

## ORAL HISTORY 3 TRANSCRIPT

JOSEPH P. LOFTUS, JR.  
INTERVIEWED BY SUMMER CHICK BERGEN  
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BERGEN: Today is November 8, 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 Kevin Rusnak.

We're glad you came back to speak with us again. We talked up through Apollo last time, and this session I think we wanted to look at your work in long-range planning. So why don't you start by telling us how you got involved in that area and what were sort of the first things that you worked on.

LOFTUS: I guess there's one thing I'd like to mention about Apollo before we leave that. One of the real challenges was building the simulators that we were going to use for the command and service module [CSM] and for the lunar module [LM]. It was right at the time when we were transitioning from analog to digital computers. I had the assignment to acquire the simulators, and I went to Wright Field [Wright-Patterson Air Force Base, Ohio] to get a guy I had worked with there by the name of Bill Geckler [phonetic]. He's still in the area, and he might be worth talking to. We held a competition, and we had a protest. I had to go mop my notebooks and go up and see George M. Low to deal with the protest, and that went away.

But the big significant thing is that we were building the simulators essentially on the same schedule we were building the flight software and the flight computers, and so it was a

very difficult question. Normally what you would do is you would buy a flight computer and you would imbed it in a simulator and simulate its environment, but we couldn't do that because there wouldn't be any flight computers.

So a young man by the name of Jim [James L.] Raney built an emulator of the flight computer and its software, and that was the most significant accomplishment because it really allowed us to do the training and to train well. The reason we had gone digital was that we had such a scale factor when you're dealing with going from Earth to orbit and to the Moon, you have to be able to deal with distances that are orders of magnitude. That's quite different from an aircraft simulator. I just thought after our conversation last time, that Raney's accomplishment was not widely understood, not even widely known, but was most significant.

When we were landing on the Moon, I had the assignment to go look at what we would do for the F series missions and then the J series. So when we had laid out the plans for those missions and made the modifications to the vehicle, I was sort of already doing advanced mission planning. At that time it was desired to look more formally at what we were going to do as we went ahead, because the things that we had currently on the books were Skylab and ASTP [Apollo Soyuz Test Project]. And then the question would be, what followed that?

One of the things we looked at was doing a Skylab recovery mission with the [Space] Shuttle, because we had abandoned Skylab without a full appreciation of what that meant. We were very fortunate that when it reentered, it mostly landed in the Indian Ocean and the outback in Australia. But to give you some idea of the hazard, we recovered 26 metric tons. That's why one of the reasons that I'm embarked on right now is the reentry of *Mir*, because

that's 135 metric tons. So we want to very carefully put that in the South Pacific. We've been working with the Russians to do that. I've been working with a number of the DoD [Department of Defense] people to do an observation campaign so that we can better understand how these things break up and where the pieces really go.

We just finished doing that for the Gamma Ray Observatory [GRO]. That was the heaviest payload we ever flew in the Shuttle, and it was 14 metric tons. We brought it down the night of June 4<sup>th</sup> [2000], and five and a half metric tons made it to the surface. So that was a controlled entry, and it was probably the best observation campaign I've ever run. Everything worked. We got good telescopic views from Maui [Hawaii]. We got good radar data from Maui. We had an aircraft on station, and all of its instruments worked, everything went tickety-boo, and we got really good data.

But at any rate, we were looking at things like what do you do about Skylab, and we looked at a follow-on with ASTP where we would maybe take a Shuttle to *Salyut* so we had some predecessors for what later became the Phase 1 [International] Space Station program which we called Shuttle-*Mir*.

We were looking at station, and, as a matter of fact, all of the momentum was in station. We had contracts at both Marshall [Space Flight Center, Huntsville, Alabama] and JSC [Johnson Space Center, Houston, Texas] with the contractors doing Phase B studies, but the more we studied the station, the more we came to realize that you can't have an effective station if you don't have effective transportation. We also began to realize that things like "Big Gemini" and "Big Dumb Boosters" and other fashionable concepts all had serious shortcomings. We started out trying to look at a fully reusable system, and it just proved beyond what we had in the way of technology.

One observation is fairly significant. We couldn't have built the Shuttle had we not had the experience and the technology base of Apollo. Building the Shuttle main engines would have just been too big a stretch if we hadn't built the F-1s for Saturn V. And there were similar things. We had started using fuel cells in Gemini. These were vacant cells. They use a catalytic membrane to mix hydrogen and oxygen and create electricity and water. The fuel cells we developed for the Shuttle had five times the power, five times the life, and one-third the weight. So there were those kinds of significant technology developments that went into making the Shuttle feasible.

The Shuttle tiles were an interesting development. They had originally been developed as a material to make a radar transparent window in an aircraft. They were very light and they were thermally very efficient. We decided that that was the way to go, that it was the only way we could make the weight. If we had gone with a metallic TPS [thermal protection system], it would have been too heavy. We knew that we had a problem in terms of learning how to match this brittle ceramic with a flexible aluminum airplane. We had actually bought and refurbished a DC-3 to use as a test article to train ourselves how to do that.

