ORAL HISTORY 2 TRANSCRIPT

JOSEPH P. LOFTUS, JR.
INTERVIEWED BY SUMMER CHICK BERGEN
HOUSTON, TEXAS – 27 OCTOBER 2000

BERGEN: Today is October 27, 2000. This oral history with Joe Loftus is being conducted for the Johnson Space Center Oral History Project at the offices of the Signal Corporation in Houston, Texas. The interviewer is Summer Chick Bergen, assisted by Carol Butler.

We're so glad you could be with us today, Mr. Loftus.

LOFTUS: You're welcome.

BERGEN: Why don't we start by giving us some background. I'd like to hear about what you did in the Air Force, but even before that, I was wondering how you—what led you to study psychology, that you got your bachelor's and master's degree in.

LOFTUS: Well, I grew up in Washington, D.C., a small suburb of town called Greenbelt [Maryland], and went to Jesuit High School in Washington, D.C., Gonzaga. When I left there, I went in the seminary for three years. There had been several members of my father's family and my mother's family who had been clergy. But I decided, after three years, that that wasn't for me. So I went to Catholic University [of America] in Washington, D.C., then, to complete my undergraduate work.

At that time the Korean War had a draft on, so in order to avoid the draft, I joined the ROTC [Reserve Officer Training Corps], finished up a bachelor's at Catholic U., and had a fellowship offer at Fordham University [Bronx, New York] and went to Fordham in New York, to pursue graduate work. Since I had only two years of ROTC as an undergraduate, I had to finish the other two years while I was in graduate school.

Then, because I wanted to fly, I had to sort of drop the graduate studies and go on active duty. Went through pilot training at McAllen [Air Force Base] in Texas, and then at Vance Air Force Base up in Enid, Oklahoma. By that time the war was over. When wars end, fighter pilots become a glut on the market, so I was assigned to Dover Air Force Base [Delaware] to be the adjutant in the operations squadron, the squadron that's sort of responsible for all the functions of the base.

I was just going to do that and finish up my Air Force time, but I got a query from the folks at Wright-Patterson Air Force Base [Dayton, Ohio]. They had a number of joint projects between the aeromedical laboratory and the avionics laboratory, so they asked me to come out and chat with them, and I did. They said they'd like to offer me a position, so I said, "That's fine. Let's talk about the position." And in the Air Force, everything is doing by a manning document. So a position is not a vague verbal description; it has an explicit number as well as a description.

So I went back to Dover. Since I was a pilot, I could get a T-33 to fly out to Wright Field and fly back to Dover. So eventually a telex message came in that sort of said, "Would you like to take assignment to Wright Field?"

And I responded that I would accept the position, and I spelled it all out for the manning document. The major who was the personnel officer at McGuire [Fort Dix, New Jersey], which was our headquarters, said, "You can't do that. That's only done for general officers."

I said, "Well, your suspense date is tomorrow and I'm not going to change."

So the thing went through and I got orders, so I went to Wright Field and went into the personnel office to check in, and the fellow that greeted me said, "Am I glad to see you. You're my replacement."

And I said, "No, I'm not. Read the orders."

So that was a useful defensive move, and I did get, in fact, the assignment that I had wanted. Basically I was assigned to a group in human factors in the aeromed lab, but I did most of my time working with the avionics laboratory. The reason is that the kind of psychology I studied is probably not what you would envision. Basically what I was doing is looking at kinesthesis. How do you know where your arm is when it's behind your back? How do you know when a car is handling properly?

So most of the work I was doing involved things like applying servo theory from electrical engineering to the way people function at their interface with machines. That was of some considerable interest to people in the avionics lab, because at that time we were still having a lot of trouble with the control systems on jet aircraft.

So I started doing that kind of work at Wright Field, participated in a lot of the test flights that we were doing for developing Category Three landing procedures, which is landing with only 100 feet of elevation for clouds and 100 feet of visibility on the runway, severe weather kind of landing conditions. And we were doing some studies of how would you control spacecraft, or could people control launch vehicles, and things of that variety.

Because of that, I got involved on the X-15. The X-15 was typical of the X aircraft of that era, that they were joint projects of the NACA [National Advisory Committee for Aeronautics] , the Navy, and the Air Force, and so through that activity I began to get to know quite a few of the people at the NACA, many of whom were later parts of the Space Task Group. So I worked on the Air Force side of trying to get man in space soonest.

Mr. [Dwight D.] Eisenhower decided that he did not want to export the Cold War into space. He wanted a civil space agency. So he, in effect, gave the assignment to the NACA, which became NASA, and that was a fairly big jolt for them, because it meant they had to have a whole host of new skills and large numbers. So through those interfaces, Charlie [Charles J.] Donlan, who was the Deputy Director for Space Task Group, said he'd like to have me, and that went up through Mr. [Hugh L.] Dryden and across to the Air Force, and in due course I got orders assigning me to Space Task Group.

But I did not want to move my family at the time. We had a couple of new children, and I had a nephew that I was raising. It was not a good time to move the family. So I worked out an arrangement that I would commute from Wright Field until such time as a new home for the Space Task Group, which eventually became the Johnson Space Center [Houston, Texas], was designated.

So I'd pick up an airplane Monday morning and go to work, might be at Kennedy [Space Center, Florida], it might be at Langley [Research Center, Hampton, Virginia], it might be at the Draper Lab [Massachusetts Institute of Technology] in Boston or Cambridge, Massachusetts, or it might be at what was then the Lewis Research Center or Glen [Research Center, Cleveland, Ohio], or it might be at McDonnell [Aircraft Corporation] at St. Louis [Missouri], or two or three of those places in the course of a week. So I did a lot of traveling, but it was very exhilarating.

Then when the site was selected here in Houston, at first we were just renting buildings all over the southeast part of Houston, and my first office when I moved down here was what is now Oshman's warehouse at the corner of I-45 and OST [Old Spanish Trail]. So it was a hectic time, but it was fun.

BERGEN: When you first started with the Space Task Group, what were your—what was your original assignment and responsibilities?

LOFTUS: Well, we were a very, very small organization, and I was assigned to what was called the Astronaut and Training Office, and basically worked on such things as the controls and displays and the caution and warning and the development of interface between the crew and the vehicle. We had such things, for example, as how do you decide whether or not to abort. In Mercury we had a launch escape tower which could pull the capsule off of the rocket.

