ORAL HISTORY TRANSCRIPT

ROBERT G. CHILTON
INTERVIEWED BY SUMMER CHICK BERGEN
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BERGEN: Today is April 5, 1999. This oral history interview with Robert Chilton is being conducted at the offices of the Signal Corporation in Houston, Texas, for the Johnson Space Center Oral History Project. The interviewer is Summer Chick Bergen, assisted by Kevin Rusnak and Sasha Tarrant.

Thank you for being here with us today.

CHILTON: Happy to be here.

BERGEN: Let's begin with how your interest began in aviation.

CHILTON: When I was growing up. Were there any events in your early life that led you to aeronautics or the space program? I guess I'd better take off my glasses. I read much better with them. Is that okay?

BERGEN: That's perfectly all right.

CHILTON: Well, I was born in 1922, late '22, so those are pretty early days. A comic strip, there was one called "Brick Bradford" that I used to read religiously. There were radio shows, series about stories from Randolph Field [San Antonio, Texas], aviation cadets, and stuff like that. Of course, [Charles] Lindbergh flew, I think, in '27, and I was just four years old then, so I had no recollection of that event, but I had little books all about Lindbergh and the hero stuff. So that was another factor.

In those days, I lived in the outskirts of a very small town, about 6,000 people. When you heard an airplane—I thought everybody did it—you'd run out the screen door and look until you spied it and you'd watch it until it went out of sight. That's just the way things were. I used to think, maybe he'll land in the field over there and I can go over and maybe he'll give it to me. We even had a local hero named Clifton Tribble, who was a barnstorming stunt pilot who went around. He was eventually killed, I think, in doing a dangerous stunt. So that was how I got interested in it, because I was just always crazy about airplanes. I drew a little and bought books about them and stuff.

When I was in the second grade, a Ford trimotor and some about 200-horsepower biplane landed in a cow pasture outside of town. Everybody went out and we got rides. I was always kind of conservative, I guess, so I didn't go for the biplane, but my folks let me ride in the Ford trimotor. So that was my first airplane ride, was in a Ford trimotor. It took off from a cow pasture.

In fact, I was over in San Angelo [Texas] last year and there's an old museum over there, I mean, a country store that had a lot of airplane models, and I bought a model of the Ford trimotor, which is the first time I'd ever owned a Ford trimotor model. It was a neat airplane, had a lot of great history behind it.

Okay. Now, it says military. Oh, you want to phrase that one, do you? It says, "How did you come to fly B-17s?" Recount my training experiences, when and where I trained, and so forth. So here's the history of that. When I got out of high school in '41, in order to…avoid entering the labor market, there was something called the NYA. [That] was the program…that Lyndon [B.] Johnson worked in. It was Depression times, and there was this program where you could go to school and the government would pay for your expenses NYA. I got $[10] a month for working in a band, keeping instruments clean, choir robes and all.

So I went over there, because in those days the Army Air Corps, Force, whatever it was, required two years of college as an eligibility to become a cadet. A lot of schools, including Austin P [Austin Peay State College], it was a normal school the year before I went there, but they had a program that was called CPTC, Civilian Pilot Training something or another. If you went to this school, then after you finished or completed the…freshman year successfully, your sophomore year you started flight training. Then when you completed that second year with so many hours of flight training, you went directly into the Air Force. I think it was called the Air Corps then. So that was when I was there. I was a great math student and everything.

But that fall, of course, that winter, Pearl Harbor. So the next year that program was canceled. So I finished up that year and then that following summer, summer of [19]'42, I got a job helping build an Army camp, Fort Campbell [Kentucky] it's called today, and I was working for the contracting engineer, figuring stuff.

In odd times I'd run over to Nashville [Tennessee]. My first effort was to go to the Navy to see if I could become a Navy pilot. I figured that Navy pilots had to do their own navigating, and I was so smart mathematically that I probably should be a Navy pilot. So I passed my test in Nashville, and they put me on a train to Atlanta [Georgia], and I flunked the eye test, first crack out of the box.

So I came on back and I said, well, I'll try the Army, and went on back to Nashville and flunked the eye test again. [Laughter] But the flight surgeon, or whatever they called them in those days…gave me a prescription for vitamin A and B12 tablets, he says, "Take these for a month and come back." And I did, and I passed. So that was great.

But by that time they [had] abandoned the two-year college requirement and for a long time they only took current military personnel and they trained them. When they got their wings, they'd become flying sergeants, since they'd had no college. So they had pretty

well established a pipeline of trainees, so when I was accepted they said, "Well, we'll call you when we're ready for you."

So I went on back to college that fall, and at Christmastime, at the end of winter term, I got called. So I went in in January, I guess it was, of [19]'43. January of '43 is when I finally entered the service.

The procedure in those days was first I went to Miami Beach for basic training, GI shoes and everything. Then you went to Nashville for what they called classification center. That was to decide whether you'd be a pilot, a navigator, or a bombardier. I figured, well, I'm going to flunk the eye test, so I'll probably be a navigator, but I passed again, so I was put in the pilot pipeline. The next step was Maxwell Field pre-flight in the summertime. Boy, that was rough. In Montgomery, Alabama, Maxwell Field, Montgomery, Alabama.

Then primary flight training. That's the Stearman biplane in Arcadia, Florida. That's on the west coast of Florida down south of Tampa, way south of Tampa. Two months there. I won't go into details of my adventures and training, but I finished that up finally. Then the next step was Courtland, Alabama, for basic training. That was the Vultee [Aircraft, Inc.] Valiant, they called it, a BT-13. I got held up a month there, so I was there for three months. In fact, I got to go home for Christmas, because it was not that far from my hometown.

Then the final step was Seymour, Indiana, Freeman Field, for twin-engine training. As I said, I was sort of conservative. I never figured myself as a gung-ho, so I'd always put down P-38s, so I'd…have two engines. Actually, in basic I got to where I liked the hot stuff, flying formation and stuff, so I changed and put down fighter pilot, but anyway, they sent me to twin-engine school, as opposed to single-engine school, at Freeman Field.

April 15th of [19]'44, I became a second lieutenant with silver wings and all that stuff, and they shipped me to Lockbourne Air Force Base. Today it's called [Edward V.] Rickenbacker Field in Columbus, Ohio. That was a B-17 transition school, they called it. That's where you learned to fly B-17s.

Another slowdown in the training cycle—see, this is summer of [19]'44, and by this time things are really hot and heavy over in Europe and there was a slowdown in training again. I lost another month in waiting for my turn to get in the cockpit of a B-17. Everybody there were student trainees, were second lieutenants and going to learn how to fly B-17s.

From there, then, when I finished up there, we were sent to a—you asked, did I start out with the 486 Bomb Group. The answer to that is, of course, since it was pretty well into the war that I went to a replacement training center, which is where they train crews to be shipped over to be put in units that are already over there. That was at what is now the Tampa Municipal Airport. It was called, in those days, Drew Field in Tampa. That's where I collected a crew, co-pilot, navigator, bombardier, five, I think, gunners. We got to know each other and started flying training missions in B-17s. We'd go out and we'd fly formation and we'd go to high altitude and we'd run down along the beach and have them shoot guns at the targets in the water and stuff like that.

In fact, we were doing so well there, that we got so far ahead in our training requirements one time that we were picked Crew of the Week in the local weekly newspaper. As a prize, we got to fly overnight to Havana, Cuba. So I met some Cubans down there in the days of—what was that guy's name [Batiste]? Anyway, the big dictator down there. [We] met some college students and they took us around. It was fun.

So, let's see. Then the next step, in between, to get [from] one of these places to another was always troop trains. I rode a lot of troop trains. This next one, of course, was with my whole crew, although they kept officers and enlisted men pretty well separated. It was not encouraged too much to get too friendly with the enlisted men.

Anyway, the next stop was to go to Savannah, Georgia. That's from Tampa, Florida, to Savannah, Georgia, which is a place where you were equipped, you were given final flight gear to be—at that point I might have been given an airplane to fly over there, to ferry over there, with my crew, but by that time there wasn't too much of that being done, so we then

got another troop train from Savannah up to Camp Kilmer, New Jersey, which was a port of embarkation.

We went to Europe on the *Queen Elizabeth*, and that was quite an experience. We docked in the Firth of Clyde, that's the mouth of the Clyde River in Glasgow, on my birthday, December 21st, 1944. I was twenty-two years old.

Then we went to a place where they decided where to send you, and they sent us to the 486th Bomb Group, which was near Sudbury in Suffolk, which is the hometown of Gainsborough. There's a little monument there in the square…[dedicated to] Gainsborough [the painter of The Blue Boy and others].

You ask about my squadron. It was 835th Squadron in the 486th Bomb Group. That bomb group had gone over to England in the previous fall as a B-24 outfit, but they finally established that the B-17 was a little more effective, although it couldn't fly as high or as fast as a B-24, or carry as many bombs, but they withstood damage much better.

So, by the time we got there, there were three divisions in the 8th Air Force. The first and the third were B-17s and the second was [B-]24s. So my outfit, which started out as B-24s, they flew a mission or two, I think, but then they converted them to B-17s. When I got there, we were all B-17s.

Okay. Let's see. My first mission, you asked about, it was a target in Germany. By the time I got there, by the time I got to my base, it was late January of [19]'45, and we had air superiority. I only saw one enemy fighter in my tour, but flak was the big thing. So that first mission was in the early part of February in '45.

I like to tell this story. I went to Berlin two or three times, I think. In the movies they'd show the briefings, you know, and this big chart and the map of where to bomb. They would say, "In Berlin there are 800 guns, but only 600 can bear on you at any one time." [Laughter] I like to tell that story.

So I flew my twenty-fourth mission on April 7th, 1945. That would be fifty-four years from day after tomorrow. It's also my daughter-in-law's birthday. We celebrated it yesterday.

In fact, they wrote a book about it. I saw it in the Barnes and Noble catalog, and I ordered it and bought it. On that day, that's when I saw my only German fighter, the Luftwaffe made its last effort on that day, April 7th, and somebody wrote a book about it from the German Luftwaffe standpoint. My bomb group was mentioned. I'm not mentioned, but my bomb group is mentioned, and I was in it.

So anyway, when we got back from that mission, I had really flown fast and furious. At that time I had flown more missions in less time than anyone in the history of the group. I was flying on the average of about every other day, sometimes two or three days in a row. Then we'd have weekend passes to go to London. They sent us to R&R, rest and recuperation, something like that, to a big, old mansion down south of [London] run by the Red Cross. While we were there, [Franklin D.] Roosevelt died, so we were flying our flags at half-mast and everything.

When we returned from that seven-day thing, when we got back, the battle lines had become so fluid that the 8th Air Force went off operations. I might have flown once or twice after we got back, but they didn't call my number. The biggest thing about those, that I remember most about that, it was cold. Nissan huts, we called them, is where we lived, and we burned bomb ring casings, heavy cardboard, in these little old stoves to try to keep warm. I had to sleep with all my stuff piled on top of me. I never spent much time in the Officers' Club. So I'd go to bed not knowing whether I was going to fly the next day or not, so I'd lie there and I'd wake up, because if you're flying, then some guy would come in, shake you by the shoulder, and say, "Briefing at such and such a time," 04:00 or something like that, four o'clock in the morning. Then if I had been shaken, we'd go down to the mess hall and we got

real eggs for breakfast. On mission days you'd get real eggs. Other days there was powdered eggs. Then we'd get the briefing, take off and fly.

After we got back to our base, I didn't get called [the] one or two times that they flew. So the last mission that my bomb group flew was probably around mid-April or maybe about the third week of April maybe. That's when the 8th Air Force just stood down. VE [Victory in Europe] Day then was not too long after it, so it was in May of '45. We began to prepare to fly home as a group and eventually to train in B-29s to go to the Pacific. We didn't do any celestial navigation during the war because we had little radar things, and so we had to do practice missions to learn to get back into flying celestial navigation.

When the point in time came for us to leave, we went from our base, which was [in] what's called East Anglia [phonetic]. Gosh, every ten miles there was a B-17 base. We were really something coming home from a mission peeling off over the overcast, coming down through the clouds and breaking out, trying to find the airfield and B-17s from other groups all milling around trying to—it's a wonder we didn't have a lot of accidents.

Anyway, so we went up to Wales, which was a rallying point to take off, and then the mission home, the itinerary was a stopover in Iceland. I was in Iceland on the Fourth of July of [19]'45. Then to Labrador, Goose Bay, Labrador. Both of those stops were made anywhere from one day to two to three days, depending on the weather and everything. Then finally to Bradley Field, Connecticut, where we left our ships and were put back on troop trains to go to, depending on where you were from, to some central place for processing for thirty days' leave. My co-pilot and I, [he was] from Birmingham [Alabama], were sent down to Atlanta and home for thirty days, back to Atlanta to get reordered and sent back to Drew Field.

