# ORAL HISTORY TRANSCRIPT

EDGAR M. CORTRIGHT
INTERVIEWED BY RICH DINKEL
HAMPTON, VIRGINIA – 20 AUGUST 1998

The following interview of Dr. Edgar Cortright was conducted on August 20, 1998, Langley Research Center in Virginia.

DINKEL: Dr. Cortright, let's start today with talking to you about you yourself as a person. We know your birth date and the place and everything, but what about your life, prior to Lewis? How about high school—sports, organizations? What can you tell us about high school? Pennsylvania, if I remember correctly.

CORTRIGHT: … I was born in a coal mining district in western Pennsylvania, moved to the Philadelphia area, went to high school there, a small high school. I was in golf, basketball, and miscellaneous sports. Not only lettered sports…

DINKEL: What about some significant events? Any significant about the high school years? The war was rolling up about that time, I guess.

CORTRIGHT: Well, I always knew, from about age five on, what I wanted to be, because my father flew in World War I. I used to put his uniform on as a little kid, and I decided I wanted to be in aviation. So I was sort of directed in high school. I had a lot of activities and all that, but I didn't flounder around as to what I wanted to do and be. That's sort of was the way I was in high school.

DINKEL: Where did you go right after high school?

CORTRIGHT: I got a scholarship to Rensselaer Polytechnic, went there, took aeronautical engineering, met my wife, married her. Oh, I enlisted in the NROTC before Pearl Harbor. My father talked me into that. He said, "There's going to be a war. You better get involved early." So I enlisted in September of '41, and Pearl Harbor was December of '41. And then I was pulled out in my junior year, because they had run short of ensigns, and assigned to the *Saratoga*.

DINKEL: As ship's company?

CORTRIGHT: Yes. Well, I had a dual commission. I was aviation/deck. I worked the flight deck crews, and I also stood officer-of-the-deck watches on the bridge.

I had met my wife before I was called up to active duty and married her on my first leave, married her in '45. We've been married for fifty-three years.

DINKEL: Congratulations.

CORTRIGHT: That's a long time, isn't it?

DINKEL: A very long time, yes, sir. Well, tell me about the *Saratoga*. Was that basically the ship you served on for the whole time?

CORTRIGHT: … I [boarded] in the Indian Ocean, and we went up and raided Surabaya and Sabang to hit oil depots. I never heard a shot fired or saw an enemy aircraft, and we came right home. Actually, I didn't get married that leave. We went back out, participated in raids on Tokyo and supported Iwo Jima. [At] Iwo Jima we were kamikazed heavily by five planes and a couple of bombs. We took a lot of casualties and a lot of damage and went back for repairs, and then I got married on that trip.

DINKEL: At Pearl?

CORTRIGHT: No, we went to Bremerton for repair. It was too [damaged] to do at Pearl. The ship was pretty wrecked. So I went home and got married and brought my wife back out to Bremerton and then down to San Francisco, and then we went out and she went home.

DINKEL: Was that in '44, probably?

CORTRIGHT: … [No—'45]

DINKEL: It was all wound down by that time, wasn't it?

CORTRIGHT: [Not quite.] Our ship was not very maneuverable, so they … assigned us … to training air crews in Hawaii, and then when the war ended, we hauled servicemen back … from Hawaii to the West Coast.

Then I was assigned to a Naval Air Modification Unit [NAMU] in Johnsville. I ended up a Program Manager … putting turbo superchargers into F-4Us. And then I got out and went back to school and finished my education.

DINKEL: At the hangars there at Johnsville, which then became a war minster.

CORTRIGHT: Oh, you've been there?

DINKEL: Yes, sir. In fact, I lived there. When I retired from the Marine Corps, I worked at the Naval Air Development Center [NADC], which is now closed, all except for the centrifuge.

CORTRIGHT: Well, that was the same as NAMU [Naval Air Modification Unit]. It became NADC [Naval Air Development Center].

DINKEL: Yes, sir, it did.

CORTRIGHT: I was there for less than half a year, as I recall. I think I went there in the spring and went back to school in the fall, and then I got my graduate degrees.

DINKEL: I was always amazed at the Brewster Aircraft Company hangar that's still there today—they haven't wrecked it—where they actually made the Brewster Buffalos, quite an airplane.

CORTRIGHT: They flew like a buffalo. Their dive-bomber wasn't any better. I [don't] think the Brewster Buffalo ever shot down anything. It was so busy getting shot down itself.

DINKEL: The Marines flew them at Wake, if you remember, and didn't do well.

CORTRIGHT: Did they do anything?

DINKEL: No, sir, I don't think so.

CORTRIGHT: It just was underpowered. I always liked the looks of it. It looked like it would be fun to fly that airplane, but it wasn't.

DINKEL: I guess I knew too much about it to think I wanted to fly it. It was before my time.

So after the war, up to Rensselaer to finish your degree, then?

CORTRIGHT: I got a master's degree and then went to work in NACA Lewis as an aerodynamicist and did propulsion aerodynamics for most of my ten years out there. At the end, I took a nuclear engineering course and got into plasma propulsion… And then Abe Silverstein took a few of us down to Washington to help set up NASA.

DINKEL: You're getting way ahead of me. That's right where I was going to go.

I see you went back again in '75 for your Ph.D.

CORTRIGHT: … My Ph.D. is an honorary, earned in the school of hard knocks, not in academia.

DINKEL: Oh, Doctor of Engineering, Rensselaer, honorary. Okay, gotcha.

Tell us about the NACA nuclear school. In '57, that was right up at the pointy end of the stick in those days, wasn't it, nuclear propulsion. It must have been just coming around.

CORTRIGHT: It was a gleam in the eye. As far as I know, the only way they've ever used the plasma physics propulsion is for stationkeeping and attitude control, because [it has] very high-specific impulse and uses very little propellant. The nuclear rocket got quite a bit of attention for the early years of the space program, but was never developed, and I wasn't involved with that.

DINKEL: I was afraid you wouldn't be able to talk much about it. When I saw nuclear school at NACA, I didn't know exactly what that was going to be.

CORTRIGHT: They were preparing us—some of us—for the entrance of nuclear into aviation and space. They considered nuclear power for an airplane. Remember the B-36, with that big eight-ball reactor in it? I mean, some of these ideas were wild, and that was not a good idea, but they did it. How would you like your planes flying around overhead with nuclear reactors [on board]?

DINKEL: It would be just fine, as long as they didn't crash into my house.

CORTRIGHT: Well, … assuming that one or two crashed occasionally, you'd have a big mess.

DINKEL: They would certainly shun away from that quickly after you had a couple crashes.

Well, that was '48 that you went to NACA.

CORTRIGHT: Right. I stayed there for ten years.

DINKEL: How were you lured over there, from Pennsylvania to Cleveland?

CORTRIGHT: [I applied to both Langley and Lewis. I was offered a higher starting grade from Lewis.] I could use [the extra money], so I went to Cleveland. As long as I could work in aerodynamics, I didn't care. And I knew I could work under John Evvard and Abe Silverstein at Cleveland. That was fine.

DINKEL: Can you tell me about some of the people who were there, now that you mention Abe Silverstein and that bunch?

CORTRIGHT: They were great people there, as there were down here. Silverstein is my hero, really, and my mentor, because he brought me along, gave me opportunities, took me to Washington with him, gave me important responsibilities at a very young age.

Evvard is a character, but a bright one. He was a Cal Tech [California Institute of Technology] graduate, and I worked directly under [him]for most of the time I was there.

DINKEL: Oh, I can't let that go by. You have to tell us why he was a character.

CORTRIGHT: No.

DINKEL: [Laughter] You're right, you don't.

There are lots of instances in the research we did that said that—

CORTRIGHT: … I was put in a group of theoretical aerodynamicists and physicists. It was called the Applied Mechanics Group, and it was set up by Silverstein. And these are names you probably never have heard of, but it included Sy Ostrach, Steve Maslen, George [M.] Low. You must have heard of George Low.

DINKEL: Yes, sir.

CORTRIGHT: Wolfe Moeckel, Barry Moscowitz, [Hal Mirels, and] Frank Moore. About five or six of that group of eight ended up in the National Academy of Engineering, which is pretty hard to get into. But I only stayed in that group for less than a year, and then I got an assignment to run an experimental aerodynamics branch, where I did the [unclear] work that you asked about. It turned out I was better at experimental work than theoretical. I really didn't ever think that I was a creative mathematician. I could do the work in college and get

A's in it, but I couldn't apply it as creatively as the best, and I didn't do as well. But I did fine in the other stuff.

DINKEL: Tell us some more about Abe Silverstein, if you would.

CORTRIGHT: He's ninety years old, just turned ninety. Talked to him the other day.

DINKEL: You two were portrayed as quite a team back in those days. Was it the personal relationship that made the professional relationship so good?

