ORAL HISTORY 2 TRANSCRIPT

KENNETH A. YOUNG
INTERVIEWED BY KEVIN M. RUSNAK
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RUSNAK: Today is June 13th, 2001. This interview with Ken Young is being conducted in the offices of the Signal Corporation in Houston, Texas, for the Johnson Space Center Oral History Project. The interviewer is Kevin Rusnak, assisted by Carol Butler and Kirk Freeman.

YOUNG: Since I was a rendezvous guy, I don't remember, you know, the details about like, when the lightning struck [Apollo] 12, and I remember it, but that was pretty scary. Then that mission went great.

RUSNAK: When they did that precision landing on the Moon, did you have anything to do with that?

YOUNG: Not directly. That was the Math Physics guys, Emil [R.] Scheisser and his crew. Of course, you know, the same as all of the landings, we had to know precisely where it touched down and recalculate the rendezvous maneuvers for NASA and everything, but no problem. That was by the Surveyor, wasn't it?

RUSNAK: Yes.

YOUNG: That was really a great feat of precision landing. I can't remember much about that flight, or, like I said last time, 13 I didn't really work because I was not needed for the non-rendezvous flight. Some of our MPAD [Mission Planning and Analysis Division] guys really contributed, primarily to the trajectory adjustments and decisions about coming back to the

Pacific [Ocean] versus the Atlantic and getting them back as quick as possible. That was the key, really, because they knew they could live there, even if they damn near froze in the LM [Lunar Module], but the water and oxygen were running out.

So Ron [Ronald L.] Berry and Hal [Harold D.] Beck and the Lunar Branch guys were the ones that did all the calculations for return, for adjusting it back to off the non-free return that it was on and figuring out the maneuver for getting them back as quickly as possible. The other big thing that our MPAD guys did was Marty [Martin D.] Jenness and a guy named Al [Alfred N.] Lunde, who was the only Norwegian NASA guy, I guess, that we've ever had up or until recently, probably, but anyway he was in Marty Jenness' section where they calculated all the look angles and stuff. So he was very instrumental in running some software programs that had actually just been kind of developed to figure out how to look at the SM [Service Module] after it was—well, first before that, was how to navigate without all the CSM [Command and Service Module] nav tools and make sure they got all the maneuvers and the mid-courses correct coming back.

Then what Lunde did was run a series of attitude studies, I guess you'd say, or look angle things where once they jettisoned the SM right before entry, they could get some decent looks, if not pictures of it to see what the heck had happened because, of course, they were still just guessing. They knew that the oxygen tank and most of the SM was gone, but it really helped to have those pictures for post-flight analysis. MPAD did that.

RUSNAK: On the post-flight?

YOUNG: Well, not the systems post-flight, but they got those pictures possible and helped with all the trajectory reanalysis. So that was, of course, a remarkable mission. I didn't hear the Fox [Television Network] conspiracy guys explain why we faked that whole thing. I mean, why wouldn't you have faked another successful mission to the Moon, not just for the drama, so Tom Hanks would have—

RUSNAK: Improve ratings.

YOUNG: Yes, I guess they could claim it blew up in Earth orbit and that we made up the whole thing. Ridiculous.

Let's see. Then it was 14, I think 14, maybe even 13, I was pretty much, personally, off of Apollo and was starting to work Apollo Applications [Project], AAP, which I'm sure Cathy [Catherine T.] Osgood—have you interviewed Jerry [Gerald L.] Hunt?

Kevin: No.

YOUNG: He's another MPADer. He and I and Cathy worked, starting, actually, in '66, on AAP to use the spare hardware from Apollo to do a Space Station or an interim Space Station. It evolved into Skylab, but originally there was a whole series of missions involving even the LM in Earth orbit to, in essence, use up the spare Apollo hardware. In about '70, of course, they had already changed the name to Skylab and everything. But from '66 to about '69 or '70, we worked that part-time, that series of missions and all kind of variations, using the S-IV and the S-IVBs, and finally ended up with what became Skylab in '73. '73, we launched.

So as far as the later Apollos, I really didn't have a whole lot of direct shift work, anyway. My guys were doing the rendezvous all that time. Took my family to Apollo 17 launch, which was the midnight launch. Actually, it ended up past midnight as I remember because of a delay. But I took my kids. Disney World had just opened like in October of '72. So I had four girls. Our youngest one was six months old, and she just turned twenty-nine last month. So that's how long ago it was.

But we got out in the field, you know, rode a bus out near the pad, which at that time they let you get within about three miles, I think. I'd never seen, except on TV, a Saturn V launch. It was just awesome, just indescribable. And at night, it was the only night launch. So we stand out in the field, and they had delay after delay, some computer problem, I can't remember. At least an hour or two delay, and they were getting close to the end of the launch window, but they got it off. I remember standing there in that field, holding my little daughter, and her eyes, when that thing lit up, we probably all looked like that, but her eyes were just like that. Of course, she had been sleeping off and on, six months old, almost exactly.

The earth just shakes. I mean, that thing sits there for, it seems like, thirty seconds. It's only probably ten, but it just sits there and the whole earth shakes. It lit up just like daylight, I swear, almost like this room, for probably thirty seconds or forty. Her eyes got so big, I remember telling her, I said, "Try. I know you're only six months old, but try to remember. I'm going to ask when you're five years old whether you can remember." Of course she couldn't. [Laughter] But it was an awesome sight. The Saturn V was just amazing. I've been to Shuttle launches, and that's pretty impressive, but doesn't compare to the Saturn V, especially at night.

RUSNAK: I can only imagine.

YOUNG: So that was my last Apollo thing, if you don't count ASTP [Apollo-Soyuz Test Project], anyway, 17, Apollo 17.

Of course, by that time, we were heavily into Skylab, and believe it or not, we were already starting to work the international docking mission, which became ASTP in '75.

So Skylab was the next one I remember, launched on her, that same daughter's first birthday, May 25th. Of course, we almost lost it. I don't know how much Cathy told you or other interviewees have told you about the Skylab experience, but I really, thinking back, and I don't know if Cathy would agree with this, I think she would, that's the hardest we ever worked, was the two weeks or four weeks after Skylab 1. I guess that was the right number. It was one of these numbering things where it originally was 2, even though it went first, the workshop, and Skylab 1 was the CSM. I think they finally changed it.

But, anyway, due to a screw-up of some sort over at Marshall [Space Flight Center, Huntsville, Alabama] on the meteorite shield loading for ascent, they misfigured the aerodynamics or something, but it ripped off the meteorite shield and one of the big solar panels on the side that supposed to spring out, and damn near, you know, didn't make it to orbit, but it did.

Then we had to redo everything in terms of the first rendezvous and the first mission, because, of course, the whole objective became can you keep it alive and lower the temperatures in the lab, because the thermal shield, which was the micrometeorite shield, was gone, and it was eighty, ninety degrees in there in a few days.

Unfortunately, Skylab, you had to fly solar inertial, you know, with the top to the sun because of the solar panels, one of which was at least partially deployed. The other had been ripped off. So the only power, really, they had was that partial one, and then the Apollo Telescope Mount [ATM] panels, four panels that were sitting on top of what used to be the LM, part of the LM, that was what became the Apollo Telescope Mount. So they had that power, which was probably equivalent to half of one of the big panels.

So they barely had enough power to keep the CMGs [Control Moment Gyros] and some of the critical systems working, but the main problem was the temperature. It was just heating up big time. They couldn't go out of that attitude, or at least for very many hours, because then you'd have no power because the panels wouldn't be looking at the sun.

So that was really the only time that I ever got paid overtime in my whole career at NASA. We worked like eighty hours, it seemed like, three weeks in a row. Anyway, it's probably still this way with civil servants, but you can only carry over so much leave, and we already had our thirty days of leave accrued, and you can't carry more than that. So we had that year's leave. Then we were used to working comp time, but you had to use it or lose it in comp time.

So we worked, I don't know, probably sixty to eighty hours a week for three or four weeks, planning rendezvous so we could be over Hawaii at a site when the crew got close, what the thing looked like, because, of course, they could only guess at what its situation physically was from the system's telemetry. So we had to replan the rendezvous and all that, get the lighting right for that. Hawaii and continental U.S. were the only areas where we could get TV down from the CSM when it got there, so we had to adjust that for the setup for rendezvous lighting. That was pretty hard work.

But, anyway, the bottom line is, Dr. [Christopher C.] Kraft [Jr.] finally gave us some special dispensation that we didn't have to lose all our leave, and, finally they ended up, I think they paid us for forty or sixty hours of overtime pay. It's the only time in twenty-five years I got paid overtime. But it was a challenge and a half, and I'm sure many of your interviewees will tell their own parts of Skylab.

I guess our part, mine and Cathy's in particular, was that we picked the orbit that we had planned to put the workshop in, and, of course, we had picked it two or three years earlier. We did that for Earth Resources purposes. That was the only kind of fun part of Skylab, frankly, was the daily conflict between the Earth Resources guys who had all these Earth targets, and the solar scientists, who wanted to point the ATM at the sun and get these flares and all that. Due to the delays and everything, unfortunately, the thing was launched in the solar minimum time frame instead of the max, but they ended up actually getting some great data on some pretty big flares.

But every day I had five guys assigned to the Earth Resources, to do the targeting and look angles and all that of the Earth targets. Every day they'd meet in the back rooms there and argue about who had priority, because to look at the sun, of course, you had to stay in solar inertial, and with the bad power situation, that was the preferred and it was the nominal attitude anyway. So the Earth Resources guys wanted to fly LVLH [local vertical/local horizontal], where you can constantly point the ground-looking cameras at the Earth. Otherwise you get this half the orbit you don't see the Earth with the Earth observation cameras and the crew just looking, but it got a little old after the first mission.

The first mission was really a repair and hardly any science. I remember those guys really did a great job, not only on the rendezvous, but a fly-around and see how they could—and then the EVA [extravehicular activity] was done, snap the panel so that it would go on out to full deployment. Well, all the Skylab crews were good, including the third one, which got really annoyed with the ground, which was, of course, we all said then, there was a lesson to be learned for future Space stations. But these Montessori flight controllers that are flying the ISS [International Space Station] are repeating the same mistakes, trying to tell the crew exactly what to do when, every second of their waking hours. That's just not the way to fly. Even Mir didn't. Even though the Russians, of course, knew that for years, they tried to plan too much. So the crew rebels once in a while, which you can't blame them.

RUSNAK: Did you have any involvement with planning for the rescue mission that they had?

YOUNG: Yes, yes. We had a guy named Alex [Alexie H.] Benney [Jr.], actually, he worked with Cathy at USA for years till he retired five years ago. He was our rescue mission planner. Every one, we had these two or three alternate plans for if they couldn't get down in the CSM. Of course, we all knew that the CSM was so proven, especially the SPS [service propulsion system] engine, that there was never any real worry that they couldn't do a de-orbit, but we had to plan the rescue missions each time. That was a full-time job for Alex for a year or about a year. We started on it before the launch, and then nine months of manned occupation.

One thing about that or sort of related to that is back in '72, I think it was '71, I think it was two years before we ended up launching, Bill [Howard W.] Tindall [Jr.], at our urging, and out of MPAD particularly, went to the Associate Administrator for Manned Flight—that was Dr. George [E.] Mueller then—and proposed several times over about a year that we put a propulsion system on the back end of the workshop, which, of course, had this one J2 engine back there, where it was. They took it off, of course. It was useless weight. But we wanted to put a small propulsion package on there to do reboost and particularly reboost to leave it in storage after we quit manning it, which was, of course, the plan was never to man it for more than nine months.

That's one of my great dis—I don't know. How can I politically, tactfully, put this? My great disappointments in Marshall and Marshall management, particularly then, was that they had a solar scientist who thought he had the—it was just a regression analysis technique, but he thought he had the answer to the eleven-year solar cycle. It was a cycle in a cycle, which is actually true statistically. Of course, the problem was there were only twenty-something cycles of data, and half of that was suspect because it was old sunspot counts from the 17th and 18th century.

But, anyway, he claimed that he had predicted, this guy named Bob Smith, he claimed that he knew that the next solar cycle was going to be one of the lowest in recorded history, and, therefore, this 235-mile orbit that we picked, Marshall had also wanted 220, but we forced them up to at least to 235. The main reason for 235 was that at the 50-degree inclination that was a five-day repeating orbit precisely, and we kept it at that during the whole manned phase so that the Earth Resources guys would get to see exactly their same targets every five days.

Cathy and I, I guess, were mainly instrumental in deciding on that and picking that. We had long fights for a year or so with Marshall about whether they could get it to 235 and why did we want that and so forth. But that was just normal technical disagreements. The one about the solar cycle, we had a guy here named Don [Donald E.] Robbins, who had been studying some other techniques. It's still, to this day, really more of an art than a science to predict this solar cycle, especially the magnitude of it. I mean it's pretty well known it's going to occur every eleven, plus or minus two years. And it has.

But the peak is very essential as to what the drag factors will be for the heating of the atmosphere. So we, Don Robbins and the JSC solar experts and myself—not that I'm an expert, but I was fairly knowledgeable about solar cycles because we'd been studying drag things for years and working with NOAA [National Oceanographic and Atmospheric Administration] and some other agencies. Don Robbins had seen this paper by a Russian name Ohl. I'm sure he's dead now. I think he was dead maybe then. He probably died in the seventies. But, anyway, he had a different technique for predicting the upcoming cycle amplitude, primarily the magnitude. He was predicting a very high cycle, which would be heavy drag.

So, consequently, armed with those predictions from here, and NOAA, even, had some guys that tended to believe that—he had predicted the previous cycle within 10 percent, which was remarkable, and it involves some technique detail, geodynamic index and some other data rather than just sunspot and regression analysis.

Anyway, so we went to Dr. Mueller and said, "Hey." I wasn't a systems guy, but our propulsion guys said, hey, for, I think it was, $6 million, we could put a little package on the back where the J2 engine had been to do a reboost of probably fifty-mile altitude, or a series of them, you know. There was arguments about when to do that and so forth, but after the last crew leaves, we wanted to jack it up to make it through the upcoming solar cycle, which was, at that time, predicted to be in the late seventies.

The Marshall guys, I've convinced myself since then that they probably believed Bob Smith's data, for one thing, but I think there was more to it than that. I think they wanted to make sure the Skylab didn't last, because it was just an interim Space Station. See, at this time, especially in '70, '70 and '71, they were still pushing—[Wernher] von Braun was still pushing for the big Space Station, you know, with the Shuttle resupply. So Shuttle and Space Station were going at it head to head for the budget considerations. Skylab was interesting and a good demonstration and a good use of the last test S-V [Saturn V] and all that, but they just really wanted a bigger station, and they didn't care, really, if it came down in two or three years.

Bob Smith was saying, oh, it'll last fifteen years. I forget the exact predictions, but, anyway, it was well into the eighties that he was predicting it in this low solar cycle. So they turned us down. I remember Tindall being very upset about that for months, because I kept telling him, "This is going to jump up and bite us." And so did Don Robbins.

