

ORAL HISTORY TRANSCRIPT

KENNETH A. YOUNG
INTERVIEWED BY KEVIN M. RUSNAK
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RUSNAK: Today is June 6, 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.

I'd like to thank you for taking time out to join us today.

YOUNG: My pleasure.

RUSNAK: We're glad to have you here. If we can start out with you telling us maybe a little bit about growing up, what kind of interests you had in either engineering or aviation, the things that perhaps guided you into a career in the space program.

YOUNG: Okay. I'm out of Austin, Texas, grew up there, went to school there, went to the University of Texas. "Hook 'em." [Chuckles] Actually, decided in high school—I've always been more or less pragmatic kind of scientifically minded person, although not deep science or not academic science, more engineering, curiosity about how things work and such. So I decided in high school that I wanted to be an engineer. I wasn't sure what that was, but I decided that's what I'd enroll at UT in. Actually, I enrolled in summer of 1957 as a civil engineer. I really didn't know one engineer from another.

Then fortunately, I guess for me, the Russians launched the Sputnik in October, October 4th, 1957. Just like Homer Hickam, the Rocket Boys fame, that inspired me to

change my major, and I went the next day and changed to aeronautical engineering. There wasn't even an aerospace at that time, degree.

So that's what I did, and throughout my five years there at UT I think I had two space courses, one in orbital mechanics and one in astronomy-type subjects. But that's what the curriculum consisted of. Then the rest was fundamental engineering, which, of course, is really the key to any engineering, the math and calculus and some of the higher, tougher, harder math. Analytic geometry, that's a good one.

So by the time I graduated in '62, they had added quite a few courses to the space part of it, but I was mainly aeronautical, although I never really liked it. I disliked high-speed aerodynamics, it's so tough, for one thing. Fluid flow and all that is really a tough subject. So I was always more interested in orbits and trajectory work.

I had an excellent guy for that, he's still there at UT, I guess, he must be a professor emeritus now. A guy named Byron [D.] Tapley, he's in orbital mechanics. In fact, it was engineering mechanics that he taught. He was really good, and I kind of owe my real love of orbital mechanics to Byron.

Got out in the summer of '62 and had several aeronautical offers. I had worked two summers in Silver Spring, Maryland, for the Navy in the wind tunnel, the high-speed wind tunnel. They called it then the Naval Surface Weapons Lab. I think it's changed the name. It's still there.

I came out of there thinking I didn't really want to work aerodynamics, and NASA gave me a pretty good offer. It wasn't the best I got, but they just said they were moving down here to Houston. I decided that was great. I didn't really want to leave Texas, so it worked out. So it's been, what, thirty-nine years this month that I started with NASA down here. Not here, because this was all cow pasture, I mean literally all of this. NASA Road 1 was a two-lane gravel road from the Webster railroad tracks to Kemah.

I remember coming down in late '62 just to see what it looked like, the site looked like, and going on this gravel road and seeing all the cows out there in the site. They hadn't started building anything at that point in the middle of '62. So we were housed in several office buildings mainly off the Gulf Freeway [now Interstate 45]. I'm sure you've heard about some of those. I was in one called HPC, Houston Petroleum Center, which distinguishing feature is its oil derrick. It's still out there, or it may not be the same one. That was thirty-nine years ago. There's still an oil derrick out there. I think they've changed the name of the place. There's a gas station in the old parking lot. But those one-story buildings are still there where we were in offices for, what, two, two and a half years. I joined Flight Operations Division under Chris Kraft at that point.

RUSNAK: What kind of job was it that they had offered you? What were you expecting to be working on when you signed up?

YOUNG: Just space, manned space. Of course, they had already picked Houston as the Manned Spacecraft Center in the fall of '61, so I knew that much, but as far as particular task or job, I just came in. The personnel [office] was in the East End Bank on Telephone Road, upstairs in this little office no bigger than this. I went up there and a guy named Les [Leslie J.] Sullivan, who, unfortunately, passed away not too long ago, helped me decide where I wanted to go. I mean, they just hired a bunch of us. I went in there without a real idea, just knew I wanted to work on trajectories and orbits, so he put me in a mission planning branch. That was Mission Analysis Branch, as it was called in those days. John [P.] Mayer from Langley was our branch chief.

Shortly after that I showed a real interest in a rendezvous problem which was just being worked. Nobody had ever done a rendezvous, including the Russians. I started working for Bill [Howard W.] Tindall and with a couple of guys that really were the best

guys I ever worked with at NASA, Ed [Edgar C.] Lineberry and a couple other guys. Bill Tindall, he was probably the best boss I ever had. That guy was so dynamic, it was unbelievable. He wasn't always technically right. We had to set him straight a few times. Of course, none of us knew what we were doing. We were just trying to make plans for Gemini. I started working on Gemini.

But one thing I was thinking through these memories, and people find it hard to believe, but in the summer of '62, I think it was in August, I used to have the notebook from this series of meetings they sent me to, but I've lost it in the garage somewhere or something. But it was a series of meetings on space stations. It was General Dynamics [Corp.], Boeing [Co.], and one other big contractor. It wasn't Lockheed [Aircraft Corp.]. It might have been McDonnell [Aircraft Corp.], but I don't think it was even McDonnell. Convair [Division of General Dynamics] maybe. This was in the days where [Wernher] von Braun was still the guru of NASA future, anyway, and he was expounding on the artificial gravity rotating 2001. That's strange. It's 2001. A station like that, where of course everybody thought you probably would have to have artificial gravity for man to survive in space. It turned out to be at least for a short time, that's not really that necessary.

But anyway, there was a series of meetings and I took notes for our branch, and nothing came of it. It was just some concepts of how these big stations could be launched and set to rotating and accommodate men. But that was in the summer of '62.

So my first real task—well, I actually worked on a couple of Mercury flights, the last two Mercury flights. I say I worked on them, I plotted data and did some curves through the launch trajectories. I didn't know what I was doing there, but I was plotting data. So I'd say that really was my first task, was working on Mercury.

Later I was happy to say that I worked on every manned space program from 1962 on. I guess I can still say that, except for individual Shuttles that have been flying. I worked

on Apollo-Soyuz with the Russians in '75, and Skylab. I've seen it all and pretty much done it all.

But in the fall of '62 we started working Gemini, which, in my opinion, is the best program we ever had as far as real what you'd call basic preparation for manned space. Mercury, of course, was important to catch up with the Russians, for one thing, but as far as technological advances, they were mostly made in Gemini, that made us able to get to the Moon in '69, in eight years from nothing.

I imagine some people have probably told you this, at least it's my opinion that if we had to go, if we wanted to go back to the Moon, which, of course, a lot of people do, I don't think we could do it in eight years. The red tape and the politics and the bureaucracy is so bad now that I really don't think we could do it. For one thing, we don't even have a manned vehicle that could go there, much less land there. I mean, we can do a circumnavigation of the Moon, manned, for four or five years, because we just don't have the vehicle. But anyway, we did actually land on the Moon, though.

RUSNAK: Those of us here don't have much doubt about that. We've talked to enough people that—

YOUNG: First of all, it would have been an even neater trick to fake it. I mean, I don't how anybody could have done that. Steven Spielberg maybe can now, but not in the early sixties, there was no way. That Fox program ["Did We Land On the Moon?"] made me so incensed, I shot them off a couple of e-mails, which they probably just discarded with the thousands of others. But I told them I'd never watch another Fox program. I lied, because I watched the [Houston] Rockets on Fox Sports Net. That's the only one I'll watch. But I mean, that thing was so absurd it just—I told them the producers were irresponsible morons to just get ratings and whatever. Just every point they had is refutable.

In fact, I was reading one today, I'd heard this before from buddies that are radiation experts, and there are only two to three of them in the world, but one of their claims, I didn't watch the whole thing, I couldn't take it. But one of their claims was if Apollo so-and-so had launched in August of '70 or whenever it was, '72, I guess, the crew would have been killed by this big solar radiation event. Truth is, even if it had been launched then, which it wasn't, if they were in the command module, they would have had no problem and would have had enough protection.