When I got back to Atlanta from my thirty days' leave, VJ [Victory in Japan] Day happened. I was walking the streets, down Peach Tree Street, when everybody was going crazy. So a day or two later, I was back down at Drew Field and the people were coming in

at random. As soon as you got there, they'd say, "Well, you want to stay in or you want to get out?" I said, "Well, I'll get out." I had enough points, they called it, from my missions and time and everything, so they sent me back to Atlanta and back to Springfield, Tennessee, by mid-August, or sometime in August I got back home. At least that just about covers all that. I was separated and all that.

Now, your next question is about, "Why did you choose MIT [Massachusetts Institute of Technology] for your education?" So forth and so forth.

You haven't interrupted me for any questions for clarification. [Laughter]

BERGEN: You're going along just great.

CHILTON: Okay. Well, it's a funny story about MIT. Coming from middle Tennessee, I hadn't even heard of Texas A&M. All I knew about was Georgia Tech and University of Tennessee. You're from that part of the country, too? I never heard of MIT, for sure, but I heard about it from [all] of these Yankee boys I got thrown in with in the service, and they were talking about this is the best school and all that sort of stuff. So when they passed the GI Bill, I think I was at Lockbourne in Columbus at the time, and so I just sent off a letter to MIT and they sent me a preliminary application blank. So I had a preliminary application to go to MIT on the GI Bill.

When I got home that summer in [19]'45, I wrote them and said, "I'm ready." They sent me a final application. Actually, I got applications from—I didn't just go there blindly. I said, well, I'll give Georgia Tech a chance and University of Cincinnati. Georgia Tech's letter came back, and on the application you had to say a lot of stuff like, "What do your parents think about this?" That insulted me. [Laughter] Georgia Tech, I'm not going to fool with them, I'm a mature war veteran. [Laughter] So I went off to MIT.

It turns out that at MIT, all during the war, instead of having the usual two full semesters and a short summer semester term, they had three full semesters to expedite training. So each fall, the fall term would begin a few weeks later. That year, in '45, the fall term didn't begin until November. So it was very timely for me. I got on another train. Airplanes weren't—commercial aviation wasn't too much—I couldn't afford it anyway. But, anyway, off to MIT I went.

My profs [professors] up there, you ask about that. "Were there any specific people or events?" Anyway, there were a lot of good profs up there, including mine. Among mine were [C.] Stark Draper and Bob [Robert C.] Seamans [Jr.], Dr. Draper and Dr. Seamans. Their special areas were in instrumentation and automatic control and stuff like that. Those are my main—well, actually I had some other good main mentors, but they're not well known. Well, one guy I really liked was Walt McKay [phonetic], and he was an instrumentation man, too. In fact, while I was there—during the war there was some guy who became very famous and very wealthy by inventing the ballpoint pen. I don't suppose y'all remember that. I forget his name, but he was a real colorful character. All during the war, these rumors kept coming back from the pilots that were flying the hump over India in the Himalayan Mountains, that there was a mountain over there taller than Mt.—what's the tallest mountain?

BERGEN: Everest.

CHILTON: Is it Everest?

BERGEN: I think so.

CHILTON: That's in our country, isn't it? Okay, anyway, taller than the tallest mountain. So this guy, this ballpoint-pen guy, got together a bunch of experts, including my professor, Walt McKay, who was going to do the actual measuring with barometric devices and so forth, to go over and test this out. Well, it turned out that before they got there, even, the whole thing fell apart. They became fairly well convinced it was a false rumor. My professor just left and came home, but the pen guy, actually, to make some more headlines, he took off from China or someplace without authority [without] clearance and everything and came home, more big headlines. Anyway, Professor McKay told us all these stories, and he was one of my favorite profs. He liked jazz, too. In fact, I learned to love jazz myself while I was up there. I'll mention that at the very end here.

Well, anyway, I really breezed through. MIT was not very hard for me, but at the beginning of my senior year, I was up there four and a half years total. They took me in. I'd had a year and a half of college, but I only got credit for about a half a year, but that allowed me to start off as a sophomore, which is where first you could declare a major. So I was an aeronautical engineering student from the beginning, but I was behind in everything technical.

Being in my senior year, between my junior and senior year I got this notification from Dr. Draper that he invited me to join…his "honors group." The honors group was maybe thirty guys, and when you joined it, you were with it for two years. You'd spend your senior year as a member of the honors group and a graduate year for a master's degree, and the summer in between, you had to work somewhere. So that was what I got into. The perk was that Dr. Draper was a real culinary artist, so once a month we'd go to some fancy restaurant from some ethnic background, and that was great fun. Of course, that's how I came to know Draper pretty well and admired him tremendously.

The deal was, then, that at the end of your graduate year, you got both degrees. You didn't get your bachelor's degree when you should have, you waited one year later and you

got two at once, bachelor's and master's. And that summer I worked in his instrumentation laboratory, didn't have to, but that was—

That summer I had—you talk about getting a fork in the road and you wonder which way to take. Well, that summer was when the Berlin airlift took place. In fact, I flew reserves there for a while at what's now [Hanscom] Air Force Base, I think, but it got to be too big a hassle. I had to hitchhike out there.

Anyway, I went to try to re-up. I thought as a B-17 pilot, I could be a co-pilot on DC-4 or 6, or whatever it was they were flying in those days, but I couldn't find anybody who knew what to do with me, so I never did it. But I always wondered what it would have been like to have flown in the Berlin airlift.

Anyway, upon graduation—let's see. Okay. That just about covers MIT, I think. Well, of course, you can see the contacts I had that led later to Apollo.

"How did you get your first job in NACA? What was my first job? What projects did you work on?" And so forth. Okay. Well, Charles [J.] Donlan was an MIT alumnus. Has he been interviewed? I guess he's still living.

BERGEN: I'm not sure if he's been interviewed. He's on our list, if he hasn't been interviewed.

CHILTON: When you hear the things I have to say about him, I hope you can get him. It's been about three years since I last saw him at one of the reunions.

Anyway, he came to MIT on a recruiting trip from NACA [National Advisory Committee for Aeronautics]-Langley [Research Center, Hampton, Virginia], and he recruited me. So I went down there and I had a choice of working at the Flight Research Division or the Pilotless Aircraft Research Division [PARD]. They gave me my choice. The Pilotless [Aircraft] Research Division, which was PARD, was headed by a division chief, Bob [Robert

R.] Gilruth, and their thrust was to obtain aerodynamic data, particularly transonic data that you couldn't get in a wind tunnel by rocketry. They'd put a model on a rocket and shoot it up in the air and you gather very high-speed data.

Again, my conservatism, I thought that sounded militaristic, so I loved airplanes, so I wanted to be with airplanes, so I went with the Flight Research Division. The chief there was Mel [Melvin N.] Gough, who later went down to the Cape [Canaveral, Florida], and Fred [Frederick J.] Bailey [Jr.]. I'm sure you didn't get to interview him, I think he died. He was assistant division chief. Both of them wound up in the space program, both at the Cape, although Bailey spent some time here, just a short while, but mostly his career was at the Cape. They assigned me there to the Stability and Control Branch. The branch chief was [W.] Hewitt Phillips. Have you ever heard of Hewitt Phillips?

BERGEN: I don't think I have.

CHILTON: He's, in my opinion, one of the smartest—he's a genius. His assistant was Charles [W.] Mathews, Chuck Mathews, branch chief and assistant branch chief. There were about twenty-five or so engineers in that branch in one big room. I'm going to mention the ones that later became space people. Harold [G.] Johnson. He probably died before you all started, too. Sig [Sigurd A.] Sjoberg had just returned from his stint out at Muroc, X-1 Program. Have you interviewed Sig?

BERGEN: Yes, we have.

CHILTON: I'd like to see his interview. Chris [Christopher C.] Kraft [Jr.]. Don [Donald C.] Cheatham, who died also too soon for y'all. Arty Assadorian, Walt Russell (who later came to work for me), Jack [John M.] Eggleston (came later), [B.] Porter Brown, and Helmut

Kuehnel (came later). So, about a third, at least, wound up later in the space program. Have you heard any of those names?

BERGEN: Yes, I have, some of them.

CHILTON: My work there was mostly—well, I had several tasks. My first one was working with Porter Brown. Chuck Mathews had done the first examination of putting powered controls in the B-29. That's where you don't have to exert all the force necessary to move the surfaces around. My job that I got put on with Porter Brown was to—at first, those boosters were partial boosters. They would design them so that the pilot would exert maybe one-tenth or one-fifth of the force required, and the hydraulic actuator would take the remainder of the load. But it was obvious that they were fairly complicated linkages, so obviously it would be better if the pilot didn't have to exert any force, but then he wouldn't have any feel. So they conceived the idea of an artificial feel system. So my first job was to develop the criterion for what should an artificial feel system be like and so forth.

BERGEN: Did your experiences as a pilot help you with that?

CHILTON: Not really. [Laughter] There was really no connection between being a pilot and being an aeronautical engineer, and although I flew in the reserves for a while up at MIT, while I was at MIT, I really had had enough of flying. I had done it. I mean, I'd always wanted to do it and I did it. After flying B-17s and all, I didn't think I wanted to fly Piper Cubs or Cessnas, and it was very expensive. So I didn't, really.

My next experience with flying was when I sent my son over here to—one of my sons, over here to a guy who runs a school over in Pasadena, I forget his name, to learn, to

get his pilot's license after he got out of high school. He took me for a ride and his grandmother for a ride.

Anyway, let's see. Then I also worked in statistical dynamics of random processes, gusts, we were interested in gusts, how you characterize them and what you had to do to design for them. Hewitt and Chris worked on a big project, too, called gust alleviation, and I aided somewhat in that myself.

And human pilot response analysis, could you write a mathematical equation for a human pilot, you know, and optimal remote control theory [investigations]. Actually, I was getting pretty desperate to think of spending the rest of my life doing like that, you know, so the space program was a god-send to me. Anyway, that's sort of what I did, and it really was no experience to prepare me for [space] flight.

You talk about spaceflight research with NASA. You said, "Who were the critical people you worked with?" Obviously, Hewitt and Chuck and all those guys.

"How did this experience prepare you for space flight research with NASA?" Well, here's my response to that. We weren't in space flight research. [Laughter] NASA didn't do space flight research; we did project management to develop and evaluate. My job was in development and evaluation of the systems.

I guess that came later, but, anyway, I'll throw it in now. The point was that NACA's closest experience to anything like that before was their part of the X-1 Program, where the Air Force bought all the hardware and ran the program, and NACA just did the engineering study to see what the results were. The guys that were out there from my group, like Sig Sjoberg and Porter Brown were both out there under Walt [Walter C.] Williams, and they didn't think too much of Chuck Yeager. He was a [poor] test pilot, but he had to fly for [the record]. When they wanted data, they would use the NACA test pilots, because they knew how to fly with precision and so forth to optimize the quality of the data that you got. So the

space program was the first time NACA was given the job of developing and procuring hardware. It was totally different. It was a completely totally different thing.

Sputnik. "What was your reaction to Sputnik? What was the general mood?" You mean changes in my work as the result of Sputnik. Okay, that's a good story. The reaction was that at Langley some of the people who were knowledgeable in the area of orbital mechanics and so forth started organizing little seminars, study courses, you might say, at night, to teach the rest of us all about orbital mechanics and so forth. I never had the faintest thing about orbital mechanics.

One night we were in some local elementary school for our classes, and…Sputnik was going to pass over, so we all went outdoors to see if we could spot it when it went over. Chuck Mathews asked me would I like to work on a project for man in space. Of course, I said, "Oh, yeah." So that's how I got involved.

Prior to this time, which I had no involvement, there were three studies carried out at Langley. You probably know about this, anyway. Max [Maxime A.] Faget headed up one study from PARD, and that was the zero lift entry, a vehicle that came [in] with zero lifting capability. Mathews, from my outfit, headed up a study, and Don Cheatham was the only one I knew who worked with him on it, which had moderate lift…during reentry, which is kind of like the Shuttle. It didn't look like Shuttle, but it had the same amount of lifting capability as the Shuttle. Maybe not quite as much.

Then there was another guy from [a] wind tunnel, I can't remember his name, but he had a study of a high lift, one that, once it came in, it could choose which country to land in, almost. Well, obviously it was Max's study who won out. It was the only one that was practical with the rocketry capabilities that we had. Then they decided to get more—Gilruth and Charlie Donlan and Charles [H.] Zimmerman were kind of the senior engineers involved in paving the way for this, and Chuck and Max, Mathews and Faget, were their senior technical people. So each of them sort of drew in a few people and we got ten people and

Chuck picked me and Johnny [John P.] Mayer from our division. Johnny was in the Loads Branch of the Flight Research Division. We were told to spend half our time over in the Unitary Plan Wind Tunnel and start writing a specification—it wasn't really a specification, but we called it that—that would be used toward procurement of the Mercury capsule.