CORTRIGHT: Well, his team was small. I mean, the people he surrounded himself with he handpicked, and I just was lucky enough to be one of them. But they include George Low and Dee Wyatt, now dead, Harry Finger, John [H.] Disher, now dead. He took down less than a dozen people. Then he brought some up from Langley, [but] not quite as many. Bob [Robert R.] Gilruth came up and led the Langley part of it, but the Langley guys were harder to get to stay in Washington. Lewis guys were more inclined to stay under Abe, and since Gilruth wouldn't stay there, the Langley guys went home, except for a few notable exceptions. They were outstanding people who stayed.

This is semi-interesting, I think. The way NASA was started was in a little room, with maybe a dozen guys all crowded into it around desks, and that was at 15th and H, in the old Dolly Madison House in Washington. And we started with clean sheets of paper, and we sat down, basically, and the questions were asked—okay, I was given space applications, which was meteorology, communications, navigation, and geodesy, and told to put a program

together, figure out what you can do in those fields and rough out a program, and I did. I didn't do it by myself. There were universities working in it. The Air Force was already in it. It was a matter of getting all that information pulled together, because people had started working on space some years before that, as you're well aware. But we had to pull it all together and lay out a program and budget it and organize it.

Well, we did that for applications, and then I got lunar and planetary, and I did the same thing for unmanned lunar and planetary. The manned lunar was separated off. I can go into some of that later, but that's enough for starters.

We roughed the whole thing out in those early days, put budgets together, which we sort of made up out of our heads, went into Congress and asked for the money, figured out where we might get the work done, went out and sweet-talked these various organizations into taking [the assignment]—it wasn't hard to talk them into it. I shouldn't have put it that way. But parceled it out, and where there was no one there to do it, we identified the need for a new center. And then we went site-searching and got the people pulled in to organize the construction of that center.

That's how Goddard Space Flight Center was built. I still remember that. I went on a search, with five other guys from different centers, all over the country looking for the sight for the new NASA center. We looked in California, and we looked down at the King Ranch in Texas. We looked at the Outer Banks of Georgia. … Sea Island area was pretty unpopulated. We looked at the Cape. And when we got all done, we brought a report back in and Hugh Dryden said, "Well, last week I went out and looked around Beltsville and talked with the Department of Agriculture, and they said they'd give me the land out there. Let's just put it there."

His reasons were very different. His reasons were brilliant—namely, it's got to be close to get the Congressional people that are going to put up the budget a way to see what you're doing. … We were looking for a launch site, and we were going to put it at new launch sites. They didn't have to do that. We'll use the Air Force launch sites, and we'll put the research up here. And that was how that decision was made. It was made by [Hugh L.] Dryden, who's dead, of course. Silverstein participated, and the rest of us said, "Gee whiz."

DINKEL: This group that you talked about, that was the Advanced Technology Group, right? Is that what you called yourselves?

CORTRIGHT: No. That was an *ad hoc* committee put together. The Advanced Technology Group, I had guys like Leonard Jaffe, who you may have heard of, who [led] communications work for me on communications, and Morris Tepper, who worked on the meteorological satellite program. We didn't have a formal effort in geodesy at the time.

I guess the big winners were communications, meteorology [and] navigation. Who worked on that? I can't even remember anymore. Because navigation was very simple at the time. You remember the early transit satellites. They didn't do much, but the Navy used them for navigation for submarines for a while. You'd come up and listen to the transit. They used the Doppler method of getting position. It wasn't really too accurate. But you see, that's blossomed on into GPS [Global Positioning System], which is everywhere.

All the communication satellites, that really did originate—well, I keep saying our small group, and that's definitely not fair, because we were fairly limited in our capabilities of doing real in-depth work. But all of the country people responded, and the first thing you

knew, we had the little relay satellite up first. Then we had the balloon that Langley developed, the Echo, where you bounce radio signals off the satellite. That was for communications I'm talking about. Then SYNCOM was a little spinning satellite developed by Hughes Aircraft for us.

We identified that the place to put the satellites was at synchronous orbit, 22,300 miles out, so they sit overhead. We identified that almost from the beginning, but we had no way to get them up there at the time. We didn't have the launch propulsion capabilities, but it came pretty quickly. And the little SYNCOM up there was relaying [messages] almost halfway the world, a third of the way around the world, almost half, very early in the sixties. I don't remember the dates anymore. And we even had a direct-broadcast satellite, developed and built by Fairchild, which deployed a huge antenna. We put it over India to broadcast health-type messages down to the little villages, and in cooperation with the Indian government, they put little receivers and television sets in the public square. So we brought TV to all these little Indian villages. Those were very early things done.

Now we've got direct-broadcast satellites into the homes. You probably will have, what, 150 communications, maybe 200 communication satellites up there, maybe more within a year or two. You know, what do they call it, the Iridium? Is that one of the new systems?

They're just filling the sky with all sorts of systems for communications, for not just television, but for hand-held radios and beepers and everything else. It's just flooded. The meteorological satellites are somewhat similar to what we did in the first place. They went up and looked down and give you visual coverage. But they're getting more and more

sophisticated to measure temperature profiles and surface winds, and even surface pressures by certain types of instrumentation.

I've covered them all, the main ones. Even geodetic is used, but you never hear of it. They use that to detect whether the Simi Valley fault in California has moved a quarter of an inch in the past year.

There are amazing things that have come out of all this, is really what I'm saying. But those were the first years. When I … headed up the lunar and planetary effort, JPL [Jet Propulsion Laboratory] was the leader, and it didn't belong to us. We got hold of it, though. Basically, JPL became a—I don't know what the right word is. But they came under [NASA] control with some problems with Cal Tech. There was a little feeling there. Cal Tech didn't like anyone [interfering]—they're strong-minded people, like Langley. But it worked out all right. Most of my years in Washington were working on the lunar and planetary program.

Well, after a couple years of that, I got all of the unmanned under Homer Newell. Homer [E.] Newell was my boss direct. I was his deputy. He was the scientist, and I was the engineering manager. So I got into all those programs from then on, until I went over to the manned space program, where I only spent a year. I worked with JPL primarily.

DINKEL: I'd like to go over some of those in detail, but before I forget, let me go back to Langley and ask the question I've been wanting to ask you about the boundary layer control and inlets. I understand you were the subject matter expert back in those days. It must have been the days that you were running the wind tunnel.

CORTRIGHT: I was at Lewis…

DINKEL: That must have been exciting work in those days.

CORTRIGHT: Yeah. But they were doing work at Langley and Lewis on that subject, and some of the other [labs]. The Air Force at Wright Field, I think, was doing some work in it. We weren't the only ones.

DINKEL: Is that where the variable ramps and cones came up? Is that how we—

CORTRIGHT: Yeah.

DINKEL: Was it invented at Lewis and Langley in those days?

CORTRIGHT: Well, my section, I guess, did more inventing of how to control that than any other group at Lewis. The eight-by-six-foot tunnel did larger scale tests on actual aircraft that used these systems. You know, the F-106, the F-104. And they would work on trying to correct problems, like the old McDonnell-Douglas Voodoo.

DINKEL: 101.

CORTRIGHT: 101 had boundary layer problems in the inlet, and the eight-by-six worked on solving that. They are different things you can do on how you bleed off the boundary layer. The reason you have problems with it is, when you go through an inlet, you're compressing

the air from low-static pressure to high-static pressure, which creates a pressure gradient back against the flow. Well, the very high-energy flow coming in can overcome that and go right on through.

Boundary layers, the bottom of [it is] barely moving. It has no energy at all. So that high pressure buildup tends to push back out, down where the boundary layer is; and when it does that, it separates the boundary layer. The boundary layers run along nice and smooth. The pressure from inside that builds up as you go in tucks under it and separates it. So obviously the thing to do with it, you can suck it off or divert it off or just scoop it off, and we came up with all sorts of schemes to do that.

The only one specifically that we came with that I know was used was the F-104, and I think I did the first report with a guy named Fred Goelzer on the half-spike inlet. I'm not sure, because there may have been some other lab. But we did the work there, and Ben Rich [phonetic] and, what was his name, Stroud came back from Lockheed and worked with us on that. And the only twist we had, instead of having a diverter that was this way, we let the half cone go all the way down to the surface, the half-cone inlet went down to the surface and we just cut a notch in the side and the half-cone itself diverted the boundary layer. That was no great shakes, but it worked, and it worked on the 104.

DINKEL: Interesting, very interesting.

CORTRIGHT: You're apt to get more than you want on some of these questions.

DINKEL: No, that's fascinating.

CORTRIGHT: You're bringing things up I haven't thought about in years.

What I worked on more than inlets was exits, exit nozzles. If I had any claim to fame, it was on exit nozzles.

DINKEL: Oh, really? Tell us about that.

CORTRIGHT: On base pressure.

DINKEL: Base pressure?

CORTRIGHT: Base pressure. That's a long subject, but basically, if you have an exit nozzle, there's some area around it that there's no flow-through, or may not be. And that's a solid base, and it generates a low pressure in there, and it's a high drag. Well, the back of a bullet. You don't have to talk about jet face pressure. The back of a bullet is blunt, and it's very draggy.