Sure, enough, when it did happen, which was late '78 and '79, the cycle was the second highest in recorded history. It was twice as intense as Bob Smith's prediction, and, consequently, that's why Skylab came in and didn't last to the eighties. In fact, it would have come in in early '79 if we hadn't have done some neat tricky things to keep it up there for a few more months.

But their other argument, by the way, was, "Oh, the Shuttle will be flying by then and we can go up and boost it with the Shuttle." Well, for one thing, none of us here believed the Shuttle would fly in the seventies. But the planned date in '71 was like '77 or '78, but nobody really believed that, including probably Marshall, but it fit their arguments.

So I've always claimed that NASA has made three big mistakes in my time frame, and that was one of them. The other one was giving up on the Saturn V as a launch vehicle, which, of course, Shuttle is mostly to blame for that and this idea of, you know, putting all cargo up or payloads up with the Shuttle, which none of us who worked Shuttle from '69 on really believed that that was the right thing or that it would ever happen and be that cheap to fly per pound. But that's the first mistake they made, was getting off the Saturn V, because that was just a fantastic vehicle. And give Marshall all the credit for that.

The second one was not letting us put a propulsion package on Skylab, because we could be flying it today, really. That's no exaggeration. It had a multiple docking adapter, and we could have stuck modules, at least three or four more modules on that thing in the eighties when we finally got the Shuttle flying, and it could be the core station, just like Mir was, for fifteen years.

The third one was NASA going to a policy of putting astronauts in management positions. That's my third personal opinion. Not that there hadn't been a couple of good ones, but, overall, I think it was a management disaster to put astronauts, or certain ones, in big, important decision positions. I guess some people would argue with me on that because they'd argue, "Oh, well, they're sharp." Sure, they were. They all were sharp. But most of them had a test pilot brushfire mentality, and tunnel vision, no real vision for the strategic planning. Personally, some of them had terrible management styles, you know, confrontation or embarrassment of their employees and so forth. With the exception of Dick [Richard H.] Truly and maybe Bob [Robert L.] Crippen, I'd say that was a mistake to ever make many of those guys managers. But I digress. That's one of my personal feelings.

Let's see. So we did three missions on Skylab, and they were fun. But, like I say, they got a little boring in terms of the daily routine of these fights for viewing or attitude. Every morning, "Are we going to fly solar inertial today for X hours, or are we going to have Earth Resources get four or five orbits' worth of looking at the Earth?" You know, the scientists just all argue their position, and, of course, you know the earth scientists' argument was, "Hey, you know, that volcano is not going to erupt again for maybe fifty years. We've got to get the shots of it today." Or the hurricane, you know. Whereas, the solar side, they said, "You know, the sun will be there tomorrow and forever. Just let us take these opportunity targets, and you can get your pictures of flares." But, of course, the solar guys were going, "Well, those flares are rare at this time of the cycle anyway, and we can't miss it." So it was a continual battle, but that kind of made it fun. Otherwise, it's pretty boring for the day-to-day guys.

I guess about toward the start of the third mission, I had switched off once again to future missions, namely ASTP, which initially had a guy, Bob [Robert S.] Merriam, working for me, doing what we called at that time the international docking mission, which had been, I guess, postulated in about 1970 by somebody, I can't remember who right off, but between us and the Russians as a goodwill kind of thing and some technical arguments, too. But we had worked that for about a year, and then, finally, [Richard M.] Nixon's whoever was working that, [Hubert] Humphrey or whoever was working that with Nixon—no, he wasn't the vice president then, was he? Different parties.

Anyway, somebody made an agreement with [Leonid I.] Brezhnev, and they finally got this tentative agreement. Ed [Edgar C.] Lineberry and Chris Kraft and a bunch of systems guys went over there, I believe in December of '72. I lose a year here and there, but, anyway, maybe if you interview Chris, I think he mentions it in his book some. No, I guess he doesn't.

Anyway, a bunch of them got this parasite from the drinking water over there, and it was really bad. I remember Ed Lineberry was sicker than a dog for weeks. It was one of the bad parasites. It wasn't just Montezuma's revenge kind of thing. About eight or ten of them had gone over there—Caldwell [C.] Johnson, Max [Maxime A. Faget]. Maybe Max didn't go with them. Caldwell Johnson, Max's spacecraft designer, and Chris, and Ed Lineberry from MPAD had gone as the trajectory guy, and he had had Bob and myself working the details, but he was the lead, of course. He was branch chief then.

They came back, half of them sick. Some of them, I don't know if they ever got over it. I can't remember. But all I remember is they're telling us they flew out of there on the Japanese airlines on December 7, 1972, I think it was, maybe '71. Might have been '71. Anyway, they thought that was kind of ironic. They couldn't get a Western carrier flight out of Moscow for some reason, and half of them were sick, or at least half, and they wanted to get out, so they took this Japan Airlines. I think they went the long way around. They went to Japan and then back to the U.S. I don't remember.

But, anyway, shortly after that, it seemed like in the spring of that next year, Ed Lineberry decided to take a year's sabbatical for personal reasons, and he actually quit NASA, but I think they ended up somehow working it to where he really didn't quit. Anyway, he went to Colorado and he dumped all of that on me. [Laughter] So I became the ASTP trajectory lead, working with flight control, guys like Pete [M. P.] Frank and Glynn [S.] Lunney, who, of course, became the program director.

My first trip to Moscow was in fall of '73. We had previously worked with some guys, some Russian engineers coming over here, which I had a great—turned out to be a great friend, a guy named Oleg [G.] Sytin. He's a technical genius, probably close to Ed Lineberry as far as knowing orbital mechanics, and just a super guy, great sense of humor. We had a great time our four or five years of working together.

Of course, they were over here during the Watergate time frame, and we were still working Skylab. They were quite interested in Watergate and how that all could happen and how the press and the public could really do anything about what the leaders of a country were trying to pull, because, of course, I think they were under Brezhnev at that time and there just wasn't much freedom, put it that way.

So we went over there in the fall of '73, the whole working group set of us, probably forty or fifty guys, Leonard [S.] Nicholson, Glynn Lunney, of course, and a whole bunch. I was the sole trajectory person. Oh, no, I take it back. Marty Jenness, I believe, went with me on that trip because he was a pointing expert, and there was some scientific experiments in addition to the docking mission, and Marty worked those, along with some other MPADers, for the attitude and definition and so forth. So we worked that for, what, five, four years before it flew. I could talk for hours on my Russian experiences, but I guess I can stop.

RUSNAK: Maybe you can give us an example or two, just to add some color, I guess.

YOUNG: Oh, it was just so bizarre, you know. It was the height of the Cold War, but we were treated as VIPs, you know. Our first trip over there, Vance [D.] Brand was with us from the crew, I think Tom [Thomas P.] Stafford. All the crew didn't go with us. I think Vance was with us and some other crew backup guys that were helping Vance prepare the astronaut training.

He lived in El Lago [Texas] where I did, and we used to run together occasionally. Of course, he had to run all the time. I hate running, but I play basketball a lot. So he told me, "Hey, take your shorts and running shoes, because I've got to work out every day." Did I tell this story last time?

RUSNAK: No.

YOUNG: So we get over there, and we were staying at the Rossia Hotel, which is right off of the Lenin—Red Square, right by the Kremlin, in this gigantic hotel. At the time, it was the largest in the world, like 4,000 rooms, and they were all for foreigners. No Russians, except the workers in there, were permitted in the lobby, even. It was just a little foreign compound, so to speak, Rossia Hotel.

We worked out at a space institute, which was outside of Moscow. It was an hour bus ride over these horrible roads every morning. We'd leave at 7:00 and get out there at 8:00 and work till 5:00 or whatever. So Brand says, "I'm going to run in the morning at 6:30. We'll run around the Kremlin, which is two and a half miles, and get a quick shower and make the bus by 7:00 or 7:15," whatever the bus time was. I think it was maybe 7:15. I said, "Okay."

So I meet him in the lobby, and we start out across the Red Square and we're running around. We look back and we see these two guys in black suits and hats running behind us. We go, "Well, there's our buddies, our watchdogs." So they followed us to the corner at Lenin Prospect, which is this huge avenue down the town side of the Kremlin. The other side is the river, which is where the Rossia is, right on the river. So we look back after about a half mile, and they were just standing there watching us. We go on around, and, sure enough, when we come around, which took a good fifteen, twenty minutes, there they are in the middle of Red

Square by the church there, St. Basil's Cathedral, waiting for us. Of course, it wasn't that hot. This was in September. But it was probably seventy degrees.

So the next day we take off. Vance laughed about it. He said, "Well, those guys are supposed to protect us, you know." So the next day we take off, and they took off their coats and jackets. They were following us, and they followed us for about a mile, and then they gave up and came back and met us.

Then the third day we come out there, and they've got their shorts and running shoes on, too, and the followed us all the way around. [Laughter] We just laugh every time Vance and I see each other. He goes, "Remember our running buddies in Moscow?" They were KGB [Soviet secret police] guys, and they were supposed to protect us.

So we had those kind of things, and we had all the bugs in the rooms, you know. The Rossia is eleven stories high. Up on the top floor is this penthouse, and it has probably fifty antennas and stuff sticking out of it. [Laughter] We could just picture these guys up there with their TV cameras and such in each room, you know, and at least their microphones. They were sitting, trying to translate what we were talking about in our rooms. Of course, we had been warned by security. They knew we were being bugged.

I imagine you've heard stories like—I think it was [Eugene A.] Cernan or somebody was on a trip, and he went to hang up his clothes in the closet, and all they had was wire coat hangers, which in Russia it was a luxury to have any coat hanger. He goes, "Oh, no plastic coat hangers. I can't believe that." The next day there were plastic coat hangers in his closet, the only one in the whole wing. It was fun.

There was a guy—this happened later, but Bob [Robert W.] Becker, one of our MPAD guys, ended up being on the flight control team as a trajectory guy in Moscow for the mission. So they were sitting at a table. They used to put us at these tables in the restaurant, with a little American and a Soviet Union flag. They sat at one table. Of course, there were forty or fifty Americans there, so they had eight or ten tables. One of the tables, the guy goes, "Hey, we didn't get our American flag today. I can't believe that." Well, the next day—then the guy decided he'd move from one table to the other to talk to somebody during breakfast. He pulls the chair out, and he pulls these wires out. [Laughter] It was an experience.

But, really, other than that, we didn't have any trouble with the KGB. In fact, we put them on, and so did the Russian engineers. Oleg, it turns out, I found out later, was a member of the Communist Party, probably because of his position. He was pretty high up. He probably made, oh, $50 a month.

He was in the Party, but he was just because he had to be, I'm sure, because he'd tell jokes. We had some great jokes. I can remember one joke he told way back. He says, "This guy in Moscow, this Russian is out of toilet paper, so he goes down to stand in line." This was true then, particularly. We had to do it, to buy our own stuff in the meat store, the sausage or bread or anything, you have to stand in line. "So this Russian goes down to get some toilet paper, and he stands in this long line, you know, just a mile long. He's getting so frustrated, he finally says, "I can't stand this anymore. This is ridiculous that you have to stand in line to get the basics of life, I think."

"The people in front of him say, 'Well, you know, that's just the way it is. If you really don't like it, go complain to Brezhnev.'"

"He says, 'I'll do better than that. I'm going to just go shoot, just kill Brezhnev.' So he leaves. About three hours later, he comes back, gets back in line."

"They go, 'Well, what happened?'"

"He says, 'That line's longer than this one.'" [Laughter] Anyway, they made jokes.

One more story here. We always had these KGB guys in every working group. They were, of course, politicians or whatever. I don't know what their skill was, but it certainly wasn't engineering. So, of course, we and the Russian engineers knew who they were, because they didn't know anything technically, and yet they posed as they had to outwardly. Two things, they never spoke English unless you forced them to, and yet they all spoke perfect English. We could tell.

Secondly, they always, if you engaged them in a conversation about what's your field, you know, well, some part of engineering, which the one that they assigned to our trajectory working group didn't know a thing. So after a couple of meetings where he just kind of kibitzed around and, you know, like at parties, he went around, but you couldn't really have a private conversation with a Russian engineer very easily, because the KGB's job was to just nose in on things and kind of stand around and make sure they weren't talking politics or something. Of course, they asked us about Nixon and all that.

But at one party, we knew this one guy. I mean, it was not frustrating, but it was annoying that he wouldn't admit that he wasn't technically educated. And, you know, we'd talk some work things, and we'd scribble stuff on napkins and stuff about some plan for the rendezvous or something. So one time he came up and we were talking about rendezvous plans or delta Vs [change in velocity] or something, and Oleg takes this napkin and he draws the Earth as a globe and then he draws a halo around the North Pole. And he says, "I think maybe we should change to a halo orbit, where we fly just around the North Pole." And this guy is going, "Da, da." And I'm going, "I don't know. I think it takes too much energy."

And Clarke Covington, I remember, was in this conversation. Clarke was working for Lunney as one of the program guys. Clarke says, "Yes, I think it would be better if you flew a square orbit."

I go, "You're right, Clarke." So I took it and drew a square around it. I said, "You know the change at the corners here, delta V-wise was pretty severe, but I think we can work it out."

And we look at this guy, and he's "Da, da," and, of course, we were all trying to keep from just howling. The guy, he just didn't know.

Clarke goes, "So you agree with this? Are you an engineer?"

He says, "Yes, da, engineer, engineer."

Clarke says, "I thought you were in politics."

He says, "Oh, no, no politics. Engineer."

So, Clarke goes, "Well, so you're going to endorse this change if we take it to Professor [Konstantin Davydovich] Bushuyev?"

He says, "Da, da, da." Of course, Oleg didn't mind pulling his leg, but he couldn't let it get to Professor Bushuyev. But I later told Glynn Lunney about it, and, of course, Glynn's an old trajectory guy. He got a great kick out of it. So we used to do that all the time with the guys. They were just a nuisance.

The other thing that was a nuisance was the paperwork for all our planning, because not only did we have to technically get it in both languages exactly the right wording, the rules, the flight rules, and all the agreement about how we were going to fly and who's going to do what, which that alone probably did add 50 percent to our normal planning time, but then on the Russian side, they had to run all that through the Party to get a sign-off, so it took months to get them to sign, even when the technical guys totally agreed and the wording, which is another big problem, was having translators, interpreters who understood enough to get the wording correctly like—well, this isn't so much because of the Cyrillic language or anything, but attitude and altitude has been one of the, I don't know, stumbling blocks, at least in English on our side, because people don't realize that when you're planning the trajectory and you talk about attitude, that you mean attitude or the orientation, not altitude of the orbit or something. Even our own editors would go in and change—think they're doing a spell check and change the wording.

But, of course, if you knew the context of the sentence enough technically, you'd know those were two different meanings entirely. But in Russian, you know, *orbita* and all that was fairly close to some of the same terms. But even so, you have to get all the language just right about what the agreements were, so it took months. I'd say the Party sign-off delay, and, of course, they didn't—you know, it was just red tape. Literally, they'd just have to go through and send it through two or three bosses that didn't know anything about space, but had to sign off on it, make sure it didn't have some commitment that they didn't want to commit to. But that probably added another 20, 30 percent.