So we spent a lot of time trying to devise the right kind of instrumentation to be able to say that you never abort on a single cue, and preferably what you'd like to have is something that is sensed by an instrument, seconded by something which can be sensed directly by the crewman so that he could confirm that he was not getting an erroneous indication from an instrument. That was a very long and arduous debate, to try and devise the right kind of scheme to do that, but we did it successfully.

BERGEN: Who did you work with to make these decisions?

LOFTUS: Well, there were a whole group of people that would be involved in these kinds of things. Because we were so small, the crew was involved in a great deal of this, and that was not a particularly onerous problem, because I had done a lot of work and flying with Deke [Donald K.] Slayton and [Virgil I.] Gus Grissom when we were at Wright Field.

At Wright Field one of the things I had done was some of the flying in zero-G aircraft, and Slayton on occasion would fly those missions for us. Again, it was partly to understand whether or not in a weightless environment there were significant changes in motor behaviors, but we also did all kinds of other things that were of interest.

One family of experiments I did involved cats, to sort of say to what degree is a cat's reflex for landing on its feet a visual thing and to what degree is it a gravitational thing. So we did experiments with cats that had tails and cats that did not have tails, like the minx cat. My colleagues from those days in the lab have never let me forget that. [Laughter] But it was clearly demonstrated in that family of experiments that it was a visual cue that the cat was using, and cats that had tails could get themselves properly oriented more rapidly than those who did not. So it was an interesting demonstration of the conservation of momentum.

At any rate, this zero-G aircraft was a lot of fun, and we used that then later on for training the crews and eventually we moved the aircraft down here. I guess we're now in the third generation of the aircraft.

I guess one of the other interesting things that I did in the years at Wright Field was a special assignment for General [Bernard A.] Schriever. At that time the economy was good, and when the economy's good and there's no war, it's hard to keep people in the military. So the military was having a great deal of difficulty with that. Over several years there were commissions appointed to advise the Department of Defense [DoD] on what to do and how to do it, to solve this retention problem.

There had been a commission chaired by the chairman of General Motors that had made a report, and when they finished looking at the report, General Schriever said, "It's the lieutenants who don't stay in the Air Force. Why don't we ask the lieutenants." So he recruited a group of sixteen lieutenants. At that time he was the Commander of the Air Research and Development Command, and he was headquartered at Andrews Air Force Base in Washington [D.C]. I was at Wright Field, which was one of his stations, as were other stations such as Electronics Research Center in Cambridge and the laboratories out at Kirtland, New Mexico, and Eglin Air Force Base [Florida] and so on. So in order to have proportional representation, he took one lieutenant from the smallest installations and then proportionally more lieutenants from the larger installations, so that's how, with twelve installations, we wound up with sixteen lieutenants.

His Deputy Chief for Personnel was General Dougherty [phonetic]. So the first day we met, we sat down in the conference room with General Schriever and he explained to us what his concern was, why did people not find the Air Force an attractive career, what could be done about it, and then he told General Dougherty, "Give them anything they want."

So we were then left to ourselves and tried to get ourselves organized. We basically decided that we would look at the problem and several working groups, one on compensation and one on career progression and things of that variety. Then we met with General Dougherty and said, "We don't really understand very much about compensation systems. Have you got somebody who can help us?" And we went through half a dozen subjects on which we needed things.

So the following morning he met with us and said, "Okay, we're going to start by giving you guys a series of briefings." So they had some colonels come over from the Pentagon to give us briefings on the future of the Air Force, what was then, and still is, called Air Force doctrine. They had others come over and give us briefings on how was the Air Force budget structured, how was the Air Force as a whole structured, what was going on in research and development. The way they got us people on compensation was General Dougherty ordered to active duty the vice president for personnel of TransWorld Airlines [TWA] and a vice president of General Electric, and a number of other organizations, so they came and did their reserve tour of duty educating this young group of lieutenants on compensation schemes and how were career progressions managed in General Electric and things of this variety.

So it was an interesting experience to have that kind of talent at your command. We decided that we had to do something to sort of quantify a lot of the observations, so we did what we called twin studies. How did two brothers, twin brothers, one going in the Air Force and making a career of the Air Force, and one going into the Air Force, then leaving to go into some other line of work or, in another case, never going in the service, but progressing in his own profession, and that had some revealing insights, because the general belief was that the primary competitor to the uniformed services was the industry.

Turned out the primary competitor was the civil service. So that was an interesting thing, to sit down and, in effect, do a lifetime income stream for somebody in the military. We did an average Joe, above average, below average case, compared them all. It was very instinctive.

So we wound up concluding that pay was part of the issue, but the largest part of the issue was that people didn't like the job assignments because mostly what would happen is that people would come out of engineering school and they'd want to go do engineering, but they'd be assigned to some project office where what they'd be working on was configuration control paper and change orders that were going out to contractors to do various modifications to equipment. So they weren't getting any real hands-on engineering experience.

The other thing was, the Air Force, because, like all military organizations at that time and still to some degree at this time, sort of believed that it had to train its professionals so that in the event of a war you could expand the numbers very rapidly, so your core, you wanted to have a broad knowledge of everything that the service did. So that meant they'd give you a new assignment every three years or so. Well, that's disruptive to families and for people who believe they were learning an engineering profession. It was disruptive to developing the kind of skills.

So one of the recommendations was that for your first assignment you ought to be doing hands-on engineering and you ought to do it for at least four or five years before you get another assignment, so that you can take the skills that you learned in school and really develop.

So we went off and prepared a report, and at that time you did all your briefing with flip charts on an easel, and that meant that any organization like General Schriever's had an office which could do very fine lettering and graphics of all sorts for this kind of thing, so we prepared a briefing and it had seventy-nine flip charts. We were told by Schriever's aide that he just didn't have time to sit through that kind of thing. And we said we didn't know what to take out.

So we went in and we were scheduled for two hours, and the meeting lasted about four hours, but General Schriever listened to it all and he said, "Well, this is what I was looking for, is what really is it that we have to do to fix the system." And he turned to General Dougherty and he said, "I want you to take this across the river to General Ferguson [phonetic]," who was the DCS [Deputy Chief of Staff] for Personnel for the entire Air Force.

So the following day, we went across the river—the Pentagon is across the river from Andrews—and briefed General Ferguson, and General Ferguson said, "This is very good. I want the old man to hear it." The old man was Curtis [E.] LeMay.