That's when the first of the major budget crunches came in [James E.] Carter's administration due to the war in Southeast Asia. So the only major subcontract that we did not have let at that time was the contract with Lockheed [Aircraft Corporation] for the thermal protection system. So while we knew it was a long pole in the tent and one of the major technology obstacles we had to overcome, we weren't able to work on that part of the program for several years.

That showed up, of course, then in '81 when we were ferrying the [orbiter] vehicle from California to Florida, and we were losing tiles and the tiling job was incomplete. It wasn't because we were dumb; it was because that was the only contract we hadn't let, so it was the obvious place to conserve resources.

The development of the Shuttle was sort of interesting. One of the things that we had started doing in advanced studies was to look at some of the things that had been significant constraints in Apollo. In Apollo we had done all the programming in machine language. That made it very, very difficult to verify and it made it very, very difficult to change.

So one of the things we embarked upon, Jack [John R.] Garman and I and a couple of others from Draper Lab [Massachusetts Institute of Technology, Cambridge, Massachusetts], was the development of a higher-order language that could be used for programming. We called it HAL, and we said that stood for Houston Aerospace Language. But if you're familiar with [Arthur C. Clarke's] "2001," you know that's IBM minus 1. So it's one of the standing jokes in the community. But that turned out to be a very, very successful effort. It enabled us to do the programming for the Shuttle in a most efficient way.

The other thing we did that made things possible is that we made special modifications to the computers so that we could run them one cycle at a time, so if we're having trouble with bugs in the code or what have you, we had very powerful diagnostic instruments to go in and clean up that kind of problem. That was probably one of the major accomplishments in that area.

What it brings to my mind is the fact that there are two things that characterize JSC as a space center. You go to Langley [Research Center, Hampton, Virginia] or to Ames [Research Center, Mountain View, California], you see all these big wind tunnel buildings,

and wind tunnels are what aerodynamics is all about. Here what you find are all sorts of vacuum chambers. The reason you find all the vacuum chambers is that thermal balance is to spacecraft design what aerodynamics is to aircraft design.

The other thing is that our flights are rare and short, so what you find is that we do an enormous amount of analysis. If you really think about it, JSC is a very large software factory. We do all the software to animate all the displays and controls of the Mission Control Center. We do all the software to animate the simulators. We do all kinds of software in order to do all the thermal analyses for all the various attitudes and orientations of the Shuttle and all the various configurations of the payload. So it's a very large analytic effort, and most people don't think of JSC of being that kind of a software factory.

But it was for that reason, for example, that when the DoD was developing ADA [phonetic], which was a software code, we were one of the beta sites for that kind of testing. So it's, I think, a fairly important thing to recognize how much we do.

It turns out a few years ago we had to build a second heating, ventilating, and air-conditioning plant and put it on the eastside of the campus. The original one's on the westside of the campus. The reason we had to do that is because of all the computers that we had brought on site, and every one of those things sits there and generates as much heat as a person does.

To put that in perspective for you, we have more computers than we have people, because, in several cases, people not only have their personal [computer,] PC for routine work, but they have workstations which are Silicon Graphics [Inc.] or Hewlett-Packard [Company] or others kinds of more powerful machines because of the kind of analytic work they do. I think that's a facet of the Center that is not widely appreciated.

It was sort of interesting to sort of say what were we doing when we were doing two or three Apollo flights a year and we were doing it all with card decks and big central processors. It turns out we couldn't do the Shuttle missions if we were doing them the way we did Apollo, because in some ways it's more complex to fly an orbital mission in Earth orbit than it is to fly to the Moon.

Mission planning for a flight to the Moon is fairly straightforward because you've got the trajectory and it acts like a clothesline and you just hang everything in its proper place. When you're trying to do things in Earth orbit, you have a much more complex situation in terms of scheduling activities. So all this computational power is what makes it possible for us to do three or four times as many flights as we could do in Apollo because we can do with one or two engineers in a couple of weeks what it took us several months and a whole roomful of engineers to do during Apollo.

So a lot of the things we were doing in the advanced mission planning was recognizing that this computer revolution was upon us and trying to figure out how to exploit that so that we could do all the things we're doing today.

BERGEN: When I was looking at some of our research, I noticed somewhere it stated that you helped to transition JSC from a research and development phase in Shuttle to the operational mode. What was involved in that?

LOFTUS: Well, we call the Shuttle operational, but that's capricious, maybe. The question was is what would you really have to do in terms of instrumentation and flight execution in the early flights of the Shuttle to, in effect, explore your performance envelope and define

that you had in fact demonstrated the adequacy of your design so that you could go on to more productive activities.

So the original vehicles had large amounts of instrumentation. Some of it was straightforward kinds of things, but it took a lot of weight. My recollection is that we had about 15,000 pounds of instrumentation in [OV-]101 [*Enterprise*]. These were all the things you'd expect to measure. You'd expect to measure structural vibrations, you'd expect to measure temperatures. In order to verify that the vents in the payload bay were working, you'd want to be able to measure flows.

