ORAL HISTORY TRANSCRIPT

EUGENE E. COVERT
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is June 22, 1999. This oral history with Dr. Eugene Covert is being conducted at his office at MIT [Massachusetts Institute of Technology] in Cambridge, Massachusetts. The oral history is being conducted by Rebecca Wright, assisted by Carol Butler for the Johnson Space Center Oral History Project.

Good morning. We thank you again for taking time from your busy schedule for us. We'd like to start this morning, if you could give us some background. How did your interest in aeronautics begin?

COVERT: Well, I guess, like all of these things, it's probably ill-defined, but my first memory of awareness of airplanes and things like that was when I was about seven years old. At the time we lived in a small town in southern Nebraska, along the Republican River. I heard this thing flying overhead. My mother was not well at the time, so I pretty much had the freedom to roam this small town. The motor stopped as I was watching it, and it crashed into a tree about two blocks from where I lived, so I hustled down to watch and, of course, got in everyone's way, I sure. The guy was not seriously hurt. But at that point it seemed to me that airplanes are something that had some excitement associated with them, and I think that a lot of times it's the potential for excitement that causes people to originally get interested in things.

Then as I got older, we moved around. My father worked for a railroad company. So I'd build model airplanes and hung around airports and did all the usual things. So I decided probably that I'd like to be an engineer, and if I was going to be an engineer, I might as well be an aeronautical engineer. So that's the size of it.

WRIGHT: Did you ever have an interest in flying yourself or just studying about what made them fly?

COVERT: I had some interest in flying. At one point I knew how to steer. Also, one of the things we've done here is over the years have had a number of student projects associated with man-powered airplane. I was involved in starting about six others, six students the first attempt, and finally after about eight or nine years we got good enough, so I actually have peddled myself into the air three times, flown 100 yards or so each time.

WRIGHT: Ever think about that gentleman in the airplane when you saw when you were a young boy when you are up there?

COVERT: No. That's his problem.

WRIGHT: You, of course, grew from a young boy to a young man and went to school, I believe at the University of Minnesota.

COVERT: That's correct.

WRIGHT: And then you came here to study at MIT.

COVERT: Well, there was an interim period when I was in the Navy. At that time I was involved partly as a flight test engineer and partly in project engineering. Of course, flight test engineering was exciting and fun and so forth. I had been away from school for some time before I came here, and I originally came here for a short period of time. At the time I believe it's correct to say we had moved six times in seven years, and my wife was getting a little tired of this. She liked Cambridge and announced to me that she didn't know what my plans were, but when I was through being educated, she was going to stay here. Seemed like a rational thing to do.

I originally was going to just stay for a couple of years. Then I decided that maybe I ought to get a doctor's degree. Then I worked for an aerodynamics laboratory down the street, which MIT ran. I was the associate director of that. Then I got invited to be on the faculty here. I've had several other places where I could have gone. We agreed, my wife and I, that we'd stay in Cambridge. So I got on the faculty and got promoted a couple of times, ultimately got to be department head, and gave that up after five years, and retired a couple of years ago, and I'm still here. It's a rut that I got into. It seems difficult to get out of.

WRIGHT: But at least you're pleasing your wife because you're still in Cambridge, right?

COVERT: I believe so, yes.

WRIGHT: So that's good.

COVERT: Right.

WRIGHT: During the time that you were associate director, NASA formed. Did that have a bearing on what you were doing at the time, or anything that you were doing, did that start becoming an involvement? Could you tell us how you got involved with those aspects?

COVERT: As always, there's always been some interrelationship between NASA and the university community in terms of research and interests. Sometimes it's collegial and sometimes it's competitive, but the interest has been there. At the time that NASA was formed, I was, of course, quite familiar with the old NACA [National Advisory Committee for Aeronautics]. In fact, Dr. [Jerome B.] Hunsaker, who was one of the early founders of that, was still alive, still had an office here, and came in every day, so there was that kind of an awareness.

We were doing some classified work on heat transfer for the Naval Ordnance Laboratory; Naval Weapons Center, I guess it was called, at China Lake at that time. It involves some infrared sensors. So, among other things we did, we tried to see if we could see a hot Sputnik against a cold sky, which turned out to be more of a challenge than we had anticipated, but it worked out. To answer your question, that's what we were doing about the time that the Space Act was passed by Congress. That was about 1958, if I recall.

WRIGHT: What was your reaction to Sputnik?

COVERT: I had done some studies in 1947, I guess it was, which suggested that it was not impossible. This was early in the ballistic missile program. So it was not unreasonable. Of course, about that time or a little earlier, I don't remember the sequence, but NRL [Naval Research Laboratory] had a program to put an artificial satellite in space. It was called Vanguard. To create some interest in this, they had a technical essay contest, so I entered it. You know, what the hell. I'm a student at that time, married, had four kids. Why not? And so that's how I got the money to purchase my first television set, was the prize money from that essay contest.

So, you know, it's not like it was a big shock or anything, but I guess, like everyone else, I was a little bit surprised by it. We were very fortunate, we were able to go out and see it quite early on, and we listened to all the people say, "Why don't we shoot it down?" The answer is, of course, with technology available at that time, you couldn't do that. In fact, you may not be able to do it yet. Anyway, that's my first reaction to it.

WRIGHT: From that interest, of course, as you already mentioned, in '58 we had the Space Act and NASA was formed. You were here, and then your career moved, though. You were no longer associate director.

COVERT: I became a faculty member.

WRIGHT: Were you still involved in research at the time?

COVERT: Oh, yes. It's a question of how you divide your effort. MIT encourages faculty to be involved in research, because I think it makes for better teaching. You're not just isolated and rereading last year's notes; you're doing something. And engineers have to do something, in my view. It's very character-forming to do something, because often it doesn't work right the first time.

WRIGHT: Some of the research you were doing, were you working on some of the early stages of NASA work?

COVERT: Well, no, not directly. My work was mostly at that time funded by the Aeronautical Research Laboratory at Wright Field [Dayton, Ohio], and so I don't know that—some of the outgrowth of that was ultimately funded by NASA, but originally at that time there were two problems. I was seriously concerned. As part of my assignments down at the laboratory, I had designed and supervised the construction and did the calibration and so forth of a transonic wind tunnel, not a very big one, but big enough. And one of the problems that we had was interference between the support that held the model in the wind tunnel and the model. So it seemed to me that it would be possible, indeed possibly practical, to build a configuration of magnetic coils and fields to hold the model in the wind tunnel without any visible means of support. We were able to do that with a great deal of success.

That work ultimately was transferred to Langley, where it, I think, still is. The other thing I was interested in is, as you know, to accelerate a gas to high speeds, in the conventional sense, use a nozzle. It sort of starts out big and then squeezes down and then it gets big again. Well, it turns out that there's a lot of technical problems with that squeezing-down process, and it works well up to certain speeds, let's say mach numbers of 4 or 5 or 6 or even 8, but at higher speeds, the temperatures get so high that there's a lot of difficulty controlling the shape of the nozzle, in practical terms.

So the other thing I was interested in was to see whether or not we could, since I had already hung a model using magnetic fields, could we accelerate a gas to high speeds in a constant area tube rather than one that came down. We worked for a while on that. By, I suppose, 1974 or so, we had enough success so we could show that the thing was feasible and demonstrable.

About that time—in fact, just before then, there was a decision made that the Air Force was not interested in hypersonics, so we sort of did that on the tail of all this, and then threw all the equipment in a scrap heap and stopped.