A word on Ferguson. He was sort of an interesting fellow. When NASA was created, he went over to see Dr. Dryden, who was the [NASA Deputy] Administrator, and he said, "I've got 600 engineering graduates this year with ROTC obligations. Any of them that you want to take, I'll relieve them of their service obligations." That was a tremendous infusion of youngsters into NASA that was sort of a class act by Ferguson.

So at any rate, they decided they couldn't take all sixteen of us to see LeMay because it was going to be done in his office, so they took six of us. We went in to see LeMay and set up our easel, went through all the charts. He sat there, never made a word, never cracked a grin or anything, just sat there and puffed on his big cigar. When I finished the briefing, he said, "That's interesting." And he stood up and he walked over to the easel and he flipped all the charts back to the beginning and then he went through them, making comments. Very interesting. Again, it was one of these things, we'd been on his calendar for like an hour, an hour and a half, and four hours later we marched out of his office and in the anteroom, in the corridor outside were full of all these flag-rank officers who were looking at all these young lieutenants, sort of saying, "What's going on?" So that was a rather interesting experience to get exposed to those kind of people at that point in my career.

So at any rate, that was sort of the background as to how I got to NASA. We worked very closely with the McDonnell folks, because we were a small team and because at that time the community was small. I knew a lot of the NACA people from X-15 times, knew a lot of the

McDonnell people from other contacts, so it was very much sort of a team kind of an arrangement.

One of the fellows I worked very closely with at Langley was Al [Alan B.] Kehlet, who was an aerodynamicist who was very involved in this business of trying to figure out how do you do abort criteria and so forth. Another was Dick [Richard R.] Carley, who was involved in a lot of the simulations of the control systems and so forth.

And it was interesting to work with John [F.] Yardley, who was the McDonnell program manager and one of the most impressive individuals I've ever known. When people would be making presentations or talking in meetings about some system or part or what have you, he would say, "Do you mean this?" and he'd give you the drawing number. He had the entire drawing tree committed to memory. Absolutely phenomenal mind. And there were other people that were a big part of the operation—Chuck [Charles W.] Mathews, Chris [Christopher C.] Kraft [Jr.] , Sig [Sigurd A.] Sjoberg. I'm sure you've talked to a lot of these people or heard lots of anecdotes about them.

There was an interesting culture in the NACA, and it was not unlike some of the things in the Air Force, and that is, since the primary product of the NACA was research findings, they had very demanding standards as to how you would put presentations together, what type sizes to use on slides, and how many words could you use on a slide, all of those kinds of things, and they were very demanding in the way they wrote their technical reports in the interest of clarity. So it was an interesting culture in which to work.

They also, like the DoD, had a real premium on being able to make a good presentation. People who made good presentations did well, because it was believed that if you could make a good presentation, it was because you really understood what you were talking about. So it was a way of testing people's knowledge of substance, and it worked.

We had lots of interesting things happen. It was interesting to be preparing for a test launch before we were going to fly people, and have the launch escape tower take off all by itself. That put a little of the fear of God in you. [Laughter]

[Alan B.] Shepard's flight was quite exciting. We obviously regretted that Mr. [Yuri] Gagarin had gotten ahead of him, but we were very pleased with the results of Shepard's flight. Then, of course, we had the problem on Gus' [Grissom's] flight that we lost the hatch and we nearly lost Gus. The reason we nearly lost Gus is that for some reason he had not put up the neck dam in his suit. In the suit, inside the hard cover, we had a rolled-up rubber neck dam, and the notion was that when you're in the suit with the helmet on, you wanted the atmosphere to move freely back and forth between the body area and the helmet.

But when you took the helmet off, particularly if you were in the water, we didn't want to get water in the suit because you couldn't support it. Well, the neck dam was supposed to take care of that, but Gus had not unrolled the neck dam, so when he took off his helmet and the hatch blew, he was taking water into the suit at a great rate. Turns out the helicopter pilot who pulled him out was Dr. Jim [James L.] Lewis, who worked here for many, many years, and just retired recently. So that was a pretty close call.

And I tell you that little story because the next flight, of course, was John [H.] Glenn [Jr.], and after the flight, a number of us were sitting in a conference room, looking at the camera films, and one of the fellows said, "Look. John doesn't have his neck dam up." And about five minutes later, John walked in, and we told him, and he didn't believe us. So we backed the film up and showed him. He just shook his head and walked out of the room. But it's the kind of thing that can happen to you in an operational setting.

When I was at Dover, one of the things we had to do was to check out bogies who were in air defense identification zones. These are zones where civilian aircraft were prohibited. As the case with such things, we'd regularly get intruders and then we'd send somebody out to identify them and chase them out. I went on a flight to do that one night, and I had the nagging feeling that something was wrong and I couldn't remember what it was, but we found a twin-engine Cessna, waved him off, got him out of the area, came back and landed. It was when I undid my seatbelt and harness that I recognized that I had failed to fasten the leg straps on my parachute. I think I lost five pounds right then and there. [Laughter] Just pure cold sweat. So that's why you have checklists and why no matter how often you've done it, you need to follow it.

It was about that time that we had selected Houston [for the site of the Manned Spacecraft Center]. Have you heard all the stories about how that came to be?

BERGEN: I think every time we hear one, we learn something different, so we'd love for you to share that with us.

LOFTUS: Well, at that time the land that is the site and much of the land that is Clear Lake city was all land that was owned by Humble Oil Company, the predecessor of Exxon [Mobil Corporation]. There was a developer who was an imaginative fellow, and he went to Humble and said, "You don't really need the surface of the land. All you want is mineral rights and what have you. So why don't we form a corporation to develop that land." And that became

Friendswood Development Corporation. And they then took a large chunk of the land and gave it to Rice University, because Rice, as a 501(C)3 corporation, an [unclear] institution, could give things to the government that a profit-making corporation couldn't do. So that was part of the Houston package as a bid for the new Manned Spacecraft Center, was that they gave a piece of land via Rice to the federal government as the site.

The city council did a gambit that Texas has an extraterritorial jurisdiction law that says you can annex an area equal to 10 percent of your land area each year and you can bank that for three years, so you could, in one year, annex 30 percent of your present land area.

At that time Houston was in its third year, and as part of their activity to capture the Manned Spacecraft Center, they annexed a strip of land ten feet wide and drove straight south down south Main Street till the edge of Harris County, and then east along the margins of Harris County until they just had enough left to get back to the eastern side of Houston and, in effect, put a fence around the whole southeastern part of Harris County.