All the other flights, the ten flights of Apollo, were minimal radiation problems. Now, if they'd have been in the LM [lunar module] or on the lunar surface, yes, they'd have had a problem. I mean, we had a warning system for that flare, which occurred in August. We didn't launch 17 until December. But they could have gotten in the LM and gotten back to the CSM [command and service module] in time to be protected. So that's just one of the many fallacies that they tried to throw in there.

RUSNAK: We've talked to Rod [Rodney G.] Rose, who is one of the people working on the radiation at the point, and he had no concerns that the Apollo crews there were going to be overly exposed to anything like that.

YOUNG: Rod's one of these few experts. Art [Arthur] Reubens is another one. In fact, he's probably the best EMI [electromagnetic interference] guy, electromagnetic guy, there is. He's still doing some consulting over at NASA. Another guy is Cal [Calvin F.] Herman, he's an ex-NASA guy. Then there's a guy, in fact, what I was reading today in the Apollo write-up, was [J.] Vernon Bailey, who worked for me at Grumman, worked on the Space Station. Vernon wrote a summary of all that, not because of the Fox thing, but back in the nineties he wrote a summary of the Apollo radiation threat or lack of threat and so forth. That's all

documented. I think it's on a Marshall [Space Flight Center, Huntsville, Alabama] website. I didn't take it down. But there's a lot of proven data out there.

So anyway, I started working Gemini, and we worked, what, three years, two and a half years, before we flew the first Gemini in '64, I guess it was. Gemini III. John [W.] Young and—

RUSNAK: Gus [Virgil I.] Grissom.

YOUNG: Gus. Yes. I was in training then, and I was still in MPAD [Mission Planning and Analysis Division], but several of us had been recommended to be training for flight controllers. I was a FIDO [Flight Dynamics Officer] flight dynamics trainee. We had the option, it turns out, to either stay in MPAD, be a mission planner, which was my basic role, trajectory expert, or switching to flight control and become a flight dynamics officer or a guidance officer. Some of us were guidance officers. Jerry [C.] Bostick was a flight dynamics guy. He's one who did switch. I decided, me and Bob [Robert W.] Becker, were the two that decided we'd rather stay in mission planning for our various reasons, and so we did.

But we went through, like, Gemini III, we went to the Cape. Our first trip as NASA employees, we were GS-7s and went to the Cape with Chris [Christopher C.] Kraft [Jr.] and Glynn [S.] Lunney and John [S.] Llewellyn [Jr.] and John [D.] Hodge and the whole original flight control team, [Clifford E.] Charlesworth. We went through all the rules. They have these flight rule readiness sessions where you have to memorize all the rules.

My fondest memory of that is after, I don't know, a week of that kind of training and rehearsal, we were out our last night prior to flying back home and we were having kind of a celebration party at a restaurant there. Everybody was sitting at this big long table, the whole crew, and Cliff Charlesworth asked Bob Becker, he said, "Well, Bob, you know, what do you

think of flight dynamics officer now?" Cliff was the flight dynamics officer, he and Glynn Lunney. Becker, he's always been kind of an outspoken Missouri mule, he goes, "Piece of cake." [Laughter] And Cliff never let him forget that. It was like, "Oh, so you don't think this is a task or a job?"

We just really, both of us thought it was just kind of boring and really too simplistic. I mean, not to knock those guys, because they did great work and you had to do all this immediate response and all this quick action kinds of things and make abort calls and that kind of thing. It's very critical, of course. But we really just thought it was—console time was just too boring and too regulated, so we preferred to work on mission plans and future concepts and that kind of thing.

Which I've always said, and I hope Cathy said the same thing, Cathy [Catherine T.] Osgood, that MPAD to me filled a role that now there's a void at JSC, anyway. We didn't know it at the time, because we didn't have this terminology down, but actually MPAD, which is Mission Planning and Analysis, it was an operational entity, but it was really an engineering integration, systems integration entity. So we were the bridge between engineering and spacecraft design, so to speak, and operations. Not only how to fly, but how to design it to fly it easier or better. Now that's called systems integration, but we didn't really have that term in those days. So they always called us mission planners, but we started like at least five, sometimes ten years ahead of the final launch, or the launch, working on a mission, set of missions.

For instance, in spring of '69, before we landed on the Moon, I spent three weeks in Washington working on Space Shuttle concepts and mission requirements. So from '69 to April of '81, I worked twelve years on Space Shuttle off and on, when I was doing Apollo, Skylab, and some other missions. We didn't even call it the Shuttle then; it was called the logistics vehicle. Its prime mission was to resupply a big space station, which was Marshall's baby, and they were pitching ahead and trying to get money to build it. Von Braun was still

pushing it. It turns out the Vietnam War and everything killed all those ambitious plans and the country could only afford one big project, and for various reasons we won out. I say "we." JSC won out over Marshall in terms of the Shuttle over the Space Station.

But that's just an example of how we worked way ahead of the flight controllers, or the operations people, although most of our guys either worked on the front console some, or definitely in the back rooms in real time. So to us it was the same as flight control, but instead of boring simulations, not that they were all boring, we went back to our offices and did future planning concepts and such. That's basically why we chose to stay in MPAD, and haven't regretted it.

Another thing I remember, I don't know how personal I'm supposed to get, but that flight we took on the NASA airplane down to the Cape [Canaveral, Florida], that first time we'd ever flown. Like I say, we were just lowly GS-7s, Bob Becker and I, Jerry Bostick, and a couple of others. Here we were flying with Chris Kraft and John Hodge and the NASA elite. So we take off from Ellington [Air Force Base, Houston, Texas] early in the morning and it's like a four-hour flight down there in the Gulfstream. This guy, a steward, is in there and he asked us if want some sandwiches, some drinks. So, sure. So we ate our fill of sandwiches.

Then Becker discovered Kraft was having a drink, and everybody was having a drink, and he ordered a couple of drinks. He said, "Hey, this is really good." So we all had a few drinks and he kept ordering them. We got there and that was great. About a month later when we got back, we got a bill. His bill for that flight was like \$93, and our take-home was like \$200 every two weeks, so it was like a week's worth of pay. [Laughter] He didn't realize that we had to pay for that stuff. He thought it was all just a perk. I didn't either, but mine was luckily only about \$30. That was funny. We never let him forget that.

RUSNAK: Maybe it's a perk for the guys like Chris Kraft sitting up in the front of the plane.

YOUNG: No, he paid. The Gulfstream just holds thirteen passengers and, no, they all paid, had to pay. I'm sure Chris had to pay also. Which, by the way, I'm sure you've either interviewed him or plan to, I would hope. His book is excellent. I just finished it about a month ago. To me, the most interesting part was his early—it's pretty autobiographical really. It's more about him than the program per se, although that was interesting to me to see how he got into it, how he ended up at Langley, and his early years there as the test engineer. I told him the other day—of course, I got him to autograph it, but I told him, "It was great, Chris, but you had one mistake in it."

He said, "Well, I think I had a lot of mistakes."

I said, "Well, I didn't see but one."

He said, "What was that?"

I said, "Well, you talk about the landing of Apollo 11 landing on the Moon was at 3:19 or something central standard time, 1969, July 20th." I said, "It was central daylight time. We went to daylight savings in '66."

He said, "Yes, I thought, you know, I had it that way and my editor changed it."

I said, "Well, he was wrong."

So he said, "Write that up on an e-mail and ship it to me so it will remind me." I haven't done it yet.

RUSNAK: He'll fix it for the next edition.

YOUNG: Next edition. It's really a good book. Of course, he is a great leader, great guy, he and Bill Tindall, which he gives Bill a lot of credit. Bill was really the best guy to work for. Chris was a good guy to work for. I mean, he was pretty tough.

RUSNAK: It's unfortunate that Bill Tindall is not around for us to talk to.