But anyway, at that time the activities I had with NASA were not great except for some of this interaction. As I say, they took over and actually we built the magnetic balance for them, which they now have down at Langley and they use from time to time. So I didn't get involved with the space program until somewhat later, and that was partly because of the heat transfer work I had done, that sort of thing, and partly about some other things.

WRIGHT: Of course, I'm sure many of your colleagues were involved with the work that was going on with NASA during Apollo days.

COVERT: Well, yes, there was a great deal of effort up here in the Draper Laboratory with the guidance systems. There was a great deal of effort in other places, but we were not at that time. We were much more strongly focused on problems of development of high-speed testing and instrumentation and that sort of thing, so if we ever got there, we'd know what we had.

WRIGHT: Through your many years of expertise, you've served on various advisory boards or chaired others.

COVERT: Right.

WRIGHT: Could you share with us of how you moved into that role?

COVERT: Well, it's fairly simple. I was Air Force chief scientist for several years, and at that time I got involved in the propulsion business, because there were a certain number of problems associated with engine developments. I had, I suppose, a modest bit of good luck to be able to solve some of these problems, and so when Court [Courtland D.] Perkins was president of the National Academy of Engineering—let me back up one step. There have been advisory committees and so forth at NASA and workshops and that sort of thing, mostly related to either education or aerodynamics or aerodynamic testing that I had taken part in over a number of years. So it's not like we didn't know each other; it was more like I was interested in one thing and the guys down in Houston were interested in something else. I knew some of the guys early on, like [Robert R.] Gilruth, and I had met Max [Maxime A.] Faget and some of those folks. I knew who was who.

But anyway, to get back to the point, there was a problem in the development of the Space Shuttle main engine in about the middle seventies or late seventies, and Court Perkins, who was the head of the National Academy of Engineering, was asked by the Senate to form a committee to look into this. Because of the success I had had with the Air Force engine problems, why, he asked me if I would chair this committee and look and see what the nature of the problems were.

So we gathered together some people who seemed knowledgeable about this, went out and looked at the problem. There were two problems, I guess, in my mind. One was technical. It was a very difficult technical problem. Then the other was a problem with resources; it was not being adequately funded, as far as I was concerned.

The engine itself, in order to save weight, was not bolted together the way other engines were built, but it was all welded together, so it was a big deal. The other thing, because the resources were limited, they used the engine as a test stand rather than—and doing bits and pieces to see how they worked, and then redesigning it. But one of the big issues was a very practical one, and that is, I think they burned about—the fuel, 1,000 pounds a second at about 3,000 psi, and if you even have a pinhole leak in this structure where it's welded, you can probably see it for ten miles. Most of the testing was done at night. So rather than being allowed to make mistakes in private and so forth, why, really the development was being done primarily under the scrutiny of the world.

I think one of the things I had to do was to convince Congress, and particularly Mr. Stevenson, that a lot of these problems are normal when you're trying to solve such ambitious propulsion problems. We had some success at that.

Then the other thing that happened was that Alan [M.] Lovelace, who I had worked with in the Air Force off and on, became the associate administrator, later the administrator, I believe, for NASA. So between these two things, I sort of got involved in the Space Shuttle Program, and this continued on for some time. We were fortunate, I think, we were able to make some suggestions that seemed to be helpful. I got a couple of awards from them for that. I can't decide whether they wanted to shut me up or set me off to the side or what.

So anyway, that continued, and I think because of Lovelace I was on the Safety-of-Flight Certification Board for the first four flights, and I was involved in other certification activities which the group made some recommendations, the people at—I suspect it was down at Huntsville, chose to not take seriously, and that's their prerogative. You don't have to take advice that you're given.

Then as this wore on, I was involved, I suppose, somewhere in the neighborhood of nine or ten years, and most of the things I observed and some of the suggestions that I made, or committees made, were accepted and included in a redesign. The pace of all these thing is glacial. It's only been the last few years that some of the things that we suggested in the middle eighties have finally wound their way through the funding agency and so forth.

One interesting that, or at least it's interesting to me, it was at some point, I don't remember when it was, it was clear that things weren't going very well, and I thought that the OMB [Office of Management and Budget] really ought to give us enough money to buy another engine just in case we had another failure. So I'm trying to think of the name of the man who was—it'll occur to me later—the associate administrator for the Space Shuttle Program at that time, a big tall fellow from McDonnell [Aircraft Corporation] that had been involved in the Gemini Program [John F. Yardley]. I told him that I thought we needed more money, and he told me he thought that the President understood how he wanted to spend money and he wasn't going to take any more money. I finally convinced him that if I went down there and got some money, would he accept it, and he grudgingly admitted that they went down there.

So I went down to see the President's Science Advisor about this, and I convinced him that it was meritorious to do this. This was in the [James E. "Jimmy"] Carter administration. The OMB chairman was a man by the name of Beauregard M. Cutter, and Mr. Cutter, at the request of Frank Press, who was the science advisor, came down and said what was going on. We talked about the needs for the new motor. Mr. Cutter's final observation was, "It's only 37 or 38 million dollars. Let's spend it." He also had some other sage advice about how, "All you technicians, if you got into an agency that had more money, all your sandbox playing would never get any scrutiny and you'd never have any problem." That was great advice. It wasn't very helpful, but it was great advice.

So then I had to wander back down Pennsylvania Avenue and point out they're going to get this money and they'd better plan to spend it properly. As luck would have it, why, sometime later, perhaps fifteen months or so, an engine blew up on the stand, and Dr. Press called up and said, "There's just been a fire out there."

I said, "I just heard about it. But you're lucky."

He said, "Why is that?"

I said, "Because the engine that you bought me is going to be delivered next week, so the program is going to move ahead without any material delays." There's always some delay.

Anyway, so that's about how it went. Then, of course, by that time, I guess there were several administrators had come along, and I had gotten acquainted with all of them. I had come down at their request, or go to [Marshall Space Flight Center] Huntsville [Alabama] or go to Rocketdyne [Division of Rockwell International] or Pratt [Pratt & Whitney Division of United Technologies Corp.] or wherever. Of course, because of that, I think, I got invited to be on the Shuttle Challenger Investigation Commission. So that's about it.

WRIGHT: I guess being one of those thirteen on the Rogers Commission [Presidential Commission on the Space Shuttle Challenger Accident] was a little different environment than when you had been before on an advisory committee. Can you share some of the details of how that committee came together and what you all went through to formulate your opinion?

COVERT: There are a number of different ways that these committees work. Some of them, like the Air Force Scientific Advisory Board or that kind of an organization, they're required to report back on what they think of your recommendations of what they're going to do about it. In NASA, you make a recommendation, and it's sort of like dropping a stone into an empty well; you may or may not hear about it, depending on how they feel about that. So this, in some ways, makes it more interesting. In some ways it makes it less interesting.

But I think the key to all of this was that Mr. [William P.] Rogers was able to get between us and Congress and was able to allow us to do the technical work without any interference or help that they wished to give us or to be involved. This is a highly political activity, so it was a shield for that. That worked out very well. It was a good committee. I had known or worked with a substantial fraction of those people, so it went together well.

For example, General [Donald J.] Kutyna I had known when he was a graduate student here and when he and I worked together on the Scientific Advisory Board. And Joe [Joseph F.] Sutter I had known from Boeing. He was the man who built the 747, I believe. So all these threads crossed. There are a couple of people I hadn't known, but for the most part they were people—and I think therefore we were all experienced at this. In fact, Kutyna had just finished safety investigation for an Air Force rocket that had blown up, so he was pretty knowledgeable.

WRIGHT: Was this a continual task for you, day in and day out?