Well, Webster and little places like that couldn't argue with them, but they captured a tract of land Pasadena [Texas] could have captured, that is now Armand Bayou. So Pasadena took them to court because all the little places like Nassau Bay and El Lago and what have you could not incorporate because they were now within the extraterritorial jurisdiction of Houston.

So when Pasadena won the lawsuit, Nassau Bay and El Lago and all these places incorporated the following day, because they were now outside Houston's extraterritorial jurisdiction. The only ones who didn't escape were Clear Lake City, because they still were within reach. So it's been an interesting gambit to watch that kind of thing go on.

Houston was very proud of having won the Manned Spacecraft Center, and they had big doings for us down at the coliseum and the Astrodome. The night they opened the Astrodome was a disaster. They had offered free admission to all Boy Scouts and Cub Scouts and what have you. Of course, most of us had kids who were in the Scouts. I started driving towards the Astrodome, and pretty soon all the radio stations were saying, "Everybody please stop, turn around and go home. We've got gridlock." [Laughter] And literally it took maybe three hours to get untangled and get back home because there was such a horde of people all headed for the Astrodome, because nobody had ever seen it, and it was interesting.

Well, while we were at Langley, after Glenn's flight, a number of us were put to work to go look at how would you go to the Moon. I was part of that group that was looking at how would you go to the Moon. There was a fellow at Langley, John [C.] Houbolt, who was an advocate for lunar orbit rendezvous. I got to know John while we were at Langley. It was an interesting scheme. John's lunar module didn't look at all like the one we wound up building, but that wasn't the point. The point is that he was pursuing the strategy of staging to its logical conclusions. So that was an interesting introduction.

We started laying out something that was very, very similar to the command module we eventually used, and as is always the case, we had advisory committees. I can remember sort of a difficult afternoon with an advisory committee that says, "Why are you selecting a three-man crew? Three is a bad number, because two of them will gang up on one of them, and that's sociologically not a good arrangement."

We said, "Two's not enough and we don't have room for four." [Laughter] It was that kind of thing that sort of made you wonder about the value of advisory committees, but part of it is they made you work and made you think things through and recognized that that was the situation that could develop and that you'd better think about that in the way you trained and organized things.

We began moving down here. I came down just after Christmas in 1961, brought my family down, and we bought a house up in Overbrook, lived there until the site got built down here, then I moved down to Nassau Bay. We were spread out in lots of places around Houston, and, of course, that's where we were when [President John F.] Kennedy came through. He visited a number of our facilities, looked at some of the mockups and things that we were doing. That was my first encounter with the Secret Service contingents and the press. The press have no regard for anything. The photographers are particularly bad. They'll knock things over, including you, to get at the picture.

Then, of course, the following day, he [Kennedy] was assassinated in Dallas, so that was a pretty memorable event.

BERGEN: What do you remember about when he first made the commitment to go to the Moon?

LOFTUS: Well, we knew that it could be done, that it was a question of did you have the right kind of resources. You have to remember that World War II really changed the structure of the world. By the end of the war, we were flying bombing raids at 36—38,000 feet in unpressurized aircraft, which meant that you had to develop breathing systems for crewmen and what have you. So that sort of all of the things that you would need to sustain a human being in space, you had learned to do.

The second thing is, is that the military is a training machine. Amateurs think that wars are won by tactics. Professionals know that wars are won by logistics and training. So one of the things that was done is we trained literally tens of thousands of people to be pilots and tens of thousands of people to be mechanics and tens of thousands of people to do scheduling and route management and what have you. So before World War II, there was essentially an embryonic civil aviation, but out of World War II was this tremendous endowment of equipment and people that literally, within years, two years, created the aircraft industry as we know it.

During the war, there were 12,000 DC-3s built. There's still some of them flying in South America. But that meant that after the war, you could get a group of pilots and a group of mechanics and a group of schedulers, and you could set up an airline. And they did. Dozens of them, most of them gone, now embedded in much, much larger airlines, but there must have been, in 1947, maybe fifty airlines in the United States because of this endowment of equipment and people. So that changed the way transportation was done, both air cargo and air passengers.

In effect, that led to the Chicago Convention of 1947, which is the International Civil [Aviation] Organization [ICAO]. It's now part of the United Nations. But that is the forum in which people agree as to what will be the rules of airport operations, flight operations, traffic management, lighting schemes for airports, what have you, and that's where English became the language of aviation worldwide. And that's still how it's done.

So literally every nation in the world belongs, because whether they have an airline or not, they want other airlines to service their country, and that means they have to meet specifications about bearing strengths of runways, what are the heights of the passenger exit doors, and all of the accoutrements that interface the terminal to the airplane. So that was an interesting development out of all of that. And it works well. Nobody tells anybody else what to do; everybody gets together and decides what's in the common interest. That's sort of a diversion, but I had gotten into some of that when I was doing the research at Wright Field on the CAT-3 landings and was very interested in all of that.

So at any rate, we knew how to sustain human life. We had learned how to build pressurized cabins so we could build them for spacecraft. Two-thirds of the atmosphere is below you when you're at 30,000 feet. Half of it's below you when you're at 8,000 feet. So going the last 10 percent out of the atmosphere to go into space was not a particularly significant thing in terms of a lot of the design parameters of the vehicle. The biggest issue was going to be how heavy were you going to be and could you build a launch vehicle big enough to do it.

Well, of course, after World War II, the U.S. has succeeded in capturing most of the German V-2 team from Peenemünde and put them out in Fort Bliss out at El Paso [Texas], but then they wanted to build some Army launched vehicles, so they brought them up to Huntsville, Alabama, to the [Redstone] Army Arsenal there. Wernher [von Braun] had the notion of some really big launch vehicles. I'm not sure whether he and [Sergei Pavlovich] Korolev had ever had any contact with each other, but they both had the notion of multi-engines in the first stage. So Wernher said he could deliver a big launch vehicle, and they did a marvelous job.