So, overall, it probably took twice as long as normal, but you know, it was a fairly technically challenging flight in terms of the overall flight, coordinating the rendezvous and so forth.

RUSNAK: The way I understand it is the impetus was on the U.S. to rendezvous with the Russian in orbit.

YOUNG: Yes. They served as the target. Well, I've told some of these stories. I can't remember now. I haven't told you any of the ASTP.

The other funny one that occurs to me is, oh, this was probably a year before the actual flight. We had finally made an agreement with them. We were worried about their orbit determination, their tracking data, because even to this day they really—in fact, in those days, they had two or three tracking ships around the world for their actual missions where they could get data on the other side of the world from Russia. Well, if you only have a set of tracking from Russia, even though it's huge and a quarter of the Earth's circumference or whatever, you get kind of ratty orbit determination, and that makes your rendezvous error sources go up and so forth. Not that you probably would fail a rendezvous, but just more costly, so you have to budget more propellant and so forth.

So for years, a couple of years, we were worried that they wouldn't give us a good-enough orbit, so we hashed out an agreement with them to track for us and the NORAD [North American Aerospace Defense Command] people at Cheyenne Mountain [Colorado Springs, Colorado] to track—of course, they'd do it anyway—to track a Soyuz and send that data to us here at Houston, and we'd compare it with the Russian vectors that they sent us. So we wanted to do that to verify that their basic tracking, augmented by NORAD, was going to be sufficient. So we had an agreement to do a tracking of a Soyuz.

But in that day and time, the policy was they would never tell us when a launch was coming up. Professor Bushuyev would hint at it to Glynn Lunney, like, "I think we'll be able to pull this test off, you know, in a couple of weeks or a couple of months." So the agreement was that he would call Lunney within ten minutes after they launched. Of course, they knew that we knew when they launched because of NORAD and our other resources, but he would call Lunney and tell him, "We just launched a Soyuz, so get up and start tracking it."

So, sure enough, of course, they launched it in the daytime over there, it was the middle of the night here, and the agreement was Glynn was supposed to call me, because I was our interface with NORAD, and we were supposed to alert NORAD. I would alert NORAD, and they were already tracking it with certain resources. But when they put these other radars on it, they'd send us some data and so forth over a twelve-hour period or whatever.

Bill [Wilburn R.] Wollenhaupt, who was a navigation guy in MPAD, really good guy, unfortunately he's no longer with us, he was the nav guy that I was working with. So, sure enough, about 3:00 in the morning, one summer, I get this call from Bill Wollenhaupt. "Is that you, Ken?" He lived up in LaPorte or somewhere.

I said, "Yeah." I said, "What's going on?"

He says, "Glynn Lunney's been trying to get a hold of you."

I said, "Really? Well, he hadn't called me directly."

He says, "Well, the professor called and said they'd launched a Soyuz, and we need to get on it."

I said, "Well, why did Glynn call you?"

He says, "Well, your phone number is wrong or something."

I said, "Really? I've been in the book for—." At that time, I'd actually only been in the book for two years maybe, three years, since I moved to El Lago in '71 or so. But I'd never looked at my own phone number, but it was one digit wrong.

He said, "Yeah, Glynn tried two or three times, but he kept getting this woman down in Kemah, and, boy, is she mad at you," because it was one digit off in the phone book.

So I finally called Glynn and I called NORAD, but I told Glynn. He said, "Boy, I woke her up twice, she was really hot, asking for Ken Young."

I says, "Do you think I ought to call her and say, 'This is Ken Young, have I got any messages?'" [Laughter]

My wife says, "Don't you do that. Your address is right in that book." [Laughter] So I never to this day knew who she was. I never even looked. Well, I did look, and it was one digit wrong under my name, but I never did find who the gal was in Kemah. I didn't want to bother her any more.

But, anyway, we did the test and we found that we really needed to use the NORAD data, but we had made all these agreements, you know, as a matter of pride or whatever, technical pride. So we always had on paper that the Russian vectors they sent us would be the source of our rendezvous calculations, but we used the NORAD data. It was much better. So that was another funny—we had some great times there, but Moscow, in those days, was just the pits.

I went back in '94 for Space Station, and it was way better, but it's still not the garden spot of the world, I'll say. But we had a good time. And the guys I worked with, including a guy that had originally on ASTP worked on the tracking ship, his name is Viktor [D.] Blagoff, he's the director of the MCC-M [Mission Control Center – Moscow], the Moscow control center. Well, his title is actually deputy director, but he really runs the place. The other guy's kind of a figurehead, an ex-cosmonaut. But Viktor's moved all the way up.

But he and Oleg were great friends and still are, I guess. So each one of them would have me pull jokes on the other one and such, and taught me some Russian phrases, you know, that I could use on each one of them. I never did really learn to speak Russian particularly well. Read a little bit of it, but that Cyrillic alphabet stuff is tough. The endings are really tough. It's hard to figure.

But that was a great experience. Actually, my wife came over and met me in '74 when we were over there. For four days, she came over. We toured Europe afterwards, but also saw the sights in Moscow. It's an interesting city, that's for sure.

I see Victor every once in a while because he's still working Space Station and Mir and so forth. But Oleg, I've talked to him. In fact, I saw him about three or four years ago here. He was here for some German satellite experiment. He's working different programs. We had agreed to meet for a twentieth reunion in Paris, but we didn't make it, neither one of us. I don't know if anything happened about ASTP, but, anyway, we had that as a running joke that we would meet in Paris for our twentieth reunion. Let's see, it's coming up the 25th or 30th, pretty soon. Well, it would be 2005.

So that was ASTP. That was a challenge, to say the least. I guess technically you probably read or hear that we gave away the farm, that we gave them too much technical information, as opposed to we could barely squeeze out stuff about the Soyuz and so forth. But, frankly, we got the docking mechanism from them that is used on Station now, in effect, a follow-on of that same mechanical device, not that our mechanical docking guys aren't pretty expert on that, but I mean that's one of the things that's benefited both countries and is benefiting the Space Station now.

If anything we gave them, it was probably process and techniques about how to plan a mission, not that they hadn't planned a hundred more than we have through the years and successfully, most of them. And they were totally hindered by the Party red tape stuff. But I think we taught them some about the way to set up working groups and do this logical approach to a mission plan and technical agreements and that kind of thing, a little more structured than they were used to working, which is mainly because of their budget. They don't have that many guys in the industry, and they just sort of fly by the seat of their pants.

That's a good joke. Tom Stafford—talk about learning to speak Russian or not. He's an Oklahoman, you know. He, of course, had to learn pretty much to talk with the crew, his counterparts, [Alexei A.] Leonov and so forth. But he never could get over his Oklahoma accent, and he really would screw up some Russian words.

They were having a big post-flight celebration dinner. I wasn't at it, but I heard this later. He got up to make his talk about how great it was working with the cosmonauts and such, and they were, and still are, great friends, I guess, he and Leonov. Of course, he insisted on doing his speech in Russian. He meant to say that Leonov was a great pilot and that he flew by the seat of his pants. In Russian he used the words that essentially said that Leonov was a flying asshole. [Laughter] And, of course, Leonov just laughed, cracked up, and it supposedly cracked up the whole audience. I don't know if Tom has ever lived that one down. "Seat of the pants" didn't translate very well, at least the way he translated it into some Russian words.

It was fun. We learned from them some things, some technical things, and, really, some philosophical engineering things like we probably go overboard on testing, especially hardware testing, and again, they're forced by budget constraints not to do all these elaborate tests, which we've done that and skipped some that had great consequences and problems. Basically, they build their hardware and do some testing, physical testing, but they don't do a lot of analytical testing. They don't do a whole bunch of Monte Carlo runs, as they call them, to test the envelope, the thermal envelopes and so forth. They just try to hit it in the middle. Then they get it on orbit, and if it turns out that it wasn't right, they fix it. If it's not broke, you don't fix it.

I think we're into some of that now with Space Station, because it's just so expensive to test everything on the ground and, especially, analytically test it or analyze it to death with all possible failure modes. It's just there's a point of diminishing returns on all that. I think some of the people at Johnson, anyway, have learned that the Russians do some things pretty cleverly.

We didn't really give away anything. I mean, the Apollo technology was not only twenty years old by then, but, you know, it was well-known public record, and they could get Mattel models that were better than anything we shipped them. So it wasn't a giveaway, and neither really was the Shuttle-Mir thing, in my opinion. In fact, I think we benefited because of the automatic docking technology that we've got now in the Station. That's all Russian-built. The U.S. still cannot do an automatic docking, rendezvous and docking, for that matter. I mean, we could if you had to, but you'd have to build the hardware and software and do it, whereas they've been doing it for twenty years now with their Salyuts and the Progress resupplies. That alone, that technology alone, has got to be worth millions. Of course, we've given them billions. Anyway, it's not a one-way street.

We learned a lot about the Russian character, too, or at least the guys we worked with, just really upright patriotic people. You didn't hear any communist propaganda or anything. They didn't believe in communism any more than anybody else. And most of them have a great sense of humor, that's one of the things I've found, particularly. Very patriotic. They are patriotic, but then we are, too.

And they're paranoid. The Russian people are probably—it's a generalization, but they're particularly paranoid as a people, and it goes back before [Vladimir I.] Lenin. It's because of the czars and the serfs and slavery, but they always seem to think you've got an ulterior motive to any kind of goodwill that you might do for them or being a good Samaritan or something. They always think, "Well, you know, nobody's this nice. There must be a hidden motive." They're sort of paranoid, is, I think the best term for them.

But the engineers suppressed that, and maybe some of their managers were such that when we had to write these agreements, that's why they made sure they were read exactly right, because they didn't want to be giving us anything or getting snookered. I imagine they don't know that word in Russian. [Laughter]

RUSNAK: [unclear].

YOUNG: So that was a great five-year experience, basically, five years, I guess, we worked on it. Flew it in July of '75, and highly successfully.

That flight was the first flight that we in the U.S. used a geosynchronous satellite like the TDRSs [Tracking and Data Relay Satellite system] that we have had now for Shuttle for years, and Station. I can't remember the acronym now. ATS-F, I think it was Applied Technology Satellite. But it was the first one. It was the ComSat that was capable of talking to the CSM, or vice versa, getting data.

So I used to have one my trivia questions that I threw at some people for a few years after the mission was, "How did the Indian birth rate affect the docking of Apollo-Soyuz?" Well, the ATS-F was the only operational geosync satellite that could beam down TV programs to foreign countries, anyway. There were a couple that worked over the U.S., but this one was out over the Indian Ocean and had only been launched like a year before we flew. The time had been sold to various countries and just happened that in the July time frame of '75, Goddard, who operated the ATS, had sold the TV time downlinks for several months to the Indian government to beam a bunch of birth control videos to the whole country, and especially the backwoods, where they'd bring in a little TV and an antenna and play this birth control propaganda to the populace there to try to control Indian birth rates.

So it turns out to do that optimally they had to move the satellite, which isn't real easy, you know. It has a limited amount of propellant to move it around in geosync orbit. They had to move it several degrees to the east to get the optimum coverage of India. So we had planned to rendezvous over the U.S. or the Eastern Coast of the U.S. because we had U.S. sources for

TV and the lighting was going to work out better, because we had to agree on the liftoff times and all that, months, years, before because of aborts and so forth.

So when they moved this ATS-F to India, they moved it, I don't know, 10 degrees or more, and that meant that we couldn't get a live TV picture of the handshake right after the docking occurred over Paris, I think. It was over France or Spain. So we had to move our whole timeline about ten minutes, ten to fifteen minutes, so we could have the ATS-F coverage and get the live handshake between Stafford and Leonov. And the reason for that and changing the docking time was because of the Indian birth control program. [Laughter]

RUSNAK: Certainly not an obvious connection to make.

YOUNG: No, no, but it worked. I mean, it was a really good picture. I mean you're talking '75. That's one of the first live in-orbit, if not the first, probably was the first live in-orbit TV other than right over the U.S. like we did on Skylab in Hawaii, because of ATS-F.

Now we have TDRS. In fact, we have one over near there and two or three others around the globe, so you get almost 100 percent coverage for Space Station and Shuttle. So that was an interesting little tidbit.

We tried for months to get them to delay that move. They wouldn't budge. This was an economical thing, because the Indians had paid them X million dollars for this air time. So it wasn't that hard to shift, but some little details that you have to go through.

I remember we did that on Apollo 11. We had to change the landing area a little bit so Nixon could be shown on live TV shaking the crews' hands when they got on the aircraft carrier. Had to move the entry point a little bit, but that's just part of the business in a free society.

So, '75. Then what did I work on after that? I guess we were working Shuttle, of course, all during this time, off and on, different planning things. I personally didn't work the Shuttle ALT [Approach and Landing Tests], the atmospheric test phase in '79 and '80. Of course, at that time until mid-'75, we thought we could get Shuttle off maybe in the seventies, maybe. But due to the tiles and the main engine problems, all of that slipped several years.

So the next fun thing I did was Skylab reentry in '79. Well, we started in '78 maneuvering it and trying to predict when it was going to come in. Myself and Ed Lineberry devised the technique. You had no propulsion capability, although it did have what they call the TACS [Thruster Attitude Control System]. I forget what T stands for. But, anyway, a cold gas system of attitude control that they could roll and pitch the vehicle a little bit.

Back when we left it in '73, storage orbit, we fought for, oh, several months, with Marshall again, and mainly with the program office people that were afraid that if we boosted it with a CSM, we'd have a loads problem because of the interface, of the docking interface, on the front end was not designed to take an SPS firing, which is a fairly—it's 900 pounds of force, fairly good kick. That was one fear, is that it might buckle something and cause a CSM safety problem for the last crew.

The other problem was if you used RCS, you could probably get it high enough to—by that time, of course, in '73, even Bob Smith was going, "Well, maybe this cycle isn't going to be quite so low." Of course, it was still five, six years away, but he had seen the Russian Ohl stuff by then and was maybe having second thoughts about what his predictions were, but it was too late. So we came forth with another argument: "Let us put it to 275 miles. We think it will survive a really high peak of solar cycle drag."

They were fearful that if you did that with SPS, you not only might have a loads problem, but then when you went to de-orbit, if the SPS didn't work for de-orbit, you wouldn't have enough backup RCS fuel to get down except in one part of the orbit, because it would be elliptical. They wouldn't consider circularizing at 275 miles. So we lost that battle after several months of trying.

Then they finally made a decision to let us use the RCS excess that was still in the third CSM and boost it, what we could, and not eat into any of the backup RCS propellant. So that's what we did. We took it up at like, I believe, three miles, 238-something, and that was all we could do, but we did do that. That probably gave it six months of life time, as it turns out, but we needed about thirty to fifty miles to make it over the hump, so to speak, in late seventies.