That's sort of an invention I did with a fellow named Al Shroder. I called it "base bleed." If you could find a way to let some air leak in to that base area, it would break up the suction. The suction's created by external flow coming together, and it's like a little jet pump. It wants to aspirate that open area. So I said, "Well, we'll just let a little air leak in there, and it'll raise the pressure." So Al and I went over the wind tunnel and set it up. We did that, and sure enough, the base pressure comes right up. We wrote a paper, the first paper on what we

called base bleed. We tried it out reducing the drag of artillery shells, which it does. Not a lot.

Do you remember the name of a ballistics guy named Ball who did work for the Arabs and the Argentines and became sort of notorious? He developed the artillery pieces with the longest range so they could out-range our artillery pieces, and he sold some of these, built them, I guess, for the Argentines and the Arabs. I think the Iraqis had some. But he used it.

I just was listening to a program on Ball one day, and they said his shells used the technique called base bleed. I said, "Gee, did someone finally use it?"

But we did a lot of work in predicting base pressures of not just blunt bodies, but where jets are interacting with external flow. You know, the jet comes out, the external flow comes around, they interact in a shockwave pattern, and there's disturbances. So we found some empirical ways to predict those drags and pressures. That's probably the best paper I ever wrote, and the only one I ever saved.

DINKEL: Oh, good. The only one you ever saved?

CORTRIGHT: Yeah.

DINKEL: You must have been proud of that, then.

CORTRIGHT: Yeah, I was.

DINKEL: You should be.

CORTRIGHT: Sort of.

DINKEL: In '58, you were selected as a member of—we touched on this before—a member of the special team of scientists and engineers that developed the program plans for what was—

CORTRIGHT: Can I interrupt?

DINKEL: Sure.

CORTRIGHT: I'm talking about things that I think I did, and you're going to have all this. That's how I got into trouble with my last interview. I don't mind saying it, but for God's sakes, don't make it come out like "Cortright claimed he did this." That what [James R.] Hansen did. If I ever see Hansen again, I'm going to tell him. I don't really appreciate that, because—

DINKEL: We'll send this tape to him.

CORTRIGHT: I'm not basically a credit-grabber, and I'm only telling you these things because they were interesting to me at the time. I never did any of these things by myself. I always had a team of people on everything I ever did in my life. My professional life was working with people. I almost never did anything by myself that was noteworthy. But my people did,

my little teams did some very good work, and I figure I helped them. I'm not just saying that to sound generous. That's the honest-to-God's truth, the way it was.

DINKEL: We believe you.

Where was I? We were talking about the team that decided what NASA was going to look like. That was in '58. Can you tell us who selected that team and put it together. I think we covered just about what it did.

CORTRIGHT: Abe Silverstein. Hugh Dryden brought Abe down, and he gave Abe carte blanche authority to put together the organization and the plans, and he retained oversight. Dryden was the wisest man in the agency, probably, and Abe was, he was a dynamo. He doesn't get half the credit he deserves for putting NASA, its programs together, starting the new centers, getting it organized. They asked him to head up the Apollo program, manned program, and he said, "Okay. I'll need Johnson. I'll need the center (either they were going to build). I'll need that reporting to me, and von Braun's organization and the Cape have to report to me directly." He couldn't sell it, so he turned the job down and went back and became Director of Lewis Research Center, and [D.] Brainard Holmes came in from RCA and took the job, without those centers reporting to him, and it took him three months to get those centers reporting to him. Brainard, he didn't have to have it his way from the beginning. He just was very smart and he knew how to get his way, and he got it. That's the way it ran from then on.

DINKEL: Interesting. And you started to touch on it right there. What are your most vivid memories of the atmosphere and the attitude during those formative years when NASA was deciding what it was going to be?

CORTRIGHT: During the first few weeks, after NASA was officially established, a lot of that planning work I talked about earlier was done before we really had a NASA. We would commute from our centers. Like I'd go down from Cleveland once a week and stay for a week or two, with all the other guys, and then go back.

When NASA was finally created—I started to make a point. This is a senior moment I'm having here. What was the question again?

DINKEL: The atmosphere.

CORTRIGHT: Yeah. [T.] Keith Glennan was appointed the first administrator, T. K. Glennan. He was RCA background, also, very distinguished gentleman. He called us all together in the Dolly Madison House, in the little auditorium, and I still remember him saying, "Gentlemen, this is no place for tired men." There weren't many ladies then at the time. They came later. "This is no place for tired men. If you're tired, go home." So that set the tone.

I'll tell you another anecdote with Keith Glennan. At one of his Saturday morning meetings, Oran [W.] Nicks, whose name you may have run across, when I became the deputy to Homer Newell, I got Oran to take over lunar and planetary. I hired him from [Vought] at the time.

We went in for a Saturday morning meeting, and Oran had to make some presentation at the time. He got up, and he was dressed like I am. I think he had a shirt with buttons, but he had no tie on. And Keith Glennan said, "I'm glad to see you're relaxed today, Mr. Nicks." And Oran said, "Dr. Glennan, where I come from, anyone who wears a tie on Saturday morning is a slob." And with that, Keith Glennan tore his tie off and threw it on the table and said, "Let's go."

The reason I tell those stories is, you asked to set the tone. The tone was one of constant work. I did some papers on this. I wrote one on the life and death of project managers once. Only about 10 to 15 percent of project managers who start a program ever get to finish it, because they get going …, and then the first you know, their project reaches the test phase, where hardware starts to come together. You run into all the problems, cost increases and delays, and then he's replaced by a new face. It's done everywhere, in the military and NASA, everywhere. And then if he's a good—most of them were good men—they recycle back, either took [on] another project or picked up another at an intermediate stage, and we just kept recycling them. But that was the way of life in Washington.

But I did a study, a fairly accurate study, of the time put in on the job. The average time put in by most people working in those tight little groups was seventy hours a week, for years. In fact, I think I worked between sixty and seventy hours a week in every job I ever had except Owens, Illinois. But at Lockheed I did, and I was old by then. I mean, pretty old. I'm old now. But at Lockheed even, it was very hard. I worked sixty to seventy hours a week. I spent eight and a half years away from my wife in dribs and drabs on travel, and I'm counting travel as work. And we worked.

The reason you had to work so hard in Washington—this is sort of a little thing of mine—is, in Washington it's so hard to get anything done. You have to work twice as hard as the ordinary person just to get the same thing done, because you have to get through the bureaucracy and all the crap that goes on up in that place. We didn't have any plans, that I know of, or knew of.

DINKEL: I think that bureaucracy has spread. We see a lot of that now in all the centers these days.

CORTRIGHT: I know. You probably thought you might ask me about that at some time. I don't know whether I'll comment on it. But I do not get good reports from people I respect who are still working at the centers or with the centers. I think the downsizing has created a very high ratio of support contractors to in-house people.

DINKEL: Oh, yes, sir.

CORTRIGHT: And that means the in-house people are more paper-pushers than researchers, particularly at research centers. Now, Johnson always had so many big projects that they always had to have that high ratio. But even at Langley now I saw that there are almost half as many support contractors as there are in-house, maybe more than half. I saw the number a while back. It was like 1,500 or 1,400 support and 2,400 in-house or something like that, a very high ratio. It's very hard to keep a large fraction of your people doing research when you have to pay attention to all these support contractors. It's a problem.

Well, that's off the track again.

DINKEL: It's interesting, your attitudes on that, because Johnson Space Center now is 80 percent contractor and 20 percent government. It's very skewed right now.

CORTRIGHT: So you're pretty much a project management organization. Well, it always was sort of that way, but you also had some very good creative people down there, most of them coming from Langley. You know, the old crew under Chris [Christopher C. Kraft, Jr.] and Max [Maxime A.] Faget, Scott Simpkinson from Cleveland, and that type, George Low, Gilruth himself, they were outstanding people, and they were creative. They could have, and did make it, in the research field before they went into managing these big efforts.

DINKEL: Max Faget keeps up busy with his NACA stories. He's a pretty colorful character.

CORTRIGHT: He is. I like Max. I'm very fond of Max. He always was stimulating.

DINKEL: Yes, sir, that's the right word.

CORTRIGHT: Full of ideas. Didn't necessarily buy yours or the party line. But very creative, and if problems came up, Max always had a solution, and frequently they were the best ones. Smart guy.

DINKEL: In '59 and '60, your group developed plans for, and initiated at least six that I know of, lunar exploration projects. I am most familiar with the Ranger, the Surveyor, and what later became Lunar Orbiter. But I'd like to talk briefly about each one, if I could. I believe Ranger was first in the chronological sequence, so let's start with that one. What can you tell us about Ranger?

CORTRIGHT: You mean beyond what I told in the book? I wrote that chapter in *Apollo Expeditions to the Moon*, and you can find a lot, not on tape, but you can find a lot about those programs.