We kept trying to get the weight down, and that led to a number of significant things. One of the big debates, of course, that prevailed during those years in Apollo were, should you go Earth orbit rendezvous and build a space station and depart from the space station to go to the Moon, or should you go lunar orbit rendezvous and not spend any time in Earth orbit? That was a very profound kind of decision that had to be made, and the fellows at Marshall [Space Flight Center, Huntsville, Alabama] were developing the Saturn, and we were trying to develop the command and service module and lunar module. All of our weight targets were being exceeded, which meant that he had to keep building a bigger and bigger vehicle or he wouldn't be able to lift everything we had. So there were lots of fairly acrimonious discussions and lots of real learning experiences.

BERGEN: Were you involved in any of that decision-making process?

LOFTUS: I was going to tell you a few stories about that.

BERGEN: Great.

LOFTUS: One of the things that happens after there's a big competition is that some company wins the competition and then it has to staff up to actually do the job. So one of the things that happens is people who are working for the losing corporations may go to work for the government agency that's going to supervise the program or they may go to work for the contractor who won the award. And that's where a lot of the NASA people came from. They came from the [Glen L.] Martin Company in Baltimore [Maryland], which had bid for Apollo and lost to North American [Rockwell Corporation], or they had worked for General Dynamics [Corporation] in San Diego, lost to North American, and rather than go to work for North American, they came to work for NASA. So we had lots of that kind of infusion of people. But that was sort of the second major infusion.

The first major infusion of people for NASA was the Canadians. Shortly after Mercury was started, the Canadian government, under Mr. [Prime Minister John] Diefenbaker, decided that they were too small a nation to compete in building front-line fighter aircraft. At that time they had a company by the name of AVRO, which is a truncation of A.V. Roe [Ltd.], who was one of the early developers of aircraft. They had an aircraft called the [CF-]105, which was undoubtedly at that time the hottest fighter in the world. They had, I think, five airframes, and they had just started their testing when the government said, "No more. Cut them up. Get rid of them. We're out of the business."

So we promptly went up to Canada and hired as many of them as we could, and McDonnell hired a group. So there was a big infusion of Canadians into both NASA and McDonnell and Rockwell. That was sort of an interesting thing because it had some echoes of the development of the jet engine, because Frank Whipple was a young Brit who wanted to develop a jet engine, but the establishment really didn't think there was much in his proposal, so they didn't want to give him any of the good young British engineers, so they gave him a group of these young colonials from Canada. Of course, Whipple was successful in developing the jet engine.

Part of the consequence of that is that those young Canadians went back to Canada and established the Orenda Engine Company with what they had learned. So they became a fairly significant player in the jet engine business.

So in the space business it was much the same way. We got this young bunch of colonials. Of course, many of them became U.S. citizens, though many did not. But they were an interesting group.

Then, of course, when the contractors lost the Apollo bids, again the winners picked up a lot of those folks and so did we. So there's a lot of that kind of cross-fertilization within the community because of the nature of the business.

We started out on the lunar module, and everybody started out with what looked like a sphere, because everybody knows that that's the most efficient shape for containing volume with minimum weight. I can remember numerous sessions where we'd be trying to figure out how we were going to build this sphere and at the same time have fields of view that we would need to do the landing, need to be able to see down, so you'd have to have windows on the bottom. You'd have to dock, you've have to have windows in the top. Pretty soon the whole front half of the thing was windows and window frames.

It was just getting more and more complicated, and so we kept modifying the design. I think before we were finished, we had done something like nineteen different configuration designs. What's impressive about that is that if you go over to Space Center Houston and look at the ascent stage of the lunar module and you've learned how to look at it, you will recognize that it's a replication of the human skull, that it's got two triangular windows and an eyebrow, it's got a nose, it's got a mouth, it's got a hatch in the top. Turned out that all the same considerations that make the skull the way it is were the right way to make a minimum weight, smallest possible window, symmetrically balanced vehicle. So the crew's in front, the equipment's in back, the fuel tanks are on the side so that everything is balanced. Very efficient design structurally and thermally. Small as possible windows so you don't have a big heat load. It took us a long time to get there when the answer had been sitting across the table from us from the beginning. So that was a real learning experience for a lot of us.

We got into all kinds of things. I had a lot of responsibility for the controls and displays in both vehicles, and had a group in the program office that did that. I can remember very carefully writing the language that said, "You shall not design any instrument so that it fails in place." If you have a needle and the needle is supposed to indicate something if the instrument fails, you fail it off-scale so it's out of sight, so that the crewman knows he doesn't have it. We had worked very hard to get all of that language into the specifications.

I can remember discovering, about two years after we started to build the lunar module, that Grumman's instruments all failed in place. I went ballistic. But it was too late to do anything about it, so we had to try and find ways to deal with that.

The evolution of the lunar module is, I think, one of the most interesting stories. I can remember being in Cambridge for some meetings with the Draper Lab people, listening to a radio show in the morning while I was getting dressed and ready. One of the critics was saying that it was demonstrable that NASA really didn't know what they were doing and we really couldn't afford to go to the Moon because they couldn't even afford to provide seats for the crewmen who were going to land on the Moon. [Laughter] So it's interesting, the kinds of perceptions that you have to deal with.

Probably the two biggest system-wide decisions that had to be taken into the program, at least from the prospective that I was involved in, were who is going to be primary for navigation. Were you going to rely on onboard navigation, and would that be primary? Or were you going to rely on ground navigation, and would that be primary and the onboard system would be a backup?

A group of people up at MIT Draper Lab, [J. Halcombe] Hal Laning and Dick [Richard H.] Battin, had developed a scheme for doing onboard navigation, and we had done a lot of work with the Draper Lab people, so we tended to be in favor of an onboard solution. But there was another group of people who felt that with proper Doppler ranging in the S-band signals, that they could do the navigation from the ground and do it more precisely. They felt that by not having to do that on board, you could substantially offload the crew from work that was involved in that task that could be better done, so that you could distribute the workload between the ground and the flight crew more efficiently.

That was a major exercise for several years before we finally resolved that we would make the ground primary and onboard secondary, but we kept working on the onboard system so that if for any reason we lost communication, we could do everything on board. That was an interesting development.

At that time, computing power was very limited, and that was one of the arguments that helped the advocates for a ground-based system prevail, because it didn't matter how big the computer on the ground had to be, you could make it. In those days, the computers on board were small and you had to do all the programming in machine language, which was very cumbersome to do, very cumbersome to check out, and even more cumbersome to change. But the onboard computers that we had had less power than your hand calculators do today. So it was an interesting exercise to develop those controls and displays and the sextants. I spent an awful lot of time working on that with the Draper Lab people, and we had a very good system.