COVERT: I was at that time department head, and I asked my boss if I could do this, the dean of engineer, and he said, "Yeah." I said, "Do I get some relief from my other job?" And he said, "No." So I spent, on the average, about three and a half or four days doing that, and three and a half or four days a week doing the other.

One thing—I don't know if you're interested in this kind of stuff, but one time we got through an early shot on Wednesday, and I called up my wife, I said, "I'm going to come home." She said, "Why don't we go to a play tonight?" I forget what it was. I said, "Sure. I'll meet you at the theater." So I got there, got a cab, got to the theater, and met her in the lobby, and sat down. Suddenly I realized I was feeling for the seatbelt, because it seemed like every time you sat down, you put a seatbelt on. So you got this sort of thing.

But it was a fairly intense sort of thing, and there was a lot of interesting cross-currents going on. Of course, people who might have had some responsibility felt that they might not have wanted to have some responsibility and all that sort of thing. But it was very intense because we had to get done, because of a federal law, within 120 days, because if we didn't get done with it in 120 days, then this would have to be made into a permanent commission, which is what President [Ronald] Reagan did not want, and we didn't want it either. So it was pretty compressed.

WRIGHT: Were you assigned a specific area?

COVERT: Well, they broke us up into teams, with General Kutyna and Richard [P.] Feynman. We did some technical stuff.

WRIGHT: And all the work and all the information that y'all brought together, were you in concurrence with—

COVERT: By and large, we were in concurrence. We actually wrote the report. We had technical editors, but the technical editors didn't seem to understand. We actually wrote it and nit-picked it and edited it ourselves, sitting around the table. Mr. Rogers and Richard were pretty good with words. Sally [K.] Ride is very good with words, as you would expect an English major to be. So it worked out all right.

We had some help with some people from the National Transportation Safety Board and some people from the Astronaut Office and so forth, so you could farm out details. The other thing is that I had a number of former students around in the world, so certain things I would farm out to them in order to cross-check stuff and so forth and so on. Nobody ever said they could do that, but nobody ever said they couldn't either. So that worked out all right.

I guess one of the things that happened, there was a solid rocket stacked out at the Cape that we went down to see, and I went down to see specifically because we wanted to mark a hole where it had burned. Maybe you have seen this picture, maybe you have not, with the big cross-hatching where the hole was. So I'm up there minding my own business and marking this rocket, and a guard came up and was going to arrest me for defacing government property. I finally was able to convince him that he can go ahead and arrest me if he wants to, but I was under the impression I had friends in high places. [Laughter] And he finally reported it. I think Bob [Robert L.] Crippen was the director down there then. Maybe not. Anyway, nothing ever came of it. He was just doing his job, you know, and I'm not putting the knock on him. But I was doing my job, too, and I was going to finish it, which we did.

WRIGHT: At least utilizing your students' expertise helped you because you knew what to expect from them, and I'm sure they knew what you expected from the, so they were able to turn back good information for you to use on the committee.

COVERT: Right. And, of course, you have to be careful to define problems fairly precisely so that they can be solved in a precise way. But the world was very helpful at that time. I guess I'm a curmudgeon, but I didn't feel that the press was particularly helpful, but they had their job to do, too. But I did on several occasions, when I didn't want to talk to them, find a way of eluding them successfully.

WRIGHT: Did you have a first thought when you heard about the Challenger accident, of what you thought might have happened? Or did you withhold your judgment?

COVERT: I've off and on over the years been involved in this kind of thing. One of the things I know is, you don't jump to any conclusions. The way I learned about this was actually that I was on my way to the MIT Faculty Club from a doctor's appointment. They give us a physical every so often, make sure we're warm. You know, nothing's worse than a cold professor.

WRIGHT: It comforts the students, doesn't it.

COVERT: Right. And I met my wife, and we were talking. A man I was in the Navy with, his daughter at that time was an assistant professor in the nuclear engineering department, and she came up to me, all shook up, and she said, "Gee, the Challenger just blew up." And I said, "Oh?" So I said, "Are you sure, Lynn, that this is what happened?" Oh, yes, she was absolutely sure. She had just heard it on the car radio or something like that.

So I went up to the Faculty Club, said goodbye to my wife, and went up. Sure enough, they had some film that they were running. Of course, the television doesn't want a blank screen, and the talking heads don't want silence, so you sort of saw and heard the same thing over and over again, and I watched it very closely. I was reasonably well convinced it was not the Space Shuttle main engines, which, of course, those were the things I had worked on. They were, by and large, fairly tender. So I was concerned about that. I actually was going to have lunch with an alumni. We were going to try to convince him to give us some financial support, but that luncheon was a failure. It didn't work out. We didn't talk much about that. Mostly the thing to talk about was the Space Shuttle Program.

I think a lot of people don't appreciate the Space Shuttle is a much more challenging technical problem than going to the moon was. It's like a lot of other things, it seems to work for a while, so you forget about it, take it for granted.

I was going to say one other thing that we had a serious discussion about in the committee was statistics, which, of course, Feynman is expert and knowledgeable at. I had in the past done some consulting, in fact, for many years with the Hercules Company that builds solid rockets. I suppose that contributed a little bit to their interest in me. We were involved in a program. I was helping them in a program that involved some ammunition, and it turns out that to qualify ammunition, if my memory is correct, which it may not be, seems to me you had to fire 300,000 rounds, and of those 300,000, some very small number, one or two, could misfire and that would be accepted, but if the number was bigger than that, they would not be accepted.

On the other hand, when we were getting the rockets—and I was a little bit involved in that—to be certified as safe for flight, they only fired six of them. I was of the opinion that that was (A), not enough firing; (B), there were some other weaknesses in their procedure. This group that I was on that did the certification, we wanted them to fire a couple of them on a cold day out at Provo [Utah], which they never did.

Anyway, it turns out that Richard was very upset, because he didn't think six was enough either, and he got very agitated about this. But it turns out that the cost of firing 300,000 projectiles from a gun is about the same as the cost of firing six rockets, so one can conclude, perhaps falsely, that the safety is determined by the cost of the amount of things you have to test and not the number that you have to test. But that was an interesting side issue. I think he ultimately wrote an appendix for the report suggesting that they should test more. It's so expensive.

The other thing I think people don't recognize—and I think it's still true—I think the Space Shuttle Program is a flight-test program. It is not like catching a bus on Massachusetts Avenue to go to Harvard Square. And because it's a flight-test program, there are always going to be risks associated with it, and there's no way that can be made absolutely safe, President Reagan's speeches to the contrary. Sooner or later, there's bound to be another accident. It may not cause a loss of life, may not even cause much attention, but these things are not as safe as driving your car, which heaven knows is a pretty risky activity in some areas. But it gets to be routine, and you go along without an accident for a while. You kind of worry. Do people get sloppy? Discipline is a very important thing to maintain among the people when you're working with hazardous materials. I think you'd agree that the Space Shuttle booster system is full of hazardous material. Anyway, that's my personal feeling about that.

WRIGHT: You sound like you have quite a fondness for the Shuttle and even mentioned a few minutes ago that it was much more of a challenging program than the Apollo vehicle. Would you elaborate on that a little bit more and tell us about that?

COVERT: Well, I think it's technically more difficult, because, first of all, you want to use most of the equipment over and over again, you know, just throw it away for one trip.

Second, the environment is more challenging, and that's because you had a lot of time in the Apollo Program to correct small mistakes and make decisions and so on, but Space Shuttle missions are often established very early on by what happens as soon as you get into orbit and the people have tasks to perform. You have to be right. You have to be correct right away. The time lines are tight.

