need may be technology benefits of humankind. It could be something that would benefit people. I felt that what we needed to do was see how we could solve human problems on Earth from space. So that was my theme and that's what I tried to do. That's what I tried to do when I worked with the Greater Houston Partnership. That's what I tried do when I tried to do the Visitors Center. That's what I tried to do with the technology we'd work on.

We used to have open houses. I do think the things that they're doing now are very, very good. I really think some of the things they're doing, I really encourage and I like them. I think that's what needs to be done, because I do think that NASA needs to establish a need. It needs to establish where it's going. We have a very difficult job. We can't lobby. We can't do this, we can't do that. But we can show people that we're doing something to help them solve their problems. So I think it's extremely important.

BERGEN: I think that's important, too. What do you think is in store for the future of NASA? You were involved in the Moon and Mars Initiative Study. Where do you see that going?

COHEN: Well, you know, it turns out I was asked to be on the Mars Architecture Study Board at the Jet Propulsion Laboratory, which is looking at the future of robotic missions to Mars and the human mission to Mars. Of course, if you ask me, I feel very strongly that we're going to send robotic missions to Mars in terms of a Mars sample return and look for the existence of life, using the theme that if you can find water, you can find the existence of life. I think that's extremely important. I think it's important because I think it would let us understand our planet better, understand what the frailties of our planet are. It will also allow us to understand where our existence may have occurred. So I think it's important.

I think we will send robotic missions. In fact, I think we will have drilling on Mars that will go to a subsurface level of anywheres from 100 meters to 2 kilometers, and bring back samples of surface samples to the Earth. Then I feel eventually we will send humans to Mars. I'm sure not all the people agree with me on that, but I feel very strongly that's what's going to happen. I think the plan is there. I think we have the people to do it, we have the resources to do it. I think there are some technologies that need to be developed, like *in situ* propellant development on the Martian surface, like physiological effects on humans, solving that problem.

But I do think it will be done. I think certainly people coming out of school today will have the opportunity to do it. In fact, what I tell my students, I tell them that I was very, very fortunate to work on the programs I just described to you. I say it was just a fantastic feeling. My biggest desire is that I hope they're able to do the same things I did. There are programs that they'll be able to do, that I did. That's how I end my lectures.

BERGEN: That's wonderful. How do you feel about the current international cooperation that's going on in the space industry?

COHEN: Of course, I don't know too much about it, other than what—I don't want to sound like Will Rogers—what I read in the paper. But I do know a little bit about what's happened in Space Station, with the international participation in Space Station, because I read that. In terms of the French, in terms of the Italians, I think it's extremely positive. Even in this exploration program, the little bit I see there, the French are getting involved, the Italians are getting involved along with the Space Station. So I think it's very positive.

The Russians are very tough. I think they've having tough times. I think they can contribute. I don't know enough about their economics or where that stands, but there's no question that the Russians have had a very, very solid, good space program in terms of launch vehicles, in terms of their technology for their on-orbit capability. So I think there's some benefits there. What does the economic situation do? I'm really not sure I can answer that.

But I do think countries, in general, if they can bring things to the table, like the French can and the Italians can, I think international cooperation is a benefit, because I think space is an international program.

BERGEN: It looks like it's going to be that way in the future.

COHEN: Right. I don't think there's any question about it. Going to Mars will be an international program, both robotic missions and in the human missions.

BERGEN: Tell us something about the project you're working on with Kistler.

COHEN: That's a very interesting project. I'll get a chance to talk about my good friend George [E.] Mueller, which I didn't mention. Again I would say that George Mueller—there are a lot of people the Apollo Program couldn't be done without, and George Mueller is certainly one of them. It definitely could not have been done without George Mueller. He has the tenacity, the engineering capability, and the drive to make it happen. Had it not been for George Mueller, we wouldn't have made Apollo on the date we wanted to make it. So I think the world of George.

It turns out that I was in Washington at an AIAA Fellows dinner, I guess it was about three years ago, and George said, "Aaron, I've got something interesting." I was teaching at A&M. "I've got something interesting that I'm working on, and I'd like you to come up and see me in Seattle and talk about seeing if you'd be interested in doing it."