But then we realized, even the Marshall guys, when we got into '78, realized that we were approaching a new high peak, and it wasn't going to make it. We left it in what we call a gravity-gradient attitude where we're sort of end-down to the Earth and no active attitude control. Since it only had one panel, it sort of served as a fin, and it just kind of goes LVLH, as we say, but it's gravity gradient with the heavy end pointing down. Of course, that didn't take any attitude, fuel, or anything. But it presents the broad side of the whole station, which is 150 feet long, actually, more.

You've probably been in the Skylab mockup over here. Anyway, that's almost the worst drag. The worst drag you could have would be that with the panel out to the wind, so the drag was maximized, practically, in that gravity-gradient attitude. But to take it out and fly it nose on, head on, which would be the minimum drag, was just impossible for many orbits because the TACS system was just a few pounds of helium, nitrogen.

So we devised, Ed Lineberry and I, working with some Marshall guys, devised what we called the variable drag attitude. There's an attitude that you can put it in. Modulated drag attitude, that's it, that you can put it in by taking it to different attitudes and changing the drag such that we could, hopefully, make it reenter at least maybe on the right orbit, if not in the right place on the orbit. So we worked for about a year trying to figure out if we could do that and then at least extend the lifetime a couple of months into better conditions for the eventual entry in terms of the ground tracks. Really, truthfully, we didn't have much control at all.

By taking it out of the gravity gradient and lessening the drag for X number of months, we could give ourselves more time to figure out what to do. We went through in '77, '78 all kinds of ideas, one of which was Marshall's, to fly an automated device up there and dock with it, automated even though we didn't have that capability. Then, in fact, Marshall paid Martin

Company in Denver [Colorado] millions, I think $25 million to build this prototype tug, I guess it what you'd call it. I can't think of the acronym name for it right now, the tug.

There were some arguments on the DoD [Department of Defense] side that the U.S. needs to have that capability anyway. Of course, the Russians had it, not so much as in a tug, but a Progress had been docking with their Salyuts' stations. But we didn't have that and still don't. The main problem with that, other than the technical risk, was the schedule risk. We didn't think it could be built and tested and flown in time to save it because it was going to come in in '79, come hell or high water. In fact, that was the month that we had the high water, in July of '79.

So we did this modulated drag thing and sort of played with it and lengthened the lag time out about a few months and kind of kept it alive. Some guys went to Bermuda, the only station that you could talk to it from, practically, and got it activated, which, in itself, was a big achievement for flight control. To their credit, they did a pretty good job. They and the Marshall guys did a pretty good job just getting back alive after six years dormant, drilling holes in the sky.

So we decided, you know, hey, all we can do is work with NORAD and hopefully apply this modulated drag to force it into one of the least populated orbits. I have to admit, in hindsight, we were just damn lucky. We didn't have that much control, but by great fortune it was on the least populated rev that it came in. Our only other control was that we had a plan, and we had analyzed the heck out of this, we and Marshall, and to a certain extent, the NORAD guys, but they really weren't. They were just responsible for tracking it and doing their prediction of entry points and all that. But we had different tools. Anyway, our only other control was that we knew if we just left it in this attitude, torque equilibrium attitude is what we ended up with, where you balance the gravity gradient torques with the aerodynamic drag torques.

Of course, that changes as you come down in altitude. So it's certainly not a static attitude. But it's called TEA, torque equilibrium attitude. You can predict the drag, I mean, very well, because you know what its wetted area is. So we knew that if we tried to hold it in that all the way down to eighty miles or so where the drag gets unbelievably high, sensible atmosphere at sixty miles, anyway, that it would tumble up randomly and then we wouldn't know what we had, whether it would trim out like an arrow, maybe with that panel, or whether it would go broadside, or, more likely, just a total tumble. There again, you don't know what parts are going to be exposed as drag areas.

So we knew in the last day that it would be better to tumble it deliberately at a given time so then it would probably just stay in a known tumble, and we knew from analysis about what that drag was. Of course, the other big unknown really is the upper atmosphere densities, which changes diurnally with the sun and the sun's position and not so much the ultraviolet heating that caused it come down from high altitude. That's free oxygen and drag effects. But down in the sensible seventy to eighty mile altitude range, we don't have that much data on what a local density is, as where it comes through in the Earth's atmosphere.

So, anyway, we knew that we had that last-ditch kind of tumble thing that would maybe extend it, compared to a different attitude or random tumble. We could extend it, its entry point, by a few thousand miles, or hundreds of miles, anyway. So in the last hours, I can't remember the timeline—I ought to go back and look sometime—but it's in the last twelve hours, we had planned to tumble it at a certain time and then, you know, hopefully get it into the ocean, one of the oceans.

We got down to twelve hours or so to go, approximately, and found that it was maybe going to reenter and come in on an orbit that went over Nova Scotia [Canada] and Newfoundland. It was missing the U.S., but our Canadian neighbors wouldn't have been too happy. And the Atlantic shipping lines comes through there, not to mention air traffic, but the odds of hitting an airplane are pretty small. But when us and NORAD and the Marshall guys all started to agree that, "Look, it looks like it may come in in the North Atlantic," we said, "Well, we can fix that." So we tumbled it early, by two or three revs, like three or four hours early, and we knew that would push its entry down into the southern hemisphere. Unfortunately, we overshot by a few hundred miles, and the tip of it hit Australia.

RUSNAK: Actually, that might be a place for us to stop, because we're almost out of tape, and we can pick up from there.

YOUNG: Okay. So we tumbled it at least three or four revs early, and that forced it into the southern hemisphere. We were actually aiming, so to speak, for the mid Indian Ocean, you know, way south of India and that continent, and did pretty well. If anything, we deliberately biased it that direction and made sure it didn't come in in the North Atlantic or even the Atlantic. Of course, it passed south of the Cape of South Africa. So at least on that orbit, it wasn't a fear of hitting Africa or anything.

So, came through mid Australia. The footprint, as best an analyst could figure out, was the total length of, well, 500 to 1,000 miles long, from the toe to the heel or vice versa. Of course, everyone knew that the heavy pieces that were more likely to, you know—the surviving pieces would be the heavy pieces. Like they had a lead-lined safe where they had stored photos, film. Then the helium tanks, or titanium or something, I don't think they were titanium back then, but some stainless steel, I believe. And parts of the structure of the S-IVB workshop, even though they were aluminum, they were highly protected by outer skin and all that. And part of the Apollo Telescope Mount was pretty hefty.

There was a great fear that several thousand pounds would survive and be at the toe of the footprint. The heavier it is, the farther, the less drag, so to speak, and the further downrange it would go. And that's what happened. It flamed in over the Indian Ocean. Really, probably, 80 to 90 percent of it went into the South Indian Ocean to the west of Australia. But, unfortunately, the toe was out on about Alice Springs, Ayers Rock, although it didn't hit either one of those. Quite a few pieces survived and put on a big fireworks show for a little town on the coast called Esperance. I don't remember the population, several thousand people, and flamed in over into mid Australia in the outback.

A team went down there. I didn't go. Marshall team of several guys went. Of course, along with the local population, they found a lot of pieces, or quite a few pieces. It wasn't tons of it. But found a dead rabbit under one piece of workshop skin. We toyed with the idea of submitting a request for a bounty. Rabbits are a pest in Australia. There were millions of them, and so they were paying like two or five dollars a rabbit. So, we thought, "Well, we killed a rabbit. Let's just apply for NASA, $5 from the Australian government," but decided not to push our intrusion.

RUSNAK: I heard the Australian government sent the U.S. a bill for littering or something, as a joke.

YOUNG: Yes. There's, of course, a post-flight book on it, I haven't seen it in years now, but Marshall put out about the pieces, and they reconstructed the whole footprint. It pretty well fit the analysis. We used to also kid about it, saying, "Look, if we'd hit it all in the Indian Ocean, we wouldn't have any data to go on." We had to have some surviving pieces to size the footprint. I think one commercial liner was flying across the Indian Ocean, and actually saw—the pilots and the passengers saw a bit of the entry. It's kind of like Mir when Mir came in, some of the people actually saw. Of course, they were out there deliberately in the South Pacific.

But it ended up not hitting anyone. It was a fun thing to work on, although it was pretty political, and we had to do a lot of public pronouncements to allay the fears of people.

A couple of stories. First of all, we decided, me and a friend of mine, who happened upon a tee-shirt-making machine, decided to make a bunch of Skylab-entry tee-shirts for profit. So we did that weeks before the entry. We had predicted the day of entry probably a week before we knew, pretty much. It was July 11th. It was not of any great significance, but we put out to various people like Alex Benney and Cathy for designs for our tee-shirts.

My favorite one of all time, I wish I'd have worn it now today, Alex Benney came up with. It showed a flaming workshop, you know, with flames coming out, and it says, "Skylab: One Giant Step on Mankind." [Laughter]

Then we had Chicken Little ones, you know, "Skylab is Falling," with a chicken running around. Several other good ones. Frankly, we sold about a thousand of those tee-shirts. It was fun making them at night. We even sold them in the control center, because they had let the press in there, the first mission, so to speak, that they'd ever let the press into the control center during the mission itself. Of course, all the networks were covering it.

Jules Bergman was the ABC alleged science journalist correspondent. He was just an arrogant know-nothing, but he, of course, thought he knew everything. He would bug us constantly, I mean for a week, two weeks ahead of the actual entry date, about, you know, "When is it coming? What's it going to hit?" We kept telling him. One thing I will say, though, he and his ABC crew bought quite a few tee-shirts from us, so we didn't mind them being in there. [Laughter]

No, they were a pain, because the truth was, and we told them over and over, you just won't know within a half a rev, within a day, much less which orbit and where around the Earth. It's just not predictable. Cathy, particularly, had been caught by Jules in the hall there outside the control room, the MOCR. In fact, we worked out of the trajectory room down the hall. There was hardly any activity in the main control room, because it really wasn't a mission, you know. Several times Jules had caught her or me in the hall and was trying to get us to commit, not on live TV, but some kind of comment that he could use as to what we thought, when and where it was coming in, and so forth.

We got kind of tired of it, because Cathy would plot these ground tracks and such as it came down in altitude. Then, of course, at this time, several days ahead, it was still over 100 miles up. We had told Jules that, "Hey, due to the nature of orbital mechanics and the atmospheric drag and everything, error analysis shows that the [envelope shrinks]." We tried to tell him that what we meant was the dispersion of the envelope shrinks as you get closer to the actual entry day or rev.

He was always trying to impress his colleagues, one of which was this good-looking gal assistant that he had. She was always in tow with him. He was always pontificating about, "Well, this orbit does this and so forth." He bugged us, and Cathy particularly, about this shrinking orbit. "I'm not sure I understand how the orbit shrinks."

"No, Jules, the orbit doesn't shrink. It shrinks in altitude, yes, but the dispersion around the ground track of where you think it's predicted to come in shrinks, because your knowledge of the drag is better and better as it gets down, especially if you know what attitude you've put it in or can predict it will go into."

So, finally, Cathy got annoyed with his questions, and so she went off and plotted a ground track. Skylab was in a 50-degree inclination, which means that it goes from 50 north to 50 south in a sinusoidal curve on a Mercator projection of the Earth. Of course, until it finally reentered, it was always in a 50-degree inclination with the equator, which is like that. That's what inclination is, is the angle between the equator and the plane of the orbit. Of course, this ground track marches across the globe. That's what ground tracks are.

Well, he got in his head that the shrinking meant that the inclination was going to shrink, meaning that the latitudes would get lower and lower and closer to the equator. So Cathy, she used to plot our ground tracks on an HP thing. Now remember, this is '79. We didn't have all this fancy PC software and hardware. So she plotted up some ground tracks, and to this day she's never explained to me how she did it. But she started it at 50 degrees and then changed it so that the inclination shrunk down to zero, and then she drew an ellipse there like that was the entry point. So she showed it to me in the hall, and I said, "Oh, that's pretty clever. You've got it shrinking down like that on inclination."

And here comes Jules, right at the right time. He says, "What have you got there?"

"Well, we can't show you this."

He says, "Oh, really, really. This is important." He had this gal with him.

So we said, "No, this not for public consumption. It's for a little bit of that shrinking orbit that you're worried about, Jules, or we told you about."

"Oh, come on, you've got to show it to me. I promise I won't put it on the air or anything." So we finally, Cathy hands it to him.

He's looking at it and he's showing this gal. He says, "Yeah, see, that's exactly what I've been telling you. As it gets down lower in altitude, it gets down here near the equator." Of course, she had drawn it on a certain part of the Earth. I forget where. I think it was in the South Atlantic or the mid Atlantic. He says, "So it's coming in and shrinking down to the mid Atlantic."

I said, "No, no, no, there's no guarantee that it will be anywhere close to the Atlantic."

He said, "Can I keep this?"

We said, "Yes, but promise you won't show it on TV." He promised and so he goes off down the hall, explaining all this to this gal. We never did tell him. I think he may have figured it out later on the entry day when the ground track never did shrink. We had it on the world map, of course, as best we could simulate it. He was something else. I guess he's dead now. That was an experience just to deal with the press directly all the time. He, in particular, was a pain.

RUSNAK: What do you think of the overall portrayal of this whole episode in the press and with the public?

YOUNG: Well, it was a little bit exaggerated to sell newspapers. Of course, the typical nut factor, faction that thought that it was really a super threat to humanity. Like I said, we had to calculate kill probabilities and all that, and then try to explain that they were just so uncertain they just statistically didn't mean hardly anything. If you had ever studied statistics or were used to error analysis, I mean it's just too many variables and too unpredictable.

But, overall, it was kind of a fun thing, and we had a big splash-down party afterwards. Splat-down, we called it. Skylab splat-down. But it was fun. I guess I worked a year and a half on that, that alone.

Of course, meanwhile, everybody else was trying to do real work on Shuttle and get it flying. They had already flown some atmospheric tests, landing tests. At that time, it turns out, we were two years, almost, from launch.

The media was its usual, know-nothing, sensationalist approach, but just like the Mir was, just something to sell papers. But it was fun.

Then, I guess, we got into Shuttle, hot and heavy, right after that. Like I say, that was the month that we had the terrible Claudette Tropical Storm flood. I think that was maybe a week, maybe two weeks, after Skylab reentered. Then, of course, we had jokes about that. Well, it punched a big hole in the sky, and that's why we're getting these terrible rainstorms, forty inches, and then ten or fifteen days later it rained again. It was a bad summer in that respect, but we had a good time on Skylab.

Charlie [Charles S.] Harlan was one of the flight control leads for that. I guess he was *the* flight control. He's one of my old buddies. It was a learning experience, too, not just for press relations, but we and the Marshall guys learned a lot, and we learned a lot about solar cycles and drag and that kind of stuff, too. Some of that has been directly applicable, really, to the Space Station planning, for instance. Shuttle, I mean you worry about drag, but not in the same context because of the ten-day missions, but Station has an altitude reboost strategy that we started working on in '83 or something for it, and it's been refined to where now they've got some pretty good solar cycle predictions. Of course, we've had two of those since Skylab, and we're right at the peak of one right now, as a matter of fact. And the predictions have gotten better all around the world, and that's still a very interesting area.