One of the modifications we made was to put a special nose cap on the vehicle, which acted like a cue ball, an instrument that we had used on the X-15 and other experimental aircraft, where you could study the aerodynamic flow as it approached the vehicle. So by measuring the stagnation point on the nose, you could understand what the thermal flows were going to be and what the airflow was going to be over the vehicle. There were a number of significant questions that we had. One of the questions was would the finish that we had on the tiles, which is a boro-silicate glass called a reaction cured glass, was whether it would have a catalytic reaction, and that would greatly influence the kind of heating loads that we would have to look at.

Another major question was, how laminar would the flow be over the vehicle, because that made a lot of difference in the event that you lost a tile. If the flow was laminar, then it would flow over the opening and you would not have a problem. If it were turbulent, you could get heating and maybe burn through the structure. It turned out to be neither catalytic nor turbulent, so the vehicle tolerates the loss of a tile pretty well.

As part of that, we equipped the airplane with a special set of instruments to complement the qualification instrumentations that were called the orbit experiments, in which case we configured it the way we would an experimental airplane. One of the things we did was, up on top of the vertical stabilizer we had imagers and cameras that could look down at the wing and the rest of the vehicle and observe the aerodynamic flows and the corona during entry, things of that variety.

It had been a long time since we had had an airplane-type thing to qualify, and that was a fairly significant point, because in Mercury, Gemini, and Apollo, we, in effect, flew the vehicle unmanned before we flew it manned, and that meant that we had scars in the vehicles that made a lot of us uncomfortable because they had failure modes that, had we not had to fly unmanned to do the qualification, we wouldn't have had those junctures in the various subsystems. So the decision was made to fly the Shuttle manned the first time. So that was a very significant kind of a decision.

We did what we could with the ALT, the approach and landing tests, where we used the 747 to carry it [the orbiter] to altitude and then drop it so that it would land. To put that one in perspective for you, what we were copying was a scheme the Germans had used back in World War I. So it wasn't totally original, but it was effective.

One of the things we did at that time was a small group of us went out and spent a couple of days at Redmond, Washington, with The Boeing [Company] people to understand how they were doing their flight tests, because they were just beginning all of their flight-test program on what is now the 757, 767, 777 family of aircraft.

It was a fairly impressive difference in the sense that we had one vehicle instrumented. They had four different airplanes in each series, all instrumented so that they

could do different parts of the flight tests, because, obviously, in their case, time was money. The sooner they got it over and got the vehicle certificated, the sooner they could start selling them. So they had a very sophisticated flight-test operation where they take off out of Renton [Washington] or SeaTac [Seattle Tacoma International Airport] and telemeter all the data back to air flight-test facility in Seattle [Washington] and be able to do all their analysis of the data and turn it all around within about seventy-two hours. So they had a big flight test operation, very useful. It was sort of interesting, because we were sitting at lunch one day and talking about, you know, how much wind tunnel time they had and how much wind tunnel time we had, and they had about 15,000 hours on their airframe, and we had something close to 100,000 hours at that point. But that's the kind of thing we were forced to do because of the limited opportunities to do flight experiments.

At any rate, the instrumentation worked pretty well. The vehicle design proved to be even more conservative than we thought. So after four flights, we satisfied ourselves that we had most of the data we needed, and we took the instrumentation out and began to do more operational flying in terms of trying to take care of various classes of payloads.

One of the things that had led us to go to the Shuttle and back away from the Station was a recognition that we could do two things with the Shuttle. One, we could fly something like Spacelab and in a sortie have a short-term temporary space station. The second thing we realized is that, typical of an aircraft, we had more capability going uphill than we did coming down. This is fairly typical. If you think about an airplane on a runway with a full fuel load, it's got a lot of stress in it, but it's going from zero to 160 to get to takeoff speed. So the stresses are not too bad.

On the other hand, you can't land with a full fuel load because now you're coming in at 180 knots and you're touching down, and that puts a great deal of stress on the vehicle, so you've got to get rid of all that weight before you can land. Well, the Shuttle has the same thing. So the notion that we were trying to foster was to find a combination of sortie payloads and deliverable payloads, so that you would make best use of the vehicle's capability. You could take off and you could launch the deliverable payload, and then you could spend the remainder of the time working on the sortie payload. That way, you'd make the best use of your resources.

That turned out to be extremely difficult to do, difficult largely because of organizational interfaces, not because of the physics of the problem. But you got into the situation that the Spacelab people, being European, wanted to have a vehicle of their own that was fully capable, so they didn't want to depend on Shuttle computers and other Shuttle capabilities. They wanted to develop those capabilities for the benefit of their own experience. That made a very complex set of interfaces and made it very difficult to do this optimal scheduling, because you found that they had captured all of the interfaces and so there were none left for the deliverable payload. So it's a perfect illustration of how your organizational structure can dictate what you physically wind up with. So that was an interesting set of studies.

