Interview with Dr. Christopher C. Kraft, Jr., Director of MSC, at his home in Friendswood, Texas, 27 July '72, 1910 hours, by Robert Sherrod.

(First of two tapes)

Q. Well, Dr. Kraft, we were talking the other night about outstanding people you have known in NASA, and we mentioned Frank Borman and Jim McDivitt and Bill Tindall, I think, and two or three others. Could you elaborate a little more on that? How much farther would you extend this list?

A. You couldn't leave out Gene Kranz.

Q. Of course not.

A. Because, you know, I get a lot of credit for being the father of flight control and that sort of thing.

Q. Yes, well deservedly, of course.

A. Well, but he had an awful lot to do with that because he had a tremendous knack for that kind of thing; that is, the development of procedures and the attention to detail and he always amazed me in his capacity for work. We used to ... when the control center at the Cape was being activated there were ...

Q. You mean Mercury control.

A. Mercury control center, and I guess that was about 1960, sort of the fall of 1960, if I can remember my dates properly and I'm not very good at that sometimes. But, well, let me use some background. Back before we got to formulating in detail what we were going to do in terms of ground support, that is the in-flight ground support to the program, we had decided to build a control center at the Cape and where to build it and all that sort of thing and then when we looked at the schedule of when it was going to be ready we said that we'll never get it ready to support the Redstone part of the program--so we'd better do something special for that and we decided that we would give a special contract to McDonnell to build a mobile lab--sort of a

(037) mobile control center. We built it and never used it because it turned out that the control center was ready because of the lag in time in the development of the spacecraft, and the time we were ready to launch it on the Redstone, it turned out we were ready with the control center at the Cape. We were fortunate there. And about that time period--as I say, I think that was about the fall of 1960--we were getting ready to launch the first Mercury Redstone so we figured we'd better go down there and start working out the procedures for how we were going to fly that thing and take any action that was required by the ground. Tech Roberts, whom I guess you're familiar with, he's down at Goddard, just took Ozzie Covington's job up at Goddard,

Q. Oh, has Ozzie left?

A. Yes, Ozzie's working for me in Houston.

Q. Okay. I didn't know this.

A. But Tech Roberts was one of these fellows who came from Canada, from the Avro Corporation you've heard us talk about, like John ~~Hines~~ Hodge, he was with John ~~Hines~~ Hodge.

Q. And Rose?

A. Yes, Rod Rose. And we--he and myself and Gene Kranz and a fellow from the Navy who had been assigned to us, named Paul Havenstein--moved our bodies to Cape Kennedy--Cape Canaveral, then--to establish the procedures and work out all the details of how we were going to run an operation from that control center. And that's an awful long background to Gene Kranz but I didn't know Gene very well at that point--I mean I didn't know him like I got to know him living with him then for that length of time.

(080) praise anybody I couldn't praise anybody higher than I could praise Kranz. He's superb. And under the pressure of getting the job done he's always been superb. Of course, the outstanding example of that was Apollo 13, because when that accident happened, he was the guy in charge at that time.

Q. But actually, during the most critical times, wasn't Lunney the director?

A. No, sir. When the accident happened and the sort of the "safing" of the spacecraft and the ideas of what to do and how to bring that all about, he was the guy who was responsible at that moment. He was the flight director at that moment and for about two hours after the accident occurred. Now, when Lunney came on board, he had the next shift and Lunney sort of took the situation and, after it had been stabilized, began to do the overall planning. But Kranz, my friend, went downstairs with those people and spent the next eight hours doing the planning with them.

Q. I didn't know this, because even two or three weeks ago there was a piece in the New York Times about Lunney and they gave him the credit there for the Apollo 13 ....

A. No, sir. They were both responsible for different parts of it.

Q. You told me you were out here at your house, in the shower, when you got word that something godawful had happened.

A. Yes. Right. George Abbey called me and told me we'd lost two fuel cells and got some serious problems out here. I didn't even continue to talk with him. I hung up the phone, and I was there in a very short period. I walked in ...

Q. You drove at 90 miles an hour out there.

(057) Q. But he must have been very young then.

A. He was. He came to us ... he was an F-86 pilot in the Korean War and then he went to work for McDonnell at White Sands as a missile guy, and transferred to us. And we used to work all day long--hard, and battling all the time trying to figure out what we were going to do and writing all this stuff down and figuring out what was the best way to do each one of these things at each one of these stations that we had developed in our preplanning of who had to sit where in this control center and what their responsibilities were. We'd get there usually about eight o'clock in the morning and work 'til 6:00 or 7:00 and then we'd say, well, we'd had enough, Roberts and myself and Havenstein, and he'd say, well, I'll be along in a few minutes, and he'd stay there and work another three-four hours. I was tremendously impressed with him there and I've never been anything but tremendously impressed with him ever since. His capacity for doing work is unbelievable and his capacity for storage in his brain is unbelievable and his talents are unbelievable.