Bob Smith retired years ago, but even he probably has seen the error of his ways now, or did back then. It's still pretty much an art. Well, not an art; totally an art. This technique that Ohl devised, the problem with it is that you have to have like the last two or three years of the previous cycle of data before you can do a very valid future prediction. Of course, that still gives you five years, roughly, because of the last two years of data and then it goes into a five-year up cycle and a five-and-a-half year down cycle and so forth, or thereabouts. So the predictions have been pretty good the last two cycles, and fairly high, but not outrageously high like they were in '79.

But that has a great effect on logistics planning for Space Station and the Shuttle and Progress resupplies and all that, what altitude you have to keep the Station at to be in certain safe conditions for failure to get a refuel ship up there and that kind of thing. So we learned a lot from Skylab about all that.

So then, yes, we worked Shuttle. Of course, I'd already been working it ten years when Skylab came in. I became branch chief sometime in that time frame. I'd been section head for ten or fifteen years. My branch, Flight Planning Branch, did all the Shuttle mission planning for the first six or eight years.

Then MPAD was dissolved, which is a whole other story. I personally stopped working on Shuttle about '83 or '84, and started working Space Station. But we planned all those missions, with the help of contractors, of course, until '86 when USA [United Space Alliance], actually Rockwell at that time, won the contract to do the planning with what became DM

[Flight Design and Dynamics Division] support in the Missions Operations Division. MPAD was abolished in 1990 or something.

Anyway, we planned the early Shuttle flights, trajectory-wise. When I say we planned them, JSC has always been split in the functions of trajectory attitude and consumables planning compared to the crew planning, the astronaut timelining crew planning. They've called that flight planning for years, and for whatever bureaucratic reasons, those two organizations have been fairly separate. There are good and bad with that. I was proud to say I was never in Mission Operations Directorate. I was always in Information Systems and in MPAD, which, way back in Kraft's day, was Flight Operations Division, but never Mission Operations Division. Gene Kranz's directorate was MOD.

But, anyway, various reasons, pro and con, for why those are separate. It's really pretty inefficient and dumb, frankly, but it's just the way it's grown up and legacy is still there. It just makes the necessary communication just that much harder because of the politics between the directorates and the divisions, but now DM, a division of MOD, supervises USA in the churning-out of the mission plans. Of course, another division still does the crew planning. They're both in MOD now, whereas we were in MPAD, separate from that bunch, till it was abolished in middle '90 or whenever that was. I had already retired.

Something I never forgave Gene Kranz for, but for technical reasons, and that is that it's become obvious now we didn't have the right terminology, but MPAD was the systems operations integration function at the center, and that's, I think, a necessary and distinct bridge between ops guys, console-sitters, and systems engineering designers. If you fill that gap between them with a little bit of both, which is what we did in MPAD, you do the mission planning, like I've told over and over, years ahead of anybody, certainly way before the flight controllers learn their position and their flight rules and all that, and you work with the systems guys in the actual design in a lot of cases of the spacecraft, and, of course, you try to influence how it will be flown, because you've flown and know the operational constraints and druthers.

Then the systems guys, you know, they want their system to work efficient and all that, and a certain amount of redundancy. So in my opinion, it's essential that you have a systems integration group that melds the two and their guidelines and constraints, because, of course, there are operational constraints and system constraints. Who better to do that than the ones who plan the basic trajectory and the mission overall, how long is it going to be, what is the lighting situation around the orbit, and what orbits you go into, and what is the fuel situation, and all that, which DM tries to do now for Shuttle and Space Station, and they do a pretty good job, but it's, if anything, probably a little too tunnel vision in that they really just look at the next few missions.

They have a couple advanced planning guys, but since the Shuttle missions are kind of cookbook now, as compared to way back, you know, they can kind of churn these things out. Probably now it's overkill with detail and worry about safety, but that's just one offshoot of bureaucratic government. You fall into these.

RUSNAK: You actually hit right on one of the questions I was going to ask, which was the relationship between MPAD and engineering and the mission operations guys on the other side. You've described well, I think, your function as a sort of intermediary between, but if you can just maybe describe a little bit about how you worked with each of those, I guess, separately.

YOUNG: Yes. Well, like I say, the key is not so much being in the ops area, but at least working in the ops environment for years and years or mission after mission, so you know what the ops guys have to deal with compared to what the systems guys deal with, too. But, naturally, every system designer wants his system to be the focal point and perform efficiently and all this stuff, but the ops guy has got to fly the whole machine and trade off different system constraints to achieve the mission objectives.

So MPAD's role was, there again, the integration of that sort of two different schools of philosophy even in terms of safety and redundancy. You know, if you're too close to ops by definition, you've got this what I call the brushfire mentality. You've got to worry about today's problem and you can't look ahead to a mission five years down the pike at a time. You've got to worry about saving today's computer load or saving today's crew for whatever reasons, and you just don't have time to look down the pike. So, if anything, ops guys tend to be myopic, and they should be. Their real function is to fly today's vehicle and get through today's problems. Just by the nature of forty hours a week, or fifty, they don't have time off their shifts to go off and work.

But, see, we in MPAD worked shifts in the back room, but we didn't work all of them in all of the sims, so we had more flexibility. If they were running a routine abort sim, which doesn't involve any orbital worries, then we wouldn't even support. If it's a rendezvous sim, yes, we had guys supporting that because we knew how to do the rendezvous and what all the detailed error analysis was we had to tell them, to tell them that things were in the envelope or not.

But we had the flexibility or the time to look at missions way down the pike, and we spent that time working with the systems guys, learning if it had already been designed, how it really was going to have to be operated, and then if it hadn't been designed or is just a concept, then you try to influence them into "We'll put this much more fuel on board," or "This much more consumable in your tank," because you're going to need it because of the errors of whatever other part of the mission caused errors, you know, either launch vehicle insertion errors or bad tracking or all that kind of thing.

That's what MPAD did in the process of conceptually planning the mission. You would find out all you knew about the vehicle, and we had experts in consumables and in attitude control and navigation and everything. You go in and get them to give you, at that point, say, five years ahead, their best guess at how this thing is going to act or operate, and then you'd either try to influence that.

One story I like to tell about early Shuttle is our consumable guys, of course, worked for years, and I wasn't a consumable guy directly, but on trying to size the tanks, the oxygen tanks and the water tanks and everything for Shuttle, and one of the requirements—and this, again, is an ops kind of thing—what you have to design the vehicle for is in case of an orbital debris impact, either that or a micrometeorite punching a hole in the cabin, which, of course, is where the crew is and usually unsuited nowadays.

So you had to allow in the design phase for, well, what if a hole is punched in by whatever, debris most likely, and, of course, it's not likely at all, but it's something that's finitely possible. So you have to design, say, enough oxygen and nitrogen to pump in there if you had a certain size hole. Well, of course, it's subjective, you know. You could get hit by a rocket body and wipe the whole spacecraft out, or, more likely, get hit by a bolt off of a rocket body that's a half-inch in diameter.

So, somebody, in their wisdom, Don [Donald J.] Kessler was one of the orbital debris experts, along with several others through the years, decided that the requirements should be we have to have enough oxygen and nitrogen, enough air in the cabin to support a hole of one-half inch in diameter for an hour and forty minutes before entry interface. The thinking and the reasoning that went through all that was, okay, you get the hole in the cabin, and, of course, the crew generally would know that immediately. Well, how soon can you come down? Well, that depends on landing opportunities and the weather and orbital mechanics and some procedural things like how quickly can you go to de-orbit maneuver attitude, fire the engines, and drop the spacecraft into the atmosphere.

There again, one orbit is roughly an hour and forty minutes, so sort of arbitrarily assumed, well, you might have to take up to an orbit to not only do the maneuver, but get to entry interface at 60-mile altitude where you have sensible atmosphere. The fact that you haven't got a whole lot of cabin air is only a few minutes from 10,000 feet or so, so you can maybe hold your breath or survive that five or ten minutes. But, anyway, that was the ground rules, which are ops requirements, basically.

But the systems guys had to size the tanks. Well, believe it or not, the way the geometry and all of that, flow rates, worked out, they couldn't make them an hour and forty minutes. I forget how short it was for a half-inch hole. And, of course, you're working with a system, tank designers, the builders, Rockwell at this point on Shuttle, and it's months, if not years, before you really know how big it is and how it's really going to fit in wherever it fits in the Shuttle bay or under the bay.

So after a year or so of sizing and all that, they came up with an analysis that showed, hey, we just can't make the hour and forty minutes, it's an hour and twenty, or whatever. I forget what the numbers were. So, rather than re-size the tanks, we just made the hole smaller. [Laughter] Made it .4-inch hole, because, of course, that was subjective. Then you had to go through all the orbital debris statistics that show, well, we picked a half an inch because that was a round number, .4 is essentially as likely. So we'll use .4 because a .4-diameter hole is only going to maybe occur once in 10,000 flights or whatever the ground rules were for assuming a hit.

Of course, in fact, the Shuttle has been hit several times, and the windows have been cracked, but never anything that penetrated, and never anything nearly a quarter of an inch, a half inch, I should say. They've had a couple of pretty good pings on the windows, but they were, you know, like a tenth of an inch or something, BB-size kinds of—and paint flecks itself.

One piece of the log [logistics] module that was up there on the early Space Station the Italians built got hit while it was up there with a little BB-sized crater, but it didn't go through the outer aluminum skin. So they do get hit, but that's just an example of an operational set of requirements melded with systems. Of course, it was far easier to just keep the tanks the same size, because they've got to fit inside the air frame, and just change the ops numbers, in essence. That's just something you live with.

Of course, you've got to document all that and practice simulate all that, but that's what MPAD's role was in just every area. See, that example alone not only entails all the consumables people that we had for years, and still have, along with USA. In fact, the gal that did most of the analysis, named Cynthia [F.] Wells, she works for USA, went over there in '86 or something, '87. But you had that, consumables systems, and then for the ops side you had the nav guys to figure out how quickly can you get a decent enough de-orbit maneuver calculated on board with the onboard nav state, versus the ground, if this hole occurs out of communication with the ground. Now we have TDRS, so it's practically all the time the ground is talking to them, but they don't always have the latest state vector. Now with TDRS tracking, they pretty much do. So it would be different now.

Of course, when we designed the tanks and the hole size, we didn't have TDRS in the plans, so you had to figure out how ratty can that de-orbit maneuver be and still get you to safe entry point. Then you had the other ops people that you had to figure out what are the landing opportunities, and there are opportunities on every rev. Of course, that's been a fundamental thing for Shuttle. I mean, you can come down, practically somewhere every rev, and they've gone out to those airports. They have all these agreements and deals, technical agreements with these airports around the world. Of course, some of them are preferred, rather than just last-ditch ditch.

But you had to work all that out and conclude that, yes, if you could do the de-orbit maneuver, you can get down to a landing area in an hour and forty minutes or roughly. Well, you could get down to entry interface in an hour and forty minutes, and then it's another twenty minutes before you land, roughly.

You had to make all the agreements with foreign countries and landing airports and landing strips. They even have it on Tahiti and a bunch of islands in the Pacific. Then you have the maneuver guys and the propulsion guys to figure how quickly can the crew go through their de-orbit maneuver prep and turn the vehicle around and do the retrofire maneuver, and how much fuel can you always be assured of having, and all that. So, you do all of that integration of all of those, and you finally agree, true, somewhat subjectively, on a number, like an hour and forty minutes. I think it's still that. It might be—two hours rattles around in my head, and that may be from the whole occurring until landing, but, anyway, it's of that order.

That one little thing alone, which could have implications on the design and the fundamental loading and consumables, is just one of hundreds of things you integrate into a mission. So that's what we did for Shuttle for, really, fully ten years before we flew it. Well, yes, ten years before we even flew it in the atmosphere, much less orbital.

RUSNAK: I think that's an excellent example of just one small piece of all of this, as you say, that goes into planning one flight. It's just one small criteria.

YOUNG: Yes, and that's kind of not minor if it ever happens, but it's really minor in terms of a nominal plan. You have all these parameters that have to be worked and integrated between systems and ops, which if DM and USA didn't have pretty much a cookbook kind of set of missions for Shuttle. You know, they couldn't do it with the manpower they'd been forced into because of budgets.

RUSNAK: Let me ask, when you're looking at a new program, whether it be Apollo or Shuttle or Space Station, and you've got to come up from the very beginning—somebody's got to come up with all these requirements that they have to put in, how does that work? Is it a bunch of guys sitting around a room with some pads, "Okay, what are some factors we have to think of?" and writing them down? What's the mechanism there?

YOUNG: Well, it did, you know, back in Gemini and Apollo, certainly. But, you know, you collect these lessons learned, not that there really is a cookbook, even. But, yes, nowadays, you have a legacy of documents, memos, lessons learned, and hopefully some few old heads that have been through it a few times that know the key parts to each mission, starting from absolute scratch.

Of course, nowadays, there's really not too many of those. Well, the man-to-Mars mission, it has its own unique problems that are unlike anything we've ever flown. But, yes, essentially, you do it with a group of a set of meetings. Nowadays, of course, you have some desktop databases that you can sit there, maybe, and even project up different areas. You'd step through the mission phases with fundamental concepts or ground rules and assumptions, anyway. That's what you always start with, is ground rules and assumptions. Well, mission objectives, and then the ground rules and assumptions that go with, if it's a manned flight—personed flight—then you have to apply all these special rules. If it's a robotic spacecraft, there's different rules and different assumptions and so forth. So, yes, it's not seat of the pants, but there's no real cookbook that has all that.

You get into Shuttle here, twenty years after the first one, it is pretty much cookbook, but even that, each mission has its own problems. The payloads change and all the objectives, some of the objectives change. Right now, frankly, they're looking at—and we looked at this years and years ago—they're looking at extended Shuttle flights. The facts are, with this 4-billion-plus budget problem with Space Station alone is that they're going to pretty well quit assembly here in a few more flights. I mean, they're going to be forced to. They may get a couple more of the partners up there because they have to, Japan, particularly, and ESA [European Space Agency].

But then they're going to have to quit. Of course, they've already quit on the hab [habitation] module because that's too expensive. So the crew is always going to be no more than three, permanent. So they're going to have to fly what we called way back then mated operations, or man-tended, is what the term was thirty years ago, where the Shuttle is docked and you have a crew of eight or ten for, hopefully, more than ten days. You do all this Station stuff, using the Station kind of as a temporary lab, so to speak, and the Shuttle with its sleeping quarters and everything.

For years they've looked at the extended missions of up to sixty days with the Shuttle on orbit. There's some fundamental systems things that have to be resolved and seal reliability and stuff. The Shuttle, of course, wasn't designed to stay more than two weeks, but analysis has shown that it probably could easily stay twice that long, if not four times that long, so fifty, sixty days is probably feasible if you make a few changes and take a few precautions and tests and so forth. So I think that's what they'll be doing a lot of—"they," the missions and systems planners here, in the next—if they're not already. I'm sure they're already doing it, but I mean that's going to be a higher priority set of analyses, I think, pretty soon.