It was along about that time that we really began to get serious about some of the orbital debris things. I had gone off to school in '75-'76, sort of in the middle of the Shuttle development activity. They had sent me to Stanford [University, Palo Alto, California] as a Sloan Fellow in the graduate school of business, and that really came out of the fact that I had been asked to go off and look at how would you set up a pricing scheme for the use of

the Shuttle. I got rather interested in that as a business problem and had given several briefings to the administrator and the deputies on how you would go about approaching that problem. So I wound up going to school for a year, a good school year, really enjoyed it.

When I came back, I picked up the advanced studies again. During my absence, Jerry C. Bostick had done that. A young man came to see me by the name of Don [Donald J.] Kessler. He was a young flight controller and he had been working with an analyst up at Cheyenne Mountain [North American Aerospace Defense Command, Colorado Springs, Colorado] by the name of John Gabbard [phonetic]. They had observed that we were having explosions of upper stages, and these created large amounts of debris, and this was going on in altitudes above where the Shuttle would be flying, which meant that you would have debris raining down on the Shuttle. So we got interested in that problem.

I was active in the AIAA [American Institute of Aeronautics and Astronautics]. I was on the Space Systems Technical Committee. Then when we decided that we needed to have a Space Transportation Technical Committee, I was the original chair of that committee. So we began to work with the various launch vehicles. The first one was Delta, because we observed that the Delta upper stage was exploding with some considerable regularity, sometimes thirty days after launch, sometimes three years, in one case, twenty-seven years. We couldn't understand that, so we spent some time with the Delta people out at Huntington Beach [California] sort of saying, you know, what can cause this? We concluded that there were a number of mechanisms by which you could have either oxidizer or fuel migrate into a vent line where it could mix with its component part, or you could have failure of the common bulkhead.

We decided that probably the best was to fix that was after you had delivered the payload and done a contact avoidance maneuver so that you didn't have the stage bumping into the payload subsequently, then you could turn it and burn it to depletion. So we adopted a policy and a practice of getting rid of all stored energy at end of mission. Since we've been doing that, we've no further explosions of Delta stages.

We continue to have a number of explosions of various stages, largely from new designs or new operators. In one case, we've got a continuing problem. The Russians, in their TM stage that's operated by Energia, it is often used as the upper stage on Proton as a multiple burn capability. They can burn it as many as ten times. It's a LOX [liquid oxygen]-kerosene system. So they have to have some kind of an ullage motor, so they have a small motor called Saas [phonetic]. It's about the size of a fifty-gallon drum, and basically it's a very simple mechanism. It has an engine in one end, and it has two bladders in the middle, and then you've got your M204 and your MMH. The nitrogen tank pressurizes between the two bladders to expel the propellants. When they have finished their last LH [liquid hydrogen] burn, they jettison. Well, obviously you have a potential for explosion because you've got this highly stressed membrane between these two hyperbolic components, and so they explode with great regularity.

It went on for years before we detected it, because the Russians used this thing originally for their Molniya orbit. Well, that meant that they were low in the southern hemisphere and high in the northern hemisphere, and we can't see small things that are high in the northern hemisphere, and we have no observation assets in the southern hemisphere. So it was years before we understood this. Then once we knew how to look for it, we could find it, and once we found it, we had conversations with the Russians, and they were going to

set out to fix it. But by the time they were getting around to it, the Russian economy collapsed, so they're just using up those that they have but they aren't building any more.

At any rate, we started out after the Delta experience trying to do some consciousness-raising. I went to a meeting in Taiwan to talk about launch vehicles. On the way, I decided to stop in Japan and visit with the Japanese NASDA [National Space Development Agency (of Japan)] folks because the N-1 vehicle, which they were then operating, was a Delta vehicle built on license from the United States from McDonnell-Douglas [Corporation]. So we explained to them what we had done and why and how, and they made the same modifications to their practices and ceased having explosions.

We, working in the AIAA, wrote a position paper that sort of said, you know, "We have to protect space as an environment if we're going to operate there." That led to reviews by the NASA Advisory Committee and the Defense Science Board, and eventually we directed by the National Security Council at what was called the Interagency Group Space to develop a position paper for the United States. We did that in '88. It was published in '89.

The Security Council directed us to go talk to all the spacefaring nations, so we went to Europe and talked with the ESA [European Space Agency] folks. In November of '86, we had a major explosive event. The upper stage of flight 16 of the Ariane exploded. It had been used to launch the original Spot Image spacecraft, which was their Earth-sensing spacecraft—produced like 700 pieces [of debris]. It turned out that I got the word of that in the morning when I knew that the director general of ESA was going to be visiting with Dr. [James C.] Fletcher that afternoon.

So I called Dr. Fletcher's office and explained to him that he might want to tell the DG [Director General] that he had a problem. He did, and I was asked to go work with the

ESA folks and the Ariane folks on modifications to the Ariane to eliminate this kind of explosions, and did that. Very competent group of people.

So we were directed to go talk to all the spacefaring nations. So we went to Russia and sat down with the folks there. That was really one of the first things that began to ease up after the earlier hostilities where we had sort of backed away from all the Russian contacts. We spent a week in Moscow at what is now Kralyev [phonetic] at CMCC, the Mission Control Center there, and worked with people from their space surveillance organizations and their vehicle design people, and drafted protocols to document our discussions and what have you.