Then as time went on, somewhere along there they changed us to NASA. No, no they didn't either. The next thing that happened was, the Space Task Group [STG] was created. By that time there were thirty of us, as some orders existing, these thirty people transferred to the Space Task Group reporting directly to headquarters, but still living at Langley. Then somewhat later, NASA was created and we were part of NASA, but at that time we were still part of NACA.

Then as the Space Task Group was created, you ask, "What was the general sentiment toward nascent space flight research?" I'd have to say, again, we weren't doing space flight research.

"What were the others [suggested it a] career move? How did you feel? Was I eager? What did you think was the future?" Well, as I say, nobody thought of it as nascent space flight research. It was just going to be a project. My reference earlier saying that I was approaching quiet desperation, when getting into project management was my salvation and I learned a lot about myself, which dictated how I finally wound up.

BERGEN: You said you worked on developing the specifications for the Mercury capsule.

CHILTON: I commented on that a little bit. It was just, as I say, there was ten of us and we started just drafting a document. I was supposed to work on the controls and displays. I think I cover that—yes. Yes, I touch on that a little later.

BERGEN: Okay.

CHILTON: Here's where I mention about the fact the X-1—so we really didn't have any time to think or worry about what other people thought about it as a career move. We were too busy, preoccupied with just getting this Mercury procurement thing going. But it's true that a lot of people at Langley sort of pooh-poohed the idea. A lot of it was envy, I think. [Laughter] In fact, some people who came with us for a while later just stayed on. They went back to Langley. I can think of two or three. Some really good people who decided it wasn't for them, they'd rather stay there.

By this time, then, as we prepared for the Mercury procurement, our organization was Gilruth and Donlan were the big boss and the assistant. Charles Zimmerman—have you heard of him?

BERGEN: Yes, I have.

CHILTON: He was in charge of contracts. Do you know his role in the Flying Pancake when he was working at Vought [Chance-Vought Corporation] for a while? No? Is he still living?

BERGEN: I'm not sure if he is or not.

CHILTON: I wonder if he got interviewed. Anyway, he was really a great engineer, but he was ready for a career move, too, so his interest was in running the business side of the project. Mathews was put in charge of operations. I remember Chris telling me when Chuck asked him if he wanted to be one of this first thirty, Chris said he told Mathews he would like to do it, but he didn't want to stay in engineering, he wanted to be in operations, which sort of surprised me at the time. And Faget was in charge of engineering. So we just had the three elements: contracts, operations and engineering.

When we had the proposals come in, we had briefings and everything. There were a couple of guys from Langley's Instrument Research Division who were a big help to me. One was the son of the director, [Henry J. E.] Reid. Dr. Reid was the head of Langley at the time, and his son [Jack] was an electrical engineer, and he had a buddy [Doug Garner] who used to drive around in an English MG. Anyway, they were a big help to me.

My team of the evaluation, to evaluate proposals we broke out into evaluation teams. My area, I was in charge of the controls and displays. By that time I think I had a couple of people working—had been told to work with me, and that was Jack Funk and Fred Pierce. I think they were on my team. Hewitt Phillips from Langley was one of them. Bill Alford, who was a test pilot, a Langley test pilot, I remember, was on my evaluation team. Bill was later killed in England flying a Harrier. You know, the Harrier is a plane that goes up and down. He was over in England when it was just beginning, and was killed in a crash.

That brings us up to—by this time, of course, we were NASA and we picked McDonnell [Aircraft Corporation]. [Actually, we rated the proposals 1, 2, 3, etc., and NASA Headquarters selected the winner based upon whatever considerations they wished with our technical and management considerations. They could include local economic conditions, for example.] Mr. "Mac" [James S. McDonnell, Jr.—President of McDonnell Aircraft Corporation] was an MIT graduate, too. [Laughter] That was a really great company in St. Louis [Missouri] and they did a bang-up job on that Mercury.

Then you ask about the AVRO bunch. Of course, the CF-105 [Arrow], was it? The Arrow was canceled. There's a real good article in the Smithsonian *Space* magazine a few months back about that whole deal. So Charlie Donlan, with the approval of the State Department, went up there to see what we could recruit, and they went to big Jim [James A.] Chamberlin and he picked, I think, [30] people, and they all came down. Let's see. They sent [to] me Tom [Thomas V.] Chambers, Dick [Richard R.] Carley, Morris [V.] Jenkins, and one other guy whose name I forget, mostly because he decided when we came to Texas to

stay, to transfer to Langley. I can't remember his name. I remember one of them was John [N.] Shoosmith [phonetic] and he was assigned to Johnny Mayer, who at that time still worked for Faget. I think John Shoosmith went back to Langley eventually.

BERGEN: He may have.

CHILTON: There weren't any cultural issues. [Laughter] They were all good men. You say "Any cultural issues?" And their approach. As I say, they were all good men. Their technical approach was no different than our own.

They were a mixture of Canadian nationals and English immigrants. Morris and Tom were from England, and Carley was a Canadian from way [out] in west Canada.

CHILTON: "You mentioned in an earlier interview that you helped develop the autopilot specification for Mercury before STG officially formed. How did you approach the development of the control system and what were the key obstacles? Can you explain the evolution of the rate-damping control system?"

Okay. As I said in the spec [specification], this was [the controls and] display part, and the only really new thing that we had to face up to was how were you going to have control-power. So we had to define a reaction control system, and actually the thrusters. Chuck had really done all the early inquiries before we got started and had pretty well narrowed down that we would use hydrogen peroxide on some kind of a [catalyst (platinum)] screen bed. When hydrogen peroxide comes in contact with this metal, it evolves into hot steam, and you put it in a nozzle and it squirts out.

Most of the early constraints were then just on weight, how to have a light capsule. I suggested facetiously that what we ought to do is look for legless astronauts, because the legs constitute about 40 percent or more of the weight of the body. Nobody bought that idea.

Okay. So that led us to guys like Phillips and myself. He was really one of my mentors. We kind of came to the conclusion that this capsule, of course, the Russians had already done some work, and we thought the capsule had to be really austere, and we weren't too savvy—being control people, we weren't too savvy about structures and things. You always worry more about the things you don't understand. So we put portholes so they could look out and look down, and a periscope to look down with some things that you could use for maintaining a reference, an attitude.

We had rate gyros that drove needles like this. If the pilot had to take over, which we didn't think he would, but he would just simply use his controls to zero, null these needles and help him maintain [attitude], and looking through the periscope just like centering the bubble. That was our approach to what the pilot would do if the autopilot didn't work.

The autopilot was unique for this kind of application and it had to be what we called a bang-bang system, which is to say these jets either had to be on or off, they couldn't control them. So that was sort of a whole new—there was a lot of background in the control theory about bang-bang systems. Thermostats, for example, you turn the heat on or you turn it off. So there was a lot of ancient and traditional theory, control theory, with that kind of system. So it was mechanized. We didn't have any idea how to mechanize it, but Honeywell got the subcontract, and the approach was to use a couple of free gyros, [two-degree] freedom gyros with wipers on them, so that when they exceeded certain limits, the power would go [to] the jet, [which] would thrust or turn off or [on], depending on whether you were in the right attitude as dictated by these [gyros]. Then these rate gyros, I think, fed into it also, in addition to driving [the needles]. I'm sure they did [to provide damping].

[There was one never-before-employed concept in the Mercury autopilot. Free gyros drift because of friction in the bearings. So periodically a free gyro must be erected to the vertical. The two two-degree-of-freedom gyros maintained a pitch, roll, and yaw reference but had to be corrected for drift. The pitch and roll true reference was obtained by a horizon

scanner, a state-of-the-art device used by the military. The horizon was scanned in the infrared spectrum, detecting the difference between the cold space and relatively warm Earth, and the pitch and roll gyros were slaved to the output of the horizon scanner. (Horizon scanners were not dynamically capable of being used without gyros.) The problem was how to maintain a yaw reference. In theory, because of the orbital pitch rate, we could slave the roll gimbal to the directional gyro (yaw reference) to the roll output of the vertical gyro to keep it aligned with the local horizontal, then compare the yaw output of the directional gyro with the error signal in the roll slowing loop to keep the yaw gyro aligned in the orbital plane. We tried to verify this in the laboratory by using gyros and scanners on an air-bearing table, but we were not able to get the table friction low enough to be certain. It worked in orbit, though.]

So the autopilot was a good control system, but, of course, they wanted a means, if it failed, for the astronaut to also turn these jets on and off. As I said, our approach was, you look at the bubble and you look at the needles and turn them on and off. So that was a very spartan approach, and we thought it was the appropriate thing for such a crucial, critical lightweight system.

BERGEN: Did you interact much with astronauts?

CHILTON: We didn't have any astronauts. This was before they were picked. [Laughter] As you will see later what effect that had.

You asked about how the rate control system evolved, evolution of the rate damping control system. Well, what happened, see, I and most of my peers thought we were racing the Russians to try to get a man [in space] first, but Bob Gilruth always said, "Oh, we're not racing." His main thing was, we've got to have safety for the astronauts. And we would have beat them, except for this pig. [Laughter] What happened was, they had a—of course, I

failed to mention that in the Mercury, one of their really technical—there were two really technical problems to solve. One was the reentry heating, and the other was how to keep the astronauts alive when that big sudden stop as it was coming into the atmosphere, the high Gs. So they had the couch business and the…[heat protection] on the outside. [It was called ablative material.]

So the effectiveness of the couch was one of the things. The critical design, it was decided was, suppose you aborted the capsule on the beach and you went up in the air and came down on parachutes and swinging back and forth and landed on the beach, kaplunk. That was where the maximum critical effect on the astronaut would occur. So they put a pig in one and did it, and it broke his back. It broke the pig's back. So we had to do a whole redesign. That set us back. Obviously, we knew it would be a big setback, but it was considered essential to not break an astronaut's back. So that opened the window, the door, for a lot of improvements that people were thinking [about].

By this time we had astronauts, so they, of course, didn't like our—in our system, the astronaut was just along for the ride, and if he had to, he had a means where he really could save himself. But they wanted to fly the thing. So the first thing went were our portholes and we got a nice, big windscreen with reference lines on it. Of course, we couldn't argue that. Obviously, if you wanted to fly it, that was a good thing. Gilruth, he was very concerned about the pilot being able to do his job, so I think he may have asked us if we thought this would do it.

Of course, it was Dick Carley, really, who said we ought to put a rate damping system in the manual control. There was already one in the autopilot. You couldn't argue that either, if you're going to do all this. But he really gets the credit for bringing up the subject. I still was sort of slow to rock the boat. I like to stick with—once you do something, I like to stick with it.

So then the pilot would have a good control mechanism and he had the window to line up a line on the window with the horizon, and he could get the right attitude. There were two critical attitudes, one for [firing] a retrorocket and the other for reentering. So there couldn't be any argument about the desirability and the feasibility of it. That's on that subject.

Your next question had to do with STG New Project Panel. I think I later understood what that meant.

Your next question had to do with our branch had comparatively little work on Gemini, and what was my opinion of the system developed under Carley and so forth. Would I have done anything differently? Somewhere along in there, Chuck wanted Johnny Mayer's group. His group worked for Max in orbital mechanics. And Chuck felt like he ought to have that capability in his operations branch for the studying of missions and so forth. I'm sure it was justified. It had to be. Anyway, once that happened, Max told me to develop an orbital mechanics capability in my branch. So I built that around Jack Funk. At that time I think I had about two sections. Jack Funk was in charge of orbital mechanics and [Dick Carley], controls and displays, everything else. [Tom Chambers] worked for [Carley].

Now, this new Projects Panel, at first I didn't understand, but later on I'll talk about in the context of pre-Apollo stuff. That business about my branch not having much to do with Gemini, I've got to place this in the right context. Gemini was conceived after Apollo was under way. In fact, the guys at MIT and I, myself, were dismayed to think that they were going to do this. I'll elaborate on that, I guess, in a minute.

The ground rules established—see, the idea was to—well, I'll go ahead now. The justification for the Gemini Program was that they needed to get rendezvous experience under their belts, mostly the operations folks, as opposed to the astronauts that much. But the real reason was that it was going to be this long before Apollo would fly, and they were afraid that the public would forget, with no space flights, there wouldn't be any—they'd lose

interest. We wouldn't have a space program. That was the real reason. But the down side of that, and I really thought it was a wrong move, because they're terribly expensive, and really more so [it] diverted attention, management attention, from Apollo, and resources, but mostly attention from Apollo to Gemini.