There are those of us who claim that's the way we ought to fly the initial Space Station for five years anyway because of budget and learning-as-you-go kinds of things and extending the Shuttle to its full capability, because, of course, if you amortize that, each Shuttle flight is still, no matter what anybody says, it's roughly a half a billion dollars a flight. Some people say, "Oh, no, it's really only 200 million." Others say it's 700 million, but a half a billion is a good round number.

Ten days at half a billion, 50 million a day, is not what you call economical. But if you use it on orbit as almost a second lab or second Space Station up to fifty, sixty days, or even thirty or forty, you're starting to get really way more of your money's worth, because the cost isn't really the ten days after it gets on orbit. It is probably 20 or 30 percent of it, but the cost is the launch and the landing. The landing's not too tough. But that kind of amortizes the real use. By the same token, if you don't have enough money to go to a second-generation Shuttle, which is a whole new development, then, hey, what's the most logical thing to do, just squeeze every penny out of the Shuttle in a sense or every operational minute that you can. So I think that's what they'll be driven to, or if not already driven to. Hoping [NASA Administrator] Dan [Daniel S.] Goldin will be gone, because I'm not sure he still gets it yet, but maybe he does. Not too enamored with that Administrator.

I'm trying to think back on some early Shuttle flights. Of course, we had some adventures on them. One thing I think could talk about now since it's been twenty years, is we did some secret stuff from day one on Shuttle, some DoD stuff that I was debriefed on years ago. At that time I remember signing one of these little things that says I wouldn't talk about it for ten years, I believe it was. So it's been twice that long, and I won't go into any of the details about it, but it was, for me personally, and Ed Lineberry, it was pretty significant in our careers because it was so secret, top secret, that we couldn't tell our fellow MPADers what we were doing. That puts the pressure on you, because we had a lot of technical stuff to do.

Well, I'll just put it this way. On STS-1, Ed Lineberry and I were personally responsible for the exact liftoff time that we used for that flight, and there's a handful of guys still around here that know that. But most of JSC and the world, the public, thought it was just a standard liftoff time and we had a little delay and so forth. It was actually planned to be at a certain time, and involved DoD resources and such. This went on, frankly, for several flights, eight or ten flights starting in '81.

One of the toughest things I had to do in, I think it was the fourth flight, yes, the fourth flight, Ed had already moved on to something else, he was division chief, and he'd gone into advanced planning or something. So I was responsible for changing the mission objective, changing a mission, actually, changing the trajectory. Of course, these were test flights. The first six were basically test flights, orbital test flights.

The deputy division chief was Claude [A.] Graves, and the so-called mission manager for the fourth flight was a guy named Morris [V.] Jenkins, who was a character in himself, and really he will never interview with you, I guarantee you. But Rod [Rodney G.] Rose has probably mentioned him in his interviews. They were both English. Morris, I guess, is an

American citizen now, like Rod, but, anyway, they came over with AVRO [Aircraft Company, Canada] in the first Langley [Research Center, Hampton, Virginia] bunch from Canada.

Anyway, Morris, the reason I know he won't interview with you, is because—Rod Rose may have told you this story. Rod had been talking to a guy about writing a book, and the guy had interviewed Morris on his own, this journalist. Rod told me this story, oh, three years ago. But Morris lives up in Central Texas, Lago Vista, I think. He had done a bunch of tape interviews of Morris for his own book, and then he gave it up, and then Rod started talking to him and thought, well, maybe he'd use it. No, I'm sorry, it was the other way around.

Rod had interviewed Morris, thinking about writing a book on Apollo, and then decided it was too much trouble or whatever. But he had these tapes of Morris, and this journalist found out and came to Rod and said, "Can I use those? I'm writing a book." I don't know if he's ever finished it or done it. I don't think he did, but, anyway, so Rod called up Morris Jenkins. Rod lives in Wimberley and Morris lives in Lago Vista. He was supposed to live in Wimberley, but he moved out. He had a fight with his builder or something.

Anyway, they are, of course, countrymen and old friends and co-workers. Rod asked him if Morris would mind if he used those tapes or gave those transcripts to this journalist. Morris said, "Absolutely not. I don't want them to ever go out." I don't think there was anything controversial about them; it's just Morris. You've got to know Morris to understand his personality. Maybe you can get him in here, but I doubt it.

But, anyway, he was the mission manager for the fourth Shuttle flight, and because of this secret requirement, I had to change some fundamental mission timing, which that part of the mission was being done under a cover objective. One of the flight planners, the crew planners, he didn't even know what the real objective was, but we'd worked with him on what's called a DTO, detailed test objective, and it had to do with tracking the Shuttle with certain radars and stuff. So under the guise of that DTO, we had put in this sequence and this orbit set of maneuvers and stuff.

I wasn't even able to tell Morris Jenkins or our deputy division chief Claude Graves—who's still here, by the way, he works in advanced planning now, himself—anything. Ed Lineberry and I were the only two in MPAD that knew this objective. So we had, of course, this process, this approach to the mission plans. You do the nominal trajectory X—I forget the terminology now, but six months before the flight, you put out this one plan, and then you put out the final ops plan two months before or one month before, and then the final plan, which nowadays is just a tape, it's not a whole document, but in those days we were still putting out the documents and all this detailed data, datapack that goes with them for the simulators and the systems people.

So I had to make this change in the last month before the flight because of the DoD side of the situation. It was right at Christmastime. I remember I had to go—I went in and talked to Morris, and Claude Graves was essentially running Shuttle planning then because Ed was off doing this other advanced planning stuff, Mars mission or something. Anyway, well, he wasn't able to tell Claude, his deputy, that this was even going on. So I had to go in and tell him, "Hey, we're changing the mission."

And they go, "What?" Morris said, "I'm the mission manager, and I wasn't told, consulted. You can't do that one month before the flight." I think it was about a month or maybe six weeks before the flight. It wasn't a huge, huge change, but it was fairly significant. And they both grilled me as to why.

I said, "Well, it's this DTO we've got."

"Oh, the DTO's not that important. It's just a minor objective."

I finally had to get Ed Lineberry. I had to look him up and tell him, "Look, just tell Claude to quit asking me questions about this. We're going to change the trajectory." So Ed did, I guess, and Morris never did understand. "This was violating our process." But that was one of the fun things.

Then Hal Beck, who's another good old friend, and was Morris's branch deputy for years and took over for me when I retired, the Flight Planning Branch, I turned this thing over to Hal Beck after about six flights and I started working Space Station.

But, anyway, that was some top-secret stuff we did, which was super successful, as a matter of fact. There, again, it's another case where I had to really credit Ed Lineberry. He did all the math for our calculations. We'd go into this little secret room somewhere in the Mission Control Center that nobody knew about except about maybe forty or fifty guys around the center. We'd go through all this encryption stuff and learn all these passwords. It was a pain, really, but it worked pretty good.

So, STS-1, to get back to it, we had it all set up, but it turned out we needed to change the liftoff time by, I'll say, two minutes and thirty-two seconds. I can't remember, but it was of that order. You know, we had just picked for the planned purposes and everything, I don't know, 3:00 o'clock Eastern Standard Time. I forget the liftoff times now. But it was just an even hour, you know, because the window was fairly large otherwise. It's several, couple of hours, set by mainly the abort constraints and the downrange landing points and all of that. You have to figure out the lighting for emergency landings and all that.

So we got into the last hour of the countdown or so, and we figured out that we had to change the liftoff time. Of course, one of the flight directors and one of the FIDOs was in on this, Jay Greene, who's, of course, now still around and has become a program managing type. But we couldn't just go in and change it or it looked very strange, so we made up this problem in a certain computer on the ground in the control center. We just told Flight [Director], "Look, we've got to have about a two-and-a-half-minute delay." We had already had this agreement.

So he goes on the loop and he says something like, "Well, the backup DC computer," blah blah, "is looking funny. The guys need to stop the count for two to three minutes and make sure it's working okay."

So, of course, all the guys down in the computer center are going, "There's nothing wrong with the computer."

The FIDO, Jay Greene, says, "Look, just stop the count. We're going to check it out," and they went through some kind of make-work motions. Then two and a half minutes later, Flight comes back on and says, "It all looks good. Let's pick up the count." There was a lot of that stuff that goes on that some people don't know about.

The other funny story about that area, if you ever interview Hal Beck, which you should interview him if you possibly can, because he's got a world of stories, he and I used to laugh. I don't know if you know—you probably don't know anything about the black world of DoD. They call it "the black world," where you have all these passwords and all these secret code words for projects, even. Of course, in addition to this particular one, we worked some other DoD payloads and so forth that launched on early Shuttles.

We'd go to these secret meetings, and you had to sign these papers, you know, with what's now, I recognize, as the National Reconnaissance Office, NRO, which you couldn't even say their name back twenty years ago. Now they even have a museum up on the Beltway between Baltimore [Maryland] and Washington [DC], and they've got all this cryptographic stuff. You ought to get by there. It's a Cryptographic Museum. That's NSA [National Security Agency], but NRO is another secret agency.

So you sign these waivers and swear that you will never reveal, or at least for ten years, you will never—you couldn't even talk about it being a project, or you couldn't even acknowledge that there was such an office, or that there was such any kind. You couldn't even say this was a secret payload. You couldn't talk about it.

But, anyway, they all had cover names. Sometimes during the early Shuttle flights, we were doing future planning for four or five of these things, and they were being built by various contractors all over the U.S., mainly on the West Coast, Sunnyvale [California] and Seattle [Washington] and L.A. [Los Angeles, California]

But what was funny, and the thing that Hal and I always laughed about, is, you know, we, of course, were just doing Shuttle mission planning and didn't have time to really get into the details of those things. We'd show up at the secret room in Building 1, and these guys would come in that we didn't know from Adam, and they'd tell us what their payload objectives or their problems were, and we were integrating them into the Shuttle in one sense or another, either in the Shuttle or looking at the Shuttle or whatever.

They all had these code names like Lamppost and Wagon Wheel. Those probably aren't being used nowadays, but Firefly and just random things, you know, Water Glass. But when you have four or five of them, we couldn't keep them straight, and you can't take any notes. You have to memorize it all. Of course, we cheated and did a couple of notes sometimes in our own code, you know. Water Glass equals this other thing, and nobody could figure that out. Anyway, we'd laugh about it.

We'd sometimes get called over there by the program manager or flight director or something. We'd get in there and they go, "Okay, Lamppost today. We've got this problem with a system blah blah, solar panel." We'd look at each other and go [whispering], "Which one's Lamppost?" We couldn't keep them straight, you know. "I think that's the one the guys in Sunnyvale are on." We always laughed, because it was hard to memorize all that stuff. I mean, those guys worked every day, you know, with it, but we were the NASA interface.

The other thing that was, frankly, pretty sad to see, I guess you could justify it on national security, but you think NASA is expensive, the money that's wasted on that other side in the name of national security is just unbelievable. They have some Air Force people in charge that are great, smart guys, captains usually, but due to the way the Air Force rotates their assignments, these guys would come in for like two years on a future program and they were expected to make all these giant budget [decisions], I'm talking billion-dollar satellites. And they didn't have the background or the time to learn the pros and cons of these decisions they had to make and, frankly, the contract.

The contractors, Lockheeds and Martins and Boeings that are building these things, just kind of like lead the Air Force captains by the nose, because they're the experts. But when it comes to money and budget, they're looking out for themselves, you know. It was sad to see some Air Force guys just in over their heads technically. But, we, NASA, weren't. We were just integrating them with the Shuttle. We aren't responsible, even though we were government, for what the other part of the government was deciding to spend its money on. So you'd sit in these meetings, and go, "God, I can't believe he's authorized $25 million for that piece of crap." But, you know, if the contractors told him to do it, he'd do it.

That's kind of a sad thing. It's mainly because of the way the Air Force shifts personnel, not so much that the contractors are trying to stick the government. It's just that they're the ones who are doing everything and they know the details, and these captains come in and out and they're sharp, but they can't pick up and they don't have the past experience to make, in a lot of cases, the right technical decision. So we had to hold our tongues a little bit, Hal Beck and I, particularly. We were in several of those meetings.

Of course, sometimes, on the side, we'd get the captain aside and tell him, "Hey, as a fellow government employee, we think you ought to ask that contractor to really give you the data on that before you decide to spend all that money." But I mean we're talking $20 billion a year, easy, some on one satellite a billion-plus dollars. And you don't even hear about them. You hear about the Shuttle costing two and a half billion, which is true, and those things are neat. They can do some super things with those cameras and such, sensors.

But, anyway, that's an interesting part of my career. I did all that and had to memorize all this stuff. Thankfully, I've been debriefed and I don't have to remember any of that stuff anymore. It takes years to get clearances and all that, and costs money.

In fact, that's a sort of a little-known story about the Hubble Telescope. Remember the bad mirror? Well, the reason it was bad was directly due to security constraints. Perkin-Elmer, who carved the lens, was working with Lockheed on the telescope. I guess it's not classified that the Hubble Telescope has other capabilities than just looking at black holes. And because of that, not directly, because they don't need to look at it with that mirror, certainly, that is a planetary- or galaxy-looking telescope, but because of the overall security on the vehicle, when they went to do tests, and there's only certain couple of places in the United States where you could test the focal length of these mirrors, they didn't want to—either Perkin-Elmer or Marshall, I think it was Marshall Space Center, didn't want to pay the background check price for clearing certain engineers, test engineers. You only get so many slots in the NASA budget. Like here, I was cleared and Ed Lineberry and maybe forty other people around the center for top secret.

Well, they were testing the Hubble Telescope in a top-secret lab and it didn't matter that the mirror itself wasn't top secret, but the test facility was, so certain Marshall guys who'd been working with Perkin-Elmer on carving the mirror and the focal length requirements weren't cleared, so they weren't in the series of tests where they allegedly ran the tests of the focal length, and the guy had put the thing on backwards. There was some bracket on backwards so it was two inches too long or too short, I can't remember. But if one of those uncleared guys had been in there, they would have caught that. So they ended up, they thought it checked out, and it was off.

When they get it in the actual telescope housing and got it up there, they found it was myopic. That was really due to Marshall not having enough clearances to have the right guys in for certain tests. Of course, nobody liked to admit that that was really what happened. It was a goof by the guy that set up the test, in terms of the length, but really it would have been caught if they hadn't—it cost something like $25,000 or something. So the Marshall and the JSC budgets, you know, you call up and say, "Hey, I need somebody to work on this secret stuff." "We don't have a slot to give him a clearance." Just economics. Little-known facts.

RUSNAK: Yes, I'd certainly never heard that before.

YOUNG: Of course, it ended up costing hundreds of millions, and if they'd have spent $50,000, it probably wouldn't have happened. But, anyhow, it's sort of like that on the *Challenger* disaster. It wasn't security, not to my knowledge, but it was—although, El [Ellison S.] Onizuka was one of the astronauts that I'd worked several missions with on DoD secret stuff. He was a good friend.