Q. He never quite got the publicity Glynn Lunney has.

A. No, because he's a little more abrupt than Lunney and he's more the military type than Glynn. Everybody calls him "General Savage," all the people who work for him.

Q. Well, he looks like a Prussian.

A. Yes. You know, Lunney worked for him, you realize, in the organization until recently. Kranz was his boss. But Kranz' capability to run an operation is terrific. He has the utmost respect from the people who work for him and with him and above him. He's got some rough edges but they're acceptable in that kind of human being. I would say that if I were going to

(103) A. Whatever it was. I was there in less than 15 minutes from the time he talked to me on the phone, and I had to get dressed. I was wet, as a matter of fact.

Q. And it was Kranz at the time ...

A. And it was Kranz on. And I walked into the control center and he turned around and looked at me and he said, "We've got a hell of a serious situation here and I'm not sure what we're going to do with it."

Q. That's funny, because the Times piece in connection with Lunney's handling the Russian ——————→ negotiations, gave Lunney the credit for the ...

A. Well, Lunney did a great deal of the post-accident planning, as to how to get it back and all that sort of thing. He was responsible at that time period for taking a stabilized situation and then proceeding with it and finding the right things to do in the next 10 hours, which were very important. I said at the time that if I were going to choose two guys to be there at that particular time, I wouldn't have done it any different. They're the best we had and they did a superb job. Kranz is what I would say is one of the most superb and exacting planners of anybody I've ever met. That guy forces the system and himself to do the most fantastic planning of everything he does. He can tell you what every one of his men is going to do in the next hour. That's how well he has that organization planned. He's got it on a computer program and he can tell you what he can take on and what he can't take on in terms of their workload and who can do it and when they're going to produce it. It's fantastic. He's just that kind of guy.

Q. Who chose the new flight directors? Griffin and Windler and Frank. Did you choose them?

(127) A. Sig and I chose them.

Q. It wasn't Kranz who chose them.

A. No, sir. And that was probably a mistake. We told him who we wanted and he wasn't very happy about that because he didn't think we had ... what he wanted to do was establish a training ground for those kind of people. I guess our argument at that point in time was that that was such a very important position, as far as the organization was concerned, but also it was an opportunity for people who shouldn't be ignored in terms of the people who deserved a chance at it. That was our reason for choosing the ones we had chosen and Sig Sjoberg and myself really selected those people ourselves.

Q. But once you selected them, Kranz took them over.

A. Yes.

Q. Allright, who else?

A. Well, I can't say too much for Lunney. I think all of us recognized Lunney's capability, starting in Bermuda when he was the flight dynamics officer there, and bringing him in as a flight dynamics officer at the Cape and his whole progression through the business. He's a superb young man also. He has a capability of making people like him, besides being so competent. That's been evident to anybody who's every worked with Lunney. He's always done a good job, he's always been easy to get along with and responsive to top management and extremely good with the press, very articulate in his capability to explain a problem or define the way to do something.

(152) Q. Have you ever noticed something about so many of the heroes of NASA, whether they're astronauts, who are the heroes known to the public, or others, like Kranz and Lunney, how many of them came from relatively humble beginnings?

A. That's not surprising to me. I guess it's probably got something to do with their training. You know, Kranz was a military man, Lunney was of course from a humble family.

Q. His father was a welder, he told me yesterday.

A. Yes.

Q. And I've talked also with Kranz and he certainly didn't have a silver spoon in his mouth.

A. No.

Q. Borman's father--was he a policeman? I've forgotten.

A. I don't know.

Q. His father ran a filling station, and they moved from Indiana to Arizona because Frank was a sickly young kid and had to go to a better climate. I think his father ran a filling station.

A. I'll be durned.

Q. I'd have to look it up.

A. The Grissom family was much the same way, too.

Q. Yes, his father worked on the railroad.

A. They were very plain people. I've met almost the whole family.

Q. There are very few people I know in NASA who belong to what you might call any sort of aristocracy. Well, Conrad was. Conrad came from a well-to-do Mainline family in Philadelphia.

(171) A. Schirra was a Navy man.

Q. His father was a highway commissioner, or something like that, in New Jersey. Tom Stafford told me his father was a dentist, I believe. But anyway, there are very few of them who do not come from relatively humble beginnings. Which brings us to a subject-we usually start these talks with "I was born ... ." Tell me, you were born in what part of Virginia?

A. The town of Phoebus, Virginia, which is now part of the city of Hampton, and Phoebus was named after a doctor Phoebus, and was established as a sort of a place where the soldiers from Fortress Monroe and the Veterans Administration, which was part of the old soldiers home from World War I, was its main source of financial ... .