We've been meeting with them regularly ever since. So we wound up starting with bilateral meetings with NASA and ESA, and then NASA and NASDA and then NASA and the Russians. By '93, this was just eating our lunch. We couldn't go to so many meetings and get any work done, so we decided to make it a multilateral operation. So our first multilateral was in '93 at Darmstadt, which is the European Space Operations Center [ESOC] in Germany. We had the Russians and the Japanese and the Europeans and ourselves. We called ourselves the Interagency Debris Coordination Committee. We set ourselves up to have a steering group and four working groups. One of the working groups would be on observations, one would be on data and modeling, one would be on shielding and protection, and the fourth would be on mitigation.

The rules we set up for ourselves in the terms of reference were that everybody had to have a representative on the steering committee and everybody had to have a representative on the mitigation working group, and they could participate in the others depending upon what kinds of assets and resources they had that were pertinent to those kind of problems.

Well, ESA wanted to be the representative for Europe, but the French wanted to have their own representation. So CNESS [Centre National d'Etudes Spatiales (of France)] argued very strongly for that. Of course, that meant that the other major players, the UK [United Kingdom], Germany, and Italy, also wanted to have their own representatives. So somewhat over ESA's reluctance, those became members.

Then the Soviet Union came apart, and the Ukraine wanted to become a member. So now we have Russia, the Ukraine, Germany, France, Italy, the UK, ESA, the US. The US delegation consists not only of NASA, but of the DoD and the FCC [Federal Communications Commission] and the Department of Transportation [DoT] and the Department of State [DoS].

In Japan, they have multiple space organizations. NASDA is the one we see most often, but that's in the Science and Technology Administration. The Ministry of Education also has a space agency called the Institute for Space and Aeronautical Sciences.

They have a Postal, Telegraph and Telecommunications Ministry, and it has a space agency called the Communications Research Lab. The Department of Agriculture has a space organization in their Bureau of Fisheries. So the Japanese have multiple participants. Their delegation is normally led by the National Aerospace Lab.

The [Tianamin] Square incident interfered with our going to sit down and talk with the Chinese, so it wasn't for several years until we did that, but then we went and spent about four weeks in China, visiting all of their various facilities for launch vehicles and spacecraft in Beijing and Xian and Shanghai and Jiuquan. That was a most interesting trip.

At the first the Chinese were very reluctant to modify their launch vehicle upper stages because they were afraid that they'd contaminate their spacecraft and otherwise

disturb things, but a year later at a conference in Japan, they came and showed us that they'd made the modifications we had suggested, and they operated just like Delta. Then there were several years of where they did not participate as members but participated as observers, part of that because they had limited numbers of people with linguistic capability. But they have become full members and now participate fully.

The last to join was the Indian Space Research Organization. So the only launch-capable nation that's not a member is Israel, and they have been invited, but they just said, you know, "We don't have enough people to be able to do that." So this group now meets twice a year. We have a meeting of the steering group in conjunction with the International Astronautical Congress, and then we have a plenary session where we sort of get all five working groups together, and what we try to do is to make this an active participatory kind of function. Nobody is allowed to be an observer unless they're in an official observer status. If you're there, you're expected to work and contribute.

We also try to avoid making it a paper presentation session like a professional conference. That doesn't get work done. So we spend normally four and a half days in these working sessions, and we've made a lot of progress. We now have a consensus standard for disposition of geostationary spacecraft. Before we had the consensus standard, we, NASA and ESA, had developed a set of position papers. In '92, I was part of the U.S. delegation to what at that time was called the CCIR, where we took up this question of disposition of geostationary satellites, and I wrote the recommendation for the ITU [International Technology Union] that said boost 300 kilometers above the geostationary arc with another factor to allow for the variations in size and reflectivity of the spacecraft, and that was where the IADC started, was with that recommendation.

But the Russians and the Japanese objected to the 300 kilometers as being too much, so the consensus recommendation is 235 kilometers above the arc with an allowance for the size and surface area of the spacecraft because of solar pressure which causes the orbit to oscillate. So we've got that. We have general consensus. All of the members adopt this business of no stored energy at end of mission. We're engaged right now in a lot of studies and debates about do you require people to bring objects out of lower Earth orbit. We do not use space uniformly. There is a family of orbits that are sun synchronous, that have the attribute that every time you come over a point on the Earth, the lighting condition is the same. There are other orbits which are very useful for certain geodetic measurement properties because they have a stable line of apsides [phonetic]. So you find that we use a relatively small number of inclinations and altitudes. So space is very heavily populated at 900 kilometers and at 1400 kilometers, and in between there's two or three orders of magnitude difference in the density of population.

So we've been saying at end of mission, "Don't just abandon those things. Lower the perigee of the orbit so that it will be removed from orbit within twenty-five years by aerodynamic forces," and that was based upon a series of studies we had done which said the big thing you want to do is you don't want to leave something there in a 5,000-year orbit, because they'll keep accumulating and eventually they'll run into each other.