Ranger basically, we had had a whole series of failures using—some were launched with Thor, with Thor-Able rocket. These all preceded Ranger. There were a dozen failures, ten or eleven failures. Was it that high? Maybe not quite. It was maybe close to ten, where they'd go out near the moon. We called them semi-successes, but they were all failures. And so Ranger was to be the first big effort to really do it right, and the job was to find out what the lunar surface looked like in detail. I mean, down to where it could detect you if you were standing on the lunar surface.

So we got the Atlas-Agena rocket to do it and put the job at JPL. Bill [William H.] Pickering was the director at the time, and a fellow by the name of Jim [James D.] Burke was the first of three project mangers on that program. No, Cliff [Clifford I.] Cummings was first. Cliff Cummings went to Jim Burke, and maybe Ranger ended up under Jim Burke, but I'm not sure. There may have been another.

How many Rangers were there?

DINKEL: I've got Ranger 9, 7, 8, and 9.

CORTRIGHT: There were nine. Seven, eight, and nine worked. The first [six] failed. And it really was tense, because we were investigated on every one of those failures, and as it got worse and worse, we'd spend weeks in front of Congressional committees, explaining it, and I had a lot of that. I had to do most of that. I had to do all of it.

DINKEL: That's not fun, standing on the Hill and telling them about your work.

CORTRIGHT: No it isn't, telling them all these millions of dollars gone down the drain. In the beginning, see, we didn't want to contaminate the moon, so everything had to be sterilized. JPL tried to do it by heat sterilization, and all the wiring ended up like spaghetti. So that failed. Then we gave up on that and said, "They really don't have to be sterilized. They just have to be clean." So we cleaned it with a gas, radiation and a gas in general, liquids that were disinfectants, germicide-type things, and tried to keep it as clean as possible.

But they were all different causes. You don't want to hear about all of them. I can even remember some of them. Some were exotic, and some were just dumb. Some reflected inadequate testing, and at one time the program was called "shoot and hope." But finally, 7, 8, and 9 worked, and as I put in my chapter of the book, … 7 worked, [but] it came in slightly on an angle, moving across the surface. The resolution wasn't the maximum, but it was excellent. And then on the 8th, JPL wanted to do an attitude control change, so it got max resolution. We were afraid to take any attitude changes that might introduce a glitch. We

were on a mission, so we wouldn't let them do it. But on 9 we did, and they got the maximum resolution on 9, and you could get objects about this size.

But it was a difficult program. It was very tense. But then we went on to Surveyor, and we got five out of seven. And Lunar Orbiter we got five out of five, and that was done by sleepy old Langley down here, before I got here, and Boeing, with a very revolutionary approach to the camera, which I've described all that in the book. They used wet film and developed it on the spacecraft, as opposed to a television camera (Vidicon-type stuff) and it worked.

DINKEL: I'm reading my notes, and we did that with a resolution of sixty meters between center posts. That's very good for that time.

CORTRIGHT: On the Lunar Orbiter?

DINKEL: On the Surveyor.

CORTRIGHT: Sixty meters, Surveyor landed, three out of five.

DINKEL: No, my mistake. Lunar Orbiter.

CORTRIGHT: Lunar Orbiter. Yeah, that was something. It was very good. It was probably about like that. That wasn't much worse than—if I can expose a gripe here—you know, we had LANDSAT-type satellites very early that were surveying the Earth for various reasons—

geological, crop control, land development-type control. The whole Earth was being mapped. I think the early resolutions were—I don't know. I don't remember exactly, but maybe fifteen feet or something like that. And the Air Force wouldn't let us show pictures any better than like fifty meters, as I recall. Well, they were getting reconnaissance pictures down in a few feet or less. I don't know, incidentally, what they were. I don't know what they are today, but I know they're very good.

The interesting thing is, the Russians were doing the same thing, and we knew that. They were taking pictures of anything they wanted over here in the inches or a foot-type resolution. We were still held to the 150 feet in all the things we mapped, and I don't know, I don't think it's been relaxed too much yet. It's better than that. But then the French came along and they did it, and they released low resolution. The Russians are releasing, were releasing better resolution of this country. If you wanted to buy it, then we could…

DINKEL: Well, the Spot That is very good.

CORTRIGHT: Spot That is very good. [Have] you used your computer to go into Terra Server yet?

DINKEL: No, sir.

CORTRIGHT: I photographed my house in Virginia, my house in Florida, where I used to live in Washington, where I used to live in California. Pictures of the house from orbit taken off [the] Internet. Now, they don't have the entire world on their computer base yet, even though

they've mapped it, but you can get Baghdad, I think. You can get some of the Middle East stuff that's on there, if you want to look at Hussein's palace. Or depending on where you live. Probably Houston's been done. You can probably find your house out there, just on Internet. Go to Terra Server. It's fun.

At any rate, that's just a gripe, the military constraints. And then it gets in their own bureaucracy, once they set it up. I don't want to come across as a critic. I'm a great admirer of the military, actually. But they did some dumb things, like we all did, but I remember other people's dumb things better than my own.

DINKEL: Tell me about the duration of the Lunar Orbiter. How long was it up there? There was five of them. Did they all stay up for weeks or months?

CORTRIGHT: I don't remember. They mapped the entire moon at medium resolution and selected areas at high resolution. They found, as I put in the book, that the areas that were slated for Apollo landing were rougher than Earth-based photographs would give you to believe, and they did help select Apollo landing sites. A very successful program.

There's some anecdotes in that book, but you're not relying entirely on the spoken word. This is to be a written product or not?

DINKEL: It's going to be both. We're going to take the audio and put it on a Web site eventually.

CORTRIGHT: Okay. Well, I'm not trying to speak to an audience. I'm just rambling.

DINKEL: That's okay. How did Lunar Orbiter get switched—I think it got switched. Please correct me if I'm wrong—from Surveyor Orbiter to Lunar Orbiter?

CORTRIGHT: Because JPL and the Hughes Corporation got bogged down in doing the Surveyor program, and a young man by the name of Oran Nicks, who worked for me, got together with Langley, and a fellow by the name of Captain Lee Scherer, and they got Langley to agree to take over the job if I and Headquarters could be persuaded to move it from JPL to Langley. I can't take any credit for that at all, but I agreed with it once I saw the arguments. And JPL and Hughes had all they could do to do the Surveyor.

The Surveyor Orbiter was supposed to use a lot of the hardware from the Surveyor already developed for attitude control and maneuvering and the launch vehicle and all that. But we were having trouble with the Centaur, Atlas-Centaur rocket at the time, too, and Langley figured they could do that mission on an Atlas-Agena, with a little lunar orbiter, and they did. So we switched it, and they did it. So people like Tommy [Floyd L.] Thompson, who was Director down here, and Cliff [Clifford H.] Nelson was the Project Manager on that job. Jim [James V.] Martin was Assistant Project Manager, and he went on to manage Viking, a great Project Manager.

There are a lot of people at Langley that deserve a lot of credit that they don't get as a rule. They get it for the Apollo program pretty much, because [they] sent their best people down there, and then they did research for it up here. Viking they get quite a bit of credit. Well, they get credit for Lunar Orbiter. It was their program. But I guess what I was trying to

say is, in all these programs, the research people at Langley came up with fundamental ideas that were absorbed by the projects, and frequently they don't get reflected back on the Center.

DINKEL: What about Mariner?

CORTRIGHT: What about it?

DINKEL: We don't know too much about Mariner.

CORTRIGHT: Well, the program that JPL always wanted to run was planetary. They didn't care about running a lunar program, but they sort of got stuck with Ranger and Surveyor, and they had their grief on it, too. I mean, they had problems. But they always wanted to explore the planets. They thought that was the best application of rockets and space flight. So we agreed to have parallel programs, and Mariner started early on.

That was similar to Ranger in many of its concepts, and the first one that worked. The first interplanetary mission that ever worked for us was a Mariner fly-by of Venus. I remember it because I took my kids out of school and my mother from her home, bought a new car that would hold them all, and toured the United States, through all the national parks. It took five weeks to do it. Swung down through JPL in time for the fly-by of Venus, and then came home.

That was the first of quite a few. Even before Viking, there were—I think Mariner 9 was the last successful Mariner, or maybe there was a Mariner 11. I don't remember anymore, because I was down here by the time the last Mariner flew. But there were a series

of half a dozen or so successful Mariners, each one getting a little more information about Mars, primarily, including one that went into orbit about Mars and did a lot of mapping of the planet. I think it went into orbit. You better look that up, because I'm not sure. It seems to me Mariner 9 may have gone into orbit and did a pretty good job of low-resolution mapping before Viking mapped it in more detail.

DINKEL: And then the Prospector was after that, and that ran into trouble and was canceled. Do you remember that?

CORTRIGHT: I guess our eyes were too big for the budget there. I had a lot to do with Prospector.

…It seemed like the Saturn V had no application after Apollo. So I thought, "Well, let's use it to put some real substantial payloads on [the Moon]."

We [also] had a design [Voyager] that would use the Saturn V to launch payloads, one on top of the other, in a single mission to Mars. …It looked like a big Viking. I mean, it just had a lot more to it. And it was very expensive, and there were no other uses for Saturn V that anyone was really interested in. So Saturn V died and Prospector [and Voyager] died.