I said, "Well, okay," and I didn't think any more about it.

Then not too long after that, he called me. It turns out that I'm very fortunate at A&M, is they allow us to do fifty-two days a year, … a day a week, consulting. So I can do consulting. So I went up there and talked to George. Dale Myers was there. Of course, the three of us knew each other. I should mention Dale was another one of my very, very good friends in the Apollo Program. Dale used to work for me when he was at [North American]. Then I used to work for him. I've always worked for George.

So George had this idea of a two-staged orbit [vehicle] for communication satellites, and said, could we design it. So we started designing. Then I brought Henry Pohl in, because I never wanted to design anything without my good friend and engineer Henry Pohl, so I brought Henry in. We stayed up there for a while over the summer and came up with this design, which I think is very, very good. It's two-stage to orbit and return. The uniqueness about it is that it comes in with parachutes and air bags, and it can deploy [payloads on orbit]. It has a very good market in terms of communications satellites. The other thing that was very beneficial to us, as you pointed out, we could use the Russian NK33 engine, which took a big burden of development off of us in terms of a liquid-oxygen/kerosene engine, which made it very feasible to do.

One thing led to another, and pretty soon we've got a design and we've got people building it, and I think we're going to make this thing happen. So it's very, very exciting. We even brought Dick Kohrs into it, so we've got Dick Kohrs working on it, Henry Pohl working on it. Then we brought in Joe Cuzzupoli. He really built the Apollo and the Shuttle vehicles for Rockwell. So we've got Joe building it, and he got some very good contractors. We got Northrup-Grumman in El Segundo. We've got Lockheed-Martin in Michoud [Louisiana] building the [propellant] tanks. We've got Draper Labs doing the guidance system, along with Allied Signal. And we've got Oceaneering doing the thermal protection system, and Irvin [phonetic] Industries doing the parachutes and the air bags. So we've got some top contractors, good people, and we've got a good market. So it's very exciting and it's liable to be a very good success.

BERGEN: Terrific. Is there anything that you would like to mention, that we haven't already included?

COHEN: I don't really know. I think I've covered an awful lot. I'm sure I left a lot of things out. When you start talking about people, there's always a danger you're going to leave somebody out, and I probably have left somebody out. One person I know of very distinctly I left out is Bob [Robert F.] Thompson, who was head of the Shuttle Program, who I worked for. Bob was very key in the program, in getting the lead center going. If you want to talk about a person who really made the lead center work, it was really Bob Thompson. Bob, I think, was really a key n making the lead-center concept work. He and Chris Kraft, who was the Center director, Bob was the program manager, really made the lead center work. Lead center depends on a lot of people, and I think Bob was a key man in making that a success and making my job easy.

BUTLER: You mentioned that you worked on the command module at Skylab. Were you involved with developing the rescue vehicle, the command module vehicle that was modified so if they needed to do a rescue?

COHEN: Yes, I was. We implemented the hardware into that, yes. Requirements were established and then I was essentially building it, in that context.

BUTLER: There's not much reference to the rescue vehicle that we've come across. Was there a time during one of the missions, maybe the last one, where they looked at possibly needing to launch that?

COHEN: As I recall, there was, but I honestly don't remember the details of it. I do think there was a time when they thought they would have to go into—the other point was, I think, the other question was how much power did they have to keep on it to make it useable and that type of thing. Those were the issues. I think at one point in time there was a thought of using it, but it's very vague. I have to be honest, it's very vague in my mind.

BUTLER: When you first started out, you mentioned that these folks from NASA were coming down and you asked, "Who's NASA?" Would you ever have imagined where this would all lead to?

COHEN: No, not at all. That is a very good question. It turns out, people ask me how did I plan to get to where I was, and the thing is, I never did plan anything. I mean, I never did think today what I was going to do six months from now. No. The answer is no. If you would take what I did then and then tell me that some day I was going to be Center director of the Johnson Space Center, or acting deputy administrator, or even program manager, I would say, "Gosh, you don't know what you're talking about." I never had that thought.