Just in the last six months, for example, we've had four conjunction events for the Space Station where an object in orbit came close enough that we went to great trouble to understand whether or not a collision was imminent, and in three of those occasions, we maneuvered. So the notion was that if you're gone within twenty-five years, you don't become a long-term threat. That's sort of been a raging debate in our working group for the

last several years. Is that necessary? Why twenty-five years? Why not fifty? Et cetera. It's like a lot of these things, it's a matter of judgment, what's reasonable, but it's made a lot of progress in terms of getting people thinking about protecting the environment.

L\_\_\_\_ Parak [phonetic], a Czech astronomer in the late seventies, was the executive director for the Committee on the Peaceful Uses of Outer Space [COPUOS]. As an astronomer, he had concerned himself with the geostationary orbit, and being the director, he wanted to get the subject of orbital debris on the committee's agenda. NASA and Russia, U.S. and Russia, the U.S. delegation to the Committee on the Peaceful Uses of Outer Space is chaired by NASA. State Department, Department of Energy [DoE], DoD, are all members of the delegation, but it's led by NASA. And we vetoed every proposal to put it on the agenda.

Finally, in '93, after we had been getting this IADC thing going, I went to the National Security Council and said, "It's time to put this subject on the agenda for the Scientific and Technical Subcommittee, but not the Legal Subcommittee." So we began a five-year program in which the members of the IADC presented to the Scientific and Technical Subcommittee of COPUOS, of what we were doing in the world of orbital debris.

The reason we were doing that was because there are 188 members in the U.N. [United Nations]. There are about 98 of them active in COPUOS. Back in the seventies, there was a very bad thing done. The General Assembly passed a resolution that assigned to every one of the members of the U.N. a geosynchronous orbit position and 8 megahertz worth of band loop, and that has been a source of trouble ever since. The reason they did that was that they had been hearing all of this talk about crowding in the geostationary orbit.

To give you some idea, the mean distance between objects at that time was 10,000 kilometers. It wasn't crowding in a physical sense; it was an RF issue, a radio frequency issue, and it was because the spacecraft were small. So they had small antennas, and when you have small antennas, you have big side lobes. When you have big side lobes, you have potential for a lot of interference, and the only defense against that is physical separation.

But spacecraft nowadays, you know, are enormous. We just launched one this year that is seven and a half tons, and the antenna is forty feet in diameter. So you don't have side lobe problems anymore. But the point of that was, it was a perfect example of where people became alarmed about a situation because they didn't understand it, and what they did was foolish, and what it did was it created an arbitrage market for people to buy up those slots and frequency allocations from the people who couldn't use them, like Sri Lanka. So it has been a source of major mischief. But as you can imagine, there was an entrepreneurial guy with money who took advantage of this situation.

But at any rate, I would sort of say we've been very successful. We published the report of the work that we had done for the U.N. in February a year ago. That's almost twenty months now, and that's been pretty useful. We now are committed to reporting to COPUOS annually on what the situation is and how things are going on. There's still a debate about whether or not we should put it on the Legal Subcommittee agenda. At least so far NASA has resisted that, so we'll see how much longer that goes on.

BERGEN: Do you feel that the progress you've made so far is doing its part to ensure that this isn't going to be a tremendous problem in the future?

LOFTUS: Yes. We don't have a problem now, but this is one of those situations in which the only real good solution is prevention rather than remediation. It's much easier to deal with an upper stage when it's in one piece than when it's in 10,000 pieces. The issue is not trivial in the sense that we have replaced over 100 windows on the Shuttle over the course of 100 flights. On other flights, we have taken significant damage in the speed break, on the cargo bay door, and things of this variety.

So the way we've been operating the Shuttle in order to protect it is we have been controlling what we call the attitude time line. So that normally we fly with the engine down in the ram direction and the cargo bay looking at the Earth, because that way the Earth protects us on that side. The engine compartment is the strongest part of the vehicle, so if it gets hit by a piece of debris, the damage will be minimum.

We're coming up on the Space Station. If you've seen pictures of the Shuttle docked to the Space Station, you know that it's up front and its belly is in the ram direction, so it's potentially very vulnerable. So, a couple of years ago we embarked upon a series of studies to say how should we fix the Shuttle so that it's less vulnerable for these station missions.

What we've done is we've gone in and underneath the carbon-carbon, which is the leading edge of the wing, we had insulation on the spar, which was designed to deal with the radiant heat load coming off the carbon-carbon. We modified that insulation so that now we can tolerate a hole a half inch to one inch in diameter in the carbon-carbon leading edge, and the plasma can flow through that region and the insulation is now designed to protect the spar against damage from that plasma flow.

We went in and modified the radiators by taking each of the radiators as it was going through the refurbishment cycle out at Palmdale [California], and going in, and the fluid

flows through a path that has separations of about an inch or two inches. So we went in and put another piece of aluminum on top of the tube to armor the tube, but we didn't armor in between the tubes, because we didn't want the weight and we didn't want to make the radiator inefficient. Then we modified the fluid flow so that we could isolate various sections of the radiator if we had a penetration. Those have both been effective modifications. Right now we're looking at modifications of the tile to see if we can make the vehicle more robust.