There was a money crunch at the time. We were lucky to get the money to do Viking, and that was a struggle. And that cost, what, almost a billion dollars, certainly three-quarters of a billion dollars for the Viking.

DINKEL: And then there was Voyager.

CORTRIGHT: The same deal. Voyager was to be a—the original Voyager was—what was that going to fly on? Atlas-Centaur. Then we upgraded that to have a big Voyager on Saturn V. All of the Saturn V payloads died before they were born.

DINKEL: And the Voyager was going to be a Saturn V payload, also, you think?

CORTRIGHT: I think.

DINKEL: I couldn't find a lot on that.

CORTRIGHT: They were first trimester abortions, is what they were. We did a lot of work on it, but they didn't get too far. Incidentally, JPL never liked the big approach. They always argued against it. I probably was the leading proponent in using the Saturn V, and I lost. Probably very wise that I lost.

DINKEL: Well, it turns out that what I'm about to talk about you didn't lose, because in '62 our researchers revealed that you and Dr. von Braun had differing opinions regarding the Centaur program. He wasn't comfortable with the—

CORTRIGHT: Well, let's put it, it was in trouble. The Centaur was in trouble. It was not a Marshall idea. It was originated by a man named Krafft [A.] Ehricke at General Dynamics for the Air Force. It was an Air Force program, and it was in trouble. And von Braun by then was—we took it over from the Air Force, because they didn't really have the need for it.

At the time, we did. And we put it at Marshall, and Marshall was in trouble. They had their hands full with the Saturn 1B and the Saturn V. So von Braun came up and he said, "The solution to this is to—oh, it kept losing payload capability, to pretty soon it wouldn't launch our Surveyors, and we had a lot of money invested in Surveyor, a lot of time and money. So von Braun said, "Well, let's put the Surveyor on the Saturn 1. We'll put an upper stage on it." The Saturn 1 with an Agena, I think, would handle the Surveyor mission a lot better than Atlas-Centaur, and it would have in terms of weight. But it didn't exist.

So I went down there. I still remember that meeting with von Braun and Eberhard Rees, and heard their arguments, which were logical. I'd already taken a mental position when I went, and I had already talked with Abe Silverstein. I said, "If we take it from Wernher, will you do it at Cleveland?" and he said, yes, he would, because he'd been part of starting it when we took it over from the Air Force. So I had that ace up my sleeve for that meeting.

It was not a contentious meeting. They made their points and I made our points of how we could use that rocket, and then I said, "I'm going to transfer it to Lewis. I think I have concurrence." And I did. I didn't do this—I didn't have the clout to do this by myself. Tom Dixon was involved. Homer Newell was involved. And Bob Seamans [Robert C. Seamans, Jr.] was involved, as I recall. It went right up the line, and everyone agreed; and it went to Lewis, and Lewis did it.

They got not all the payload back. They set everything back a year, almost cleaned the whole thing up and started over again on many parts of it. And they amazed me. I didn't think Lewis, being a propulsion center, would have the smarts to do some of that stuff. The

whole Space Program amazed me, because you've got engineers with no background who did all these things. They went in cold, pretty much, and invented.

It's like the Apollo program itself, which I'm getting off the topic, but I can remember when that was put together by Silverstein and George Low and John Disher, the guys here at Langley, Bob Gilruth and Chris Kraft, before it was a program, just preliminary calculations on what it would take to send men to the moon, and that was in very early stages.

I hate to do this, talk about myself again, but Abe Silverstein asked me to give very early on, NASA had just been created for a few weeks, and he said, "I want you to give a presentation to the AIAA [American Institute of Aeronautics and Astronautics] annual meeting on the space program."

So in any event, I did it, and in that, I sat down with John Disher one day and I said, or George Low and Disher, "Tell me about this man to the moon thing. How can you do all this?"

Well, they showed me some rough sketches of humongous rockets. I said, "Do you really know the size?"

"Well, plus or minus 20 percent." So it was very rough stuff at the time.

So I went in there, and I said, "Well, how fast can you do this thing? It looks like a huge job."

"We probably can do it ten years."

So I went in this meeting, and I said that. I said, "We have all these programs that I've told you about. We've been sketching out on paper all these neat things we can do, including a man to the moon."

Well, then they started asking questions, "How fast can you do that?"

I didn't know my left foot from my right on that subject, but I did quote Disher, and I said, "Well, that's probably a ten-year job, but that's off the record. I don't want that used any place."

They said, "How about men to Mars?"

I said, "Well, that's certainly fifteen to twenty, at least, but ten to the moon."

The next morning, a guy by the name of Vern Hogland [phonetic] or Augland, in one of the newspapers' headlines, "NASA Plans Man to the Moon in Ten Years."

I called Abe Silverstein and apologized. I was humiliated. And how long did it take us? Ten years. It was the right number.

DINKEL: So everybody thought you were really smart.

CORTRIGHT: Everybody but me. I didn't think it could be done that fast, and I didn't think anyone knew how long it would take to do it. I don't think we could do it in ten years today, to be honest with you. That's one of my negatives on this. Our operations today have become so bogged down.

We started a fighter plane at Lockheed my last year there, the preliminary work on what's now the F-22. Are you familiar with that?

DINKEL: Oh, yes, sir.

CORTRIGHT: They call it the Raptor. We had really talked the Air Force into including a very stealthy spec. The first fighter spec had very little stealth. It was like one square meter, or

half a square meter. That's not stealthy. It either has to be this big or it might as well be that big. But I think we played a key role in convincing them to rewrite the spec. After the commanding general retired, his deputy became commander, and he rewrote the spec and put stealth in. So that complicated it a little bit.

But that's been fifteen years, and all we have is two of the first—they're not prototypes, but they're more the proof test models, the ten or eleven they built to ring out before heavy production. There are only a couple of those flying. They're behind in their flight test program. That's fifteen years. We went to the moon in ten, knowing nothing, almost nothing when we started. There were no rockets like the Saturn V. It took the imagination of people like von Braun and Silverstein to do that, and you've got to give those guys tremendous credit for even imagining they could do it.

DINKEL: Yes, sir. Well, Centaur turned out to be pretty successful.

CORTRIGHT: It's still in use.

DINKEL: The RL-10 engine is the workhorse.

CORTRIGHT: Centaur's still a workhorse. That was a good decision. By the way, that rocket blew up the other day wasn't the Centaur. It wasn't the upper stage, was it?

DINKEL: No, sir, it was not.

CORTRIGHT: It was the Titan?

DINKEL: They thought it was a gas path problem.

CORTRIGHT: In the solids again?

DINKEL: In the solids.

CORTRIGHT: Oh, gee. That's sad.

DINKEL: We're taking a look, and we think we're okay.

CORTRIGHT: Now, you say "we."

DINKEL: NASA. They're very similar in construction to the Shuttle's SRVs.

CORTRIGHT: Oh, okay. See, I got involved in that on the *Challenger*. I was by then retired from Lockheed, and I was working on a Shuttle Safety Advisory Board. And, of course, after the accident, we went through it all, also. It was repetitious.

When's your plane, by the way? I mean, I don't talk much, but once you get me talking, I'll talk.

DINKEL: We have all day. Our time is your time.

In June of '66, right after the flight of Gemini IX, Deputy Administrator Robert Seamans didn't like the way the mission went and the results, and he established the Gemini Mission Review Board. James [C.] Elms was the chairman, and you were on that board. Can you tell us something about that?

CORTRIGHT: No, I don't remember anything about it.

DINKEL: Oh, okay. Chuck [Charles W.] Mathews of the Manned Spacecraft Center [MSC] was on there.

CORTRIGHT: I vaguely recall it, but it's very vague. As I said, I have trouble with yesterday. That was a long time ago.

DINKEL: That was '66. That was a long time ago. You've been doing great so far. That's the first one you didn't remember.

Let's try two years later, '68. Joe [Joseph F.] Shea, I guess, who was the Apollo Program Manager, and was the Deputy AA for Manned Space Flight, and then you came in and took his place for a year, a couple of years.

CORTRIGHT: A year.

DINKEL: Your friend, Mr. Hansen, quoted you in the book that, when you came in to take the job, you said, "It's awfully hard to come into Apollo and make much of an impact." What did you mean by that?

CORTRIGHT: It was almost done.

DINKEL: It was rolling up by that time.

CORTRIGHT: Yeah. You know, it was close to the flight to the moon, and all the hardware. The only thing I did, I did a—and I don't know whether it was very valuable. I did a troubleshoot assignment for George [E.] Mueller, going to all the subcontractors and trying to find problems with their equipment. I don't remember whether we found anything significant or not.

And then I got involved with the space station, and you probably don't remember a program called the Apollo LEM Lab. They took a Lunar Excursion Module and made a space laboratory out of it, to be launched on whatever they used, the Saturn 1B, I think, and it was to contain some astronomy instruments and whatnot. I got involved with that, because there was some things I didn't like about that program.