Often the work that's done, at least in the early days, no one had tried to do work under these things. Guys in the Apollo Program didn't do much except put their neck on the line until they got to the moon, and then they did a few hours' worth of stuff and then they came back home. Space Shuttle, these guys are up here six, eight, ten days, and I suppose by now that they're used to it, but everything is a lot different. When you just get out of the environment you're used to, like being on the surface of the earth, things get—are you a hiker? Have you ever climbed a mountain? You get up above 12, 14,000 feet, it's a whole different world, but you've still got the world there. So that's just my view. It represents an advance and an attempt originally to make it be economical, which was lack of success, but the way it works.

WRIGHT: Do you feel the Challenger accident set a new stage or a different stage for the rest of Shuttle and its career?

COVERT: Yes. The answer is they paid more attention to what was going on afterwards than beforehand. It's always easy to be a Monday morning quarterback and look back and say, "Gee, they should have done this," or, "They should have done that, and they didn't." But I think it was in the number-two Challenger flight there was some sign of blow-by from the O-rings, and so because of the design of the seal, the pressurizing system seated one O-ring but tended to unseat the other one. This is technical. I won't get into it. So they decided that they would seat the unseated one by putting a little air pressure in there and driving it to where it was supposed to be after it was assembled.

Well, after they had the first blow-by, they decided that they would increase the pressure to make sure the second one sealed properly, and then they had a couple more blow-bys. So they decided that they would increase the pressure again. Evidence, when you finally got around to it, was that this pressurizing system seemed to correlate with a number of blow-bys, and the higher the pressure, the greater the risk. But for some reason this correlation was missed for one reason or another, and, in fact, following the launches of the Challenger that exploded, they were planning to double the pressure again because the pattern was not clear to them.

These things, as I say, in retrospect, you can all say, "Boy, that's pretty dumb," but the fact of the matter is that if you're in here and you're up to your armpits with this and you've got a lot of other things going on, it's possible to misinterpret things.

One of the other differences was, in my experience with Apollo, which was limited, was that if you had a failure, you had to duplicate the failure on the bench and make sure that you understood what the failure was. Because of the pressures of time and money on the Space Shuttle Program, there was an insufficient amount of this. People would say, "Okay, well, this sounds like the way it works," and everybody would nod their head. So this would be the fix, but it would never necessarily be properly tested.

After the Challenger accident, there was a serious effort made to improve the testing, and they redesigned the joint to try to eliminate this blow-by problem. So technically there were details that were changed because of the accident. I don't know if that's what you were getting at or what.

WRIGHT: While you were working on the commission, you shared with us the physical effect on it and the fact that you sat down in the theater seat where you should be relaxing and reached for a seatbelt that wasn't there. Of course, you were still doing your job here at MIT. How emotionally and mentally taxing was it for you to be doing so much for those 120 days? How did you manage to get it all done and do it all so well?

COVERT: You can live on adrenaline for a certain period of time, and I think that was the way that happened. Fortunately, the end of the 120 days corresponded with the middle of June or something like that, and so summer was here. But there was no formal decompression-type activities that anybody provided.

WRIGHT: Was there a time during that span which was more difficult than others, or almost every day equally as taxing as the one before?

COVERT: Well, that was a long time ago.

WRIGHT: I guess that's the good news.

COVERT: Right.

WRIGHT: Did you go into another advisory position soon after that one, or did you give yourself a break?

COVERT: Well, it seems to me that I just sort of let things keep doing it as they had been done. I think the answer to that is probably yes. When you get out of the—don't do so much technical work, this other stuff is a way of keeping current. I suppose that was the case.

WRIGHT: What's the most difficult part of being on a group with so many of your maybe colleagues or peers or counterparts or from people that you may not have met before that time period? What were the difficulties of getting so many people of your expertise and caliber together to come to a result?

COVERT: The most difficult thing is to get them to arrange their calendars so they're all there at the same time. That's the most difficult thing. Usually the people are interested in solving the problems that are presented to them. Mostly they're people who started out as young technical people and have worked their way up, so they know how groups work. And depending on what's what, you can usually, if you need to break it up, you can find people who have some leadership experience or are knowledgeable in the area that they're knowledgeable in.

Once in a while something contentious comes along, and most of the time, if it's contentious, it's because of the material, the subject matter you're dealing with. There are physical arguments that you can use, that you can rely on to help people realize that they are thinking about something in perhaps a unique way that is not quite the way everybody else sees it. Sometimes the guy goes through this carefully, you say, "Aha! That's right," and sometimes it's not right.

One of the more difficult things is getting consensus on a report, because certain words carry a certain implication, and some people regard different implications and you just have to patiently work your way through those. But maybe I've just been lucky in that kind of problem. I think that, in fact, it's fun sometimes to do these things. I suppose that's why I continue to do it. I do this for the Air Force. It's been a long time since I've done any advisory committee stuff for NASA, but I have done stuff through the National Research Council for NASA recently and for the Air Force.

WRIGHT: Could you give us some examples of what you've done with the National Research Council, with NASA?

COVERT: Well, let's parse it in a different way. In 1983, President Reagan and his advisors decided that aeronautics was a sunset industry; we knew everything we needed to know. As a result of that, a group was formed under the President's Science Advisory Office to review the situation of aeronautical policy, which was run by Jack Steiner [phonetic] of Boeing, and we ultimately put together a book of policy guidance, which had a picture of a big eagle on the front of it. It's called *The Bird Book*. Basically *The Bird Book* said that "Aeronautics is not a sunset industry. Here are some examples of some things that ought to be done. Oh, by the way, aeronautics, commercial transports are the biggest single contributor to the balance of trade, and so we ought to encourage NASA to do as much base technology studies so that these airplanes made, designed, and built in the United States are competitive with the rest of the world or even better than the rest of the world, so they can be sold." That worked for a while, and the aeronautics budget went up for a while.

Then it became contentious again. I ran a committee for National Research Council that led to a report called "Aeronautical Technology for the 21st Century," and the purpose of this was, again, to encourage a greater investment in aeronautics. [NASA Administrator] Mr. [Daniel S.] Goldin was very active in supporting this. This worked out fairly well. But it's sort of like your dog's memory of not sleeping on your new white sofa; he'll remember it the first day or two and then he'll kind of forget it. So I have been involved over the years in this kind of aeronautical policy stuff, helping NASA aeronautics people, not space work necessarily, which is what you're interested in. So we've done that.

We did several studies on Space Shuttle main engine, which obviously is there. We did some studies on the aging aircraft. It looks like these things will last longer than people thought they would. And are they safe? The answer is, if properly maintained, they are safe.

So it involves that kind of stuff with the National Research Council. I've done some similar stuff for the Air Force Scientific Advisory Board as well as other kinds of studies for them.

WRIGHT: How important do you think it is for both industries, NASA and the industries that are related to them, to have these types of advisory councils?

COVERT: Well, I think that they are probably useful, because people tend to get inbred in their thinking, and I think this tends to stretch their minds a little bit. Industry has their own kinds of problems. They have to make an airplane that is reasonably cheap and that lives up to the buyers' expectations, and they have to make money on it. So they tend to have a relatively short time perspective.

NASA, on the other hand—and this is more evident, probably, in the space program—has to have a long-time perspective, and so there is a disjoint between what the industry thinks it wants and what they probably need in order, ten years from now, to be more effective. This is part of the give and take of it. I think it's even more evident in the space program because some of these space probes launched might take ten years to get to their target and do what they're supposed to do. It requires a difference in thinking about things and what kind of a perspective you have. So you have to kind of weld this together. I think advisory groups often can make a useful function that way, too, not only stretching people's minds and getting them to look outside their own playpen, but also to keep in mind there are these cultural differences that exist. I think that's probably the two important aspects of advisory work.