I started to give you the ground rules. In order to make it go fast, the idea was that you could really get a Gemini flight in the air quick, and have this continuity, this continuum of space flights. So the ground rules were that the program office would run the whole thing. See, by this time we had evolved a system of program office with engineering support. The program office didn't have any—very little, if any, engineering personnel. So this was that they would have complete autonomy, the program office, and that the engineering support would only occur during post-flight data analysis.

So it happened about this time that Dick Carley and myself had developed a great antipathy toward one another, and the solution to this was to transfer Dick into the program office. He, then, was in charge of overlooking McDonnell's effort in the guidance and control area.

McDonnell—my counterpart during Mercury was a guy named Joe [J. W.] Twombly. He was an electrical engineer also from MIT. [Laughter] In fact, I ran into him—actually, I called him up and we visited in St. Louis a couple of years ago. He later became chief engineer at McDonnell. He had just lost his wife and he had about five children. We had a nice reunion. I go up to St. Louis, have been for the last several years, to a jazz party every March. I called him up and he came over to my hotel and we had a long chat. Really great guy.

They had already pretty well established—their idea was it was just going to be a two-seated Mercury. The big difference, of course, was that it had two seats, had an offset CG [center of gravity], so it could develop a little lift and therefore have some latitude as to where you wanted to come down. You could actually come closer to the carrier, whatever

you wanted to do. Those were the main two differences in Mercury, I think. Well, no, there was another one I shall mention later, much more serious.

To do this latter part you had to have inertial guidance system, and so control-wise they kept Honeywell as a subcontractor for the controls. I believe they used an IBM inertial platform adapted from the Titan missile program. I don't know what development they had to do it to change it for this different mission. So they had pretty well evolved the configuration. Of course, Dick, I'm sure he did a good job, but I'm sure he didn't define it. I mean, Joe Twombly probably defined it.

You say I said—I don't remember saying this, but Gemini GNC [Guidance, Navigation, and Control] learned little if anything from Mercury experience. Well, I guess that's right. It was a totally new experience, so it had to be a totally different system.

"What should they have learned from Mercury?" Well, the thing I think they did learn was not to have 100 percent oxygen atmosphere. The Gemini flew with an atmosphere more like Earth, with nitrogen and oxygen in certain percentages, whereas Apollo was coming along following the Mercury pattern with 100 percent oxygen. My personal feeling is that if we had not had a Gemini Program, there would not have been an Apollo fire, because top management was paying a lot of attention to Gemini, and [I'm sure they] had a lot to do with this change in the atmosphere, whereas Apollo was coming along, following Mercury. Had they been following Apollo like they were following Gemini, I think Apollo would not have had the 100 percent oxygen and there probably would not have been a fire. Or if there had been a fire, it would have been extinguished quickly.

I didn't approve of the Korean War or the Vietnamese War, and I didn't approve of the Gemini Program. [Laughter] I thought that whatever they needed to do in terms of helping rendezvous techniques could have been done when the command service module came on line with an orbital development phase, which developed rendezvous stuff. And I still do think that.

These advanced studies. This is what I call pre-Apollo. "What was involved with the initial guidance [and] control studies for Apollo? Compare with Mercury how far the technology developed and then, later, the selection of MIT." Okay. Jack Funk's section in my branch had by then acquired quite a few people. I mean, six or eight, maybe, one of whom was Don Jezewski. I don't guess he's on your list. Your list is probably more supervisor-type folks. There was another guy, I haven't been able to think of his name [Tom Gibson]. I've been wracking my brain trying to think of his name. He was a young engineer right out Georgia Tech. Don Jezewski, I think, too, had just got a physics master's degree from somewhere. But this guy had come from Georgia Tech and he had grown up in the canal zone and he was very brash, pretty rough around the edges. He was very unpopular in the group, because he smoked a cigar, and Fred Pierce I [know] particularly didn't like that. Anyway, those two, Don Jezewski and [Tom Gibson], were really smart kids, and very well versed in this orbital mechanics thing.

So, Jack Funk's outfit, see, landing on the Moon, we weren't even thinking about that. Now that I think of it, those early studies, what you call, was a group of us, come to think of it, headed by Bob [Robert O.] Piland and [H.] Kurt Strass. He's dead, too. [Myself and]I forget who else, but there were six or eight of us that [brain-stormed and] made trips and talked to other NASA Centers and so forth about what could constitute a lunar program.

Our thought was that the least we should do is go up and circle the Moon and then come back. We didn't think we had the capability, the payload capabilities, to do anything more than that. So Funk's group were developing techniques for analyzing what we called cis-lunar flight analysis, developing trajectories that would allow us to do what I had just said, to take us from Earth orbit to lunar orbit and back, and to define what we came to call the entry corridor, because coming back from the Moon, you're going real, real fast and if you come in too shallow, you skip out and you're lost. If you come in too steep, you go down and you break apart. So the difference between was the top and bottom, was the entry

corridor. I forget, about eight or ten miles or twenty miles, that you had to hit to successfully complete your mission. So we were defining the corridor and developing analytical techniques.

They heard of a concept called matched conics. Conic is a trajectory, it's a broader term for like parabolas and so forth. A former NACA guy that was a good friend of mine, who had gone up to some research place in Wisconsin, he had started to think of this matched conics idea. So we gave him a little study contract, Jack did. Working with him, they developed a very effective system of going from an Earth orbit to a lunar orbit by this matched conic technique, and were doing some really good work.

In fact, in later times when Johnny Mayers' outfit began to have time to think about, they were all tied up in Mercury and, I guess, Gemini, and when they got time to think about the Moon, they were very impressed themselves. They [wanted and] got, of course…these analytical techniques for themselves, too.

Comparing the early Apollo and early Mercury stuff, I'd say that in Apollo, the initial studies were mostly in orbital mechanics, whereas in Mercury it was configuration and control, and technology was not an issue. As I mentioned earlier, in Mercury the big technology drivers were the high G entry for the seats and the entry heating. In Apollo, the big technology driver was the even higher, greater heating, because…[of the much higher reentry veolcity] and the precise guidance and control to hit that corridor and to do all the other stuff to make sure these trajectories did what they were supposed to do. As it turned out, the Polaris, that was a submarine-launched missile, it had an inertial guidance system and it turned out to be the best current technology that would be available to us, but even it would be limited.

In Apollo, we needed a lot of device improvements, solid state electronics had to be more widely used because of the long mission. One of the biggest, toughest problems was we had to put the astronaut in the loop. The astronaut had to have something to be able to

help out, or to do things himself if he had to. And to integrate a pilot into these early concepts, like missile guidance, which was the basis we had to be working on, was a real challenge.

BERGEN: Why was that challenging?

CHILTON: Well, because you do things totally different. In a missile system, everything is automatic and you design devices that can acquire a star and track it, whereas the astronaut, we developed a unique telescopic system so the astronaut could zero in on a star and do whatever it took to initialize the computer and the guidance system and align the inertial platform. So it was a real challenge. [And] of course, very high-powered mathematics involved in the computer. They actually went to a lot of pains to try and develop a little hand-held computational [technique], so they could calculate this thing. They were never used. I don't think they ever thought they had a viable system. So the technology that we needed was different from Mercury and it had been developed primarily through the missile business, military mission.

"The selection of MIT was a key decision. What were the alternatives? How did they first come in the picture? Did your previous connections have a role?" Okay, we'll talk about that. [Laughter] Of course, I had worked at the instrumentation lab one summer and it turned out that MIT instrumentation laboratory had played a role with the Navy in the development of Polaris guidance system, a very intimate role, as a matter of fact. As a perk, or a [by-product] to that, they were given a certain amount of funding from the Navy to do advanced study research. So they were actively involved in what I call here as advanced studies of interplanetary probes. Even that stuff they had studied were all unmanned, how to send something to the Moon or to Mars, all unmanned stuff.

So they were involved in that sort of activity. I didn't know anything about it, but Charlie Donlan told me to go up there and meet those guys and have a meeting and see how we could direct their studies towards a manned lunar flight, to establish contact and design an Apollo-directed mission or Apollo-directed studies by that same group. Except for Draper, I didn't know any of the people involved. Even when I was there, the group I was with was different from this group, which I'm sure was already operating. And they were very impressive people—Dick [Richard E.] Battin, Milt [Milton B.] Trageser, Ralph [R.] Ragan, Davey [David G.] Hoag [phonetic]. They were really fantastic.

The role they had played in the Polaris guidance system was totally new to me. The Navy had given them the system responsibility for the guidance system, not the hardware procurement, but the studies. Then they let contracts. The Navy let contracts to companies to build platforms and computers and so forth [to MIT specifications], but MIT maintained the overall system management, putting them together, as separate from the contractor who built the missile itself. So it was MIT mostly who interfaced with the missile contractor and integrated the guidance system into the missile system.

At the time we were talking, in fact, one of their guys was on a Polaris-bearing submarine, cruising under the Arctic ice pack. See, what they needed to do was have a guidance system on the Polaris missile so that as it was stored in the submarine, wherever they went, they'd know where they are and how to reach the target. When they got to the point where they could shoot off the missile, it would know where to go. That was obviously, a very close, happy marriage for us. My only previous relation to the lab is, as I said, I had worked in the lab, but in a different outfit.

Then you say, okay, people didn't like that, some people, and that's true. "What were the objections? Why do you think there were objections from within NASA, as well?" Actually, MIT did a really terrific job, but the industry obviously didn't like the idea, because they had probably seen that they had been dealt out of a lot of business in the Polaris

program. This is the inertial guidance industry that we're talking about. The missile group, too, because they wanted to, of course, have the integration responsibility and the system engineering and everything. They wanted everything, whereas on the Polaris, the Navy had gone to one company for gyros, another company for computers, another company for platforms. MIT made sure they all worked together, which was a strange idea.

Joe—who's the guy who went to NASA headquarters and later came down here and headed up the Apollo Program for a while? One of the Apollo Program managers.

BERGEN: Joe [Joseph F.] Shea?

CHILTON: Joe Shea. Joe Shea was an MIT man, too. [Laughter] He worked for the Air Force, and he didn't like the Polaris concept at all, but he didn't want Dr. Draper to know that. [Laughter]

Anyway, so it was obvious why the industry would object. From within NASA, it was just a philosophical difference of opinion. Philosophically, what's the best way to manage a program? You have to admit that our approach was not the best from many standpoints. It was only justified because of the high technical risk of a successful program, and MIT had done it, so the time came eventually when Gilruth told me to go ahead.

I was quizzed a lot, because a lot of people later wanted to accuse Bob Seamans of doing a lot of stuff. I never talked [to Bob Seamans] about it. Seems by this time he was working for NASA headquarters. I don't know what really happened. All I know is that Bob Gilruth said, "Go ahead," and I started writing sole-source justifications and stuff like that, and MIT got this overall job as an extension—part of the justification was, of course, because they had already been working on these studies, and we just expanded the contract, but without any competition. So it was a natural controversy.

In time, much later time, some of these concepts were forced into the program. Another Apollo manager later made—what was that guy from RCA went up to NASA headquarters [D. Brainerd Holmes]? Anyway, eventually we sat down with Doc Draper and the Doc agreed there would come a time when all of the inertial hardware, the gyros and accelerometers, would be turned over to AC [Spark Plug Division (General Motors Corporation)], who had the platform and they would do the whole package. I guess that's as far as we ever went. North American was never given the overall responsibility, but one guidance contractor, AC Spark—AC division of General Motors, did eventually fold in Raytheon (Company), who did the computer, and whoever did the accelerometers and gyros [Sperry Corp.]. That was sort of a stretch and we couldn't argue too much. In fact, Bob Gardiner by that time was our major interface person, and he felt that was good, too. And it was probably right, by that time, you might say. It was too much [for] MIT by that time.

Anyway, let's see. Okay. It was because it was such a high-risk technical program that, I think, justified us going to MIT. I think the ends proved that it was, because there's nothing like that Apollo guidance system ever, before or since, especially the computer. They really had some really good guys up there.

On the other side, from NASA's standpoint, they didn't work that great in terms of pushing on the gyro, because they kept wanting to improve things. We'd say, "Oh, no, it's good enough now." Because they were very concerned about specifications, about it being good enough to do this long-term mission. That was a big challenge. So we had to improve the gyros and accelerometers and the specifications. We had to sort of make them stop.

Then I think you next ask us about going to Houston and the space technology. Let's see. We moved to Houston. What prompted you to move to assistant chief of Spacecraft Technology Division [STD]? I'm going to combine all this.