In fact, when I came to the Johnson Space Center, I didn't know what to expect, and I certainly didn't expect to be a manager of the command and service module. I never in the world thought that would happen. I mean, it wasn't even on my radar screen. To be Center Director, I never thought I'd be Center Director. I never did really plan for it. I never did say, "This is my next step. This is what I want to do." It came to me, and I guess it came to me because I was lucky. If I look at people like George Low and Chris Kraft and George Jeff and Dick Battin and George Mueller, I guess that's why it came to me.

BUTLER: When Apollo 11 landed on the moon, where were you and what were you thinking?

COHEN: Well, I was in the control room, and I just thought it was normal. I mean, I just really didn't think it was anything great. I just thought that's what we set out to do and that's what we were going to do.

I will say one thing, though, to add on to that question a little bit. One thing I would add, and you led me to that question, if I look at what are the most significant achievements I made in terms of missions, I will say Apollo 8 was probably the biggest significant mission that I was involved in. Then I would say STS-1 was the next. Then I would say Apollo 11. Then I would say STS-2. And I'll tell you why I say it in that order. I think I told you enough about what I think about Apollo 8, because that was the first time humans really left the gravitational field of the Earth.

STS-1, to me, meant really my contributions to a reusable launch vehicle, which I think was very, very, very significant, the reusable launch vehicle. Of course, Apollo 11 can't go without being noticed. I mean, it has to be noticed, and it was very, very significant. STS-2 proved that we had a reusable launch vehicle. So that's my logic.

I think a lot of people say Apollo 8 was their—I mean, I think if you ask other people, other people will say the same thing. They'll probably say Apollo 11. I have to put STS-1 in before Apollo 11, which is probably just my own feeling on it.

SWANSON: The recent effort to consolidate the operations of the Shuttle to principally one organization, USA [United Space Alliance], at the time of your work with the Space Shuttle, was there early talk of changing the Shuttle operations and moving it to the private sector, in other words, getting a Boeing to operate the Shuttle?

COHEN: No. I tell you, I don't think it was really talked about that much. What we were really talking about is doing the operations of the Cape that way, the Cape contract that way, making it one contract at the Cape. I think they did do that eventually, Lockheed. They never really seriously talked about making the operations a commercial operations of the total system. That happened about a year after I left, I think, so I never really got involved with that. That was a very tough decision to make. I think it seems to be working. As much as they've done, it seems to be working pretty well, but I really am not that familiar with that question.

SWANSON: Regarding the *Challenger* accident, in your opinion, the problems that they had with the joints, if that accident did not occur at that time, do you think eventually it would have been [unclear]?

COHEN: You know, I've thought about that a lot. I think the possibility is there. If you really look at the design and you look at the finite element analysis or the structural analysis of it, that joint really opens up. If it's like this, when you pressurize it, it opens up and that O-ring is exposed. That first O-ring is completely exposed, even under a very hot day. Now, we did fly several fights that way, but quite a few flights that you never did damage that second O-ring. You had soot on it, but you never did damage it.

You know, your question is a very, very hard question to answer. I don't know. I guess I would have to say that it should have been fixed. I think it probably should have been fixed in line, at least. At least that's Monday-morning quarterback. You're much smarter on Monday morning if you go for a first down and you make it rather than kicking a field goal. But I just can't answer that question.

I teach my students, I use that as an example for my students, and I say, "Never design a system this way." I say, "Never design a system this way. When you design an O-ring, make sure it's locked in place. If you want it to be a static joint, be sure you lock it in place, because that's the classic way not to design an O-ring joint." So it's a bad design of an O-ring. If you want to get a grade, if you'd come to my class and get a grade on it, you wouldn't get a passing grade on that joint.

SWANSON: The whole idea of using a solid rocket, I know some of the early designs use a reusable cryogenic strap-on since then. What are your feelings about that idea of having a man attached to a solid rocket that basically you cannot shut off once it gets started?

COHEN: Well, you know, there still is a lot of arguments about which is more reliable, a solid rocket booster and a liquid system that is fed, that has all the intricacies of a liquid propellant, pumps and fluid lines and so forth and so on, so it's really very hard to say in terms of reliability.