WRIGHT: What about the future? Have you worked on councils that are looking to the future of aeronautics and space industry?

COVERT: The answer is yes. In fact, we're in the middle of one now. That's what that phone call I was making, the guy's beeper number, was all about when you arrived.

WRIGHT: Can you share any information with us?

COVERT: It's premature.

WRIGHT: So you're looking ahead, and all that past knowledge, I'm sure, plays a big effort in making sure everything is in line for what we're all looking into.

COVERT: Well, I don't know. That's a hard—actually, that's a sentence, not a question.

WRIGHT: Right. Is the technology changing, though, so quickly in this field that it's hard to predict or hard to plan for the future?

COVERT: I think we know what the important problems are. I am not sure that we necessarily know how to solve them at this point. You have to look at a variety of approaches and hope that one will work and not be disappointed that the others don't. This is a very important thing. The other thing that it's important to realize is your own tendency to focus too narrowly, because in engineering you can have multiple solutions to the same design problem. They're all equally effective. So you have to make sure that you don't select yours just because it's yours. There is a little bit of "not invented here" feeling about that. I think, for the most part, we can define the problem areas broadly, and that's all you really need, that and I guess it's an act of faith, you know. You think it's going to work, and you have to work on it.

WRIGHT: You're definitely challenging the future, and your past years have been full. Has there been a time that you can remember that was probably the most challenging milestone that you had to deal with before you could move on to the next step?

COVERT: I don't think it's possible—

WRIGHT: To find one?

COVERT: —to define it in those terms.

WRIGHT: Is it also difficult for you to find the most significant one that you might have accomplished, or is there something that you feel very glad you were able to participate in?

COVERT: The answer, I think, is it's hard to grade your own paper, and it's hard to know what you've done that twenty-five years from now will turn out to be important. Often what people think is important in the short term turns out to be unimportant in the long term, and often the thing since you don't think much about it in the short term leads somebody to think about something else, and so it's sort of suddenly your technical grandson or great-grandson, or at least the technical idea that's traced back like that, turns out to be very important.

I suppose from the public's standpoint, the investigation of the Challenger accident was probably something that was very interesting and probably turned out to save some lives in the long run. I don't know. It's a hard question to ask. It's actually an easy question to ask.

I think that the other thing is that there are people with different personalities will react to these things, these kind of questions, differently, I think. Some people are more lacking in humility than others and they're inclined to put their own efforts in a somewhat inflated package, and others are less likely to do so, perhaps because they're aware of the frailties of human nature. I can't answer that any further. I'm not a philosopher either.

WRIGHT: You've worked with so many people through all these years. Those that you met, were there some that you remember of having such a significant impact on how you thought or helped formulate what you thought later?

COVERT: I think one always has people who—I think of a couple of the teachers I had in the past were very informative. I think that there were people that I worked with. I mentioned Court Perkins, for example, had an important impact on how I looked at things. It's hard not to learn from everybody you work with there.

It's so very different that each person approaches things differently, and sometimes you think you have a great idea and it turns out that you go and push on this and it's maybe a great idea, but it's not time for it. As an example of this, towards the waning days of the Challenger investigation, it seemed to me they really ought to set up a corner of the FAA [Federal Aviation Administration] to look at how you're going to manage space transportation and how are you going to do certification, because sooner or later that ought to look a little bit like the airplane business. So I trudged myself over to FAA and got to see Admiral [Donald D.] Engen, who was at that time administrator of the FAA, a very interesting guy. He initially did not think much of this, but ultimately he came to agree, except that he felt it wasn't timely. Then it was very useful for me to hear his arguments on why it wasn't timely. Of course, ultimately he's become the head of the National Air and Space Museum, so we have different kinds of conversations now. But I'm using him as a surrogate for many, many people that I've worked with over the years. I can give you a long list and then I'd leave somebody out, and they'd get mad at me because I left them out.

WRIGHT: We don't want that to happen.

COVERT: That's right. But I think that whenever you work with people, you learn something from them, even from the students. So I would prefer not to name any specific names, but I want you to understand I am beholden to probably thousands of people who helped teach me things.

WRIGHT: All those students that you have and had, were they aware of all your involvement in all these different areas when they'd take your classes?

COVERT: Some were and some weren't. I tried to arrange things so I never missed a class. However, I often would arrange things so that I would be out of town while they had the examination. Once in a while you couldn't do that. Of course, when I was department head during Challenger investigation, I decided I couldn't teach anyway. It takes me too long to write lectures. So at that time I was not teaching at all anyway, so it didn't make any difference. I had good people working for me in the department, so I could wander off and they probably would never miss me until a signature was needed or something.

WRIGHT: Sometimes an important signature.

COVERT: Possibly, yes. Payrolls are important.

WRIGHT: That's what I was thinking of, yes. [Laughter] So many firsts have happened in the space industry while you have been alive, and so many of those you've touched one way or the other, especially in those last twenty, thirty years.

WRIGHT: When you first started, did you have any idea that the short time you were going to spend at MIT would develop into this long career with those—

COVERT: No. At the time I was working as a designer, and I figured that supersonic flight was going to be important, that I'd come here for a year or so and learn something about supersonic flight, then go back and be a designer and see what happened, but it didn't work out that way. It's been very interesting. One of the things, of course, is that the young people are interested in new things, so that you can't help but be forced to think about new things in a much more continuous way than if you didn't have students around. They help keep your mind open about things. So that's a great plus for being in the university environment.

As far as—well, I've seen some very interesting—I don't know. In the question you ask, one of the early wind tunnel tests I did was associated, when I first came to MIT, was with McDonnell Company-sponsored twin jet airplane, and then later on that turned out to be the F-4 [Phantom], which was one of the most—I think more of those have been built than any other fighter plane in the world, probably, around 5,000 or more. So it's kind of interesting to see that. It's always fun to see something you've worked on fly. I don't care whether it's a Space Shuttle or the Apollo or the B-1 [Lancer] or the B-2 [Spirit] or the F-15 [Eagle], it's always fun. You say, "You know, I've got a thumbprint on that." But I think if you haven't done it, you can't appreciate it, I suppose. Maybe you can. It's hard to know.

WRIGHT: In all those travels, were you able to go to the launches to see?

COVERT: I have been to launches, yes. I can arrange that.

WRIGHT: Good.

COVERT: I haven't been to one recently. I was at the first Space Shuttle launch, then the second.

WRIGHT: It must have been a special thrill for you to see those engines.

COVERT: Well, I think in one sense it was more of a relief, because you sit around the table and Lovelace would say, "Is it safe to go?" And you'd say, "Yes, let's go." Then you thought about all the things that go wrong, and hope that none of them do. So it's exciting, but also it's a little bit of a relief. I mean, there are people in there, after all. That goes with the territory.

WRIGHT: And that was from the first time you saw a Shuttle launch to the first time that you saw—or the only time when you watched Sputnik that night, so much had happened in between that time.

COVERT: Well, that's right. I think we live in a time when we take for granted that there is change. I don't think that people, by and large, realize that this century is different from other centuries. My father died at eighty-eight and he had seen the electric lights come, the television come, he'd seen the steam replaced by diesel and locomotive. He saw the airplane come. He saw television come. You know, these things just keep coming. I don't think most people worry about it.