BERGEN: Okay.

CHILTON: Was I eager to come to Houston? Were there any personal issues? Did I consider staying at Goddard [Space Flight Center—Greenbelt, Maryland] or Langley? We never really got assigned to Goddard. Well, we did, too, [on paper] but Gilruth didn't want that. He didn't want to work for Harry [J.] Goett, had been moved from Ames [Research Center—Mountain View, California] to Goddard. He was really great guy, though, Harry Goett. Gilruth didn't want to work for anybody, I guess.

Did I consider going to private industry? Then what prompted me [to enter Spacecraft] Technology Division? Okay. Well, somewhere along in there, they brought Walt Williams in from whatever it was called, [Edwards AFB (Dryden)]. He moved in down at the Cape, and that resulted in Chuck Mathews sort of getting shoved aside. So there was a big reorganization and Chuck was made division chief of the Spacecraft Technology Division reporting to Max Faget, which he'd never done before. Chuck picked two assistants, Ralph [S.] Sawyer and myself, to overview certain ensembles of branches. I think I had [four] branches by then, so I was assistant chief of STD with the Mission Branch, I guess you'd call it. I don't remember what we called it, headed by Jack Funk. He was branch chief. The Control Branch with Tom Chambers and a Simulation Branch. [Also, an Aerodynamics Branch with Bruce Jackson.]

See, back at Langley I had been very active in analog computer simulations, [of] which there were real-time and fast-time. Fast-time, you just program it in on a computer and push buttons and it would go cchh-cchh-cchh, and you'd solve a problem. Real-time meant that things happened just like in real time, and those were used in man-in-the-loop simulations. That's [how] we justified all the Mercury stuff. We'd put a thing up and put a pilot in and move and tie in the analog computer and we'd say, "Oh, yes, he can do it."

By this time, of course, with Apollo we had a lot of that. So I had a whole branch to develop, a Simulation Branch, we called it, which meant we bought millions of dollars worth

of analog computer equipment and developed, with GE [General Electric Company] and some others, all these sophisticated—what do they call it nowadays? Reality—you know, when you think you're in the real world. What's that word that they use in computers nowadays?

TARRANT: Virtual reality.

CHILTON: We sort of invented it. In fact, Fred Pierce had a lot to do with some of the concepts in virtual image creation. You generate a scene with a computer and you put it onto a mirror, focused to infinity, and you really thought you were there. All that we developed. Of course, it was later, the same sort of thing would show up later on in crew ops, flight crew operations. They had their own training simulators. Ours we used for development. [Laughter] So I had those [four] branches, I think. Then we hired Jim Van Artsdalen from Boeing in Wichita to head up that branch. Is he on your list by any chance?

BERGEN: I don't think so.

CHILTON: He's very ill. He's moved over to near Houston now. No, near where Blinn College is. What's that town?

TARRANT: Brenham [Texas].

CHILTON: Yes, he lives over near Brenham.

Anyway, I had these three branch chiefs, Jack Funk, Tom Chambers [Bruce Jackson,] and Jim Van Artsdalen in the old Rich Building. Ralph Sawyer and I had private offices downstairs with a connecting bathroom. That was my most prestigious position. [Laughter]

I shared one bathroom with Ralph Sawyer. I think I never had a perk like that before or since. [Laughter]

So, let's see. In answer to your question, yes, my job became much more management as opposed to my personal technical input, although I could always challenge my guys. It got to where they could do a lot more things than I could do, but I could always say, "That's not right," or something like that.

Then you asked a very key question, which said—oh, I didn't answer the question about coming to Houston, because I didn't—I came down a week after Hurricane Carla, to select these temporary buildings. I picked the Rich Building myself. I wasn't too impressed with the scene over at Texas City where there were bedsteads and mattresses were all out on the [street] and all this flat terrain and rattlesnakes running around the place chased out by the floods.

In fact, my assistant branch chief at that time, he didn't become an assistant chief until we got in the Rich Building. But my assistant branch chief was a guy—what was his name [Alan B. Kehlet]? …[An aerodynamicist who had worked for Faget in PARD] and we two came down together and he decided not to come. He stayed at Langley and later went to work for North American [Aviation, Inc.]…

Anyway, so I started exploring some alternatives to coming to Houston. I talked to Charlie Donlan, who had already elected to stay at Langley. He was like assistant director or something by that time. He offered me a job as an assistant branch chief to a loads guy whom I didn't really care that much for, so I figured I had no alternative. Plus, by that time, I had twenty, thirty people working for me and they were really great people and I didn't really want to leave them. I didn't want to leave that leadership role.

By then I had decided not only had I been a good project manager, because that's what I was in Mercury to start with, I later turned it over to [Carley and] Chambers… But I had decided I also had leadership skills. [Laughter]

BERGEN: If it's okay we could take a short break, so we can change out our tape?

CHILTON: Sure.

CHILTON: You've asked me to describe my move to Houston. Were there any personal issues? Did I consider staying at Goddard or Langley or to private industry? I have already described why I decided not to stay at Langley. We didn't even have an option of staying at Goddard, because we were never there. I didn't even consider going to private industry, because back in about [19]'54 or '55, I had my first son, and I perceived that it was going to be quite a while—I was working on a project that I didn't like, for Chuck. I had an offer from Honeywell in Minneapolis [Minnesota]. I was making 7,000 [dollars] a year as a GS-12. Honeywell offered me 10,000 [dollars] to come to Minneapolis, so off I went. That's a big raise from 7,000 to 10,000.

Hugo Schuck was the head of their research department, and they were really fat, because they were building the autopilot for the Saber Jet, F-86, and they were given a lot of money just like this Polaris, extra money they could spend any way they wanted to further the state of the art. But just as I got there, the Air Force decided they didn't want to do business that way anymore. From now on, they were going to make Honeywell bid, propose a study, and then the Air Force would decide if they wanted them to do it.

So I hardly got there before I was put on the proposal preparation job, writing proposals. It was called a Wagtail missile. The idea was that an airplane would fly [low] over a target with an atomic-powered thing and the missile would launch up. The plane would go on safely and the missile would sort of do like this and then blow up the target. I think they won the contract finally, but I'd had enough of that.

I got a letter from Chris Kraft. By this time Hewitt had developed—organized his branch into sections. Chris had one of those sections. I got a letter from Chris saying—Congress had passed a law, because there were a shortage of technical people, so that everybody in the Civil Service were going to be pushed to the top of the grade, if you had a certain background. So all the guys at Langley were going to be pushed to the top of the grade. So Chris wrote me a letter said, how would I like to come back at 10,000? Same pay as at Honeywell.

Well, we got to Honeywell in the summer and we left in the spring, because it was terribly cold there. My wife got pregnant again. We never really got a good location there. We rented. So I had a choice of either coming back to Langley, working for Hewitt again, or I could have gone—Honeywell was just building up a new facility down in St. Petersburg. I could have gone with them, but I decided that I would rather be the customer, the government, than the guy trying to sell to the government in industry.

When I got back, Hewitt kept me as a—I was reporting directly to him, I wasn't put in [a] section, just in time for this space business. So that was … fortunate—but I never considered going back to industry. I just didn't like it. I wasn't suited for it.

And then you ask—let's see. There weren't any problems, except I sent most of my people down in the fall of [19]'62 or '63, whatever it was, '61. I was the next to the last person in my group to come, because of selling a house. The last person was Jim Lawrence. Is he on your list, by any chance?

BERGEN: I don't think so.

CHILTON: He later ran that Simulation Branch. His wife was expecting, so we let him stay until she delivered. But he was the last one. I came on ahead to the Rich Building. While I was gone, a big northeast storm blew into—were [we] were living in Seaford, on the water.

A big northeast storm came in, and my brother-in-law had to come and carry my kids out on his shoulders to wade through the water and to the car out in the street, because the house was about to be floating away. Anyway, that was the biggest challenge.

Okay. "How did this promotion to Spacecraft Technology Division and with the Guidance Control Division, which was later a spinoff, affect your job responsibilities? Did you find yourself doing more management than engineering? How did these new forces affect your prospective on Apollo?" The answer to that is that we really—of course it was more management, but it was a different kind of challenge. Our big job then was to develop a—Gilruth said, "We're going to build a facility like Langley." And there I was, top of the heap, and all these great people working for me, and we started writing up what the—Chuck still is the chief of Spacecraft Technology Division. He was going to have a whole building. I told what part of the building was going to be like, and—what's that loads guy's name [Joseph N. Kotanchik]? Anyway, the other people…outside [of] guidance control, the power and propulsion. I guess power and propulsion and [we] were the sole occupants of the building. Building [16], it later became.

So that was a big challenge, writing up this stuff to go to Congress, what the building was going to be. In fact, Chuck, I thought, sort of shortchanged us, and a year or two later I also went on my own, put a big annex onto Building 16, which is the big part behind there with all the guidance labs and big high bays. We had a high bay. Anyway, that was all big stuff. The big thrust, to me, now my job was to [develop] an institution and to protect that institution and the people in it. I had people go out to do that hands-on stuff.

"During Mercury and Gemini Projects, did you have any direct involvement with mission operations?" If not, what was I doing and were any missions too taxing? Let's see. We supported the missions. I would always have people in different technical areas sitting in the back room of Mission Control, along with the mission people. They [sat] side by side with them my people, somebody on platform, somebody on computer, somebody on this,

that, and the other to act as consultants or to put a word in. Like during the Apollo 13 business, I don't know what they did, but they helped form opinions and contribute to decisions. Decisions were made by the operations people, but they based it on what they thought and what they were told by [others] and whether or not they believed them.

Then after the flight, we did the post-flight analysis. We'd have a crew to write the report on how the guidance and control and stuff performed. So that was our involvement. I don't even remember where I was when—actually, we were still in the Rich Building when John [H.] Glenn [Jr.] flew. In fact, I think I was meeting some MIT guys when John Glenn flew. I was meeting with some [MIT] guys in the Rich Building. Actually, we had gone to lunch when we heard about Jack [President John F.] Kennedy getting killed.

I remember one of the smartest things that NASA did, and I guess it's attributable to George [M.] Low, was we did, as our first mission, what we had thought was all we could do a long time ago, was to just go up and circle the Moon. I forget which mission that was called, but that was a brilliant stroke.

BERGEN: Apollo 8.

CHILTON: Apollo 8. I think I failed to mention that, as I said, during our advanced studies we never thought about landing on the Moon, we just thought about going out and circling the Moon. But when Kennedy said, "We're going to go to the Moon in this decade and come back," we were delighted, Max and all his people. My impression is that Bob Gilruth thought that was a big mistake. He didn't see how we could possible do that. Besides, he always favored space stations. The Moon wasn't his bag.

Anyway, in all of the missions, all of them within my organization, we just helped back up the mission and did or wrote the technical parts of the assessment of how the system performed.

"What was your involvement in Skylab?"

BERGEN: Before we go on to that, I have a couple of things that I don't think I put on your sheet. The decision on how exactly we go to the Moon and lunar orbit rendezvous, how did that affect you in guidance and control?

CHILTON: I'm glad you mentioned that. I had thought about answering that question, but I guess I never got around to making notes on it, because it wasn't really put out here.

Well, Gilruth, his big thrust, mainly, after the decision was made was, how are we going to do it? I guess our first thought was that the—in fact, the command service module contract was given to North America or Rockwell, whatever it was called by then. [I told you how Headquarters made the selection. My colleagues and I were surprised that Martin did not win because they would build a factory in the Clear Lake areas and build the CSM here.] In fact, the service module, I don't know if Max ever mentioned this or not, but the service module thruster, which was, I think, 20,000 pounds of thrust, … was sized for taking off from the Moon, and way overpowered for what it ended up doing. In fact, it was almost a disadvantage, it had so much thrust. It was designed, and was finished, because it was too far gone to change, to be the takeoff agent from the Moon's surface.

We hadn't figured out how to get it down there. That's where we were going to take off from. Of course, I guess, we assumed that we would orbit the Earth, and that it would do, probably Earth Orbit Rendezvous [EOR]. No, I guess we hadn't even thought about that. We just thought that the men would only occupy the command service module from beginning to end. Of course, that's what Rockwell wanted, too.