And then at one time—remember the Skylab?

DINKEL: Yes, sir.

CORTRIGHT: The Skylab initially was to be launched full of fuel, and after the fuel was burned out, it was to be shut down and vented. And then we were to go up with another mission and open it up and go in and make that tank into a lab.

Well, I opposed that, and I argued against it. I don't know how much weight I had to carry on it, but it didn't get done. They launched it properly, completely outfitted on the ground beforehand. I thought that was a crazy idea. It added tremendous work and some risk for no obvious gain. You'd end up probably with not as good a facility and my never get it to work. So I did that before I left.

I didn't do a whole lot. I did the best I could under the circumstances. You didn't know George Mueller, or did you?

DINKEL: No, sir.

CORTRIGHT: Well, George is a very, very dynamic leader, and a stylish one. By that I mean he has his own style, and it doesn't really need—I mean, a deputy is sort of nice to have, categorically. The people who really got the work assignments were the Apollo Program Manager, Sam [Samuel C.] Phillips, and the Project Manager, George Low, after Joe Shea, and the Gilruths and the von Brauns, and George's deputy was a high-level helper, and it didn't turn me on very much. That's all I meant by that. I found a few interesting things to do, but when Langley came along, I was very happy to get it.

DINKEL: That's a good place to take a break, because we're just about out of tape right here.

CORTRIGHT: I'd like to make a phone call to see if my wife—

DINKEL: Let's talk about the Viking. The Viking program was run a little differently than other programs, and there was some contentiousness about the way it was run. Do you want to talk about some of that, regarding who the work force was and the relative sizes of them?

CORTRIGHT: Well, I don't know that it was run all that differently from most projects. It maybe was run a little differently than Langley was used to doing it, because I very much projectized it and pulled it all together rather than having it all parceled out. I think the Lunar Orbiter had a smaller project office and had more tasks parceled out, but it was a smaller job.

Basically, my experience said that you should have a very strong—it's so difficult a project. You only get two shots. You're going to spend $750-plus million dollars. You've got to make it work, so you've got to put the strongest possible team together. There was a Project Manager here that I thought was outstanding, Jim Martin, and he agreed to do it; he wanted to do it. He's the best Project Manager I've ever known, and I've known a lot of them. He's a fantastic guy. But he was demanding and he was an inspirational leader, but you don't want to get in his way. He did run over a few people, but he did get the job done.

At any rate, I pulled it together, and I had the Project Manager report to me, which the Lunar Orbiter Project Manager reported to the equivalent to the director for flight projects. He was at least one level down from the director. Well, he reported to a director, and a director reported to the office of the center director, which included the Deputy Director, Charlie [Charles J.] Donlan. So there was a little bit of layering. I had none of that. I had

him report directly to me, even though I had a good deputy. I left other work to that deputy, and I spent an awful lot of time, maybe half my time, on Viking.

The size of the office was pretty big. I don't remember exactly anymore, but it was at least 250 people or so, and then we had a large number of support people throughout the laboratory also supporting it. So maybe at one time—we had 4,000 people at the lab at the time. Maybe as much as 400 or 500 were involved in Viking, which is a big commitment.

There was a lot of complaints that we took time from the shop people from building research models to do special problems. We had a lot of special projects of our own. We did work on the computer, which was running into deep trouble. We used an Air Force computer using a plated wire technology. It didn't last long. But it came out of the Minuteman program, and we used it. You wouldn't believe it if you saw it today, but every bit store was a little cast-iron donut, magnetic donut, that had two wires threaded through it, and that's how you got your either/or, and there were thousands of these things. And of course, the computer, there was not much room for error. I mean, you drop a few bits in the wrong place, your mission's gone.

I won't go into too much. I don't remember all that much about it. But they had trouble plating these wires. They were nickel-plated in a certain way. And they had trouble getting them to store data properly. Well, we brought that in-house and we did research on what the landing rockets might do to the Mars soil and that environment of a very low pressure, because this vehicle landed with rockets ablaze.

We did a lot of in-house. We did structural work. We did the parachute for Viking in-house. Working with a company, but, I mean, we did a lot of the design and final testing. That parachute was packed to the density of hard maple, and it was baked. We did work on

the heat sterilization. You could talk for hours on Viking, because that vehicle was actually baked at temperatures over 200 degrees Fahrenheit, over the temperature of boiling water, for hours, in its full configuration—wiring, computers, instruments, everything—in addition to other sterilization techniques. And then it was put in a capsule that was sealed, and we had a high probability that there were no living organisms in there at all. We had to do that because the life defection experiment might pick up our own organisms and say, "Ah ha, something's living here." That was an unbelievable challenge to get that thing to the—get me to the church on time sort of thing. The life detection experiment didn't make it to the spacecraft for that all-up test. It went through its individual test, and it got to the Cape just in time for the flight. That's a whole story in itself of the relationship with TRW in getting that thing out. We had a lot of struggles with them. They did it, finally. Everyone did it. Everything got there, some of it at the last minute, and everything worked. I couldn't have believed it.

DINKEL: Worked as advertised.

CORTRIGHT: Worked as advertised.

DINKEL: Well, about the general same time frame—

CORTRIGHT: You asked me a question at the beginning. I didn't totally answer it. We had internal—we had struggles. There were people who didn't like the project, never liked it being here, and still don't think it was a good idea and that it had a bad impact on the Center. And some good people. These are not fly-brains. These are very good research people,

whom I like and respect highly, who think that project never should have come here. So there was that much contention about it.

DINKEL: I want to got back and ask you a question that I almost missed, and this is about Apollo 13. Apollo 13 was classified as a successful failure. I made a note here that that's an oxymoron if there was one, a successful failure.

CORTRIGHT: I never said that.

DINKEL: No, you never did. Why is it classified as a successful failure?

CORTRIGHT: Nobody got hurt. That's all you can say about it.

DINKEL: You wound up being the chairman of the Apollo 13 Review Board. We mentioned earlier, I think off line, that you kept that report. Can you tell us something about that in a *Readers Digest* version? One of the things I wanted to specifically ask you about that, you determined that the root causes were obsolete thermostatic switches and improvised ground test procedures. Since I work safety these days, I'd kind of like to know what was learned from the test procedures.

CORTRIGHT: That's probably an oversimplified statement. The failure occurred—it starts out with the oxygen tank. It was on a shelf, and they were taking it out and replacing with it or doing something with it at the supplier who built that thing. I can't remember who it was

anymore. And they dropped it. Not off a table or anything, but the way it was mounted slapped down hard. It didn't seem to hurt it, so they went on.

In that tank, there's a tube. It's a fill tube, where you fill the oxygen, and it's a tube from the outside meeting a tube from the inside, with a sleeve, a loose sleeve over it. And during that drop, we determined that that sleeve probably popped out, because if you took all the tolerances in the wrong direction, it was barely in to start with. And maybe even if there weren't all in the wrong direction, it could deflect enough to pop that thing out.

Well, when that was out, you could still fill it, but you couldn't de-tank it by pressurizing it, because if you blew in over here, and you wanted it to run out this tube and there was a missing connector, nothing would happen, just pressurize the whole tank.

Down at the Cape, they ran into a problem with the vehicle that extended to the length of time that they had to de-tank. They tried to pressurize it, and the oxygen didn't come out. So they determined they could—they didn't know why for sure, but they determined they could boil it off, just by putting some heat in. Liquid oxygen doesn't take a whole lot of heat to make it boil, and it just bubbled the gases out. And they did that.

Now, there is a heater in the tank, was a heater in the tank, and it had a thermostatic switch on it that, if it overheated, was supposed to break the circuit. It turned out that the thermostatic switch was not rated. They had changed the voltage, what, from 30-some to 60-some, 65 volts. I don't remember the numbers exactly, but they had changed the rating on that on that switch, and the subcontractor hadn't done it; he'd missed it. So the switch that was in there was not capable of carrying the new current loads that were used on the heater, so it overheated the heating wire, which boiled the oxygen. But it couldn't shut it off, so it just went on up to over 1,000 degrees. And the wire had a Teflon insulator on it, and we

determined that that Teflon insulator would char and gassify at those temperatures. And when it charred and gassified, some of it would break off and leave bare wire exposed. And that happened at the Cape, and it was refilled. It could have conceivably blown at the Cape, but it didn't, because those wires didn't come in contact with each other.

By the way, that overheating—you're talking about testing procedures. The mechanic who oversaw this room in which some of the equipment was had a temperature gauge with a red line on it. Maybe it wasn't redlined, but it was marked at the temperature that that was not supposed to exceed. But it just so happens that that mark was very close to the stop on that gauge. There wasn't enough distance to really tell that it was there. It had hit the stop, and it looked like it was probably just at the limit of what was allowed. So he didn't notice it. That was an opportunity to notice that something might have happened. He missed that. He was never blamed for that, but that was another opportunity to catch it.