The year that they landed, in 1969, on the moon, I was teaching summer school, and so one of the students complained about the quality of the television pictures that were coming back. So I gave him a homework problem to calculate how much the frequency of the signal was shifting when the moon was going away and when it was coming back. Normally, you see, when you get a television picture, it's stationary, effectively, and you're stationary, so the frequency that the antenna has to be tuned for, both the transmitting and the receiving are the same. In this case now, there's enough of a time lag and the velocities are such that you have to allow for that in your signal processing. It was a good homework problem. We had a lot of fun with it. So we got done with this, and they decided that they really ought to do color.

I don't think—and when I say this, I don't want to appear to have thought of something no one else thought of, but I think that most people watched this on their television set and they didn't realize the level of difficulty that was involved in doing this. This was you turn on the damn television and it's there, you know. What the hell. So I think to answer your question, I didn't worry about it. It was just part of the job, you know. It's part of the fun.

WRIGHT: It sounds like you were able to have quite a fun time learning everything that you have all the years that you have been in this business, especially, like you said, from your students and from all the folks that you've met.

COVERT: Right. I think so. Maybe we accomplished something; we don't know.

WRIGHT: I was going to ask Carol if she had any questions for you.

BUTLER: I do. I have a few.

COVERT: That's a—

BUTLER: This is mostly notes. Going back—and my questions will probably jump around a little bit.

COVERT: That's all right. I jumped around.

BUTLER: Going back, you mentioned that you worked on an essay tied in with the Vanguard.

COVERT: What was your essay specifically about, if you remember details of that?

COVERT: It seemed to me it was something relatively elementary. I think that what I was doing was comparing the energy that something had in orbit, when you work it against gravity, to the energy that if you dug a hole straight down to the center of the earth, would you accumulate the same amount of energy going inward, falling, as you had to put into it to get out, to get into orbit. Something like that.

BUTLER: Interesting.

COVERT: Well, yes. Thank you.

WRIGHT: That was worth a television set.

COVERT: That was worth a television set.

BUTLER: You talked about your work on the Space Shuttle main engines and that it was a relief to see them go up and that everything was okay. Were you involved at all when there was the one flight that ended up having the abort to orbit after some problems?

COVERT: I was involved in the investigation after that, yes. That was a gutsy thing to do, but it was the right thing to do.

BUTLER: Had that been something that in your early studies on the main engines that you had looked at? When you then looked at the problem after the flight, did you—

COVERT: Well, you sort of try to think about all these things. The real problems you have are not the problems you anticipate. The real problems you have are something dumb that you don't anticipate, that leaps up and bites you in the arm or someplace. So we worried about these various things. If I recall correctly, that was a decision was whether a sensor was working correctly or not. That was the basis of this. The reason that finally the controller on the ground decided, based on several other pieces of information, that it was possibly the sensor and therefore let's go for it, as opposed to the fact that if all the other information had been negative, why, then they would have had to not do that. But most of these things you work out.

The other thing you have to do is, you have to practice all kinds of terrorizing situations, shall I say, and the reason you have to practice all these things is not so much that the ones you're practicing will come up, they may, but it's the idea of problem-solving on the fly. Don't be paralyzed by these things. So that's a very important thing, is to have these rehearsals and these rehearsals and have somebody intentionally screw something up. Then you have to work your way through it. So sooner or later, somebody has to make a decision that is a difficult one to make, but if you had enough practice at this, at least you understand the risks that are involved, and you have a lot of help from a lot of people, some of which is real help and some of which is not much help at all. But the practicing is important. I don't know if that's clear or not.

BUTLER: I think it's proved out in the flights, in that most of them have been so successful.

COVERT: That's right.

BUTLER: Practice pays off.

COVERT: I think so. In fact, that's one of the reasons it's so expensive, because you have all these people. The question is, how can you do this without all these people? Of course, the computer wizards say that's easy; you just have a computer. If you have a computer at home, you're not sure it's that easy after all.

BUTLER: Yes, the computer's only as smart as the person who tells it—

COVERT: Right. And therefore, but only as smart as the person who tells it really is and not as smart as they think they are. In my experience, the computer is great as long as everything is going just exactly the way—you checked in to come in here on the airline and everything was fine. But there have been times you got on an airplane to go somewhere and things weren't fine. Then things really screwed up. That's the difficultly with the smart person making the codes. They've got to anticipate some of these other things.

BUTLER: Absolutely.

COVERT: And they don't always do it well. Grumble, grumble. What's your next question, Carol?

BUTLER: You mentioned hearing about the Challenger accident and then going up and watching it on television. But when were you actually invited to be on the commission? How did that come about?

COVERT: I guess what happened was, Saturday night about 11:30, I got a telephone call from Bill Graham, who was the President's science advisor at that point. He said that the President wanted to form a commission and they wanted me to be on it. I said, "Well, I'm honored to serve, but it's 11:30 at night and I don't know what my boss is going to say," and all this, so I said, "Let me know how much time it's going to take, and then I'll talk to him and we'll see what happens." But I said, yes, I would be glad to cooperate.

I didn't hear. He was going to call me back the next day, on Sunday. He didn't call, he didn't call. So finally I didn't think much about it. Monday morning was something called Engineering Council, where the dean and all the department heads get together, and you confess the sins you've committed and you confess the sins you should have committed, and all that. This particular session, I don't remember how long this meeting—but it was a long meeting.

I got back to my office about one o'clock, I suppose, and I just sat down and decided what I was going to do about lunch, and the telephone started jumping off the hook, and it was reporters telling me that I was on this thing, and what was I going to do about this, so forth, and how long was it going to be until the next flight, all kinds of questions that to the reporters were eminently reasonable, but, of course, you can't answer that kind of question at this point. Finally the phone stopped ringing, and a couple of television teams showed up. I don't think I ever ate lunch that day. But that's how I learned about it.

But meanwhile, I had a very important commitment for the next day, and so I didn't arrive in Washington until a day later. Meanwhile, they had already had one session. It worked out.

BUTLER: As you were going through, you mentioned, of course, the media was wanting answers right away, but as the people you were working with at NASA or with the contractors, as you would talk with them and trying to find answers, did you find people were open and trying to help solve the problem, or was there some reservations?

COVERT: I think it depends on the person and on the situation. The answer is yes, there were reservations, particularly—I had a very vigorous argument with Mr. Rogers, who I knew had been the Secretary of State for a while, very successfully, as a matter of fact, but I didn't know anything more about his background. My experience in accident investigations and so forth was that you really didn't need a lot of publicity, and that the best thing you could do was just kind of work quietly and collect all the information. Part of the reason that you do this is because in accident investigation in aeronautics and astronautics, the important thing is not to put blame. The lawyers like to say, "You're guilty." But what you really want to do is find out what went wrong and then take those steps to try to make it less likely to happen again. That's the important thing.

You can't find out what went wrong if people are trying to minimize their part in this, whatever the disaster is. In fact, it's well known, I think, that if you're a policeman and you want to find out what happened in a traffic accident, the faster you talk to the people before they've had time to make a good, coherent story, the more likely—or let's say the sooner, not the faster—more likely you're going to get what really happened.

It turned out that Mr. Rogers had been the chief counsel for the investigation of the assassination of President [John F.] Kennedy, and at that time they operated on the rule that they would work quietly and then make the announcement. As a consequence, he was very much bothered by the fact that there were lots of books and lots of conspiracy theories and things like that. He was bound and determined that that wasn't going to happen in this investigation, and the way that he was going to preclude this from happening would be that he would have to have this done more or less in the open. And it was done more or less in the open. I think in the long run he was correct, but it did lead to the fact that there were people who were going to be interviewed on television, who were uncomfortable because they might have the feeling that they would be judged as guilty, which probably there were, in a legal sense, as opposed to finding out what went wrong.