Then there came the big debate. Who was that guy from Langley? Houbolt. John [C.] Houbolt, he says, "Ought to rendezvous around the Moon." And Jack Funk's people started doing all these studies. I guess it looked to them the same way, but Rockwell, boy,

they mounted a terrific effort to disprove this idea, that earth orbit rendezvous was the way to go. Every week, just about, Jack Funk would get a call from somebody at headquarters, "Rockwell says this." And then Jack would have to [get busy and answer], "No, it's this way." [Laughter]

In fact, there came a time when Johnny Mayer started getting into my knickers. He was going to build his own simulation branch. So I had a meeting with him. This is after Cliff [Robert C.] Duncan came to work, and I was a deputy chief of the Guidance and Control Division. I sat down with Johnny and made a deal with him, I said, "I'll give you Jack Funk's branch if you'll stay out of simulation." [Laughter] So we shipped Jack Funk over. This is, of course, later than the time I'm talking about now. So that's how Jack Funk and Don Jezewski, and all these guys ended up working for Johnny Mayer, which was a good move for them, and I got out of the business altogether.

When Jack Funk left, I had a big going-away party. I didn't draw it, but I had somebody draw it, but I designed it, a cartoon for Jack, sitting there with his helmet on and slide rule and says, "EOR [Earth Orbit Rendezvous] versus LOR [Lunar Orbit Rendezvous]," lunar over rendezvous versus Earth rendezvous. EOR versus LOR. And over here was a camel trying to carry his pack through the needle's eye, you know, [in the Bible]. That was hitting the entry corridor. A lot of stuff like that. I still have it. [See Figure 1] I think that was a clever thing. But Jack Funk, has he been interviewed?

BERGEN: No, he hasn't.

CHILTON: Oh, you ought to interview Jack. Listen, he had a colorful career. Before he joined—you see, back during that Space Task Group days, various people in Langley decided they'd like to work, so they'd come over and Jack decided he wanted to work for us,

so they gave him to me. In fact, he was one of my first two guys to work with me. I never heard of him before because he worked in loads.

But at one time—you remember the U-2 business? There was a time when—who was the head of NACA in those days, the great scientist [Hugh L. Dryden]? Anyway, the head of NACA didn't even know what was going on, but when the Air Force established this base over in Turkey someplace to fly these spy missions in a U-2, Jack Funk was on the team. The cover for our involvement was that they were getting gust data. [Laughter] Instrumenting the U-2 and Jack and some other guys were over there for a long time, pretending. Actually, they were getting data, but it wasn't the real reason that the U-2 was there, but that's what they were pretending. They were so hush-hush that even the head of NACA didn't even know that some of his people were involved and Jack was involved. I don't know whether he was in charge of the instrumentation part or not, but he was involved. He would have some interesting stories for us, I imagine.

BERGEN: We were talking about LOR.

CHILTON: LOR. Okay. Eventually, Gilruth sat us down in a room and had people talk. I was there and Jack was there, or Jack and I were there. Jack was there, [so] I had to be there, too, because he worked for me, and talked about all this. For Gilruth, there was just no question in his mind, it had to be lunar orbit rendezvous. Yes, it had to be lunar orbit rendezvous. So that's how the decision was then. I don't know what he had to do to make his opinion [unclear], but I'm sure Houbolt helped, but I do think that Gilruth didn't really appreciate Houbolt's intrusion into the idea. I don't think he relied that much on Houbolt's results, which were, I guess, valid and convincing. I know he capitalized on it a whole lot. He wasn't a very likeable guy. Is he still living? …

BERGEN: I believe he is. I'm not positive about that.

CHILTON: He was a very bombastic kind of guy and he did not—what's the word? Didn't tolerant ignorance kindly or something. Anyway, he was very impatient with people who weren't as smart as he was.

BERGEN: So once this decision was made to go with the LOR—

CHILTON: You see, the main reason Rockwell opposed LOR, was it dealt them out of a lot of business. So as a result, of course, the final decision, the main thing was that we went out for another competition and Grumman [Aircraft Engineering Corporation] won the deal. Early on it was decided (they were still in good graces) MIT was going to play the same role in LM [Lunar Module] as they played in the command service module.

I often wondered what it would have been like had we redesigned the service modules for its ultimate mission. Maybe it would have prevented that accident on Apollo 13. I would say it would be a safe bet to say that if we had redesigned the service module for a smaller engine, that that accident probably would not have happened. Or maybe some worse thing might have happened, I don't know. It's interesting to speculate on if you'd taken that different path.

Any other question on that?

[There was one incident from the LEM proposal evaluation and selection, which could be noted. In the request for proposals, the prospective bidders were told that teaming agreements between contractors were not permitted. Grumman was selected even though they proposed teaming with RCA for all radar equipment. Bob Piland's tack was to get Grumman to compete for the radar instead. After some unsuccessful discussions, he received instructions from NASA Headquarters to give it up. So the Grumman/RCA team remained

intact. Some time later an acquaintance of mine who worked for LTV (Ling-Temco-Vought) in the Dallas-Ft. Worth area who had bid unsuccessfully on the LEM told me that RCA had approached them with the proposal to team. LTV declined or it was not permitted in the bidding instructions. RCA stated that if they could team, they would win. They did not win, but Grumman, who did team with RCA, did win.]

BERGEN: Well, before we get off Apollo, I was wondering if you remember where you were when Apollo 11 did land on the Moon.

CHILTON: No. It was on a Sunday, wasn't it?

BERGEN: I don't know what day of the week it was.

CHILTON: I didn't even go and sit in the Mission Control. I just listened to it on TV, watched it on TV, just like everybody else. I went over once or twice to see what they were doing in that observation place where they watch, but really and truly, I never even actually went down to the Cape for a Saturn launch. I went down once for an Atlas launch, but it was aborted. [Laughter] So I never saw a launch. I felt like if I was going to go, that Chris should have invited me and he never did. [Laughter] I wouldn't have had anything to do.

BERGEN: Another thing that you've mentioned a few times, but we haven't really spoken directly on, you talked about computers and how important they were and what you did and the high-level mathematical calculations you used, but computers were really in their infancy. How did that affect what you did?

CHILTON: Well, the calculations [that were] made were the brain child of Dick Battin, a real genius at MIT, a math major. The computer itself, I can't think of the guy who was in charge of the computer room [Eldon W. Hall], but they pioneered in the—one big decision we had to make, which guys like John [P.] Mayer didn't really like, was that it was a hard-wired program.

I forgot. Way back in that first meeting, this computer guy [Eldon Hall] asked me—see, they were very reliability-conscious with numbers. They had to calculate things. How reliable is this? How reliable is that? How many failures per thousand hours, or how many hours between failures? They asked me how reliable has the computer got to be, this guy in charge of computers. How reliable does a computer have to be? I didn't know anything from reliability, I said, "It has to be as reliable as a parachute." [Laughter] I thought that was a real brilliant off-the-cuff response. And I was never asked again.

It was ultra reliable. I mean, its experience throughout the mission, its use is unparalleled, I'll bet, in industry, for the reliability of that computer. The big decision that I really—was whether the program should be hard-wired or programmable. Of course, the mission people wanted it be programmable. But we just felt, and MIT—I mean, I was just on their side, really, felt that if you want to be reliable, the big thing is—because in their opinion, the unreliable thing is the program, because it's done by changes. So if we hard-wired the memory, and it was tested and tested and tested, then it couldn't change. So it didn't change from mission to mission; it was always the same. They developed a whole procedure of rope memories, using these little cores. So the computer development was a unique technical experience and, I think, a unique result in reliability.

BERGEN: What were some of the drawbacks that you had to deal with because of the hard-wiring?

CHILTON: You had to make sure that you were right the first time and that you wouldn't need to change it. It was a tradeoff in which you thought was the more greater risk.

What else was there in the computer? One of the subsequent things that happened that could have been avoided had there been—I mean, it turned out that we could have avoided an incident on the first landing had we tested more carefully. That is to say when we had our manned simulations with the computer. When Apollo 11 approached touchdown, suddenly the ground radar, the radar altimeter started feeding data into the computer and Armstrong and them saw all these lights flashing in the cockpit. Some guy made a reputation for himself by saying, "Go ahead and land." I forget his name, a young man.

What had happened was, is that when the computer sought—we had never put the ground radar in the simulations, neither us nor MIT. We mostly tracked everything and MIT did, to the extent of our facilities, and Rockwell. That's what our simulation branch was all about. By this time our division had a Control Branch and an Inertial Branch and a Simulation Branch, [Ken Cox's Analysis Branch,] and Dave [David W.] Gilbert's branch. Have you interviewed Dave Gilbert? I should go back and tell you something about how Dave Gilbert and—who was the last director before now? Aaron Cohen. How Dave Gilbert and Aaron Cohen came to Houston.

Anyway, so when that radar altimeter started feeding data into the computer, the computer overloaded and it was sending out signals, "You're asking me too much." So this guy said, "Ignore it, go on and land." That was a good decision. But we could have avoided that. We should have avoided it had we done enough of this testing. So that was the failure, the weakness, in the hard-wired memory was it had to be tested and tested and tested and tested, and we just fell short at that one point, we and MIT.

BERGEN: Was there anything else that stands out in your mind about Apollo and the work you did in Apollo that maybe I didn't address in my questions?

CHILTON: Well, I'd like to tell you the story about how Dave Gilbert and Aaron Cohen came into the picture. The early—and maybe I should mention that I don't know who was the first Gemini program manager, but there came a time when they realized that Chuck Mathews was perhaps undervalued by being [just a] division chief in Max's organization, and they put him in charge of Gemini Program. Chuck Mathews was a really talented guy. He was technically capable, but more than that—I wonder if he told you that—see, right after the war, while I was still at MIT, in fact, [President Dwight D.] Eisenhower created a commission to study the airport requirements and the interstate system and all that stuff, you know. In those days, NACA used to provide—they would create a committee. I don't know who was in charge of that committee, but Eisenhower named a bunch of high-powered people. Then to do the legwork and the recordkeeping, NACA would assign an engineer as a secretary to the committee. Well, Chuck Mathews was the secretary to the Eisenhower committee that made all those great decisions after the war.

I don't know whether, because I really didn't know too much about that, but there's a parallel to that story. When whoever created the [George B.] Kistiakowsky—do you know the Kistiakowsky Committee?

BERGEN: No, I'm not familiar with that.

CHILTON: Somewhere along the line, I guess again it was—well, it would have been—might have been Eisenhower again. There was a big debate, who's going to have charge of space, the Air Force or not? So they created the Kistiakowsky Committee to make this decision, and NACA sent Bob Piland up to be the secretary of that committee. So Bob Piland, I'm convinced, had a lot to do to influence the decision to create a civilian organization to run

space. NACA had some really good people. So, as I say, I don't know who was the first Gemini program manager.

BERGEN: Jim Chamberlin.

CHILTON: He was? Oh, he was a real controversial figure. And Walt Williams, too. In fact, the guys that worked for him out at Muroc, namely Sig Sjoberg and mostly Porter Brown, didn't like him. He was very crude and a very—Jim Matthews, he's dead, too. He was out there, too.

Anyway, so Chuck wound up the Gemini Program and deserves any accolade that he can get, because it was a well-run program. It was not a well-conceived program, in my opinion. [Laughter]

Apollo had even a more traumatic project manager and leadership. I think Bob Piland started out as a program manager. He did, yes. Bob Piland was the first program manager for Apollo.

BERGEN: I think he was a temporary program manager.

CHILTON: Okay, so he was assigned temporarily. He was the first acting program manager. So that would make the first real program manager this guy that Gilruth met on an airplane that he knew, who worked at Ames and had, I think, moved over to Convair. That was a combination of Consolidated Vultee Aircraft in San Diego. What was his name now? Frick. Charlie [Charles W.] Frick was named the first real program manager and he brought with him, among others, Dave Gilbert and Aaron Cohen, who had worked for Convair.

When Joe Shea came on board, Charlie Frick didn't last too long either, because he was another one of these—in fact, when he came on, I think Piland just moved out. Nobody

could get along with him. He had one guy, one of his lieutenants that he brought with him, I don't remember his name. By this time I had created a relationship between MIT and Walter Hauserman [phonetic] down at Marshall [Space Flight Center, Huntsville, Alabama]. He was in charge of all the guidance and control in Marshall. We would met regularly and exchange ideas. So we had a meeting scheduled and, in fact, Walter—and this guy who worked for Charlie Frick, called me up and said I was to cancel that meeting. He said I was to call up Walter Hauserman and say, "We and MIT can't come."

I said, "You call him. If you want to cancel it, you call him." So he did. And Walter Hauserman made a big stink with Gilruth. This is an illustration of how Charlie Frick ran things. Anyway that's sort of our interface with—Marshall sort of petered out after that.

But anyway, so when Joe Shea came, I think Joe Shea must have followed Frick.

BERGEN: I believe so.

CHILTON: He came down from NASA headquarters. I didn't mention that one time when I was having a meeting with MIT, one of Joe Shea's people in Washington just attended our meeting. We didn't care, but we were sitting in this meeting. I got a phone call from Bob Piland. He said, "Joe Shea doesn't like the way we're doing business and he wants Doc Draper to come up and meet with this new program manager in Washington…[Brainerd Holmes] and talk about what MIT's role is going to be in this program." And Bob asked me to set up the meeting.