YOUNG: Yes, it's a shame. I guess he died about five or eight years ago. Yes, he really was one of the best, or the best. I had some great arguments technically with him. He finally would come around. The main thing was he was just so dynamic and can-do, get things done, get to the point and get it done.

Kraft was pretty much that way and willing to listen to good technical arguments. Of course, I didn't work directly for Kraft, because he was up there managing the whole affair. Bill Tindall was my branch chief for all through Gemini. Good program.

In fact, our proudest moment, or to me my proudest moment at NASA was December of '65 when we did the first rendezvous in space, Gemini 76. Another error, by the way, they had that wrong at the Air and Space Museum in D.C. There's a little plaque about Gemini VI and VII, and they had the date as December 16th, I think. It was December the 15th that we did the rendezvous on the fourth orbit of the Gemini VI.

RUSNAK: Since you brought it up, why don't we go ahead and talk a little bit about planning rendezvous for Gemini. You mentioned this earlier as one of the big problems that you worked on early on. Then they tried some various rendezvous earlier in the program, Gemini IV rendezvous with the booster and such, and the phantom rendezvous a couple of missions later, these sorts of things. Why don't you take us through this progression and how you guys attacked the problem and came to the fruition in Gemini 76.

YOUNG: Yes, Gemini IV, I don't know who all has told you some of that story, but that, to us, was never a rendezvous, first of all. It was an attempted stationkeeping exercise. Rendezvous, to us, or the ones that originally planned it, it was taking one vehicle to a target from the ground, first of all, and rendezvousing, or at least getting close enough to dock or be

able to dock if you had the mechanisms, whereas stationkeeping was if you separated, like Gemini IV's case, the capsule from the booster and turned around and tried to fly back to it and stationkeep with it, that's what that was. The terms weren't that well defined in those days and some people mistakenly said it was a rendezvous attempt, or least in my opinion, it's a mistake in terminology.

The other unfortunate thing about it was that it was kept secret from just almost everybody. I was barely knowledgeable about it myself. Ed Lineberry and Bill Tindall were two of the main pushers for this exercise with the McDonnell Douglas people in St. Louis and the crew, of course, [James A.] McDivitt. But probably one reason that it wasn't successful is because they had some people that really didn't understand the rendezvous problem or even orbital mechanics well enough to have been planning this separate, turn around, and fly back at it, including McDivitt, frankly. No fault of his. He didn't ever have a background in orbital mechanics and didn't realize that if you thrust towards something that it will raise your altitude and it will slow you down, at least over a quarter of an orbit it's not effective. You have to really force yourself back on a linear trajectory. He didn't really realize that.

The other thing was they didn't think about the Titan venting its remaining excess propellant. Well, I think they did, but they couldn't estimate it. They never really thought about measuring it. Of course, Gemini III was only a couple of months earlier, and I don't even think they had the concept then. So they didn't measure it on III, and it probably would have been different each launch, including the Shuttle, is different depending on the ascent and loads you encounter.

So he got off of it, off the end of the Titan, and it continued to vent, and by the time he got situated and turned around, it was quite a ways off and he tried to drive at it like a car. That would have been all right if he had really expended quite a bit of propellant. Of course, he had a real restricted budget of propellant, because at that time, of course, we were still

worried about de-orbit, and you always had to keep enough RCS [reaction control system], the reaction fuel, to backup the de-orbit retro pack. On Gemini we didn't have a retro pack, but anyway, the retro fuel was separate. You made sure that you had enough, even if it failed to get back.

So he had a real restricted budget, so he thrust at it for a little while and seemed to be closing, and then he let off, thinking, "Well, I'll just coast in there." Then orbital mechanics started to take hold and he was starting to go above and slow down relative to the Titan, and the Titan was continuing to thrust because of the venting. So they got further away and then he got kind of desperate and compounded his mistake.

The simulations they did, which were totally in secret up at St. Louis, just weren't valid enough to give them any real training for that situation, both the orbital mechanics effects and the effect of the venting. That was the reason it failed, but none of us rendezvous guys ever considered that a failed rendezvous. That was just a failed stationkeeping exercise. So several of us spent months analyzing what went wrong and what the trajectory was. Of course, that was difficult to reconstruct.

A friend of mine who's one of the few still over there in what was left of MPAD, DM [Flight Design and Dynamics Division], Flight Dynamics Branch of MOD [Mission Operations Directorate], is a guy named Jerry [Jerome A.] Bell. Jerry probably spent six months of agonizing post-flight analysis on trying to figure out what the trajectories really were, looking at the telemetry of how much McDivitt had input and what direction and what the venting was doing on the Titan. It was just hard to reconstruct. But basically those were reasons it was so-called failed.

Unfortunately, the media got hold of it as if it were a rendezvous attempt, but it wasn't. They did the space walk on that mission. They were trying to match the Russians to put a little jazz in our program to do a rendezvous, because the Russians had allegedly rendezvoused two Vostoks back in the previous summer, I think, where they flew by like that

within two or three miles at hundreds of feet per second relative velocity and tried to call that a rendezvous. We said, "Nah, that's not rendezvous. That's just shooting at something." So we still feel like we did the first.

Now, the first failure, so to speak, was the phantom, what turned out to be the phantom rendezvous on Gemini V. That was with a little device that we ejected from the Gemini V spacecraft, it was called the [Rendezvous Evaluation] Pod. I forget what that stands for, but it was a little radar package, transponder. We deployed it in a certain direction and then X hours later we were going to try to acquire it with the Gemini radar and fly back to it, a true rendezvous.

They started having fuel cell problems on that flight right after insertion, so they had flight rules that you have to come home if one of your three fuel cells craps out, which it looked like it was doing. So after they decided, "Well, it's probably very iffy, we'll probably have to come home early," they didn't want to waste any fuel on the setup or phasing maneuvers to the Pod, which, of course, was out in never-never land. NORAD [North American Aerospace Defense Command], the Cheyenne Mountain guys, were supposed to be tracking it, but they couldn't find it. It was a little bitty thing. In those days it was pretty hard to track that kind of object.

So they just blew it off and said, "Well, we're going to have to come home early anyway, so all you rendezvous guys go home." We'd been practicing for months, of course, to pull this off.

That's another funny story. A bunch of us ended up going over to the Flintlock on NASA Road 1, one of the local saloons, and proceeded to get pretty inebriated, including several of us, except Bill Tindall. He didn't go. He was, I guess, hanging around for other reasons. About ten o'clock that night—this all happened in the afternoon, and ten o'clock that night they sent a guy over to get us and decided that the fuel cells were okay and we ought to go ahead and try to re-rendezvous with it, with the Pod, which still hadn't been found by

NORAD. Some of us were a little high, to say the least. Lineberry, he was too far gone, we couldn't even—we had one of the guys take him home. He couldn't even drive. We were kind of letting off steam and it was pretty disappointing, because we had practiced for months and we were sure we could pull it off, if we could find the Pod, or if NORAD could find the Pod.

Anyway, several of us, Bob Becker and I and a couple of others, managed to wobble back and we worked all night waiting for NORAD to find it, and they never did. So we ended up, Bill Tindall had this concept of doing the phantom rendezvous, where we just built a ghost or a phantom target and then did the maneuvers that we had planned to do, the phasing, a little plane change, as I recall. The Texas maneuver was one of them, over the Texas tracking station.

We flew the Gemini right through the target, so in essence we stationkept, or went into the same orbit as this phantom orbit. Post-flight analysis, we verified that we had done within just a few tenths of a foot per second matching this target. They never did find the Pod. I'm sure it reentered a few days later.

But that was pretty funny. Wasn't real funny at the time, but somehow we made it through the night, and nobody—well, they never said anything. Dr. Kraft or even Bill Tindall never said anything about wondering why we were in such a good mood doing all this work all through the night. Some of us managed to sober up about dawn. [Laughter] We pulled it off. That was the first phantom rendezvous.