I found it useful to go in and talk to them beforehand and tell them what information I had and what questions I would have to ask them, to help them to understand what was going on, and also to kind of help them to keep from making up some kind of—fairy tale is too strong a word, but you know what I mean.

So to get back to your question, then, when you'd go and talk to people, why, you'd have to often try to see things from their viewpoint and ask questions that would allow you to find out what happened, without making them feel that you were going to say, "You're guilty." That was the thing we tried to do. Sometimes it was successful and sometimes it wasn't successful, and sometimes it was much more successful than planned, because sometimes you would ask a question in just the right way, then the whole thing would suddenly tumble out from people.

This was how the Roger Boisjoly conversation started, was I asked a simple question about the mechanics of this thing, and that kind of triggered a discussion which triggered the whole thing that, in fact, there were people who were concerned about this at Thiokol long before anything happened. So you never know how these things are going to go. Of course, if I did something dumb, I suppose, and if there was an accident and the television was on me and the whole world was looking on me, I would hope I would have the courage to use active verbs, but, you know, you may end up doing passive verbs. I don't know if that makes sense or not.

WRIGHT: Yes, it does.

BUTLER: Based on your background and experience, what do you think about the transition of the Shuttle into the commercial realm? Now it's going over to USA, United Space Alliance, is doing a lot of the Shuttle work now, and there's talk about what will happen for the future. What are your thoughts on that?

COVERT: I have long been of the opinion—in fact, I have written that I did not think that NASA, as an organization, is well set up to be an operator of an activity of this kind. I think that it's primarily a research outfit or maybe research and some development, but it's not primarily an operation outfit. So I think that operations are expensive and they take a lot of money, and this money detracts from some of the other activities that the agency ought to be involved in. So I don't think that this is necessarily bad at all. I've been arguing for a long time that this may be the way to go. I don't know.

But the question always is the question of formulating the agreement properly, getting the right kind of discipline and control up and down, making sure it's safe. But the airlines work pretty well, and I haven't seen any of the details of this negotiation, so I don't know what's going on, so I can't answer the question in any kind of detail you'd like, I'm sure.

BUTLER: No, that's fine. Looking at also your background with the Shuttle and the early phases of watching it come through development and now as it's growing and aging, do you foresee the Shuttle continuing on for a while with updates?

COVERT: I think in today's budgetary environment, that's the best you can hope for. Unlike airlines or the Air Force, I don't know what steps NASA has taken in terms of inspection or in terms of maintenance processes. It seems to me the thing was designed so that each Shuttle would be good for fifty-five flights. Is that correct?

BUTLER: I think it may be that or it may even be 100. I can't remember. Several.

COVERT: Yes, several. So then there must—well, let me back off and go to a little different point of view and then I'll get back to this. On an airplane, what you do is you subject it to what's called fatigue testing. So you take this airplane and you hang it from some springs or something and you load it in the same sequence that you would get if you were flying it. You know, for example, in the engine business we use something called accelerating mission testing, and the purpose is that part of the flight doesn't contribute to the wear, so you just take those parts that contribute to the wear and you keep applying these loads to the airplane or the engine to make it think that it's flying. Then you stop every so often, then you inspect. You know those regions, by and large, where you think the local stress is high enough to be potentially a problem, and you keep inspecting those places. They're called hot spots, for obvious reasons.

Then if you have one that develops before the life of the airplane is complete, then you figure out a way of repairing it so that when the airplane actually goes into service, that hot spot has been alleviated as a hot spot. Or if you can't repair it readily, at least it's something you can inspect regularly and compare the progress of this damage, whatever it is, like a little crack, with what was observed in the fatigue testing, with the shaking of the thing. So this way you have an idea of knowing where to look for problems and how often to look for problems and so forth.

In the Air Force, for example, they determine what's called the critical crack length, and this is a length that is such that if you don't detect it during one inspection interval, it will not grow to a catastrophic length in the next inspection interval. This is how you set the inspection intervals.

Now I'm getting to your question. I do not know how they are managing this flight vehicle. I don't know that they ever did fatigue tests on it or not. I don't know how they are inspecting it. There must be some procedure that they're having. If this procedure is done properly, the air frame itself should have an infinite life, and the only thing you need to do is change the electronics as they get old. The engines are swapped out very frequently and overhauled, because they are, as I say, a little tender. I think the tiles are replaced from time to time. They usually lose a few. But the heart of the thing, I don't know how they manage that, so I can't answer. But I'm sure they have some kind of a management policy.

On the other hand, I don't think—let's see. First flight was in '81. Or was it '82, '84?

BUTLER: '81.

COVERT: This is '99. So that's eighteen years, and they probably fly twice a year, so it's a long ways from fifty-five flights yet on the average. So maybe they don't even worry much about it at this point. I don't know. I guess what I've said is, I can't answer your question. [Laughter]

BUTLER: No, you gave a reasoned background for it, and I'm sure they do a similar process of what you explained.

COVERT: Well, we hope so. I don't assume anything anymore.

BUTLER: One final question, as long as it doesn't overlap into what you're—you said you are working on a committee now, but it's preliminary yet to talk about. But just your personal thoughts on the future directions of space flight and aeronautics even might take, if you can.

COVERT: Well, I think that the important part of the Space Station Program is to find out what people can do effectively in that environment and to find out what they cannot do effectively in that environment. Because we're still at the stage of the development of the space business, we don't know. We don't know how long you can keep people in space safely. That's a contentious point. So we have to develop a whole pile of procedures and databases, if I can use that appalling term, that allow us to know what can be done, what to expect, what can't be done, what can be done only at great risk for the future.

A lot of people say, "Gee, you know, the next step is to Mars." Well, I don't know. Is the next step Mars or should we see whether or not it's worthwhile to build a little outpost on the moon? If you think back—you're both too young, but in the late forties there was talk about Antarctica and how do we go about this. Well, the first people just visited for a while. Then the next thing, they built a permanent station, and a few people wintered over. Of course, Admiral Byrd had done that by himself in the thirties. So you evolve this way.

I'm more in favor of a little bit of evolution. I'm not sure that if we could send somebody. I'm sure we could. The question is, would people pay for it, send somebody to Mars. The guy comes back, would he be able to walk out of the spacecraft? Would he need to be hauled out? We need to solve all these sorts of things. I think in solving these problems, if there are things that turn out to be an economic consequence, I think they will just sort of happen. I'm not sure at this point. All these guys have all these ideas about making money. That's nice for them. But I don't think we understand the whole, shall I say, system. And that's what it is; it's a system problem. You have to have transportation to get there. You have to have people alive to do what needs to be done. Whatever you do, if it's going to make money, it has to come back here and you have to sell it to somebody. You have to sell it at a price that they can afford to pay for it.

So I think that right now we're still in the exploration stages, and I think this is the right way to go. I think all kinds of interesting things are bound to come out of this, so sometimes I wish I were about eighteen, to see all these things happen. Was that the sort of thing you wanted?

BUTLER: Absolutely. I suspect you'll probably see at least several of these things progressively growing.

COVERT: We'll see.

WRIGHT: Part of that, you mentioned the economics of it. That was probably the one group we haven't talked about that you go to talk to on occasion, was the congressional members.

COVERT: Right.

WRIGHT: Was that a good experience for you?