But the seals on the solids [solid rocket boosters] had not been tested for cold conditions, even though it was in the Thiokol test plans and Marshall guys were aware of it, but they couldn't get the budget for it, and they skipped a couple of tests the summer before. They'd had several near burn-throughs on the seals, and even Headquarters was aware of the problem or the potential problem, but they had just cut the test budget, Marshall had, or Headquarters had to Marshall, and on to Thiokol. They had skipped some of those actual tests where they'd put them out in cold temperatures or ice them down or whatever, and tried to solve it by analysis instead. That's actually, fundamentally, what happened to the seal. They had never been tested to the twenty-eight degrees. They were supposed to be, and the fundamental Volume 10 requirement is thirty-two degrees for the whole stack. That had never been tested, and still hasn't, actually, unless you call *Challenger* a test.

That was, of course, very sad that it ended up in those circumstances. Of course, they shouldn't have launched anyway, but that was a political thing, in my opinion, having to do with [President Ronald W.] Reagan's State of the Union message, because he wanted to talk about the teacher in space that night. But, anyway, we lost seven great people because of that. Really, it was fundamentally because they cut corners on tests the previous year and, you know, a little bit of mismanagement in terms of not assessing the risk correctly.

But, anyway, that, of course, is one of my worst memories of NASA, and especially the Shuttle, is *Challenger*. When was that? '86. Yes, that was sad. I don't want to talk about it.

RUSNAK: That's all right.

YOUNG: Let's see. Like I said, I wasn't even working Shuttle then. I got off of it in about '83 or '84 and was working what became Space Station Freedom. After I quit NASA and went to Grumman, I worked at Space Station Freedom for five or six years. So until I went to Loral in '94 or so, I probably worked Space Station for thirteen years, from '83 to '94. No, that's only eleven years. I think I worked up until '96. That's right. About thirteen years I worked on Space Station and then started working on some Lockheed other business contracts.

RUSNAK: It's interesting you were working Space Station for thirteen years and by the point you left, there still wasn't a piece in orbit.

YOUNG: That's right. Oh, yes. I remember when I left NASA, on the blackboard in Building 30, or maybe it was a whiteboard by then, in '87, November of '87, I wrote a little thing up in the corner of my whiteboard that said, "Work hard, guys. We launch in seven years." That was in '87, and I was guessing that we might launch—no, eight years, I'm sorry, in '95. Didn't make it. Even my chart was wrong.

RUSNAK: You probably thought you were being pessimistic.

YOUNG: Oh, yes. They left it up there. Hal Beck took that room, and he left it up there for two or three years. When he retired, I think it was still up there. They never changed the eight, you know. He left, I think, in '90 right when they abolished MPAD, and it was still there, meaning '98.

RUSNAK: That's closer.

YOUNG: Didn't quite make it, still. But that was not that surprising, especially for the bureaucratic morass that the Station became. That's the most amazing program ever for total bureaucratic mire. There's, frankly, no technical challenge to the Space Station. I mean, we did it all on Skylab and the Russians did it on Salyut and Mir. There is no technical challenge. It's been all economic budget politics and bureaucratic bumbling is the reason the Station has taken fifteen, eighteen years—well, eighteen years, I guess, to become operational. We started in '83. Of course, we'd worked on it in the seventies, and even '63, like I told you that we had concepts. It's just an incredible story of bureaucratic mire. One of the reasons I left NASA, I just couldn't take it anymore.

I was trying to think of how to phrase it. Apollo—I was interviewed several times by Charles Murray, who wrote *Apollo: The Race to the Moon*. He was a good friend. Years after his book came out—which I just got my copy back from a gal I loaned it to over in Building 4 for four years. She dropped it on my desk. Her name's Allison, by the way. I got it the day Allison hit, tropical storm Allison hit.

I talked to Charles years after he wrote his book, which is an excellent book. I'm sure you read it.

RUSNAK: It's on the shelf right behind you.

YOUNG: Oh, is it? And his wife Catherine [Bly Cox]. I said, "Charles, you've got to—" This is probably in '88 or '90 or '91, maybe. I said, "You've got to write Space Station Freedom," at that time. I said, "You think Apollo was interesting."

He said, "I'm not into writing exposés." He wouldn't touch it. He was, meanwhile, working on his *Bell Curve*, which is also another interesting book. I'll tell you, he caught hell for that one. But he wouldn't touch it, it was so political. I could tell a bunch of stories about it, but I'm not going to. Suffice it to say, it should have taken—I had a little chart for years where I plotted the number of years from inception to flight, starting with Mercury, then Gemini, then Apollo, eight years. Skylab actually was probably four years, but it didn't kind of count because it was Apollo hardware. And then ASTP didn't count, and then Shuttle was twelve years from when we started in '69 to '81. So you extrapolate that exponential curve, and I'd predicted roughly sixteen years, fifteen years, for Station from start, from '83 or so. It turned out to be pretty close, but even it was a little short.

Once you only have three points, it's hard to get that next curve of the French curve. We used what they called French curves back in our day to do all that extrapolation. One of the old MPADers, Bob Becker, would always say, "You can make any curve with two points, three points at the most. But you can even make a curve with two points, and even one if you have to. You just take your French curve, just use the exponential," which worked pretty good sometimes like on drag predictions and stuff. That's exponential, but really hard to fit.

But I predicted Skylab entry way back in '76 or '77 within about three months. Of course, we had played with it a little bit and the drag was different, but I didn't miss it too bad. Had a bet with Bill Tindall. He never paid me. He retired in '78, I guess. At his retirement party, he—no, I take it back. I had two bets with him. He did pay me for the '79 prediction. He had predicted it would be a lot earlier than '79. But, of course, he didn't really—he'd just look at our data and make a guess. So we had a bet that he'd pay me a dollar for every day after June the 1st of '79 that it was still in orbit. So I made, what, $41 or something. He did pay me that. He used to gripe about it. I mean, he had all kind of money. His wife was really rich. It wasn't the money, of course; he was just annoyed that I was closer than he was by about six months.

So he retired right after that in '79, and he had predicted that we'd launch the first Shuttle in '80. So I was going to pay every day before June of '80, I would pay him a dollar for the launch time of first Shuttle, and every day after June the 1st of '80, he owed me a dollar. So he owed me like $300 or something, close to that, when it went in April of '81. He never would pay me. I'd always say, "Hey, Bill, remember our bet?"

"Yes, but you cheated on me."

A main engine problem came up right after he had retired and made that prediction. He'd say, "That's not fair. That's not fair." He never did pay me, but that's okay. He was a great guy.

RUSNAK: Well, we're about out of tape again, but I think if we can stop here, then I just had a couple concluding things if we can start up a new one in just a minute.

YOUNG: Sure.

RUSNAK: One of the things I was thinking about as you've been talking about the people that you worked with, I guess MPAD's sort of one of these organizations that has this kind of aura about it. Everyone who talks about it always has sort of a smile on their face or have good stories. Maybe you can share with us some of the character of the organization as a whole, I guess.

YOUNG: Another group asked me that not too many days ago. It was over in DM, as a matter of fact. But they asked it in the terms of what's different now than back then. Of course, the thing you can't change is we were lucky it was pioneering. Some of the things I've said is different, of course, is that we didn't know half or a tenth of what these young NASA workers know now, in my opinion, as far as background education and tools we didn't have. Of course, we used slide rules, believe it or not, for the first five years anyway, essentially. We had a couple of big mainframe 1620 and 7094 IBM computers, but I mean we did a lot of stuff with slide rule and back-of-the-envelope kinds of calculations.

To me, that turned out to be probably the most valuable circumstance, because it forced us—not that I can get up and do even—I can do Newton's Law, F equals MA, or is it M over A? No, it's MA. But I can't do a lot like the Clohessy-Wiltshire equations and all that. But we had to learn the fundamentals because we didn't have computer software and computers to do the number-crunching, really, although we had some of that. Certainly by the time we flew Apollo, it was pretty neat and pretty good. Compared to nowadays, it's unbelievably Model T brands of computers, but that forced us engineers to understand what was in what is now black box.

These young Montessori controllers and planners, the software's all there, in essence. They just punch it in like a calculator and get the answer. It took us thirteen steps, I believe, on a Frieden or an Olivetti calculator to get a square root of a number, and square is pretty essential in orbital mechanics. And cubed, I forget how many it was for cubed. But we had to punch in, you know, thirteen separate steps just to get the square root of a number. Of course, we knew some of them. But you get up into the thousands, and it took a while.

But having to do it manually, so to speak, you had to learn because you couldn't, of course, sit down and calculate a number in real time quick enough like in a meeting or something. You had to learn the feel for the intuitive or the intrinsic feeling for the range that the number could be. Like at 200 miles altitude, what's the orbital velocity? Well, it's 25,000, approximately 24,950 feet per second. You'd learn, what if we dropped down to 150 miles, what would it be? Well, you have little fudge factors that you figure out in your head, for each mile it changes by a half a foot per second and that kind of thing, or vice versa, actually.

So, you got, after several years, to learn these little cheat—I guess you'd call them cheat clues as to what a number ought to come out like and be able to make a quick estimate of a reasonable number. Especially true in planning, of course. Now, in flight control and real time, you couldn't rely too much on people doing it in their heads, so you had to have fundamental computers there.

But I'd say that was probably the key thing that was different, and, of course, you could almost never go back to that, or you wouldn't want to, in terms of training of a new planner. But on the other hand, that probably, even though the kids nowadays may be doing some of them right, certain equations by hand, they may even know the equation, but they've never had to program it in and play with it and use it to get numbers, because they've got this black box, we call it, that they punch in the number and they get the answer. And knowing whether the answer is right is the whole key, of course.

I remember I had an Air Force detailee guy. Overall, my Air Force detailees were great, even though some of them, a lot of them, suffered from this same problem, that, you know, the Air Force had sent them in here for two years and they'd just be getting pretty smart and useful and they'd pull them off to Turkey or somewhere. That's not a good policy, but, overall, I had great support from these sharp Air Force guys.

I had one guy, though, that I had put him on some lunar—this was after Apollo, but we were looking at going back to the Moon for something, so I had him working on some lunar calculations. I asked him to run some orbit around the Moon for an observing satellite or something, and after two days he brings me this set of numbers. They were state vectors or velocities or something. I look at him, and I go, "Twenty-six thousand feet per second around the Moon? I don't think so." It's more like 6,000 or 4,000, because, of course, the gravity is much smaller. He had no clue that he had punched in a number wrong and that he had actually, I think he had forgot to change an Earth constant to a lunar constant. So he was getting Earth kinds of numbers, 25-something-thousand feet per second for velocity, which was Earth orbital.

So I go, "Are you sure this is right?"

"Oh, I double-checked it. I think I'm pretty sure I put it in all right." And he had put it in right, but he hadn't changed his constant in the program. So he was getting bad answers, and they were not just a little off, they were just ridiculous, you know. He just couldn't believe that I could look at them and tell him that they were wrong. But, of course, I had all this Apollo in my head, sixty-mile orbit for around Apollo, and I could sit down and almost build an orbit, you know, just out of my head, velocity and flight path angle and stuff like that.

Unfortunately, nowadays I don't think many of these young people get to be forced into that kind of thing, so they have to kind of believe what they get out of it. That, to me, is probably the biggest difference in terms of the way it was. The other thing that I hope, and I told the DM crowd this, is that the sense of team, I hope, isn't diminished. I mean they got some really good guys and they're flying good missions, and the Shuttle is flying with almost perfectly nominal situations and constraints there all the time. Their team camaraderie, as far as I can tell, at least in DM, which is the successor, so to speak, of MPAD, even though it's really mostly operational, is really good, but I don't think they have the total dedication to a team concept.

To me it's like baseball nowadays, or any professional sport, frankly. The free agent killed the team spirit, in my opinion, of professional sports. [Baseball player] Curt Flood was the guy that won the free agent. I mean, there is no team loyalty of any of these guys like “A. Rod” [Alex Rodriguez]. They're all in it for the individual, how much money they can get out, ridiculous sums of money. Now, sure, he wants to win when he steps up to the plate and fields, but if he goes to the [Texas] Rangers this year and back to Seattle [Mariners] next year, he cares less. It's the individual. I, frankly, hate to say it, but I see a lot of that over here at NASA nowadays.

I can't blame them in a certain way, because by the same token, there's no company loyalty, no NASA loyalty to the employees, you know. You have all these bureaucratic rules and crap. But I mean, I hear their bosses talking about, "I asked this guy to work forty-two hours this week instead of forty, and he's whining about it all the time, 'How come I don't get paid overtime?'" I mean, except for Skylab, two or three weeks, I mean we worked forty-five, fifty, sixty hours a week all through Gemini and Apollo for sure. Now, true, when we got into the seventies and the dead period there of manned missions and then the Shuttle, we didn't work that hard.

Gemini, we worked every Saturday. It was just another workday, and half-day on Sunday, usually. We would take off Sunday morning and some Sundays, you know, if there's a ball game or something good on. But, these guys, you know, they're out the door at 4:30, and they don't have any loyalty or dedication. Now, that's a generalization. There's some really good hard workers and they work their tails off.

But I think overall, in terms of like DM as compared to MPAD, there's just no team comradeship or dedication to the mission objective. It's like "What's in it for me as an individual?" Well, it's all part of the entitlement generation that some of you may be part of, but I'm not. You know, it really is too individualistic to accomplish big things. You can't do it alone. I think there's a little too much of that nowadays, individuals not really committed.

Although, frankly, the fact that they came to work for NASA speaks tons of them, as compared to getting a job off in the dot-com company for tons of money, or did until they learned that lesson. You know, I don't advise any of them to go to work for NASA anymore, because unless they just absolutely love space and they can't stay away from it, it's just not worth it. The bureaucratic stuff you put up with is just not worth it.

But there are some exceptions, and people that say, "It doesn't matter." Of course, it never mattered to me. Money, I knew, after a while, I would never have any real money being an engineer. But I didn't even think about it for years and years.

I remember telling my dad in probably '62 or '63 or '64—he died in '65—that, "Hey, in about two years I'll be making $10,000 a year, and I'll never need another penny." I started at NASA at $6,345 a year. He had filed—he was a printer in Austin. He had filed his most income tax was $8,000, he and my mother both working. I said, "Well, you know, I'm already up to 8,000. A couple more years, I'll be making ten, I'll never need another dime." [Laughter] Inflation I wasn't aware of, never even thought about it. I remember, though, that when I retired from NASA I was making ten times what I came in for, almost exactly, and yet I didn't have a dime. Of course, I had five kids that were in college, but that's another story.

Anyway, in terms of the difference between then and now, back then, MPAD, partly because really we didn't know what to do, we had to learn it from the basics. I mean, nobody had really ever been in space except the Russians, and, of course, they weren't sharing any of their knowledge. It was tough because of that, and we made some mistakes, not that we were so smart we just learned it like that. We learned from our mistakes, which everybody hopefully does, and we had to learn the fundamentals.

In MPAD, I, frankly, didn't even think about it, what I just explained a while ago about systems operations engineering being the bridge. We were mission planning and trajectory experts and consumables experts. I didn't think about it. The term "system engineering," even, or much less "system integration" was not even a term in that industry. Maybe it was in aero, you know, building airplanes.