BERGEN: Since we were talking about the [International] Space Station [ISS], I was wondering if, in your time in the long-range planning, how much involvement you had in all the changes that over the course of time that have been made with the Space Station.

LOFTUS: Well, as we mentioned earlier, after Apollo, sort of in the light of Skylab, space station seemed like the next logical step. The thing that was characteristic of the two space station designs we had at the time was that they were monolithic. They were one great big module, the notion being that you would launch them with something like the Saturn V. That turned out to be nice in one sense that it gave you lots of volume. It had the disadvantage that it didn't necessarily give you the various appurtenances that you would like to have for observation purposes and antennas and things like that.

So at that time we were doing studies of things we call remora. I don't know if you're familiar with remora, but they're a small fish that generally accompany sharks and whales, and they clean the shark or the whale. So they, in effect, perform a service in exchange for which they have a home and transportation. So we had a lot of little satellite

vehicles that would fly formation around this big space station in order to help you do all the things you wanted to do.

Well, when we backed away from that space station concept and went off to do the Shuttle, we did the obvious thing and sort of said, "If you build a space station with the Shuttle, it would obviously be very different than launching one big module with the Saturn V." So we began to look at modular space stations such as the one that we are now building and such as the one that the Russians built on the *Mir*.

Like anything else, there are virtues and disadvantages to that, but I did a series of studies very early in the space station where what we said what I want in this space station is not necessarily a laboratory, I want a space operations center. I want a node in a transportation network, so that instead of going from Earth to geotransfer orbit directly, I would always go to the space station, and then from the space station, I would go to geostationary orbit or to Mars or wherever it was I wanted to go.

We did quite a thorough study of that kind of a space station, which, in effect, was more like an airport than a life sciences laboratory. The [International] Space Station we're building today is primarily a life sciences and a materials processing laboratory, which was a different concept than the one we were working on at the time. We essentially embarked upon this station in '84 with President Bush's direction to do that.

It's had a difficult history because of the financial period in which it was being formulated and developed and because of the economic difficulties that Russia has experienced. But not just Russia. The fact of the matter is that Japan has had financial difficulties as well, because in '96, Southeast Asia had a major financial collapse and that

had a very adverse effect on the Japanese and their ability to fund their activities. So that has made the history of the program difficult.

It's obviously also difficult when you have as many interfaces as we have in the station. I think the last time we talked, we said one of the things that made Apollo successful was the simplicity of the interface between the command and service module, lunar module spacecraft and the Saturn V launch vehicle. We don't have that luxury in the station. We have a very complex set of interfaces.

But I think we have now embarked upon an era where we will never again be without humans in space, a pretty momentous kind of change. It's also a major cultural change for the [Johnson Space] Center in the sense that previously we had worked 7/24 for a couple of weeks at a time. Now it will be a permanent way of life, and that's a very difficult and different kind of a working environment.

But I think we've got a lot of good things. We've got, in the space station, one of the things that I was a strong advocate for, along with others, was a first-class optical window, and in the belly of the U.S. module we have a first-class optical window so that you can do very good Earth observations. That's not to say that you won't continue to use automated spacecraft for that, but you can do a lot of work to develop new instruments and new techniques on a manned spacecraft that you can't do as readily on an unmanned spacecraft.

Sort of a good example of that is on one of the very early Shuttle flights, the crew reported that they were seeing a very funny appearance in the Great Barrier Reef off Australia, and they took a lot of photographs of that and what have you. We finally concluded that we were looking at was a major phytoplankton bloom, and that's very important to all of the ecology of the ocean. We said, "I wonder how long that's been there."

So there's an instrument on the NOAA [National Oceanic and Atmospheric Association] spacecraft called the very high-resolution radiometers, and that data had been in the telemetry stream from the NOAA spacecraft for the past twenty years, but we never knew how to look for it until we had a human observation. That's sort of one of the things that humans contribute, is that they are synoptic observers and they can recognize the unexpected. So there's a complementary relationship between manned and automated systems that one needs to be aware of.

BERGEN: Looking back over your career, I was wondering if you could share with us what may be some of the greatest challenges that you encountered were.

LOFTUS: It's an interesting question. Let me tell you something unexpected. I think I mentioned earlier that I had gone to Gonzaga High School in Washington, D.C., and that was a very classical Jesuit high school. I took ten periods of Latin and five periods of Greek every week. And astonishingly, that's probably the best education I've had in terms of preparation for this career, because it's enabled me to read Russian, to get along in all the European languages, and just have all those benefits that came from being comfortable with other languages. That surprised me when I recognized it, but it really has been beneficial.

I think maybe the biggest challenges have been accommodating to various phases of a program. One of the things that I've observed during this period is that quite often the team of people who can put together the proposal and win the contract are not the team of people who can deliver the product. So there's always a transitional phase that's one of the more difficult things for both the companies and for the government, and that's a difficult set of

issues to deal with because it occurs gradually over time and because it involves dealing with one of the more difficult aspects of human relations in these kinds of circumstances.