COVERT: Well, it's always a—I find that—how do I want to put this? I find testifying before Congress in the morning is, in my view, always a challenging thing because you have to be so careful about what you say. They have a lot of their own agendas, some of which you're aware of and some of which you aren't aware of. You certainly don't want to mislead them in any way or shape or form. You try to answer their questions in as straightforward a way as possible, and you sort of do that. But you get into funny things. They definitely are the masters. They know they are the masters, and they want you to understand that they are the masters.

For example, a friend of mine was down testifying on behalf of the Small Business—SBIR, whatever they're called, Small Business Innovative Research. One of the congressmen or the senators was pressing him on the fact that there was hardly any of this work that was being done in his state compared to Massachusetts. But this state has several large industries, and so my friend suggested to them that perhaps the question is not why there are not any SBIRs coming out of the state; the better question is, why is the industry there not fostering this kind of thing. The guy told him, "That's your question. You find the answer. I'm not interested in it." So this particular man had a bone to pick with the SBIR program, and he wasn't going to be deflected from this.

I once heard a guy complain that he was behind on his schedule, and the Congress was talking about him. In a moment of weakness he suggested that the Rayburn Office Building was quite far behind in its schedule as well, and it was being supervised by this particular committee. The congressman said to him, "I'm interested in your behinds; I'm not interested in my behind." [Laughter] So they have that right, I suppose. I feel that it's too easy to say the wrong things, so I find it a very stressful situation.

WRIGHT: Were the occasions many that you were in D.C. to talk to the—

COVERT: I've done this on occasion, yes, whenever asked. I don't make a point of going down there and asking to do it.

WRIGHT: One of the most recent times, I guess, was '93, you spoke to a House Subcommittee on Space and Aeronautics.

COVERT: Right.

WRIGHT: Was this an invitation as well?

COVERT: This was the outcome of the study that I was talking about, "Aeronautics in the 21st Century." They invited me down there, so I went down and talked to them.

WRIGHT: And how were your plans and your ideas received?

COVERT: I think that, by and large, they were receptive. As I said, Mr. Goldin was receptive. The aeronautics budget was increased as a consequence of that, so you can't knock that.

WRIGHT: No. They may be inviting you more often. Or maybe you'll be suggested to go down and talk more often, anyway.

COVERT: If they do, they do.

WRIGHT: Your work here is officially—you're officially retired, but, of course, you spend your time at MIT. Do you have lots of projects that you'll be working on in the future or do you just take them as they come?

COVERT: Well, I have one doctoral student that I am still working with closely, and then I am in charge of the wind tunnel. You can look out the window and see that rusting thing over there. And I'm in charge of that. So we get a number of interesting things to do that are related to that. My experience—let me put it this way. I've often encountered people who said, "Gee, I've never been so busy in my life since I retired." I don't know whether that's true or not, but I know in my own case I'm slower now, I make more mistakes, it takes longer to do things right. So it just seems like I'm busier, actually. I don't know that I am or not. I probably am not, actually, but it seems that way. That's probably the natural way that it is.

WRIGHT: So much of what you've been able to accomplish seemed to result from so many talents that meshed together—your teaching skills and your research skills, your advisory talents. Was there one area more than others that you enjoyed doing?

COVERT: That's a leading question if I ever heard one. Actually, I think that it's a good balance that I was lucky enough to maintain. I think another thing that was fairly important in all this is I think it was important that I started out as a young squirt after I got out of the Navy, working as a designer, because that forces you to look at the big picture. I think that all these things that you've just described are all part of an overall picture. If I can maintain, which I mostly did, the right balance between enough research and enough teaching and enough outside activities, whether it's consulting or advisory work or being on a board of directors or whatever, you have to maintain the right balance. These things all complement each other, and one of them doesn't overwhelm you. But that requires kind of watching things and once in a while saying no, no matter how interesting it is.

Of course, the students are very important, and so I, as a matter of routine, leave my door open, because I have seen a student standing outside a closed door, trying to decide whether he should knock or not, you know. But if the door's open and you walk by, there's no excuse. For anybody at a university, ought to have first priority, but the others, it balances. At least I try to make it balance.

There's a fourth part of this that you didn't mention, and that's family. You have to also include that in the balance. Sometimes that works better than others. One of my daughters claimed I was never home on her birthday. That's not true, but she felt that I wasn't there. But that's what it is. It all works together.

WRIGHT: I think that's a good note for us to end on, that balance is important in whatever we do.

COVERT: It's important in engineering design, it's important in space, it's important in life. So, okay.

WRIGHT: We certainly thank you for your time. Is there anything else that you'd like to add before we close?

COVERT: No, but I'll think of it at two o'clock in the morning and I'll call you up.

WRIGHT: You do that. You do that, and we'll turn the recorder on if we can find it. [Laughter] We thank you again.

COVERT: You're quite welcome. I hope you find it helpful or interesting or whatever—

WRIGHT: All the above.

[COVERT: I have thought of a couple of other interesting projects I worked on for the space program. One was the preparation for the Hubble Telescope repair mission. The Hubble Space Telescope was designed to be maintained by astronauts using the Space Shuttle as a base of operations. Hence, it has hand and foot rests and tools were designed to make the job as simple as practical. One problem was when to schedule the mission(s), and the other was to decide whether or not the necessary repairs and maintenance could be completed in one mission. The latter issue arose because the operating life of some of parts was shorter than planned, and because the correction to the mis-ground primary lens was to be installed. If I recall correctly the group reached the decision that one mission would be adequate, based upon an earlier outcome from training exercises. The other recommendation was that the mission be delayed from November to the following February, because some parts were not available until then. The crux of the matter was whether or not the parts that needed unexpected replacement would last the extra three months. These recommendations were accepted. The repair mission was flown in February, and the tasks were completed successfully in one mission.

The other was stimulated by a mission to capture a satellite that failed to go into a proper orbit. The mission finally was completed successfully even though the tool designed to "grab" the satellite failed to work properly. But the 'rescue' involved man-handling the satellite by astronauts literally by shoving the satellite around more or less the way a mover deals with a heavy, bulky piece of furniture. This was very risky, and while it ended well, Mr. Goldin thought perhaps the matter should be thought through carefully and some guidelines proposed to assist decision-makers if such a rescue effort was to be undertaken in the future. The general recommendation was to limit such rescues particularly if the satellite was not designed to be retrieved and thus lacks the proper grabbing points. If I recall correctly, such rescues were to be considered favorably if and only if the satellite was part of a military program, and its retrieval by a third party could compromise national policy. A third possibility was that if a commercial satellite was to be rescued, the owner would be asked to pay the full cost of the shuttle mission and a fraction of the income earned by the owner. The hope was that this would discourage putting the crews at risk for a third party's gain.

There was an interesting technical problem associated with the failure of the "grabbing" tool to work properly in space. The astronauts trained for use of this device and for this mission through simulation of zero gravity in the NASA Marshall Space Flight Center Neutral Buoyancy Facility. This facility had been used successfully in the past, so the failure was a surprise. It turned out that the cause of the failure of the Neutral Buoyancy Facility to simulate the grabbing process was due to a little known fluid mechanical phenomenon called "Virtual Mass." In other words, at the instant of grabber-latching the water in the facility provided just enough resistance that it held the satellite stationary enough during the instant needed for the grabber to latch onto the satellite successfully. In space the grabber just nudged the satellite away from the latch. We were able to show this was the case on a large air-bearing table at Houston, which simulated the process quite well. The agreement between the satellite velocity deduced from motion pictures taken in space, and that from the air bearing table, and calculations based upon the impulse given to the satellite during the grabbing process were quite satisfactory.]

[End of interview]