In any event, on the way they used little fans in there, propellers, to stir the fluid to keep it uniform throughout. And they turned those on and the fluid started to move, and apparently these two bare wires touched and ignited what Teflon was left on the wires, which anything will burn in liquid oxygen at those pressures. The Teflon burned and raised the pressure in the tank, and it blew it out. It didn't blow out from a chemical explosion. It blew out from over pressure of oxygen gas.

Now, to find that took two months, and the final test—this is hard to believe. We had a draft report written, and we didn't have it totally pinned down, but we were close. We knew it was a pressure explosion of the oxygen. We didn't know quite how it got lit.

There was a guy from your center, Johnson, and it was either Don [Donald D.] Arabian, whom you remember, or Scott Simpkinson, or some combination of the two—I

think it was Arabian—sort of, at the last minute, said, "Well, we can test that switch right here, and we better do that." They ran the critical test. It wasn't the Failure Review Board of mine that identified that test, although someone on the board might have participated. I didn't. They ran that test, and I remember they brought it into me and said, "Ed, we found it. They found the missing link."

So we rewrote the report, those portions of it that had to be rewritten, and it was ended up a perfect crime, perfect solution at everything. Nothing was left to question marks. The only argument that came out of that is, we insisted they put an extra oxygen tank in subsequent missions, and Gilruth objected to that, but he agreed to it. The guy who pushed the hardest for it was Hans Mark, whom you may know, I've known from Ames. He's down in Texas now. He was chairman of your state university system.*

So we got in. And a little anecdote. I was having breakfast. I went down there for two months, spent most of two months there. I was having breakfast one day, and one of the astronaut wives, whose husband flew the next mission, happened to be there. I didn't know who she was. I sat next to her at the counter. We got talking, and the Review Board came up. I still remember, she said, "Fix it right because my husband, So-and-so, is flying the next mission." That's interesting.

DINKEL: Up close and personal.

CORTRIGHT: Yeah.

DINKEL: I'm going to skip this other question and ask you the one that I know you'll have something to say about. It's the question that everybody really wants to ask, but never does. I know you've seen the movie *Apollo 13*, and I'd like to know what your opinion as to its technical authenticity.

CORTRIGHT: I thought it was very good. The only technical flaws that I saw were cosmetic. You know, when the explosion occurred and when they were changing attitude, everything was always shaking and turning violently. You can't make spacecraft move that fast with the little rockets you have on them to change attitude control. That was done for the public.

I don't know about the explosion itself. That certainly could have jarred the guys, and probably did, because that was a big release of fluid. But initially, it was released into a large cavity, and then it blew the side of the Command Module off. But that still probably would have given quite a bit of impetus to move them. But later on, when they were trying to do a final burn behind the moon or something and they were trying to find the sun or something to get an attitude reference, and everything was moving and like this and guys were shaking, that was hokey. But in general, it was very good; I thought very good. I liked it.

But I couldn't stand sitting through what was the original one on the Mercury astronauts? *The Right Stuff*. I wanted to throw up. I walked out halfway through that. That was awful.

DINKEL: So you liked *Apollo 13* a lot better, and Tom Hanks.

CORTRIGHT: The other one was awful. Did you see it?

DINKEL: Yes, sir.

CORTRIGHT: Did you like it?

DINKEL: I was young and impressionable when I saw that.

CORTRIGHT: I have a lot of friends who liked it. I thought it made some very good people look like—I won't use the word in front of a lady, but you know what it is. The only people who looked halfway normal were the astronauts, and they were only halfway normal. They were either chasing skirts, which they did some of, but they looked like super guys. But the scientists looked like jerks, and the doctors looked worse than jerks. It just made fun of people who worked very hard and were very good, and I resented it, so I wouldn't watch it. You don't use that, please.

DINKEL: I found the list at Johnson Space Center of the ten worst things wrong with Apollo 13 by the technogeeks of Johnson Space Center, and one of the ones they came up with—a good one, too—Jim [James A.] Lovell's Corvette, the '65 Corvette that he was driving, had 1995 plates on it in the movie.

CORTRIGHT: That's cute.

DINKEL: Let me ask you a couple of opinion questions, and you can not answer or give me your opinion. It's about, when you instituted those sweeping changes here at Langley when you took over as the Director, how much freedom did a Center Director have then during those years to steer his group in the direction he saw fit? What was the relationship with Headquarters as contrasted to the last twenty years of evolution?

CORTRIGHT: I just had breakfast with a guy who was involved in that on the other side, who didn't like, last Sunday after church. He said, "We always prided ourselves in being totally independent of Headquarters."*

Let me tell you an anecdote on that while I think of it. Jim [James E.] Webb, whom you didn't know, I guess, but he was tough. He was a Marine, big chin, feisty, fighting guy. He hired a guy as assistant deputy something or other up there, a good guy, Earl Hilburn [phonetic]. He called a meeting. He got the press and a lot of us other semi-officials involved in a big press conference in his office, where he introduced Earl. And Tommy Thompson, Director of Langley was there, and when Webb got all done, he looked around the room, waiting for someone to say "Good job" or "Welcome" or something. No one said anything. So he turned to Tommy and he said, "Dr. Thompson, what do you think of this?"

And Tommy looked at him and said—looked down at him. Tommy's a tall guy. He said, "What do you at Langley think of all this?"

He said, "Well, Mr. Webb, Langley's been there for a long time, and Langley will be there for a long time after Headquarters is gone."

At that moment in time, Thompson became one of my all-time heroes. It was really funny. I mean, no one laughed aloud, but it was really funny. Jim, I don't think, liked laughter, that sort of thing.

How did I get off on that subject? Did you ask me whether we had a lot of latitude? We had a lot of latitude, but it wasn't perceived by a lot of the people here as latitude because of the way I did it. I decided, if you can't beat them, join them, sort of thing. Headquarters was in a money crunch mode, and I said, "Let's really work with these guys." Like on Viking, I invited an auditor to come live with us from Washington, and he lived with the project. I said, "I don't ever want to hide things from an auditor. If you get a good auditor, he'll help you stay out of trouble."

I did the same thing with Headquarters. I said, "We're going to show Headquarters everything we're doing down here. We'll have a little dog-and-pony show every year. We'll package everything up nicely with pictures of what we're doing and what the goals are so that they can have total confidence; they know what's going on down here."

A lot of people down here didn't like that, because they had always been in a keep-it-close-to-your-vest mode. But I got to do really everything I wanted to do. The only no's I ever got were, don't work on hypersonics and, after a while, don't … work on a supersonic transport, and I didn't [stop] either. We bootlegged both of them at a reduced level. It wasn't that I'm not a saluter, but the way I got the direction wasn't that intimidating. And I know it was direction. It was politically inspired and not technically inspired. So we were able to bootleg some things.

But they let us get into lots of new areas down here that we hadn't been in. We got into more avionic type work, even though we had an Electronics Research Center up in

Boston, working via MIT there, and I don't know whether it had been closed yet. We built up things like that. We built up computer-aided mathematical research. I'm not going to go into that, but if you read the book by good old what's his name, he lists the various things we did down here. There were quite a few innovative things, and even simpleminded things like a visitor's center, which they let us do. We were able to refurbish the center, the building, rebuild old buildings that were falling down, … put cooling in the shops for the technicians, who were working sometimes in 115-degree temperatures, which is illegal, by the way.

I think I got everything I asked for from Headquarters, but I think many people read that as I was sending this stuff up on a plate and then taking whatever I got. It was not that way. In my opinion, we got a lot done. And considering how much we got for Viking, we got an awful lot for other things, too. And towards the end, you know, we got the transonic research tunnel funded. Are you familiar with that? It runs on gaseous nitrogen boiled off from liquid nitrogen, and it runs at such a low temperature that the Reynolds number on models this big are full scale.

Now, we competed with the Air Force. That was a Langley idea, brought to me by a young, not a young, but a fairly down-in-the-ranks researcher, very bright guy. It was an idea invented by a Britisher maybe twenty years earlier, and this guy tried it in a wooden tunnel. It seemed to work, and so I got him some money to build a real little, a wind tunnel, a serious tunnel, but small. It was proven to work, and we went in competition with the Air Force to do the full-scale Reynolds number tunnel, high Reynolds number tunnel.

They had what was called a Ludwig [phonetic] tube, where you have a gigantic tube … with a diaphragm that's exploded, and high-pressure gas streaks down the tunnel. Right behind the shockwave it's so high pressure it's full-scale Reynolds number on little models,

and it would run for fractions of a second, or a second, and you had to get all your data in that time. We can run here on ours continuously until you run out of the liquid nitrogen. So we won that.

There was a lot done. I'm fairly proud of what the Center accomplished. I think what happened, more than anything, is, I was able to give Langley people who wanted to change direction the opportunity to do it, who wanted to introduce some new work. Langley got pretty heavily into Earth observational work at the time from space and environmentally oriented.