That's another fundamental thing that's different between an aeronautical engineer and a space engineer, is the one-of-a-kind-ness. It's one thing to build an assembly line and design a set of 747s, which Boeing has found out, and it's another entirely engineering challenge to build a one-of-a-kind Space Station, which, of course, has many parts, but it's really just a Space Station, and it's the only one, sort of. I mean there's Mir, was Mir.

But one-of-a-kind engineering is just different from assembly line building cars or airplanes or whatever, and redundancy of systems and different approaches to reliability and safety and all that stuff is fundamentally different, and, of course, we had to do it that way to build Gemini in flight and Apollo in flight. You just learned a lot and you learned these little fudge factor cheat kind of feel numbers that allowed you to make quicker decisions about concepts and things and look at data and tell whether it was right or not.

One more Gemini story. Bob Becker reminded me of this one, back in the high point of our whole careers was the rendezvous of VI with VII, 76 rendezvous. I don't remember whether

I told you about the plug falling out. I probably did. When we started to lift off in December of '65, the Gemini VI Titan, Gemini, they had just launched VII less than two weeks earlier, ten days or something earlier, and it was slated for fourteen days. That was its maximum stay time. So they had to turn. They only had one pad. They had 34B for the Titan that flew the Gemini, anyway. So they had to turn around and get the new one out there.

So, for whatever reason, the liftoff time, the clock starts when they had literally at the bottom of the rocket, when the thing fired and the engine lifted off like two inches from the pad, it pulled this plug out that started the onboard computers and told you you had liftoff in the control center and everything. In their haste between VII and VI on the pad, somebody hadn't fastened that plug in there or turned it or screwed it in enough. It really just ripped it out. Of course, it's thousands of pounds of force.

Well, they got it right down to the count and they had "Engine fire," and the plug fell out. The onboard computers, that to it was the same as the engines not working or no liftoff, so it shut down the engines immediately, which is kind of lucky in itself that it didn't blow up or hard start when they redid it. Also lucky—and Stafford and [Walter M.] Schirra tell the story—the ejection ring and nobody ever wanted to use it because they were afraid it would kill them, and so they were supposed to pull it in that case, but Wally told Tom, "Don't pull that ring." So they didn't. They just sat there and the thing just sat back down.

So that was pure luck, but I mean, bad luck but great luck. Well, they quickly got out to the pad and figured out what had happened, that it had literally just shook and fell out. So they had to turn around. Of course, we had missed our rendezvous chance for that day and so forth.

So, like two days later they got the plug back in there and were ready to go again, but a guy in the meantime, and I can't remember who this was, he's a guy at the Cape, I didn't know him, had been looking at the engine data, even though it had only fired for like a half a second or something, and particularly the telemetry of the flow rates out of the fuel and oxidizer for the Titan, and he had detected that it didn't look right. Of course, he had seen data from six or five previous Titan launches. Just because it was his job to analyze engine data, he got to looking at it and he comes up with, "Hey, this thing, even if it hadn't fallen out, this one engine would have shut down, or the engine would have shut down in a few seconds, anyway."

I go, "Why's that?"

He says, "Well, somebody left a plug." They found out. He didn't know it. He says, "The flow rate of either the oxidizer or the fuel," I forget which, "is way low or it's not right, it's been blocked." So they went in there and they found a plug had been left when they did some checkout, had been left in the fuel line and it would have run dry in another second or two. It would have been bad news, because it would have really blown the thing apart with no oxidizer or no fuel, but yet a hot engine. So, just thanks to this guy's diligence of still looking at his data, even though it was a false liftoff, he actually saved the day ultimately.

So that's just a story of each guy's got to do his job, regardless of the circumstances, and that's the way we approached it, partially because we just didn't have that many people to do the jobs. That's the other benefit. The bureaucratic way it is now, everybody is compartmentalized into they have their one little job, run their little black box, and that's it. That's supposed to be a forty-hour week. And that's exaggerating. They do other things.

But in those days, we were not jacks-of-all-trade, but we had bunches of responsibility because there weren't that many of us. I forget what it is, we've got old pictures of MPAD, different stages, org charts, but I think at our peak we were 100-and-some civil servants in MPAD. I know when I was branch chief, I think I had 100 contractors and about 30 civil servants in my branch. I think there was like 125 in the whole division at that point, anyway, of that order.

Now there's hundreds, and they have these little pieces of the mission, and not only is that bad because they don't really learn that much about the guy-next-door's job, but they don't communicate unless they really have the self-initiative and have good communication skills. They don't know enough to know they need to talk to these other people because you can have this stuff like the plug falling out. The guy just knew that he needed to look at his data anyway and found the real problem. They don't do that much here. It's just too compartmentalized and doesn't give many people much of a range of learning experiences or responsibility, for one thing.

That's the other fundamental difference is we truly had, we didn't know it at the time, we had delegation of responsibility and authority from Chris Kraft and top management. In other words, we made decisions on a daily basis that were pretty important, and we were just—in my case, in Gemini, I was a GS-7, GS-9, you know, lowly guys on the pecking, totem pole, but we didn't have a whole lot of intrusion from above management-wise. True, we had to brief our managers and tell them what we thought was best, but we'd go to meetings and we'd actually accomplish something. Nowadays you just go to meetings as part of the process and, by evolution, things kind of finally get decided sometimes. We just didn't have that luxury, if you call it that. We made decisions and had to justify them, which, of course, is also back to the black box thing.

Nowadays it seems that some of the guys are more concerned with how neat the graphics are than what the data is behind them. It's all form and format and function instead of reality of what the numbers are, partly because of this process of, well, you know, you put on a good show and then they rumble around and ruminate and, finally, somebody passes the decision up to somebody else, and they finally come back down and go, "Yes, we ought to go that way with this mission decision," or whatever.

Well, we certainly didn't have nearly the layers of all of that. So we were delegated real responsibility. Of course, if we screwed up, it was on our case. And we did, occasionally. We had guys that screwed up. Nothing super bad. We had one guy, actually, he wasn't in MPAD. He was in the sister division, ground control, MCC guy. He rounded off the Earth's rotation to an even integer or he didn't catch it. I think it had to do, really, with the software. In those days, you know, you had integer numbers and you had variables with decimal places, X positions.

Rotation rate of the Earth, of course, isn't twenty-four hours per day; it's 23:59, whatever, seven, whatever.

Well, he had a piece of software from us for entry calculations from MPAD and we had passed it on. That's another thing that we did, we wrote the software for nine-tenths of the software that's still in the MCC in terms of trajectories especially. They still use the launch targeting that me and another guy wrote it for Gemini for rendezvous for Shuttle flights, essentially the same equations and everything.

But, anyway, we wrote the basic software. At least we wrote the equations down to what they called the Level C requirements, where we gave them to IBM and the actual programmer programmed them into their computers—I'm pointing over there to the control center—as opposed to our off-line computer programs, so they were separate in that sense. But we really wrote the basic stuff and they sort of copied it or programmed it, sometimes screwing up.

Well, in this case, this guy screwed up and put in omega of the Earth, which rotation rate is integer number. It wouldn't have mattered for like the short Gemini flights, but on Gemini V, I think it was, or IV, maybe it was IV, it's the one where [M. Scott] Carpenter landed. Well, no, that was on Mercury. He landed down range because he's screwing around looking at fireflies. Kraft tells that story real well.

But on the Gemini, it wasn't Carpenter, but it was Gemini IV, I guess. It was a pretty long mission. They had the space walk and tried to do that stationkeeping thing. Anyway, it was several days long. When they went into the control center and calculated the de-orbit, it was based on a state vector that had been propagated for X days, four or five days. In the coordinate system that's used there, you have to account for the Earth's rotation during that whole time, and it was rounding it off. So the spacecraft there ended up 250 miles down range from the target, which was ridiculous, because all the others, you know, Mercury was not that bad, except for Carpenter's.

Well, that was just a flat out—the guy, when he had reviewed those numbers from us to the IBM guys, either somebody had rounded off because they put it in as an integer number, or he rounded it off because he thought it didn't matter and he put it in as an integer instead of a floating point. That's what you called it, a floating point. So that could have been—of course, unfortunately for him, he's a good friend of mine and played ball with him for years, he never lived that down.

The FIDOs that caught the brunt of doing the orbit maneuver at the wrong time, the retrofire maneuver, never let him live it down. It didn't ruin his career, because he stayed here for twenty-five years, but he never really went up the chain any after that. They never let him live it down, which was harsh, but that's an example of screwing up despite whatever. There were plenty of others.

But that was the other difference, is that we were delegated authority to make decisions when we'd go over and talk to the payload people or the science people. We'd go, "Yes, we can do that," or, "We can't do that for these reasons." Then we'd come back and tell our bosses the rationale. We had to explain it. So if you just run the numbers in a black box, you can't explain what's behind it unless you know something about the background and fundamentals and even whether it looks right. That's the biggest difference.

But team, to me, in terms of doing really neat things and useful work and job satisfaction, I think it's mostly lack of teamwork. In MPAD, we really had a camaraderie, not that we were all smart. Really, Ed Lineberry and a couple other—Emil Scheisser, a nav guy, was just brilliant people, and they were all pretty much pretty smart guys, in the upper whatever percentage on the bell curve, but we had some dumb ones. But personality is as important as brains in a lot of cases. We had some characters and we had some—I won't call them dumb, but less than total bright lights, but they did good work, and in their area they knew the job and knew how to do it. It was just a real teamwork kind of camaraderie that's lacking, I think, because of the bureaucracy and this entitlement kind of attitude that the younger generation has.

It's amazing to me that somebody would gripe about working two extra hours for free or whatever. I mean, even we got comp time. You could generally take it off the next week, you know. It's just not much team—DM actually is one of the better ones still, because they do have some real good team. They play ball together. Of course, most of the organizations do. I think it's just the fact that it's too compartmentalized. Even though there's a big team with a whole bunch of players, it's better to have a littler team that has to expand their roles a little bit, in my opinion. But it will never happen in this bureaucracy. It's gone.

So that's it. MPAD, you've probably heard. I don't know who all you've interviewed. I can't remember your list. But Cathy and others. [Robert L.] Carlton was never in MPAD that I recall. He was on the fringes, because he was in ISD [Information Systems Directorate], not MOD, for some of his years. We just had a unique kind of group of 100, 150 guys came through there.

Another thing that probably helped, frankly, was that we got this personnel freeze. It helped in one way. It hurt in the long run. We had, in essence, a personnel freeze after Apollo where it was the same old group doing Shuttle. A few guys left, you know, for better jobs. One of them became a pilot for Delta [Airlines] and one of them was a patent lawyer in [Washington] D.C. But mostly it was the same crowd that went through at least Apollo, did Shuttle or Skylab, and some of them Skylab and ASTP and then Shuttle. So we had to relearn Shuttle or apply all we learned on Shuttle. In the long run, that hurt, because, of course, we didn't have anybody to mentor for early Shuttle, until, really, literally, early Shuttle, they started getting some new hires in.

Of course, the other thing they did was give more work to contractors. That can be okay in certain cases like now when the Shuttle is totally operational and the Station is operational. But, in my opinion, it's best to have the government engineers who really know, from the cradle on, how to run things up until the thing becomes kind of automatic, and then, yes, you can get people in to run it and can do a good job, and turn that over to contractors.

In short, I think NASA and JSC in particular ought to concentrate on future, I mean, really future stuff, R&D [research and development] and not ops, although it helps to have an ops background. But there are people that you can't take them off a console and make them an engineer. They came on with a degree in math or, in some cases, geology or whatever, and they're sharp people, but if they aren't engineering material, you can't put them on an R&D job and expect topnotch engineering out of them, which is sort of the situation here, not that they have an overabundance of flight controllers, but, you know, they talked for years, "Well, get them out of ops. Get the civil servants out," and actually they are probably 80 percent out now USA.

The good thing for USA is a lot of them are the old NASA people like Cathy and some that know how to do fundamental ops. But you can't just take a guy off a console and expect him to do an R&D design, conceptual job. It's a different kind of mindset. Some of them you can, but a lot of them not only can't do it, not because they're dumb, but just don't have the background or the vision, and a lot of them don't want to do it. They'd rather do something more operational, more today, brushfire, whatever. They just don't have the personality for it. So it's management's job to figure out who can and can't and evolve that, but it just doesn't happen in a bureaucracy. Nobody's in charge, in my opinion.

RUSNAK: That might be a good place for us to end today. I don't know if there are any concluding remarks you'd like to make, anything we haven't covered, any other people or stories you want to mention before we wrap it up.

YOUNG: I can't think of any.

RUSNAK: I'm sure it's a big can of worms to open up, but—

YOUNG: Yes, I can't think of any right now. I mean I'm full of stories, as you can tell. But I think I've hit some of the interesting ones.

Like I say, with Space Station, I could talk a long time, but it's mainly negative. I mean, that's all I will say, is that it is absolutely true that there is no technical challenge. The only thing that's close to a technical challenge, and it isn't really, is the assembly in orbit, the assembly and checkout. Well, there's only one reason you even have to do that, and that's because of Shuttle and the astronauts. It has been decided many years ago that you build things on orbit because that's fun and keeps the Shuttle and the astronaut crew busy. Don't build them on the ground and take them up there. That's too easy. And, really, it is.

There are those of us who fought the assembly of Space Station for twenty years, and it just is political. You've got to have something for the Shuttle to do. Now, frankly, some of us have concepts that say, "Hey, you can still do that and still do it in one fell swoop with a big launch vehicle or even a modified unmanned Shuttle and all that." We've been all through that over and over. It's, frankly, the manned astronaut job security featherbedding that has put us where we are on Space Station, because each one of those modules you take up there and then you do all this EVA and have to make it work, and 90 percent of that is unnecessary. We could have launched it on like two flights ten years ago.

Then what would Shuttle do? What would the crew do? And I'm certainly not a proponent of robotic, unmanned only. But I mean the truth is, manned flight has dominated unnecessarily for the last twenty years. Just like the automatic rendezvous and docking. Auto land, Shuttle can be landed automatically with no crew, and I'm sure a huge percentage of the time would land safely. But the crew will never allow it. Of course, they get these little technical arguments that reinforce their desire to fly it in.

But the Russians have proved it, you know, what, fifteen years ago, or ten, anyways, that you can land a Shuttle unmanned in a 55-knot crosswind, too. And, true, it didn't end up on the centerline of the runway, and it was off in the ditch, but it was pretty impressive.

They've got the software and the nav and everything to do it, every flight, and they've paid lip service to letting it to do it some, but crew won't let it happen.

The same with automated rendezvous. Russians have done it twenty-five years ago. We still can. If we had to do it, like the Skylab thing or some other rescue thing, it would take us two years to put together a system to do it, unless we bought it from the Russians or borrowed it.

But, anyway, that's all a whole new session.

RUSNAK: All right.

YOUNG: In my next life, I'll talk about that.

RUSNAK: There you go.

YOUNG: So thanks for this opportunity.

RUSNAK: Thank you for spending the time with us, this time and last.

YOUNG: All right. You're welcome.

[End of interview]