I saw that, for example, in the development of the simulators. We literally had to fire several people because while they had been very good for the early conceptualization, they were not the people who had the skills and the schedule discipline to deliver final products. I think those are probably the most difficult sets of issues that I've had to face.

BERGEN: You've achieved so many things during your career. What are you most proud of?

LOFTUS: I guess I would say maybe this business of having gotten an international consensus in how to deal with this orbital debris business. It's taken a lot of work to do that, but I think we have succeeded.

BERGEN: Definitely quite an achievement.

There was one thing that I meant to mention earlier that I overlooked, was your involvement with the Rogers Commission in their investigation of the *Challenger* accident. Could you share with us how you were involved with that and what you did?

LOFTUS: Okay. The Rogers Commission obviously had a lot of questions, and we had a particular kind of failure. The question was, how many more potential catastrophes might be lurking in the rest of the system and how could we find those and deal with it. We had done all the things that we do during our programs where we go through and we do fault trees and failure modes effects analysis and so forth.

Arnie [Arnold D. Aldrich] was getting a lot of questions from the Rogers Commission, and I was one of the few people available to him who had been through all of the Shuttle Program up to that point from the original conception of the Shuttle through where we were at 51-L, so he asked me to put together a story that said how did we get the Shuttle we've got. So I took a team of people who had worked with that period of time, and we wrote a briefing that was the evolution of the Shuttle, how were the decisions made one after another and what have you. I gave you a copy of a paper that I had written on that subject. The purpose of that was to just explain to the Rogers Commission that there was a rational, ordered sequence by which every decision had been made, that led us to an external tank, that led us to solid rocket boosters, and led us to all the various other configuration issues.

Not in the paper, but supporting it, were other analyses that sort of said why did we do the orbit maneuvering system the way we did, where we have two separate pods, one on each side of the vehicle, each of which is autonomous, but which are cross-strapped so that you can transfer propellant from one to the other. Those were notions of redundancy so that you always had more than one way to do something.

We had gone to a great deal of trouble in the way in which we built the vehicle in terms of its plumbing and its wiring because we applied what are called battle damage criteria, which sort of says if you have an A system and a B system, you don't have them side by side, you put the A system on one side of the vehicle and the B system on the other side of the vehicle.

We went into some detail on how we had done the computer system. The primary avionics software system runs in four computers. The thing that is unique about it is that it is

asynchronous. They are not clock-driven. Each computer goes and does its thing, and then they meet at the corner and say, “What answer did you get? What answer did you get?” Everybody agrees, and they go on. But it’s not a clock-driven system, because you wanted to keep them independent in order to have redundancy and have independent solutions. We went to four because of the time criticality of events during ascent and descent. You didn’t want to have a non-voting system if you had a computer failure, so you have four so that one can fail and you can still have three voting, and you can vote the odd man out.

So we had done all of these things, and I think we satisfied most of the commission that we really did have a fairly isolated process error. Turns out that the design of O-ring seals was not a very good design. So when we fixed that, we have had no further indications of that kind of problem.

BERGEN: We’ve talked about so many different things that you’ve done in your career. Is there anything else that you would want to bring up that maybe we haven’t touched on?

LOFTUS: Well, I guess one that was fairly significant was, for quite a few years I was involved in recruiting trips on campuses, particularly in the early years. Subsequently I served on committees that chose people we sent for graduate studies and eventually things like the Sloan programs and what have you. I always thought that was a fairly significant kind of activity. It was interesting that we would get lots of good recruits out of the co-op schools and out of the Midwest. Very rarely could we recruit somebody out of MIT or Stanford or Caltech [California Institute of Technology, Pasadena, California]. I think part of that was that they were in such a different economic environment that the salaries they could

command were multiples of the kinds of salaries we were offering, while for people from Purdue [University, West Lafayette, Indiana] or [University of] Notre Dame [Indiana], that wasn't the case. So the thing you will notice is there are relatively few of the East Coast and West Coast premiere schools. Generally, the people who we did get from those schools were astronaut wannabes, and quite a few of them made it.

BERGEN: Personnel can make a big difference in what you do. Speaking of people, I was wondering if there were any individuals who maybe made a significant impact on you during your career.

LOFTUS: Yes. Bob [Robert R.] Gilruth, because he was so consistently a gentleman. Joe [Joseph F.] Shea, who is probably as brilliant on his feet as anybody I've ever known. John [F.] Yardley, who had a prodigious memory. There have been a lot of outstanding people in the program: Max [Maxime A.] Faget, Chris [Christopher C.] Kraft [Jr.], George Low. George was probably the most disciplined person I have ever known. So, you know, there have been lots of people.

BERGEN: We have talked to many, many of the people that you have worked with, and they have been an outstanding group of individuals, and we thank you for sharing with us what you did. It's been a privilege to hear about your history.

[End of Interview]