Then the original Gemini VI, which was actually the plan that Cathy Osgood and I worked on for like a year, year and a half, she and I and Bill [William A.] Reini, who's another one that's departed us, he was one of the best software programmers I ever worked with. In those days, in the early sixties, he was a genius in software, which nobody even really knew much about. He and I and Cathy did the mission plan for the Gemini VI Agena rendezvous and worked on it at least a year and a half to two years, the plan itself.

Bobby [Bob K.] Culpepper, by the way, was the one that was my lead guy for Gemini V. He worked on it for two years, I guess. It was a disappointing mission, I guess.

But then VI was planned for the fall of '65. The Agena blew up right after insertion or at insertion. I think they didn't even make orbit, the Agena target vehicle. So that was another reason that we were really proud of Gemini 7, is we planned that whole thing and pulled it off in like two months from mid-November, maybe even a little after mid-November, to December 15th, and used VII the target, that long term.

That's an interesting story. I don't know if you've heard from Frank Borman or anyone but [James A.] Lovell [Jr.], but Gemini VII was the fourteen-day-long duration test mission in that little bitty thing that, it was like me and my co-pilot here for fourteen days in that little can. The story I always heard, on landing, I think we landed in the Pacific on that one, and the frogmen who would jump out and put the flotation collar around the vehicle. After Grissom lost the *Liberty Bell* [7 Mercury capsule], it was always a worry about losing it, sinking. So frogmen, and they usually had three frogmen jump out, put the flotation collar around, and then open or help the crew open the hatch. They say when the crewmen opened the hatch and these frogmen looked in and got a whiff of the fourteen-day-old air, that two of them got sick. And these are Navy frogmen, sea legs out the kazoo, and yet two of them threw up on the side of the Gemini. I mean, it must have been rank, really rank. [Laughter]

RUSNAK: I can only imagine.

YOUNG: But, of course, luckily, the Gemini VI was only like a day, one-day flight. We rendezvoused on the fourth orbit, which we called M=4. Actually, our apogees, the first apogee was M=1, or N=1 and then rendezvous orbit was designated in the software as M. N was the orbit count, and M was the one for the rendezvous. So that was an M=4, which was our standard planned rendezvous technique. You got into orbit, of course trailing below the

target typically, and then you do a series of maneuvers on those apsidal lines that mark apogee and perigee, line of apsides, and do a phasing and a height maneuver to get up to the target altitude. Then you do the terminal phase.

There's where Bill Tindall and Ed Lineberry, particularly, were the fathers of the terminal phase, where you do the closing intercept and actually did the rendezvous and stationkeeping in this case, because we couldn't dock. But it takes about four orbits or six hours from launch. That was a pretty great achievement.

RUSNAK: When you say, for instance, that you'd been planning the original sixth mission for two years, what kinds of things are involved in that planning? What takes up those two years?

YOUNG: Again, like I say, in '63 and '64 we really didn't know what we were doing. We knew how to work the equations of motion, plan and maneuver the right orbits, but as far as rendezvous goes, you have the relative motion solution. So they have a set of equations called the Clohessy-Wiltshire equation, CW equations. You'll probably hear sometime during this. Two guys, Clohessy and Wiltshire, derived these equations of motion with some assumptions that simplified solving them. When you had a target at the center of your relative coordinate system and then you have the chaser coming to it, you figure out some linearizations, but, of course, you have to have orbital mechanics factored in there to avoid the McDivitt kind of problem.

But you program all that in, that's when guys like Bill Reini and Jerry Bell and some other guys came in. They were mainly software guys. Bob [Robert] Regelbrugge was their section chief. Bill Tindall was the branch chief. We had to program that all into an IBM computer. Back in those days, well, when we first started in '63, we had a 1620. That's another story that's always funny. Headquarters, NASA Headquarters in D.C., had a

contractor named Bellcom. It's the Bell Laboratory guys. They always were supposed to do oversight for NASA on the centers and the center work. They got all the equations and the software from us, but they put their own program together in the 1620 up there.

By the time we started really doing serious rendezvous simulations, we had graduated to a 1094, bigger IBM number cruncher. They still had a 1620. They would call us saying, "Give us the data, your target orbit and all that for what you're simulating. We want to verify that it's all working for you." "Okay, don't bother us." But they had to put it all on like Friday afternoon and leave it running all weekend, because on the 1620 it took forever to crunch through these equations, a bunch of iterations. They would frequently come in, apparently on Monday and find that the temperature in the room had been too high or something and the 1620 was really delicate, and they'd call us and go, "Well, it went down sometime and we never got an answer, but give us your answer, the time of rendezvous or something, and what the height maneuver was and we'll go back and rerun it again." So we had to put up with those guys for about a year, and they finally quit trying. That 1094, which took ten minutes, but the 1620 took about twenty hours.

But you did that, you programmed all that in, and then you did all these simulations and a whole bunch of contingency analysis, the error sources, like venting of the target, or drag. Atmospheric drag is the toughest, one of the toughest things to work out for long-range rendezvous. Now, when you're close in it doesn't matter. When you're within a few miles or a hundred, it's pretty negligible, the differential drag on your target and your chaser, but at hundreds or thousands of miles and different lower orbits, you have to account for drag and so forth.

Another thing, of course, another perturbation, a fundamental set of perturbations are the Earth, non-homogeneous Earth, non-spherical Earth perturbations, so-called J terms or J2 terms of geomagnetic, geophysical constants. You have to factor all those in. In those days, a lot of that was just analytic. There was no empirical data that confirmed perturbations, for

instance, on atmospheric drag. For instance, people used a coefficient of 2, and finally through tons of empirical data on satellites and such, you found that 1.9 sometimes worked better than 2, or 2.1. It's a fudge factor, is what it really was.

So we had to fit all that in and run sims and do what we called Monte Carlo analysis, error analysis, where we run hundreds of cases to kind of build an envelope of dispersions, and then also derive backup targeting techniques for when the radar failed on board for the homing, more or less, or the intercept and the homing-in devices with line-of-sight targeting and all that.

That's where Buzz [Edwin E.] Aldrin [Jr.] came into play. Buzz was a big help in that arena. He had some ideas that we never really went along with and proved to him finally that we had some better techniques. But he was one of the major contributors.

RUSNAK: Yes, that was one of the things I had wanted to ask you about, since he has this sort of "Dr. Rendezvous" reputation within the astronaut corps and such, having his Ph.D.

YOUNG: Definitely in the astronaut corps. I mean, he was one of the few, if the only one that understood any of it. But, yes, I mean, I give him his due, particularly in terms of backup crew visual rendezvous, VFR [visual flight rules], or whatever you want to call it.

In fact, I still accuse him of deliberately failing the radar on Gemini XII. He claims it was a coincidence, but some of us don't really believe it. But he did pull it off without the radar. It was the only flight that it failed on, Buzz's flight.

Also on that flight we flew through a solar eclipse, a total solar eclipse. I think I still have that photo. I've got an autographed photo of the solar eclipse over South America that we set up and flew. I did that more or less personally, me and Ed Lineberry figured out how to do it. It was pretty tricky. They had like a eight-second window to fly through the path of the eclipse. But at the Gemini altitude of 120 or so miles, it was pretty tricky, because we

didn't have anything but the eclipse data on the ground. We had to translate it into altitude. Took a couple of months to figure that out.

But Buzz got a real good shot of the total eclipse and he signed one of the pictures for that. We're kind of proud of that. That was a rendezvous with a shadow. That was a true phantom rendezvous. I think it was like a eight-second window we had, or he wouldn't have gotten the full, you know, he'd only had a partial eclipse. So it worked pretty good.