I guess from my point of view—let me just talk a moment. The disadvantages of solid rockets is you can't shut it off. You can have thrust termination, as we looked at. You essentially have a ring at the top and you essentially cut a hole in the top and you've got a hole in the top and a hole in the bottom, and it neutralizes the thrust. That's basically the way you do thrust termination. You could see where there's a pitfall in doing that, because if that inadvertently cuts, then you really have a problem. Also you can put very high loads into the structure. So that's why we didn't do it. We looked at thrust termination.

So that's a disadvantage with a solid rocket, you can't shut it off, whereas in a liquid system, you can shut it off. That's the advantage of a liquid system. But then if you trade off the number of moving parts, the number of parts that have to work on a liquid propellant system vis-a-vis a solid, and you do a reliability number, I'm sure the reliability analysis, from a purely statistical point of view, from a points-count point of view, it's going to come out better for the solid than it is for the liquid.

So I guess I have to say that from my knowledge base, I still think we did the right thing, going to solids rather than liquids. There are people that I'm sure would disagree with me violently and say that liquids are the right thing to do, primarily because you could stop the thrust. That's really the issue. You can stop the thrust. So it's a very, very tough, thought-provoking question.

If we had to do the decision over, if you started over, then you put your mind in starting over, what would you come up with, it's hard to say what you would do. I'm looking at it from where we are now, which is probably not the question you asked me. Where we are now, could we go to liquids? If we start over, it's another question, and I don't know how I would answer that. I'm not sure I know. It would be a very tough one. It would be a very tough one. I'm sure technology has gotten better for liquids. I'm not sure how much technology has gotten better for solids.

On the other hand, when you go back to it, should that solid have failed? And it's so simple, fixing an O-ring, that it shouldn't have failed. So if that's the only failure mode, you should be able to fix that problem.

But let me just finish up here with a trite statement that I usually give my students. What is a failure? I say a failure is if you wake up in the morning and it's raining, and you have an argument with your spouse, and your windshield wipers aren't working, you have a higher probability to have an accident. If you wake up in the morning and you don't have an argument with your spouse, and your windshield wipers—you may not have that accident. So an accident is a combination of things that happen, and there's not one single thing that causes an accident, unfortunately. It's a combination of things.

So I don't know the answer to your question. I'm still thinking I would go with the solid, but that's just from my mind-set right now.

BERGEN: You mentioned that STS-1 was an important mission to you. That was a significant mission not just because it was the Shuttle, but it was the first time you tested a vehicle manned.

COHEN: Right.

BERGEN: Can you explain that to us, the reasoning for that, and how did you feel about it?

COHEN: Why didn't we, with the Shuttle, go unmanned before we went manned? Well, that's a very interesting question, and it was debated very hot and heavy and long and furious. It was very much like the question was asked about solids versus liquids. There's almost no right answer, because history said we always went unmanned prior to manned.

The real issue that swayed us is that if you look at doing a robotic mission, it's much easier to do a robotic mission once you've done it manned. Of course, it was very hard to do. We felt the failures modes were very significant, extremely significant, and we could not do a reliable unmanned mission because the man was too much a significant part of the system.

Now, interestingly enough, I think today we could easily do a robotic Shuttle mission. Other people may not agree with me. But mainly because we know what the man has to do. So I think it was fought long and hard, but that was the decision, and it was fairly unanimous at the Johnson Space Center that that was the right decision to do, although there were some critics of it. I think, as Chris Kraft said when he got on the microphone at control center after we landed, he said, "We're infinitely smarter today." And that probably summed it up. We were infinitely smarter today that we did land safe and sound after having done a manned mission first. That's a very, very good question. It's a very hard question to answer.

BERGEN: You've had an amazing career, and we appreciate you sharing the highlights of that with us. We've enjoyed it a lot.

COHEN: I've enjoyed talking to you. If you want to talk again, if you find something you want to talk about, we can probably get together at the Johnson Space Center or at College Station.

BERGEN: Great. Thank you.

COHEN: Thank you.

[End of interview]