We also had some fairly well-trained crews. On one of the flights, Jim [James A.] Lovell [Jr.] was the navigator, and there was an error in procedure, and he dumped the whole computer, and he had to rebuild it in real time in order to get on with the mission, and he did. That's how well trained he was, very successfully. So that was a big decision.

Another one of the big decisions was, what kind of engines were you going to use in the service module to go to the Moon. We had a whole team of people working on that, but in the end, the fellow who presented the case and won the day was a fellow by the name of Larry [Lawrence G.] Williams, passed away a couple of years ago now. Larry was interesting because he and Caldwell [C.] Johnson were two of the NACA people who came into the program, neither of whom had an engineering degree or a college degree. They had both come through the apprentice program at Langley, and yet they were two of the best engineers we had.

So Larry, in effect, put together the story that said what you want is a pressure-fed engine and a single engine, because that's the most reliable system we can build. That, in effect, dictated the kind of service module we were going to have. It was a major decision, and every virtue that Larry had claimed for it was proven true. Very reliable and it worked very, very well.

So that meant that when we built the service module, it was, in a sense, a cylindrical structure and it was in six segments. Four of the six segments were occupied by propellant tanks, two fuel tanks and two oxidizer tanks, and that left us then with two open bays in which we could put other equipment. And amongst the equipment we put in those bays were the fuel cells and cryogen tanks to produce the electricity.

The Apollo fuel cells were sort of third generation baking cells. Second generation is what we flew in Gemini, and the first generation were just laboratory units. Basically a fuel cell is a membrane which acts as a catalyst so that you put in hydrogen and oxygen, and as they react with the catalyst, you produce electricity and water. They worked very, very well for us.

But, of course, on Apollo 13 we did have the accident, and that's another one of those cases where people and procedural errors caused the problem. We had done what we call a Countdown Demonstration Test where we had gone through all the procedures of loading all the consumables such as the cryogens and propellants and everything, as a dress rehearsal for the mission. Then, of course, when we finished that, we had to de-tank everything in order to get back to an empty configuration, if you will. In order to do that, we had to heat the cryogens to remove them from the tank. The technician left the heater on after all the cryogens were gone, and so the tank kept getting hotter and hotter until eventually somebody did turn off the heater.

But by that time all the insulation had been charred and fallen off. So then when we tanked for the mission, everything was all right because the wires were separated.

And it wasn't until we were in flight and had to do what we called stirring, because the problem is, is with the cryogen, is that it separates into phases and it stratifies, and you don't want to do that, so we had a fan that you could use to stir up the cryogens. And it was when the fan turned on to stir up the cryogens that it created enough motion to bring those wires together and create a spark. At 900 pounds per square inch in pure oxygen, stainless steel burns like a candle, and that's what caused the problem.

Interestingly enough, as part of the training we had done, we had done a simulation, we hadn't postulated why, we just sort of said, "You've got to get home. You've got no power. How are you going to do it?" And we had actually run the simulation of going around the Moon and navigating back to the Earth and what have you. What we hadn't done in the simulation is we hadn't had to deal with the consequences of no power, but we had in the simulation used the lunar module as a lifeboat. We had not anticipated how cold it was going to get. But because of that, we were able to, I think, pull off something close to a miracle.

Turns out I was not here at the time. I had gone off to a school. I was in the program office, and the fuel cells and cryogens were one of my responsibilities. But we had a large General Electric group working for us, and General Electric, as a company, is very, very big on training. Up at Crotonville [phonetic], New York, they have, in effect, a university campus of their own, where they run people through all kinds of training courses. You can literally get a doctorate in engineering or business or many other subjects in that kind of environment.

But in addition to that, the office of the chief engineer ran a special course twice a year up at Saratoga Springs, New York, and the notion of the course was that they would bring in the chief engineers from various facilities throughout the G.E. system and they would bring in one or two customers along with these chief engineers. So they took over the Gideon Putnam, a resort hotel, for the six weeks before the season opened and the six weeks after the season, and would bring in their people, about thirty-five or forty.

During this six weeks, they would bring in people who were truly experts, and the notion was that here you had a group of engineers who'd been out of school twenty, twenty-five years, who were the senior engineers for various facilities or functions, and what you wanted to do was to bring them up to date. You wanted to get them trained in the same way that this year's graduates are being trained so that they would know how to make use of those skills. It was very intense. They sent you programmed instruction texts to read before you got there, and they sent you a basic manual so you could produce programming in BASIC. Then you assembled.

I was one of the guests. The other fellow was from the Navy, a Navy commander from the Polaris Program. So you'd go to class six days a week, and the regimen was, class started at 8:30 every morning and went until about 3:00 in the afternoon, and then you took a couple of hours for exercise and PT [physical training], supper, and then after supper, special lectures. Essentially, a lot of the special lectures were a fellow who was an expert in making computer chips would give a two-hour course on how did he do what he do. So everybody taught at least one evening to the rest of the class.

It was while I was at that course that the Apollo 13 event took place, and so one of the things I did every evening was to give a status report, because we were obviously very interesting. I was talking to our people back here to find out what was going on. I did not sleep very well that week.

But that was truly very educational. Just as an example, one of the courses was polymer chemistry. How do you make nylon, [unclear], things of that variety. The fellow who taught the course was Hans Mark, not the Hans Mark who was with NASA, his father. [Laughter] Who was at Brooklyn Poly [Polytechnic University of New York] and who was a marvelous teacher. And the guy who taught metals was the chief metallurgist for U.S. Steel [Group]. The fellow who taught math was the chairman of the math department at Cal-Berkeley [University of California at Berkley]. In forty hours with him, we went through set theory through tensor analysis, which is like four years' worth of math. [Laughter] But he was really good. You know, most of us, a lot of it was review, but it was quite a challenge.

Had a course called transport theory, which really is essentially all forms of conversions of energy, and that was taught by a fellow by the name of Rasmussan [phonetic], who was the dean of engineering at Rensselaer [Polytechnic Institute (RPI)]. That, of course, was the time when campuses were having a lot of trouble. This fellow had played tackle in college, and they were in the midst of a student riot when he was getting ready to come teach us, and he went through them just like he went through the other line. He just knocked people end over end. [Laughter] Big bull of a man.

One of the courses was decision theory, so they brought Ron Howard in from UCLA to teach that. It was an interesting experience to get exposed to all of these academic stars in this very intense kind of environment.