So, Buzz, yes, particularly for the crew backup techniques and the 130-degree intercept arc that we used on Gemini, the equivalent of that around the Moon for the lunar rendezvous, like 140 degrees there because of the difference in orbital mechanics, or geometry, I should say, the mechanics are pretty much the same, except no drag, he was a pretty good influence on the whole rendezvous problem. I would never say he was the father of rendezvous. That kind of goes to the guy at Langley [Research Center, Hampton, Virginia], Houbolt, Jack [John C.] Houbolt. Houbolt, I think is his name. He never really worked operationally; he was kind of an academic. Did the first papers. Ed Lineberry worked for him. In my mind, Ed Lineberry is the father of real rendezvous, because he's the only genius I ever worked with. I mean, he was a true technical genius, and he knew those equations. He could sit down and write out the Clohessy-Wiltshire equations by memory, and he was just phenomenal. Unfortunately, he died about ten years ago, heart problem. But he, in my opinion, would be the father of rendezvous, with Bill Tindall being the leader for the whole effort. Ed was the technical father, Bill was the leader, or manager, father, whatever. Those guys were something else. They were just amazing people.

So we did all that, you know, and do all these simulations, and then, of course, and this is where actually myself and Cathy pretty well led the way, was you build a so-called nominal plan. So we decided on the orbit for the Agena target based on a whole bunch of factors.

You go through a bunch of analysis, drag is one of them, but energy analysis of how much fuel it's going to take to get there and what the dispersions of the Agena will be on its insertion into orbit and all of that stuff, and then figure out what the best perfect plan is, and you know you'll probably never fly the perfect one because of errors in dispersions and failures, but you build that into the mission plan and then you have appendix, so to speak, that says alternative plans, which we had for all the Apollo flights and everything, we had alternative plans. We didn't have the exact Apollo 13 plan. Those guys I didn't work that, because I was a lunar rendezvous guy, didn't work the LM rescue, per se. But they had simulated things that were very close to that situation.

It was lucky, in fact, of course—and you've seen *Apollo 13*, pretty good movie, actually—except for the time that the explosion occurred and exactly what it did in terms of failing the oxygen tanks and everything, they had simulated LM lifeboat scenarios, and so it wasn't a total shock or revision to have to do the things they did to save them. It was lucky that it happened when it did, instead of around the Moon, or on Apollo 8, which would have been maybe the end of the space program if it had happened on 8, or at least for a long time it would have been.

But anyway, that's what you do, and you start that, or in those days we started like two years before and you revised the plans based on changing events. Lots of times the launch dates of either the Agena or the Gemini, because the program changes and so forth, put out revisions. Then you finally, X months, or a month or two before the actual launch, you put out the so-called operational plan that tries to get it down to the nitty-gritty in terms of all the detailed launch times and numbers and all that.

In those days, of course, that was, we had done one for Gemini III. Of course, they'd done them for Mercury, but we'd done mission plans for Gemini III, but my branch, because we were the rendezvous guys, didn't really have a plan that we were responsible for until V,

Gemini V with the Pod. We had to work about two years ahead to get it all integrated, I guess, is really the term that we ended up being mission integrators.

There again, the idea that you had to learn how all the systems work and what to do if they fail or partially fail, like the fuel cells and all that, is what kind of made MPAD unique in terms of its role, because it wasn't just a matter of designing those systems or operating them, but figuring out how they ought to be designed or were designed and how you'd operate them in failed modes and less than perfect modes and performance.

We had experts on all those systems. We mission planners would go talk to them and go, "What if the radar fails here?" or "What if the propulsion system is under-performing and you can't get but so much velocity out of a certain set of engines?" So that's the real challenge of mission planning, is the "what ifs."

I don't know, I haven't ever really thought back through all those missions, but I think it's safe to say we never flew one that went exactly like it was planned. [Laughter] That's pretty safe. I mean, the fundamental objective didn't go exactly. Gemini VI, even, after the Agena blew up, we tried to launch and the plug fell out of the Gemini on the pad where Schirra and Stafford, wisely, didn't pull the eject ring. That was just amazing.

Then we had to turn around and replan the liftoff time and the targeting for the Gemini, which by the way, was another thing I'm proud of. Myself and a guy named Jerry [Jerome W.] Kahanek, wrote the equations and programmed the launch targeting equations that are still used today, in essence, in the Shuttle rendezvous launches, where you put in the target orbit and you have to figure out Kentucky windage, if you will, or you don't launch exactly into the target plane, because of the perturbations, you have to drift into it. There's a thing called differential nodal regression, and we worked all that out and flew it in Gemini VI was our first demonstration that all that Titan targeting worked. A guy named Stan [Stanley P.] Mann was the Titan expert that we worked with, ex-NASA guy, and some Martin Titan builder guys.

But we did all that, and really to this day, I know they still use those same equations basically for the Shuttle. We revised them a little bit for Skylab and a little bit for ASTP [Apollo-Soyuz Test Project], but fundamentally they're the same software that's been in there now for thirty-five years.

I was over talking to Cathy a couple of months ago about an orbital debris problem actually with the Space Station. She was showing me this data on how the Space Station inclination has been dropping. I don't know if anybody's told you that. It's kind of a little detail. Due to, I think, a third-order term Earth geopotential that's neglected or it's real long-term sinusoidal, I think. But anyway, she had a curve, she has a curve where the Space Station went into 51.6 degrees for the Russian inclination and it dropped about 300ths of a degree, which is nothing, although that's about ten feet per second in plane change if you went into the wrong original orbit not knowing, which you do, of course, the latest.

But she was showing me that curve, and a young gal, USA [United Space Alliance], I didn't know her, came up and had a sheet with a whole bunch of equations on it and she says to Cathy—she didn't know me—she says, "Cathy, do you know if the launch targeting, if the target inclination is mean or osculating?" Which is whether it has the average perturbations or actual real-time perturbations kind of a detail.

And Cathy said, "I don't know. I think it's mean, but why don't you ask the guy who wrote it." She points to me.

"Oh, you wrote these equations?"

I said, "Yes, thirty-five years ago." I told her it was mean. Actually, you use the mean target and then you bias, because what you're targeting is the Shuttle inclination. Well, the Shuttle goes in at 80-something miles altitude and the target is typically like the station, it's 200 miles. So you have to adjust for the difference in altitude because of the gravitational effects. So I explained to her that, yes, it's the mean target inclination, but you adjust it for

what we call an osculating term to make it into what the Shuttle goes into, so its mean I [inclination] at that altitude equals the mean I of the target.

Then you shift the nodes because of this differential normal regression. It's sometimes significant, it's 100 feet per second sometimes. It depends on how far you're behind the target, believe it or not, not only what the altitude difference is, but how you're behind the target, because it's phase-angle-dependent. Anyway, so I explained to her how it was supposed to work. She was kind of amazed.

RUSNAK: I'm sure she was.

YOUNG: I'm sure she wasn't thirty-five years old. She was twenty-five years old.

But that's what you do, you do all that, and meanwhile, of course, in addition to doing the mission planning, we supported the flight controllers and their sims [simulations]. We would sit in the back room and tell them when they're running through a plan and the simulating people go deliberately throw in glitches and anomalies. Then we'd tell them what to do in general, because, of course, their main objective is fundamental flight control rules. You can't write rules for each situation really, so we had to be in the background to advise and run off line.

In fact, all through Gemini we ran the rendezvous out of the other side of Building 30. We had the launch-starting equations then for the Titan, but we never had the rendezvous equations in Gemini. In the big computers in the MCC [Mission Control Center], or what did they call it in those days, MOCR [Mission Operations Control Room], missions ops computer. Not the MOCR, it's the front room. Can't think of the term now.

RUSNAK: RTCC [Real Time Computer Complex]?

YOUNG: RTCC. So we ran all of Gemini out of the other side and our 1094 and then we upgraded that 30-something, I can't remember the IBM numbers. Then the first few Apollo missions, I think up through 12, Apollo 12, we at least flight followed. We did the same stuff over there that was done in the RTCC as a backup or a check to make sure. RTCC had a great propensity for going down at the most inopportune time, because it was a huge complex of all kinds of data coming through, and electronics. It was subject to frequent failure and frustration. But that's what you do to plan a mission.