So I went back to the meeting and told the guys I was working with, I said, "We've got to get hold of Doc Draper and set up a meeting in Washington, because Joe Shea has challenged our idea."

Well, this guy went back and told Joe Shea, and he went ballistic, because he didn't want MIT to know that he had initiated this, because he had come from MIT, but he didn't

approve of the way they did Polaris. So I was on his list from then on. When he came down here, one of the first things he did was move Dave Gilbert. He fired Dave Gilbert. Why, I don't know, there was no connection. He did two things, he said, "And Chilton is not going to be head of this division. I'm going to pick the head." He picked Cliff Duncan. What happened, I had known Cliff for a long time and it didn't bother me.

In fact, I was not that happy in program direct support anyway. I was more institution, as I've told you. So I was glad that if I was going to lose out it would be to Cliff Duncan. That's how I became his deputy, and we had two assistants. Personnel had trouble justifying a division with a chief, a deputy chief, and two assistant chiefs all GS-16s, I guess, or something like that.

Anyway, Cliff was a super [grade]. Congress has let us hire so many people. He had a dual—he was a Navy retiree. He was a Naval Academy graduate. Anyway, so we hired and brought Dave Gilbert into our organization and he later became one of my—he's a really good guy, Dave Gilbert.

In the project office, Aaron Cohen then was the main guy in the project office that looked over the guidance control stuff, but we didn't see a whole lot of him at that time. He was a nice guy, too. I liked him. We never had any problems.

What were we talking about at that point? I was just telling how I wanted to let you know about Dave Gilbert and some of these people and how Charlie Frick, his influence on us to bring in some good people, namely those two. Some of the others he brought in, I think, left with him.

Joe Shea was a good project manager. He just didn't like me. [Laughter] I think didn't he have a nervous breakdown when they had the fire or something like that down there? I met him—the next time I talked to him, we had a reunion up at MIT one time on the 50th anniversary of the department, and we talked a little bit. He didn't seem to have any

further malice toward me. [Laughter] And we worked well with Aaron. [Joe Shea died in April 1999]

Did you have any other—

BERGEN: We can move on to Skylab, unless you have anything more about Apollo.

CHILTON: That was your next question, "What was your involvement with Skylab and so forth?" That's very easy to respond to. Skylab didn't affect Apollo at all. I mean, the Apollo command service module. I think Marshall must have built from the Agena stage or some [unclear], an [Saturn] S-II stage? And all that JSC did was just go up there—of course, the big job, which was largely Caldwell [C.] Johnson and some others, was to design a mechanical docking interface. I'm sure they did a good job there. Caldwell, I'm sure, was a key—he's a fantastic designer. I guess you've interviewed Caldwell.

BERGEN: Yes, we have.

CHILTON: You found he's another character. [Laughter] I used to accuse him, back in Virginia he was a big hunter and fisherman. I don't know why, I think I had some—I used to accuse him of being a poacher. [Laughter] Is Porter Brown on your list?

BERGEN: Not currently, no.

CHILTON: He never came to JSC; he went to the Cape. Then he left the Cape and went to headquarters.

So all we did in Skylab was sit some people in the mission room if they needed help and to do whatever post-flight data analysis there was. The command service module weren't

changed at all, except to get a docking ring, which we didn't have any problem with. The same with Apollo-Soyuz, same old, same old.

"What was your involvement with Space Shuttle development? Were you involved with any of the preliminary studies? How did the Shuttle guidance system compared with the state of the art? What new challenges did a reusable spacecraft present? Who were the key people in the design of the Shuttle guidance system?"

Therein begins my disenchantment with the space program. Okay. The Space Shuttle was designed by committee, and by this time everybody—the whole idea about this, what the project office did versus engineering, the project office kept getting bigger and bigger and bigger, more and more in-house responsibility on the technical issues. They made all the decisions. They'd have a meeting on how to define the control actuator system and it would be propulsion people and our people. I'd tell my guys what I thought and get voted down every time. I didn't win a single battle in the Shuttle control and guidance configuration.

I wanted distributed computing systems. We have a big central computer. I mean, a huge big central computer. I wanted electrical actuators for the controls. In fact, we did a big development program on electrical actuators. We have hydraulics instead.

Guy [Joseph G.] Thibodaux—no, not Guy Thibodaux. One of the guys who became later—got Max's job from propulsion, do you know his name?

RUSNAK: [Henry O.] Pohl.

CHILTON: Yes, Henry Pohl. Henry Pohl once told me that—see, the hydraulic system, unlike an electrical system, posed a lot of difficult problems. For example, there was hydraulic fluid running around to all these pipes, very, very cold and it might freeze. So they had big insulation wrapped around every hydraulic line. Henry Pohl once told me if you got

a leak, that the insulation would soak up every bit of the hydraulic fluid in the system. [Laughter] So I was in favor of an electrical system.

There were questions of redundancy, how you achieve redundancy in actuators. We did a lot of in-house development on electrical actuators with redundancy.

What other battles did I lose? Well, of course, since Rockwell went on, and having come from Apollo, they won all the battles about—they were in charge of everything. MIT, we kept JSC even…Johnny Mayer kept MIT in a lot of studies peripheral to Apollo or supportive of Apollo, and they would attend meetings, usually meetings where we would talk about issues. But Rockwell made all the decisions. They decided on a big central computer, which IBM would build, totally programmable, everything programmable; how many inertia platforms we'd have; whether they would be fail-op or fail-op, fail-op, fail-safe. Fail-op, fail-op, fail-safe or fail-op, fail-safe, or fail-op, fail-op, fail-safe. All those things, I had no contribution to whatever.

In fact, I didn't even think we should have a Shuttle. During the time that the program was sold, Bob [Robert F.] Thompson was in charge of things, of how to get it going, I guess. I'm not sure when he moved over there. They would have us do studies, make estimates of how much something would cost, and then they would change it all, say, "We don't believe you. They won't ever buy it if we [charge them] that much," and then they would tell headquarters something different.

The whole concept was that the Shuttle was going to do everything. We were going to put all the Air Force payloads in orbit. We were going to fly eleven times a year, and none of which I believed. I certainly didn't think we should have a Shuttle take up an Agena, these little rockets that the Air Force had. So I just felt like we lied and approved that program under false pretenses.

What I wanted to do was to improve the Moon landing system, replace the Saturn V with something that was cheaper. See, they had their own guidance, [we had] two guidance

systems on board. I wanted to build—in fact, Caldwell worked with me. He, too, had [some idea] and lunar logistic system we called it, which would develop something as you go up to the Moon to carry materials up there and build a little America up there, like [in] Antarctica. I had really thought maybe one day I might go up there myself and get to walk in the lab (not outdoors) and fool around. I thought that would be great.

So I was very unhappy with the way the program was going away from the Moon, and that was all largely an astronaut influenced decision. They were pilots and they wanted to—nobody had ever built a spaceship that had wings or landed, and they thought that was a deficiency. I thought it was a big improvement, because early automobiles looked like buggies and the early airplanes weren't that different. I thought we had done a great thing, we invented this first spaceship that had no resemblance to an airplane. I thought that was great, but that was a fault in the program to the astronauts. They wanted something they could fly and land.

So I didn't believe any of the justification for Shuttle, taking over the Air Force payload. I guess you know they built a blue Shuttle, or they were going to build a blue Shuttle. They did build a big whole launch facility out there on the California coast, spent billions of dollars developing a launching facility to launch Shuttles from the West Coast. In fact, I went out there. When I went to [Texas] A&M, an Air Force recruiter got me and some other [A&M] professors to go out to a tour of—what did they call that place? Anyway, the Air Force launching facility. Do you know the name of that?

TARRANT: It starts with a V.

RUSNAK: Vandenberg [Air Force Base, California].

CHILTON: Yes, Vandenberg Air Force Base. They wanted us to go out there and we got this big tour, because in order to sell graduating A&M engineers that there was a good career in the Air Force working on Vandenberg's launch facility. Sure enough, I talked to a lot of second lieutenants who had come from A&M or from other schools, for that matter, and they had fantastic jobs. The responsibility they had was terrific. So I would not have hesitated to tell anybody they would have a good career in the Air Force, because of the Air Force's space program. Well, you know what happened there. They scrapped the whole thing, because the Air Force, finally, maybe they always felt, that that they didn't want to rely on the Shuttle to put up their spies in the sky. It was far better to have—so anyway, I just fell out of love with the space program altogether. Then we started having riffs and my job became pretty onerous. [Laughter]

I began to make plans. I was approaching the lower limit for retirement, age fifty-five and thirty years. With my military time, I was approaching thirty years and fifty-five at about the same time. I had a son at A&M. He was a sophomore, I guess, then. Kraft sent one of the personnel guys that served in my division, I don't remember his name, but he came over one time and showed me this letter that Kraft had received from the acting dean of engineering at A&M, saying that would he like to recommend somebody. They were looking for a new department head in aerospace engineering. Would he like to recommend somebody? So I told him, yes. Gosh, yes. So this person wrote a letter and Kraft signed it and sent it down and recommended me to be a department head. I was thrilled at that. I filled out a big resume and sent in a resume and everything, and waited and waited and waited. Didn't get any word. Finally, I walked in, went to the department head that was outgoing—oh, no, before that time, even before then, I had just walked in to meet the department head, and said I was interested in working up there. He said, well, he had such a limited budget, he wasn't going to be hiring or taking on any new staff.

CHILTON: It turned out he was an alcoholic and he finally went on medical retirement, and that's why they had a vacancy. So I went down to the acting dean and said, "What's up? I haven't heard anything. Aren't you going to interview me?"

He said, well, in the old department, this guy who had retired had a lot of controversy, because he only had a master's degree from the University of [Minnesota]. He had several doctorals in his department and there as a schism. Some of the teachers were not doctors and some were. No offense there. [Laughter] Did you say what your major is going to be?

TARRANT: History.

CHILTON: And yours?

RUSNAK: History of technology.

CHILTON: But anyway, he said, in fact, there had been a big schism. The [number of] aerospace students was way down. In fact, the year before I went there, they only graduated about half a dozen engineers. They were even talking about, in fact, one of these guys, doctors in the aerospace was even promoting the idea of abolishing the department and creating a segment of mechanical engineering of which he would in charge. They wanted to fire this alcoholic. Then these other non-doctors they were saying, oh, no, we don't want to be a part of mechanical engineering. So there was a big friction there.

This guy said, after all that, "I just can't hire somebody that doesn't have a doctorate." That's something that later on I could have probably gotten, NASA would have easily sent me off to—Bob Piland got one in public service and Jack Eggleston tried for one at Princeton. They paid for a lot of doctorate degrees, and I made a big mistake by not—well, I began to sort of not to do that myself. But it never occurred to me. Hewitt Phillips, who I

considered to be the smartest aerospace engineer in the world, only got his master's degree at MIT. He never even thought about getting a doctorate. So anyway, I said, "Well, how about just being a teacher?"

"Oh, wonderful. Wonderful. Wonderful." So that's how I went to A&M, and I had a big impact up there. See, I was the only guy there who'd ever been out in the world. Several years later, Tom [Thomas U.] McElmurry joined us, and the two of us were, I think, a big influence on that. Pretty soon we were graduating fifty or sixty engineers every year in aerospace. I think I played a role. Of course, the [other things] had an influence, but I think I had some influence on that effect.

Well, of course, I stayed there ten years. The last two years I cut back to half time, but it counted. I was on the budget and ten years [was the] minimum to be retired with full benefits, which were free parking and an office. [Laughter] In fact, I was professor emeritus. Let me give you my card. I'll explain this card in a minute. Here's my lottery ticket for last Saturday night. I didn't win. [Laughter] Somebody won, though. Somebody from around here, went to [unclear]. I'll give you these in a minute. See if I got three of them. Yes.

Lost my train of thought here. Oh, yes. You asked me what I taught. I didn't feel—in fact, I was totally unqualified to teach graduate-level studies, so I taught strictly undergraduate. I was a visiting professor, so I didn't have to worry about tenure or research or anything, so I just taught. I taught the theory of stability and control of aircraft. I introduced, for the first time in the department, a course in automatic control theory. Electrical engineering had one and I had to do a lot of coordinating and writing things to Austin to get approval to institute a new course, which is still going on. So I think I had a pretty big impact up there. Those were the studies I did.

Even after I retired, I kept working, first, quarter-time, and then a fifth-time. I'd go in mostly just counseling, but there came a time when I wasn't getting much business

counseling, so I just gave up my office and I haven't been back over there now for several years. I need to go back over there.