Of course, at that time computers were not as prolific as they are now, but there was a terminal assigned, an old TTY terminal, as a matter of fact, to every student, and you had to do your homework on the terminal. So that was a fairly useful experience.

When I got back, I immediately became one of the guinea pigs for our folks who wanted to introduce PCs to everybody. Back when I was in graduate school, I couldn't afford a computer, but we had card-sorting machines, so I literally did a forty-by-forty analysis of variance just by sorting card decks over and over and over again. I'd spend nights doing that. And it worked, but it was labor-intensive, to say the least.

At any rate, that was a most interesting course. The other thing you came away with was a brand-new set of textbooks, and it was like a six-foot shelf. So that was good. Enjoyed that.

When I got back from that, I was asked to go and look at what are we going to do—well, back up. Before I had gone there, I had been asked to head up a task force to say, what are we going to do after we land on the Moon successfully the first time? So I spent a couple of years with a team of people, sort of saying, what are we going to do after the first landing?

Basically we came up with a scheme that we called plateaus, where you would A, B, C, D, E, and what have you. Effectively, A were the first test missions in low Earth orbit, and then E was the first successful landing on the Moon, and E2, E3 would be different versions of that because you wouldn't have time to change any of the equipment. But then we would have the F Series, where you would be able to modify things.

So we developed a set of experiments to be carried in the service module to do remote sensing and deploy sub-satellites in lunar orbit, and we developed capability for the lunar module to stay on the surface for seventy-two hours rather than twenty-four, and to land a great deal more weight so that we could carry more complex instruments in a rover vehicle, so that we could cover a larger area around where we landed.

So I worked that definition problem. One of the things we did was to go in and modify the descent stage of the lunar module. When we started out as engineers or want to be, we were all too optimistic, but that meant that the basic lunar module descent design had originally been spherical tanks. When the weight of the vehicle grew and we needed more propellant, cut the spheres in half and put in the cylindrical section. So that was the configuration in which we landed on the Moon.

Well, now we had a box structure that was the descent stage, so we had a fixed volume in which to work. So how were we going to get more propellant? So we went in and we took off those hemisphere end domes and put in elliptical end domes, and that way we could carry more propellant. So we stretched the cylinder a little bit till we took up all the possible space and changed the end dome from a spherical section to an elliptical section, and got the additional propellant to land the additional supplies, the batteries, the rover, and everything else, so that we could stay on the Moon for seventy-two hours. That was a fun exercise.

BERGEN: If we could pause for a moment and let Carol change out our tape.

LOFTUS: Sure. [Tape change]

BERGEN: Okay. Would you like to continue telling us about your extending the mission, what was necessary to extend the missions?

LOFTUS: Well, to increase the landed weight capacity, we had gone to elliptical end domes on the propellant tank in order to increase the capacity. That was successful. That was the configuration in which we did the last three landings on the Moon.

But let's back up a little bit. We talked about the fact that there was this debate about lunar orbit rendezvous versus Earth orbit rendezvous. JSC [Johnson Space Center], both by choice and assignment, was the advocate for the lunar orbit rendezvous. Marshall [Space Flight Center, Huntsville, Alabama], both by choice and by assignment, were the advocates for Earth orbit rendezvous. There were many, many working groups and studies and what have you.

The denouement came at a meeting in Huntsville which was sort of an all-day meeting in their big conference room up on the tenth floor. Marshall presented the case for Earth orbit rendezvous, and JSC presented the case for lunar orbit rendezvous. Then there was a small hiatus. Then Wernher von Braun said, "We're going to go lunar orbit rendezvous. We don't have time to do it any other way." And the Marshall guys were crushed.

I guess at the time I thought Wernher was making a decision in real time, as a consequence of what he had heard in that meeting and over several earlier meetings. I later discovered that he had already written out that speech two days before. [Laughter] So you never quite know how these things are going to get done. I think I may have located a copy of it and I may be able to get that. I'm trying to get a copy of that because that was a memorable event.

It really was the only way to go, because we wouldn't have time to build anything bigger than a Saturn V. That was already a challenge. It had another profound effect, which I don't think we had really paid enough attention to when we were doing these studies, and that is, it meant that you could have a single very, very clean interface. So we literally wound up with a single umbilical. I don't have in my head right now the exact number of wires, but it was maybe like 600 wires that went between the command and service module and lunar module and the Saturn V.

That was a complex enough interface, but it was so much simpler than anything else would have been, that it really made many, many great efficiencies because it meant then that Marshall could make changes to the elements of the Saturn V, we could make changes to the elements of our part of the stack, and they would not domino back and forth between the two organizations. So that was a profoundly significant decision. It has lots of interesting corollaries.

I'm sure you've seen the Saturn V over on the grounds, and when you look at those big F-1 engines, you wouldn't realize that there's about 50 pounds of gold in each one of them. Actually, it was used to solder the recirculation tubes to the engine bell, and the reason you use gold is, (A), it doesn't corrode, and, (B), it is the most efficient thermal transfer medium that you could use. There were several other parts that were made out of gold because of those properties. I can remember one occasion some newspaperman asked Dr. von Braun why he made it out of gold, and he said, "Well, it's the best way I can do it, and performance is the object and it doesn't really cost that much." And he was right, because essentially we were getting it from Fort Knox [Kentucky]. So it worked well.

It was a major decision and it also set up one of the neater kinds of orbital mechanics problems, because essentially what that meant is that we had to back everything up from where we wanted to land on the Moon, and that was a fairly significant point because that set the time and day of the launch, because you'd go into orbit and you'd go around the Earth, and then when you were on the opposite side of the Earth from the Moon, where the Moon would be three days later, that's when you began your burn to go to the Moon. So the Moon was a moving target and you had to lead it by three days. Once you had made that translunar injection, you were pretty much committed.

So for the next two and a half days, basically we just rolled the vehicle to keep the thermal balance, because thermal considerations are to spacecraft what aerodynamic considerations are to airplanes; they design everything. When you're in orbit around the Earth, the part that's looking at the Earth is 70 degrees. The part that's looking at the sun is +250 degrees, and the part that's looking at deep space is -250 degrees. So thermal balance is a major design consideration in any spacecraft, and that was how we handled it going out to the Moon, was barbecue.