Then, of course, by the time we got to Shuttle, we had it pretty much down to so-called cookbook, but still there's a whole lot of preplanning. I think the cycle now is something like nine months or something, from first mission definition to launch. We did some Shuttle missions in way less than that because of some big changes.

We did a dual rendezvous from scratch on—I can't remember the numbers, but in about the second or third year of Shuttle, maybe the fourth or fifth year, we had two comm sats [communications satellites] that we put out, Intelsat and another foreign satellite, that the perigee motors didn't work or they worked partially and put them in these weird orbits.

I remember that mission, because I was out on the golf course playing my best, and I got a call from Glynn [S.] Lunney. I mean he sent a guy out to the golf course. I think I was one under par at about five holes. I was playing really good. He sends this guy in a golf cart, says Commander [James A.] Abrahamson wanted to talk to me and them about trying to recover these two satellites, which I had worked that mission, but it was just a deploy mission and our branch did the plans for it, but I wasn't personally involved.

So I get over to the secret room in Building 1. This was the [NASA] Administrator for Manned Space, former MOL [Air Force Manned Orbiting Laboratory] astronaut named Abrahamson, not the sharpest guy I ever worked with, but I guess he was a leader. Anyway, he wanted to know, they had had these problems and they put these two PAMs [Payload Assist Module] in the satellites in these weird 500-mile apogee, half a plane change orbits

down toward the equator, whether we could pick them up on that flight. [Laughter] Of course, Glynn Lunney knew better than that, he's an old dynamics guy himself. But I guess they wanted to hear it from the expert, so we explained to them, well, first of all, there's no way this Shuttle has the energy left in the maneuvering system to get anywhere close to those orbits, and even if it did, the cradles they deployed them in, you couldn't bring them back in. Even if they could do a space walk and physically bring them back into the bay. It just was totally infeasible.

But didn't take long to even convince him of that. He said, "Well, how long will it take you to figure out a plan on a future Shuttle to do it?" We sat around and figured it out, and said we could do it in about four or five months, and we did. I think that was in the spring, and we did a flight in the fall of that year. Of course, we had to get those satellites maneuvered into rendezvous orbit so the Shuttle could get to, which, as I recall, working with the guys at Hughes, who had built both of them, they did 1,300 maneuvers over a period of about five months, which we told them how to get in there, but they actually performed the maneuvers.

That was something, because you had to set up, because of this differential nodal regression, you had two targets, you had to figure out how to get to one, and then get on over to the other one. Pulled the whole thing off in six months or less. That was totally successful, we recovered them both. The insurance company was really happy. [Laughter] And I think they refurbished one of them and launched it a few years later. I don't think they did both of them.

It was one of our more "fly by the seat of the pants" kind of missions. So you can do it, it's just you try not to make a habit of that, because, one thing, it works everybody to death, and, two, things have to go perfect on that plan, because you haven't had time to look at all the alternatives. If they come off good, then you look great, and if they don't, if there's some little glitch that you hadn't analyzed or planned for, then you look kind of bad. So

consequently, they still take six, nine, twelve months to do a mission plan from cradle to grave.

RUSNAK: With that kind of lead time is one team working each mission, or are you going to be working missions simultaneously?

YOUNG: I think Cathy would have to explain now how they do it, because USA—

RUSNAK: Or even back in Gemini and Apollo.

YOUNG: In our day we did we did, we staggered them, but typically we'd be working on three or four missions in parallel, but they were spread apart by months, if not years. I mean, like I say, I worked on Shuttle in '69, and of course, we were getting ready for Apollo 10 and 11 in that same time frame. Typically, at least in MPAD, you did it three or four missions and spread over X years, even.

RUSNAK: Let's go ahead and move into Apollo a little bit. One of the questions that came to mind, from earlier you had mentioned John [C.] Houbolt working on rendezvous, and he was one of these proponents of the lunar orbit rendezvous concept versus Earth orbit rendezvous or direct descent. Did you have any involvement or knowledge of this mode decision, they call it?

YOUNG: Yes, I think the decision was made in '62, but maybe '63. I think it was '62. I had just gotten there. I never was, or I never considered myself a lunar rendezvous expert. That was people like Dave [James D.] Alexander and Jerry [Jerome A.] Bell, Al [Allen L.]

DuPont, who pretty much their first X years were totally devoted to lunar rendezvous, which is not that different, but there were some differences.

As I recall, it was in the summer of '62 that JSC went to Marshall, had a big meeting with [Robert R.] Gilruth and von Braun, and JSC was arguing, along with Houbolt from Langley, the lunar rendezvous approach as being the only thing that was feasible in the Sixties, within ten years. Then, of course, Marshall wanted to do either direct, which, as it turns out, just really wasn't feasible, or Earth orbit rendezvous. Both of those cases, of course, took a much bigger booster, bigger than Saturn V, to get there. So they went through all that tradeoff, argument, at one big meeting, I think it was June of '62 or sometime in there when I had just arrived.

The way I heard it was that von Braun listened to his guys for four hours in the morning and then JSC in the afternoon, or vice versa, and at the end of the day he conferred with a couple of his guys and maybe Gilruth, and then he comes out with, "We're going lunar orbit rendezvous." He was convinced, at least himself, that that was the only valid way to do it in the Sixties.

Then Dave Alexander, the guy that I worked with for many years, he's living up in Longview now, he came in the fall of '62, and I remember him working that same problem with all those arguments, because Kennedy had a science advisor. I forget the guy's name, Jerry Weisner or something like that. That guy was a giant pain in the ass, I mean, for years, because somebody was putting thoughts into his head that lunar rendezvous wasn't right and we'd made a big mistake.

So I remember Dave spending at least two more years going through pitches about why this still is the best way to go and all the energy tradeoffs and the size of the launch vehicles and so forth and the contingency problems and all of that stuff, mainly to convince Weisner that von Braun and Gilruth had made the right decision a year before. I think he stayed on with [President Lyndon B.] Johnson after the assassination, which, of course, those

of us who were then remember that one vividly, because Kennedy had been to NASA down here the same trip the day before. We'd gone out to HPC [Houston Petroleum Center] and watched him go by on a motorcade on the Gulf Freeway.

But I think Weisner stayed on as science advisor to Johnson and he still gave NASA hell for that for years, until they finally, either he got fired. I don't think they ever convinced him that it was the right way to go. This was, of course, '63, '64.

Then, of course, Gemini had pretty much in our mind proved, and especially with working with Buzz on the lunar version of the co-elliptic rendezvous that we developed pretty much verified that we knew we were doing the right thing for the lunar program.

But fundamentally, as I always understood it, it really came down to pretty much pure energy, in other words, how big Saturn V was a great vehicle, but it had to be that big, it would have had to been twice that big to do it the other way. The direct was probably still not even feasible, but, you know, all the subtleties about the LM as a lifeboat and the other things that ended up showing that, hey, that's really for other reasons pretty neat that we had another vehicle.

Of course, in the worst case you lost two guys, but not the third one, if they couldn't get off the Moon. But it's better than going direct. Earth orbit rendezvous was also, I mean, it was easy to do Earth orbit rendezvous or pretty feasible, and we proved that that was also a big performance thing. We hadn't really perfected orbital assembly that you'd have to do, not like Space Station, but, you know, docked together two or three vehicles and make sure that the fluids were all okay, because you do worry a lot about slosh and stuff in orbit. Transferring fluids from one vehicle to another still to this day isn't a proven technology. You'd have had to do that for Earth orbit rendezvous to the Moon.

So Dave is the guy to talk to. I'm sure he'd be happy to come down and talk to you. He's written AIAA [American Institute of Aeronautics and Astronautics] papers on all that and so forth. He just had a hip replacement about two months ago, but I hear he's doing

good. He's one of the great athletes that I ever played with at NASA. We had some pretty good athletes there.