But if I had to be quoted, I'd like to say all I did was try to accommodate Viking and keep Langley moving along with the times, and give the people here an opportunity to do good work, because a Center Director doesn't do anything but set policy and high-level management. The people are the ones who do all the accomplishments. I think I gave them a lot of opportunities. But some people get stepped on. The ones who had almost carte blanche in the shops didn't get it anymore, because they had to fight for their time.

DINKEL: Let me ask you another opinion. Last year, in 1997, the Pathfinder went to Mars and it was an unqualified success, both in the public relations field. A lot of people out there, and John Q. Public, knew that NASA existed again. Did you think it would be so long between the Viking landings and going back to Mars? And the second part of that question is, do you think we're really going to do a manned mission in the next few years, as tentatively scheduled?

CORTRIGHT: Well, on the first part, I guess I didn't think it would be quite that long to go back. One side of my mind says that. The other side of my mind is, we didn't find anything there that made us want to rush back right away. I mean, we found conditions not very conducive to life, and we found no life. We had some fairly sophisticated experiments that could have found life of Earth-type microbes if they existed. There weren't any, that we could find.

The other side of my mind says … that I knew it would be expensive to do anything better than Viking. Goldin has this concept of doing everything cheaply, which is a whole other subject. So I knew to do a bigger than Viking would probably be prohibitive financially. To do something smaller and learn more was a challenge.

I was fascinated by the mission they did, but it didn't carry much. I don't know everything it carried, but I'm told that it had a camera, it had mobility, and it had an alpha-backscatter experiment similar to what we carried on Surveyor. The one we carried was developed by Tony Turkovitch (Professor Turkovitch) and it works with an alpha emitting radioactive material that bombards the surface, and you get interactive radiation back, which has the signature. If it interacts with iron, you get one spike. We found on the moon with that most of the elements that the found in Apollo, in approximately the right proportions.

Now, I shouldn't say, because I don't know whether it had more than that, but if that's all it did was take an alpha-backscatter type experiment and go around and verify the constituents that we found with the Viking a little further away, maybe it did a little better job of identifying the mineralogic—in other words, this rock is something-ite, and it's different than that rock, which was something else ite—that's something. They say that they found

conclusive proof there were rivers. Well, Viking found all those same photographs of what appear to be dry river beds.

I have to raise the question, since I don't really know the answer, how much scientific information they got beyond what we got in Viking. I don't know. But I don't want to pooh-pooh the mission, because I thought it was a brilliantly executed mission. I had a hell of a lot of fun watching it, and I still do sometimes get it up on the Internet and look at the pictures again. But scientifically, I haven't heard the results of what it accomplished.

The other part of your question was man to Mars? I promised myself I wouldn't comment on that, because I'm not involved anymore. All I would say is this. It's a tremendously large and complex mission. We are not sure yet how man can survive at zero gravity for that long, so you might have to provide them with the equivalent of gravity. There are various ways to do that. We still are working with conventional rocket fuels, which means the rocket is either huge or it's assembled from smaller rockets in orbit. They could bring samples back. I don't know how much analysis they could do on the surface there. Probably most of their mission would be to visually, and with some instruments, select samples to bring back. Long, difficult mission, and expensive.

My point of view has been, I think people who talk glibly about doing it, underestimate its cost and the time required, the difficulty, and probably have not thought seriously about bringing those samples back automated as an alternative, cheaper and with less risk. And I would remind anyone who is thinking seriously about it that the Russians have a lunar sample, and they never landed on the moon. They landed an automated spacecraft and picked up a sample and flew it back to Russia. We don't hear so much about that because we're so busy telling ourselves how smart we are. But the Russians did that.

That can be done on Mars. That was looked at, and that would have to be looked at again, unless you're doing it simply to keep the man out in space.

And that's a philosophical question. You've got him in space in the International Space Station [ISS]. I'm sure everyone's wrestling very hard to figure out the most useful things to do with that space station once they get it. It's going to be a huge consumer of money and time, launch vehicles.

You remember when George Bush announced that the three-part space program, three goals—an International Space Station, a manned lunar station, and a manned trip to Mars. Let's just say that the International Space Station is going to take X billions of dollars a year just to feed it. Think of what a manned lunar station would take to feed it, to have escape equipment up there, to get the food and water and oxygen and everything up there to these guys. The preliminary studies that people have done to show how you can extract water and oxygen out of that barren lunar surface—if you could get enough energy to do it. These are very not fanciful—well, they are fanciful, but they're way-out studies that maybe don't have too much substance to them yet. Then that's going to be a huge consumer. And then you're going to go to Mars on top of that?

At the time that Bush said that, we had a gigantic deficit. We still don't have much of a surplus, although the tobacco companies now are quoting a $1.5 trillion surplus. Have you seen that ad?

DINKEL: No, sir.

CORTRIGHT: So if we have a one and a half trillion dollar surplus in the budget, why are we trying to raise the taxes half a billion dollars on tobacco? I think they must have taken the current budget excess and projected it out ten years and say, "Well, there's one-point-some trillion dollars." We don't have anything like one-point-some trillion dollars in a single year. The budget surplus is like $40 billion this year, something like that. I'm not up on all these things. I sort of keep my ears open.

At any rate, that question is best not answered by me, or any guy my age. … It's the people who don't know that it can't be done who do it. And that's a good theme, because when we started this program, most people thought what we said we were going to do couldn't be done, and we weren't so sure we could do it either, including Apollo. But we did it.

Now, you talk about a manned trip to Mars, and I think it's, in the current environment of money and other global problems and the difficulty and the way I see bureaucracy bogging technical developments down, I don't see it happening in the very foreseeable future. We don't seem to be geared to be able to do something like that. That's just an old man talking. I'm sure there are some guys out there saying, "Well, he's an old fogy. We can do it." And maybe they can. God bless them. I hope so.

DINKEL: And there's a bunch of them saying that. I don't how much money they have, but there's a bunch of them saying that.

CORTRIGHT: Yeah. Great. I mean, thank God for people who don't know they can't do something, in all fields. That's why things get done.

DINKEL: Here's an interesting one. The last opinion question I'm going to ask you. But if you were king, what, if any, of the space tests at NASA presently performed could be or should be relegated to private industry, the commercialization aspect? Do you have any thoughts about that?

CORTRIGHT: Not many, but I think commercialization should be all private industry, and NASA's been looking for decades to have some company step forward and want to commercialize space, as far as I know. That's a no-brainer.

Well, the launch vehicles, wasn't there a proposal by a company—and maybe it's going ahead. There are, there are some commercial launch vehicles.

DINKEL: Yes, sir, there are.

CORTRIGHT: That's good. I think that's wonderful. There's no reason whatsoever for NASA to try to hold all these things close to their vest just so they can have their little bureaucracy and keep control. As fast as industry's willing and able to pick it up, where it's not a way-out thing, where the government labs are required, I think they should be allowed to have it.

But let's take going to Mars. Now, suppose Lockheed-Martin came in and said, "We'd like to commercially undertake an manned trip to Mars." Well, you know that's crazy, because no one's going to put up the money. What's the payoff for that? That's only a government-type thing. And when the government starts putting up the money, then I'm a strong believer the government should have a good management team watching it. And my

God, there are decades of evidence to show what happens when you don't watch it, and often when you do; and some of the oftens-when-you-do is government-caused. It's not all the contractors. We have caused a lot of our own problems in terms of cost and time.

So it's a fine balance to get a government/industry team that will work together to get a job done for the least money and the most effectiveness. It's hard to do. I mean, as many projects fail doing that as succeed.

DINKEL: Well, this is my last question. It's a two-part question. Sitting here talking to you today, we've discussed many, many significant, historically significant, things that you've done in your career. Of all the things that you've done, one, what is the thing that you're most proud of; and two, what is the one thing you did that you had the most fun, knowing full well that they might not be the same thing.

CORTRIGHT: I really don't have one thing or one period that I'm most proud of. The only job I ever did that I didn't have too much fun at was with Owens Illinois, because it was out of the field, and I wasn't that good at it. But the other jobs I've done, I've always felt I did a good job and I was rewarded adequately for what I did, and they all succeeded. They all were fun. I don't feel anything was all that special. They all were pretty good and a lot of fun.

DINKEL: Well, good, sir. Thank you very much. That's all I have today. I want to thank you for a really good interview, and I appreciate your time. We'll get this to you to look at for your transcript as soon as we can.

CORTRIGHT: Okay. I'll add one little thing to that last one. I'm just sitting here thinking. I was very proud of the first lunar mission, the Surveyor. I was very proud of Viking. And I'm only citing projects. There were other research-type things that I took some pride in. And I'm sort of proud of the F-117 of Lockheed, even though my contribution to it was primarily from the Office of the President and I didn't have anything to do with starting it. But I was involved, and when I see how it performed in the Middle East and look at it flying, I feel good.

Those were hard years. We worked very hard. But it was worth it. So it's not as much pride as just feeling that effort was worth it in those things I talked about.

DINKEL: Well, that's good. That's a great interview. Thank you again.

CORTRIGHT: That depends on how you use it. [Laughter] …

[End of Interview]