Site selection for landings was a really major issue. The operators and safety people, of course, wanted a place that was big and flat, and the geologists, of course, wanted something that was much more interesting than that. So there was lots and lots of debate about where do you land. We were doing all kinds of planning. One of the things was, is that if you couldn't go on the planned launch date, could you recycle and go two days later or three days later and what have you.

In the midst of all of those debates, I created a bit of a riot because I said, "If you don't go at the planned landing time, you can't go to that same place. You have to go somewhere further east on the Moon, because the lunar surface is retro reflective." If the sunline is below you as you're approaching a point on the Moon, you get good definition; you can see clearly. If the sunline to the point you're going on the Moon is above you, it's like driving into a heavy fog with your high beams on; you can't see anything. So if you pick a point on the Moon, there is one day every twenty-eight days when you can go. Otherwise, you have to go someplace further east.

Well, that, of course, created a real riot, because that meant you'd have to train for multiple landing sites, you'd have to do all kinds of things to assure that you could go at the time you had to go, and what have you. So people were very reluctant to accept that. I spent one of the most strenuous afternoons of my life in front of the CCB [Change Control Board] with George [M.] Low, explaining why this was so. And eventually he said, "Okay. That's the way it is, that's the way it is."

BERGEN: How did you come upon that information? Was that something you had been assigned to look at?

LOFTUS: No, but in some of my Air Force work I had done a lot of work in optics. One of the problems you have in high-altitude aviation is what's called empty-field myopia. Perhaps the easiest way to explain it is to sort of say if you look at something outside, you can look through the screen or through the window and it doesn't show in your field of view because you focus on the object and the distance that you want to look at. But you can also look at the window or the screen and lose the object that's in the far field. That's one of the problems that happens to pilots at high altitude, particularly when you're on a fighter sortie and you're looking for enemy airplanes; you've looking into an empty field. So, quite unconsciously, your eyes can come back and focus on the wind screen or what have you so that even if there is a bogie out there, you won't see it, because you're not focused property. You're suffering from empty-field myopia.

So I had been interested in all of these kinds of phenomena. During Mercury we had done a lot of study of the various astronomical phenomena that you might be able to see when you were out of the Earth's atmosphere, things like gaganshine [phonetic], which is the portion of the dust in the solar system that when you don't have all of the glow from the sky, you can see. So I knew from the studies that our folks had been doing, that the lunar surface had been bombarded for years by the solar wind and by various other phenomenon, and so that the surface was powdery. When we landed Surveyor on the Moon, I went out and spent a couple of weeks at JPL [Jet Propulsion Laboratory, Pasadena, California], studying all of the views from that camera, and ye verily, it was retro reflective as you would anticipate from that kind of a surface.

So, yes, it was part of my job in the sense that I was to sort of worry about the crew's interface to the system, and obviously that meant such things as what's he going to see when he looks out the window and what cues is he going to need and how can he deal with that. So it was a combination of all those things that did it. But it had a major effect on the way we plan missions.

BERGEN: Certainly.

LOFTUS: It also had a major effect on how steep the landing approach had to be, because a typical aircraft landing approach is about two and a half degrees, maybe a little less, and the lunar landing approach to the landing site was fourteen degrees. If you were in the cockpit and watching that landing, it would feel like you were going straight down. One of the hardest things to learn when you're a fighter pilot is how steep you have to dive to deliver bombs.

You're going down at 60 degrees and it feels like you're going down at 90. It's a very difficult kind of a perception.

So part of the problem was, is you didn't want to make the crew have to deal with a steeper approach than was necessary, and we said it can't be steeper than 20 degrees because it distorts the perception and it gives you too little time to react to things. On the other hand, you've got to be above the sunline, so we've decided that ideally I would like to have the sunline and the landing site be nine degrees. That was high enough that I could begin to get some good definition, see rocks and terrain features and so on, and low enough that I could still be above it and not have too steep an approach. So that was a major consideration.

Then, of course, it backs into sort of saying, okay now, what does the shape of the window have to be so that you can see the right things when those are the geometry of what you're doing.

BERGEN: We have just a couple of minutes left. I wanted to ask you what did you think when you saw the first videos of the first lunar landing.

LOFTUS: Well, I thought it was pretty spectacular. That was a pretty thrilling event. As Neil [A. Armstrong] was going in, everybody was distracted by the fact that we were getting computer alarms, and everybody was sort of saying, "Can we proceed? Do we have to abort?" And there was a boulder field that hadn't showed up in the imagery we had. So Neil was extending the landing and fuel was going down.

There was a young man, Steve [Stephen G.] Bales, who said, "Don't worry about the computer alarm. I know exactly what's going on. It's not a problem. Proceed with the landing." That was a guts call. [Laughter]

Neil did get it down. There wasn't a lot of fuel left. I had been sweating, like everybody else. So it was an event and, ye verily, in all the photographs they brought back, you can see the retro reflectivity. [Laughter] So I was pleased to be vindicated. It was a real thrill.

But I think, in a sense, the Apollo 8, I think, was a more daring event for me. That was one of those decisions that you could never get a committee to make. George Low, in effect, said, "Why don't we do that," and he was saying it in such way that said, "We're going to do that unless you can prove to me there's some reason we shouldn't." And he talked with a number of people, and I was in some of those conversations, and we made the decision to go do it.

It was a real event to go into orbit around the Moon the first time. To give you some sense of it, imagine you're going down the highway at 80 miles an hour and you can see a train begin to cross the highway, and you keep going 80 miles an hour and the train keeps crossing the highway. You wonder, is the train going to be gone when I get there, or am I going to hit it? Well, going into orbit around the Moon means that you're going to pass just five feet behind the train. [Laughter] So you can't flinch. That was a fairly profound one.

I guess another one of the Apollo things that comes to mind is we decided that we had to have EVA activities to be able to get to the lunar module from the outside if we couldn't go through the tunnel. I had designed a set of handrails to mount on the various places, and Max [Maxime A.] Faget and I had a dog fight because Max didn't want those handrails on the command module because he was afraid they were going to foul the shroud lines on the parachute. So we had to run a whole series of tests to satisfy Max that we had designed them with all the proper angularity to keep the lines from fouling, and it worked. I've still got one of the original handholds that we used for those tests.

BERGEN: It is 3:30, so we don't want to keep you any longer. That's the time you allotted us. We thank you so much for sharing with us today. We appreciated it.

LOFTUS: Okay.

[End of Interview]