You asked me, do I still have any contact with NASA. "Do you have any subsequent jobs? What do you do now?" Well, during my stay there as a teacher, it turned out that there was a ongoing program which was rotated between U.H. [University of Houston] and A&M every summer. Do they still have a summer intern program here, or do you know? Well, the way that program was structured, NASA headquarters would contract with a university, in this case alternate years, U.H. and A&M. In the alternate years one school would…[assist] and the other one would be in charge, get all the money.

So one year an A&M prof [professor] was told to manage this program for the summer and I helped him, because he wanted me to help him because I was—and then the next year, UH had it and I helped him. Then the next year I had the program. I ran the program. The program consisted of some engineering society sponsored it and NASA headquarters paid the bill. They would advertise among the university community for these opportunities for summer research for faculty members and then they would receive applications.

Let's talk about my year. The application would come in to me and I would sort of compartmentalize them, and then I would come down and have a meeting with various division chiefs or their representatives, guidance [and] control, power and propulsion, whatever, all of them that were interested. I'd hand them these things and they'd sit there on the table and they'd study them and they'd make lists. "I'd like to have this guy and I'd like to have this guy." Then I would take it all and make it fit the pattern, and then I would write letters to the lucky guys and letters to the unlucky guys.

I was being paid. Actually, the university was being paid and then they paid me full-time. I would get the money. I would write checks for their travel expenses. When they got here, they'd do travel expenses, and every month I'd pay them their monthly stipend. Then at

the point that they came, we'd come down and we'd have a big meeting and I would introduce them to where they were going to live.

I guess I'd come down about Monday and stay until Friday. I had two sons in the neighborhood, so I'd stay with one of them. Had three sons. Let's see. No, just two of them here. Anyway, I would stay with them, spend the night with them for the four nights, I guess. We would have get-togethers of these faculty people and we'd go out to lunches, we'd have lunches at various places. I'd get reports, what they were studying, what they were doing. They were required to write a report and I would evaluate that report.

During that time I was coming down here—it was a ten-week program, I believe—I'd be wandering around the campus, and the biggest thrill I got was I began meeting more of my former students than I was meeting of my former colleagues, just encountering them on the sidewalks or in the cafeteria or something. That was a big thrill. That's the most gratifying, one of the more gratifying things I've done. Of course, being on the Moon team is the big thing, as my card will show you. I'll give you those now and then I'll tell you how they came into being.

So that was my contact. Actually, I haven't had that much contact with NASA since then. I retired from A&M in [19]'88, so it's been eleven years since I've been down here on the campus.

"Did you have any subsequent jobs? What do you do now?" Well, as I said, for several years after I retired I worked part-time at A&M just doing counseling work, and that finally sort of petered out. As I said, I turned in my office. I've had a lifelong interest and love of jazz music, traditional jazz, not modern jazz, the kind Louis Armstrong used to play. In fact, when I was at MIT, a young couple of boys, some four black traditional musicians played a gig in Boston, and a friend of mine and I used to go down and listen, and I got to know these two white boys, Dick Wellstood and Bob Wilber.

So, anyway, once I was up in Ohio visiting my sister, in Circleville, Ohio, they had an annual music festival in Lancaster nearby. It was a complete coverage of music, classical. It happened that a band led by a girl named Banu Gibson and one called the Dixie Land something were appearing. While we were listening to Banu Gibson, I was sitting next to a guy and he says, "Boy, she's great. I follow her all around the country."

I said, "Well, how do you know where she's going to be?"

He said, "Well, I get the *Mississippi Rag*."

So that sounded good to me. He told me then how I could subscribe to this monthly newsletter about what's going on in the world of traditional jazz. It turns out that then—this is ten years ago or more—and now, every minute of the week, just about, there's a jazz festival going on somewhere in this country or in the European world. So I read this magazine for a year or two, and then finally when I totally, no more A&M stuff, I said, we might as well go to one of these things. So we went to St. Louis, and this year we went back there again. Every March for eight or nine years we would go to a party in St. Louis.

At our first one, we met a gal who said, "Oh, you ought to come out to Odessa." This year, actually rotating, alternating between Odessa and Midland [Texas], this year they're going to have the 33rd annual jazz party. I've only been to the last eight or nine. Then we would go to Atlanta in April, which we're going in a couple of weeks. Then in May we go back out to Midland this year. Then Labor Day we go to Los Angeles. That's a big party.

So I would meet these people and we'd get friendly. So I invented this card. I designed this card to pass out to people I met at jazz parties, and that's really about all I'm—of course, I found that once I got on Medicare and all that, I don't know in the world I managed when I had to work, because so much time is consumed in paperwork and just running, keeping pace with the needs of living, everyday living, plus enjoying some music. I have a huge library, of course, of recorded music, starting with 78 RPMs and LPs and now tapes and CDs. It's been a great life.

BERGEN: Sounds like it. I have one final question I'd like to ask you. What was the most memorable aspect of your career? Did you have a particular moment or event that really stands out in your mind?

CHILTON: Well, of course, the accomplishment that I feel I participated in was the lunar landing, but I didn't really have a personal memory, [except] like the average TV-watcher. Well, I better give some thought. Your question was, what's my most memorable experience. Actual experience. Actually, I believe that the most pleasurable experience I had was that summer I spent down here helping NASA with its summer intern program. That, I felt, was a very gratifying, pleasant experience.

In the space business, I guess that night when Chuck Mathews said, "You want to be on the man-in-space team?" was very pleasing, too.

Recently I received from Hewitt Phillips, he's written a NASA report on the first part of his technical career. You ought to see if you can—it's a series, I think, that NASA is putting out of some of their pioneers and telling of their technical contributions. You'd be amazed at what Hewitt Phillips did in the first half of his career, and he mentions me. [Laughter] He asked me for a picture once that caused a little bit of confusion actually; I sent the wrong picture. He was a great guy. He grew up in MIT [area], Brookline, I think, was his home.

What else did I get a lot of pleasure from? I remember when we first came to Houston and I bought a house down in New Meadowbrook, just a few blocks from now what's Hobby Airport. Our house, there was nothing between my house and the airport. We could look out our back door and see the planes, maybe. I was working in the Rich Building and I got promoted. I think I started making 20,000 [dollars] a year. That was my richest

period of my life. I think I bought a Buick Electra and a Volvo station wagon all in one year. [Laughter]

I had three boys. My oldest son went to the Coast Guard Academy, and he's struggling right now because he's in the process of entering the ministry, Episcopal Church, and he's living in my mother-in-law's house. She died a couple of years ago. He's costing us. We're sort of subsidizing his career.

My second son served eight or nine years as a jet pilot in the Air Force and he now is a captain for Southwest Airlines. That's really how I get to go to all these jazz parties. Most of them are places Southwest flies. [Laughter]

And then my youngest son started out in the oil patch, but he got disillusioned with that and he went to school down at Moody.

Dave went to the Coast Guard Academy, Donny went to A&M, and Larry went to the Galveston campus [of Texas A&M], but he didn't last long in the—he qualified as a third mate or some kind of a [Merchant Marine Officer]. He didn't like that, so he went back to school and he went into teaching. He teaches English and stuff like that in Conroe High School. Not now, but there was a period in time when I subsidized him, too, a good bit. So they are all three fine boys.

When astronaut [Kevin P.] Chilton was there, he and my son Donny, who now lives not too far away [where he] grew up where I'm staying, they went to the same church. So they became real good friends and described themselves as cousins, which they were not, of course. I met him and his family and that was pleasant, too. He had had people ask if we were related and when he met Donnie, he had all that [explained].

BERGEN: If it's okay, I'd like to ask Sasha and Kevin if they have any questions for you.

TARRANT: Actually, yes, I have a couple. First off, you mentioned the Flying Pancake earlier. I would like to know more about that.

CHILTON: Well, I thought that might come up. Charlie Zimmerman, I think, worked for NACA, and then he, like I, decided to work in industry for a while. So he went with Chance Vought, which was up in the Fort Worth [Texas] area. He was a designer. The Flying Pancake, it was [XF5U-1], didn't have any—it was like a pancake. It had two engines, [reciprocating] engines [2 Pratt & Whitney twin WASPs 1350 hp buried in wing driving 2 propellers out at wing tips by geared shafts] … [I]t was very revolutionary and high performance [425 mph top speed—20 mph landing, it could almost hover]. I saw one once. I think it was in Atlanta when I was finishing up my [military career]—anyway, I saw one on a ramp at an airport once. They only built, maybe—I don't know how many, two or three or four. It never went into service. [It flew November 1942, was scheduled to fly at Edwards in 1947 but canceled.]

But he was the guiding design genius behind that concept of the Flying Pancake, which is not too much different from like the B-2 configuration, no tail and all that stuff. And that's really about all I know about it. I don't know whether Charlie Zimmerman even is still alive or not.

I failed to mention that there came a time, especially when Jim Chamberlin came down here, after he left being the first Mercury project manager or Shuttle, I mean, Apollo—no, that's not right. Zimmerman was in charge of contracts, [then Chamberlin]. Oh, I know. What was that guy with a Swedish name, who was here in charge of contracts [Wesley L. "Wes" Hjornevik]? Anyway, when we came down here, we had hired a whole bunch of contracts people and the one guy in charge [Hjornevik]… Anyway, he took over all that [and all administration] and somewhere along the line Charlie Zimmerman went back to Langley. He left us, because the whole contracts business … got too big, I guess, for his taste. So I

never really knew too much. That's about all I know about the F-something or another, Flying Pancake design. I'd be interested to know if he's still living. I kind of doubt it. He was a little bit older than I, and I'm seventy-six. Charlie Donlan is still living. Have you interviewed Charlie Donlan?

BERGEN: I think we have, but I'm not sure. I think we did that in the early part of the project.

CHILTON: I'm sure he would be among the first, because, as I said, he and Gilruth were the two first in how we got in space. But he never came here. He stayed at Langley. When we moved to Texas, he didn't move with us; he stayed on at Langley as the assistant director or something like that. Great guy.

Fred Bailey was a good guy. He went to the Cape and he spent a little time back here at JSC for a while. He died early, too.

TARRANT: Milton Trageser headed MIT's effort.

CHILTON: Yes, against some—Bob Seamans didn't want him to be the manager, he wanted Ralph Ragan, because he had headed the Polaris. But Doc Draper picked Trageser, because he was sort of a genius-type guy. Big. When we first came down here, there was a place out there on the ship channel, it was a seafood place where you paid a price and you ate as much as you wanted. They'd bring out this huge platter of oysters on the half shell, huge platter of boiled shrimp and dip and fried fish and fried chicken. I once saw Milt Trageser eat two whole trays of oysters, must have been two dozen of them. He was quite a [trencherman]. He's still up there, I guess. I haven't had much contact with him. Why did you ask about him?

TARRANT: Well, I looked at some of the meeting minutes from when he was working with MIT and you obviously had regular contact with him.

CHILTON: Oh, yes, we were quite close.

TARRANT: He shifted in 1966 from the Apollo Project to other research, and I was wondering why.

CHILTON: Well, I think it was probably the fact that Seamans probably had the right idea, that he was not really as talented in management and managerial skills as he was technically, and that Ralph Ragan would have been [a better] choice. Actually, I think Dave Hoag replaced him, and he's first-rate, too. So I imagine they just decided that they needed a better—there came a time in the program when Milt could no longer quite cut it managerially and he was probably being wasted there, in fact. He had a very great ego and so he went into—one time I think he defended to me the Stars Wars concept. Is that what it was? [President Ronald] Reagan's idea? He was the kind of guy who'd say, "We can do it." He was a good guy, but probably overmatched in that context. That was your question, how come he got taken off?

TARRANT: Right.

CHILTON: What did you say he went into?

TARRANT: He went into advanced studies. In fact, the exact kind of work you were talking about earlier, where they weren't necessarily attached to a specific program, but just concept design and development. I was just wondering why that transition took place at the time.

CHILTON: I'm sure Draper or whoever was making the decision, because by that time another guy that I knew from, an Air Force retiree, began to assume more of the management of the instrumentation laboratory.

I just got a letter from John Miller, MIT, a few days ago. They're going to spend a lot of money to redesign the instrumentation laboratory in honor of Doc Draper, and if I wanted to give $5,000 I could be on a plaque or something, to contribute. I haven't decided how to respond.

Dave Hoag retired a long time ago. I expect all those guys are retired, because they were all my age or slightly older.

BERGEN: Kevin, any questions?

RUSNAK: No.

BERGEN: Is there anything you would like to say in conclusion, anything we forgot to talk about?

CHILTON: No.

[End of interview]