So Apollo, like I say, I was mainly a rendezvous expert. We'd do our portion, namely the rendezvous plan of each mission. Of course, we did the Earth orbit missions, by the way, Apollo 7. Ed Lineberry and I pretty much replanned Apollo 1 into Apollo 7 after the fire. The fire was really traumatic for everybody. I don't know that the Shuttle was any worse or better, but it was pretty traumatic.

But I spent like probably two years after the fire reworking the Apollo 7 mission and we did a rendezvous in it, by the way. We did more than a turnaround stationkeeping with S-IVB spent stage. We actually moved deliberately off hundreds of miles and then came back to test the CSM VHF [very high frequency] for rendezvous. Because we, of course, proved the Gemini transponder radar, which was essentially what was flown in the LM, but we had never really—well, on Apollo 9 we did some rendezvous with the CSM.

But on the 7 was the first time we had used the VHF for a ranging device and we did that with a spent S-IVB, or its spent S-IVB. We had a venting problem on that. We thought we had it down right. Of course, you never know how much is left in there because of the launch trajectory. But we thought we had that bounded as to how much was still in there, but what we didn't figure out very precisely was how the thermal fluctuations of day-night, the diurnal effects would have on when it vented.

We separated from it by fifty miles or something and were going to come back the next day with a couple of small maneuvers, and found that every time it came into daylight out of darkness, it did a little burp. It kept getting further away from us. By the time we figured that out, it was several hundred miles away, so we had to revise our plan. But we pulled that one off, or Schirra and the gang did. That was a successful long-range stationkeeping rendezvous.

RUSNAK: This might be a good time for us to take a break to change out the tape, if we can.

YOUNG: Sure. [Tape change]

YOUNG: We're up to Apollo.

RUSNAK: That's right, just talking a little bit about Apollo 7. Apollo 8 you mentioned a little bit earlier, this is sending the command module around the Moon the first time we were going to do a manned orbit of the Moon. What kind of challenges did this present, or did you have to do any new kind of work for this type of mission?

YOUNG: Well, that was probably the gutsiest decision NASA ever made, or we ever made. It had been talked inside of MPAD for at least six or nine months, but, of course, we knew better to ever talk about it outside, because it was so controversial. But after we flew 7 and saw the CSM was really a good vehicle, it's reminiscent now of the Shuttle. Once we got STS-1 under our belt, we saw what a great machine the Shuttle basically is. There was talk mainly by John Mayer and Bill Tindall about flying circumlunar, not necessarily going into lunar orbit.

I was, of course, working mainly rendezvous Earth orbit, Apollo rendezvous plans then, so I wasn't directly involved with that concept. I knew about it, but it was mainly Carl Huss and John Mayer and Bill Tindall that had obviously talked to Chris Kraft and some others through the months after Apollo 7, and says, "Hey, it's a pretty good machine and we can probably do it."

But I guess actually, I mean, I'm a little off there. Seven and 8 were two months apart, but it was the planning, I guess, for 7 that said, "If everything goes really well with 7, we ought to at least, or the agency ought to at least consider flying a circumlunar flight." As

far as I ever knew, it was because we thought surely the Russians were going to beat us, even at that stage, I guess a little paranoia, but they had pulled off some pretty neat stunts, and, of course, they were so secret, we didn't know much about what their real progress was.

I think most of us were skeptical that they could land a man on the Moon, but we thought flying around the Moon wouldn't be that tough. Gutsy, but you could envision that the Russians would be gutsy enough to pull that off, not land, but just fly around it. In fact, to this day it's still surprising to me that apparently they didn't ever think of that, or think that would be a great stunt. So our Apollo 8 really surprised them. I don't think that was the intent; it was just beat them to the punch, as I recall.

And like I say, it was primarily based on being convinced that the new CSM was going to be a really trustworthy vehicle. Of course, Saturn V had already been pretty well proved that it was going to be a pretty reliable vehicle. I recall hearing talk about it in like May of that year, '67, '68.

RUSNAK: It launched in '68.

YOUNG: May of '68, I guess. I remember hearing talk about that somebody ought to look into it, but Ron [Ronald L.] Berry and Hal [Harold D.] Beck and the lunar guys, as we called them, "the lunatics," were working those, the basic Apollo lunar missions. My branch was working primarily the rendezvous parts of each lunar mission and the Earth orbit plans. We had an A, B, C and C, D and F and G, all these lettered alternative missions. I guess G was the lunar landing.

So I wasn't directly involved with those plans, but I do remember hearing them in April or May of '68, and then in the summer of '68 I remember some of the guys in MPAD going to meetings, or talking about it. But I was so absorbed in 7, myself, and this rendezvous with S-IVB that I didn't really pay much attention. Again, all I remember about

it is that we kept thinking the Russians were going to do something pretty spectacular pretty soon if we don't really keep going. Of course, the fire had set everybody back just tremendously.

So we flew 7, and then I guess it was John Mayer or some of those, George [M.] Low and Kraft decided, along with Gilruth, to see what the Administrator would think of a gutsy plan like that, and for whatever reasons, they went ahead and did it. That was gutsy, because Apollo 13 could have happened, and that would have been disaster.

But I guess really a sub level of what was really gutsy in terms of MPAD was that John Mayer and Huss and company convinced NASA management that not only should we fly around it, like on a free return, but we got the confidence to go into lunar orbit for a day or so and then come back, and get out of lunar orbit because we were sure the SPS would work, which was the main engine on the CSM.

The technical argument for that was to get a better fix on the gravitational anomalies, what later became to be called the mascons. But that was, as I recall, Emil [R.] Scheisser and John Mayer's argument is, look, if you go around the Moon into 60 or a 100-mile lunar orbit, then you really get a feel for these perturbations that weren't very well defined. I guess they had had a couple of lunar landers, not rovers, but they had a couple of lunar orbiters, at least, that had shown signs of these perturbations that the nav guys had not known about or anticipated, so they really wanted to get as much data as they could before the landing mission. So it was a double gutsy call, really, to go into orbit and so forth. Turns out it worked great. I didn't work it directly, because I was a rendezvous guy. Can't take any credit for that, really.

Then after that we flew Apollo 9, which our branch planned and pulled off, which was all the LM rendezvous and the fire in the hole and the LM ascent stage firing with the descent stage still attached, and testing out some CSM rescue scenarios and techniques. All that worked wonderfully, too, really a good mission. That was a good mission.

RUSNAK: Of course, you've got all that going on in Earth orbit, rather than out at the Moon.

YOUNG: Yes. Then, of course, that was followed by 10. Ten was the first lunar rendezvous. They say [Commander Thomas P.] Stafford wanted to land, but I don't think he ever really seriously considered trying that. He would have loved to, but it wasn't like he was going to suddenly go, "While we're down here, let's go ahead."

We worked that one pretty hard, because the rescue worries about, you know, we tried to simulate the LM coming off the surface, and if the CSM had to do the rendezvous, because the LM couldn't get into the CSM orbit. Did a whole bunch of contingency planning for it. Then when they went down to the 10,000 feet or whatever it was, can't remember the perilune on those, but they really did pick up a lot of the data on the mascons. That was really helpful for the final targeting stuff on 11. I don't know that it was make or break, but it sure didn't hurt to have a case that was pretty close to the orbit of the final landing. That was a good mission, too. That was John [W.] Young and Stafford and—

RUSNAK: Gene [Eugene A.] Cernan.

YOUNG: Gene was on that, yes, then he came back on 17, yes.

So meanwhile, some of us were working on Shuttle, also, defining. Carl Huss and I were two that went up there to spend three weeks in a workshop to define the logistics vehicle parameters and we were assuming for the purpose of sizing in the energy trades and all that, a space station in 55-degree inclination, 270-mile orbit, I think. I think that's a one-day repeating orbit, by the way. There are certain orbits that repeat their ground tracks. That has certain advantages to Earth observations and other rendezvous aspects, too, because your launch window every day is the same predictable azimuth and so forth.

So some of us were already working those kind of concepts, in addition to working the lunar rendezvous stuff. One story I sometimes tell—Buzz probably won't even admit that I suggested this to him—but right before the Apollo 11, we ran a detailed rendezvous meeting of some sort, and afterwards I said, "You know, Buzz, if you want to guarantee the long-term success of Apollo and that we'll continue to go back to the Moon just forever, all you need to do when you get up there, just take a little pouch filled with gold dust and just spread it out on some of those rocks when you collect them." [Laughter] I said, "We'll be going back there for 100 years trying to find that gold."

He said, "Oh, I couldn't do that. I couldn't do that."

I saw him not too long ago and I reminded him of that, and he said, "Yes, I couldn't have done that."

I said, "But if you had, just think, we'd been back to the Moon by now." [Laughter]

Well, let's see. On 11, I mean, you know, talk about your nominal missions, that was about as amazingly close to the plan as I can recall us ever flying. It was pretty amazing in itself that it was the historic flight. And except for Armstrong's problems with the rocks, and the alarm, the famous Steve [Stephen G.] Bales hero-making, which [John R. "Jack"] Garman probably talked about. That's another case of the back-room guys knew what was happening and even had simulated it, the same stuff.

But it went as close to perfect as you could imagine. I guess the rocks were pretty scary and he was running pretty low on fuel when he finally sat down. But other than that, everything went great. Pretty amazing feat still. I don't think you could have faked it. I just don't know how you'd have done it.

RUSNAK: Where were you when they did the landing and the first EVA [extravehicular activity]?

YOUNG: We were in the back room, in the rendezvous room, because, of course, we had to be constantly be ready for emergency liftoff of the LM if something went wrong or the Moon bogeyman came after the crew and they had to jump back in. So we had shifts. Dave Alexander and I and Ed Lineberry were the prime shift, and then Bob Becker and Jerry Bell, I think.

We had one guy on each shift that was an expert in CSM, what we call the mirror-image rendezvous techniques for the CSM to come get the LM, which is tougher than you might imagine because of the orbital mechanics and the constraints on the vehicles. Pretty tricky stuff, but we were all set to pull that off if we had to. Never did have to do that. So, yes, I was in the back room when we it landed and did the EVA.

Dave reminded me a while back of something I'd forgotten about, but when they landed, the pods kind of sat down uneven and the thing was tilted I forget how many degrees. But we had even considered that as a potential problem of when you came off at a tilt, what it would do to your LM trajectory and so forth. He reminded me the other day that we had to make a call in the back room as to whether we thought the tilt was within limits. I don't even remember what they were. I think Dave must have made that call. I remembered after he reminded me of it, of him asking me. It was pretty exciting at landing, you know, "Houston, this is *Eagle*." [referring to Apollo 11 shirt Young his wearing] (I got this at the twenty-fifth or thirtieth reunion thing).

He reminded me that he had asked me, since I was the shift leader or whatever, if they read him an angle and he looked on our chart, and we converted it into what that meant for a rendezvous trajectory. He reminded me that he had asked, he said, "Well, the value is this." I forget what it was, not very many degrees, and that I had said, "Yes, it's okay. Go ahead and tell them to land." So it was along with Bales' "Go flight." That's the part I remember most is because we're all back in the back, listening to him, and he always had a high voice, and he was so nervous when they asked him if it was still go for landing, "Go

flight." So we kidded him unmercifully from then on. "Hero" Bales and his, "Go flight." [in falsetto]

RUSNAK: It's funny, because Bob [Robert L.] Carlton was in here a little bit ago and he brought in his tapes from the LM console and you can hear everybody make their calls along there, and, sure enough, there's Steve Bales.

YOUNG: I haven't seen Bob in a couple of years. I've got a funny story about Bob that I thought of, I remembered the other day because I was working on orbital debris assessment for Space Station. We used to have one of the bureaucratic things you have over at NASA now to the nth degree, is all this safety stuff. But back in those days, actually, this was in the Shuttle days, pre-Shuttle days, we had a fire marshal for each building. They still do, of course, two or three of them. Bob Carlton was Building 30 fire marshal for several months or a year or something, he got stuck with that. So he'd come around. Guys would have literally mountains of printout and computer paper just unbelievably stacked. Some guys, you couldn't even get in their office, you couldn't even see their desk or table for the paper. He'd write a note, you know, "Fire violation." [Laughter] He was obligated, I guess, to do that. As branch chief, I had to go in at night and dump a bunch of guys' stuff, because they would refuse to ever get rid of it.

But anyway, we had a guy that is an orbital debris expert, although he's retired now, Don [Donald J.] Kessler. This was the late seventies. He had a stack of Shuttle tiles in a cardboard box that he had taken to Tullahoma, Tennessee, one of the high-velocity impact gun facilities. You'd put these tiles in these wind tunnels and fire these meteorite or orbital debris particles, BBs and stuff at them, to see how well they stood up. Of course, they're just that foam stuff. He had a whole stack of them in a box in his office, and he was gone off to a meeting.

Bob Carlton goes by and sees this cardboard box with all this stuff, he didn't even know what they were, just little squares, looked like foam. So he writes up Kessler for fire violation and leaves it on his desk. Kessler came back and saw that and broke out laughing and called some of us over and we read that. He says, "You know, those tiles are fire resistant to 2900 degrees Fahrenheit. This whole building could burn down and they would be the only thing still sitting there. The box would be gone." [Laughter] Bob never—I don't know if he ever outlived that one. "I didn't know what they were. They looked like they were flammable to me." [Laughter] That was funny.

RUSNAK: We'll have to ask him about that next time when we run into him.

YOUNG: Yes. So let's see, you know, what can you say? It was the greatest fake mission we ever pulled off. The flag, the wind was blowing that flag. I gave a talk to some Pearland high school students, oh, about three months ago now, as part of Engineering Week. This, coincidentally, happened the day before that Fox first presentation of the conspiracy thing. I'm glad it wasn't the day after. But I had one kid ask me—I did it on orbital debris, actually, because that's what I was working, about Space Station and the threat of orbital debris and how we take care of it, or how NASA takes care of now, and does avoidance maneuvers, and what the protection shielding is and a lot of that. So they had some good questions, and I had a bunch of little Space Station emblems that I said I'd give to the students that asked good questions, you know, incentive to them. So I had some good questions.

Then right at the end, this guy, this kid in the back asked if there was wind or air on the Moon. This was now the day before that program. Of course, people have asked that a lot, and I said, "No. There's no atmosphere as you know it. There's certainly no oxygen, no air. There's some particles and radiation. You could call some of it atmosphere of sorts."

He said, "Well, how come the flag looks like it's waving?" He said, "How comes it stands out, like the wind's blowing?"

I said, "Did you ever hear of a stick, a wire just holding it out there?"

He says, "Yeah, but it looks like it's waving."

I said, "Well, when you put it down in the lunar dust and you twist it or try to orient it, it puts in dynamics, and in a vacuum, particularly, it doesn't damp out, so it just sits there until physically mechanical damping damps out." That's because they were sticking it down in the dust.

He says, "Well, it sure looks like the wind's blowing it."

I said, "No, that's a common fallacy."

This other kid on the other aisle, he had already asked about three questions and I'd given him one of these things, he was just trying to collect these, he goes, "Hey, I got a question."

I said, "What's your question now?"

He says, "Do they send monkeys into space?"

I couldn't resist. I said, "Why, you want to sign up?" He took it pretty good. I went up and high-fived with him. Of course, his peers just hoo-hawed. "Yeah, send him out of here!" I don't know why he asked that question. I mean, we did way back. They were a pretty knowledgeable bunch, the ones that weren't asleep, two or three, which you have in every group.

RUSNAK: It's a little bit after 5:00 now, so I don't know if we want to wrap it up for the day.

YOUNG: Probably should, I guess. Eleven is a good milestone.

RUSNAK: Okay. Let's go ahead and stop the tape.

[End of interview]