Q. Well, it's over on the coast.

A. Yes, right on Hampton Roads.

Q. On the Virginia coastal ... ?

A. Tidewater region.

Q. Farther down South they call it ... you got a "geechie" accent.

A. "Guinea," "guineaman." Guineamen are from Poquoson*, which I guess is a British derivation, I'm not sure.

Q. Sounds Indian.

A. No, it wasn't Indian, I don't think. It could have been, but Poquoson means some kind of inlet, or something like that in some language, I don't know what.

Q. But you were born in Phoebus, Virginia, and you told me your mother was German.

A. No, my mother was from North Carolina, and my father was born in

*spelling per Kraft on the tape

(196) New York City and his mother was a German woman, came over here from Germany.

Q. Oh, it was your grandmother then.

A. Yes, my grandmother came from Germany.

Q. I thought you said your mother, I'm sorry.

A. No, she was a North Carolina woman, born in a small town called Lenoir, North Carolina, which is about 50 miles east of Asheville, North Carolina.

Q. What did your father do?

A. He was in the finance department of the Veterans Administration in what is now known as Kickatan--an Indian name--Kickatan Indians. Kickatan was an old soldiers home from World War I and of course it extended into World War II and is still a part of the Veterans Administration. He went to Mt. St. Joseph's College in Baltimore for a while--I don't think he graduated.--And then got into the Veterans Administration--I don't know what his first job there was, but he found his way into being a clerk in the finance department.

Q. Did you tell me he died when you were fairly young?

A. No. He had a nervous breakdown when I was a freshman in college and was in the veterans hospital in Roanoke for about the last 10 years of his life and he died of a heart attack in that hospital ... never really regained his mind in that time period. He died about 1956, so that was about 15 years later.

Q. Then you went to VPI?

A. Yep. I started there in 1941 and never stopped because of the War. I was there in September of '41, Pearl Harbor was in December of '41 and I had a burned hand which I got when I was three years old ... .

Q. I read about that in Time magazine. I never saw it.

(222) A. Yep, burned that hand.

Q. I never noticed that.

A. Pretty badly.

Q. And you still play golf?

A. Oh yes. Played baseball with it because I'm right-handed. I play golf left-handed, but I'm a right-handed man. That happened when I was three and they wouldn't take me in the service. They put me in limited service, what they call I-A-L, and because I was in college studying engineering, with that kind of limitation, which frankly never bothered me in my life, and never prevented me from doing anything, but there was an old Marine doctor that noticed it. I didn't show it to him, but you know when you have to squeeze your hands together- ... ?

Q. Your right hand's smaller isn't it?

A. Yes, and he noticed I couldn't get it open.

Q. I bet not one person in a 1,000 would notice that.

A. No. As a result of that I finished VPI in a shorter time. I was in the class of '45, but I graduated in '44 and went to work for NACA at Langley Field, Virginia, and then I was interested in stability and control problems so they put me in the flight research organization.

Q. Did you study aeronautical engineering?

A. Yes, studied aeronautical engineering.

Q. So you've been interested in airplanes even before you went to VPI?

A. No, not really. I went to VPI hoping to study mechanical engineering and I really was interested in engines and engine design. I figured I'd end up being an automobile engine designer. The only reason I didn't

(243) proceed with that was because I didn't get much out of the people who were leading the mechanical engineering department, and there was a guy named Seltzer who impressed me because I took an elective to study aerodynamics my sophmore year--the last part of my sophmore year--and I liked him and decided I would get more out of college if I ~~studied~~ went into aeronautical engineering than I would out of mechanical. Of course now, I'd been raised around Langley Field all my life and I'd messed around with model airplanes, and all that sort of thing, when I was a kid, but I really never had any real desire to be an aerodynamicist until that part of my career happened.

Q. How far is Langley from Phoebus?

A. Just seven miles. As a matter of fact, my uncle laid out the runways at Langley Field, Virginia. He did the surveying at Langley Field.

Q. Your uncle Kraft?

A. Yes, August Kraft, his name was.

Q. Well, that's very interesting. You were brought up on airplanes, weren't you?

A. Uh-huh.

Q. Well then, in '44 you went to work for Langley?

A. Yes. January '45 I went to work at Langley.

Q. They were glad to get you ... .

A. Yes, they were. Engineers were darn hard to get and I went to work in the flight research division which was a very progressive organization. As a matter of fact, they had a division chief when I went there who had been Floyd Thompson , who I'm sure you're familiar with, and he was at that time--just about the time I arrived there--replaced by Melvin Goff

(262) who had been previously the chief test pilot for the National Advisory Committee for Aeronautics. And they had a man they called a chief of flight research, and that was Bob Gilruth.

Q. You came across him early, then?

A. Yes. He left about four months after I got there, and went into the hierarchy of NACA and then he was asked to establish the pilotless aircraft research division, which was the first experience that NACA had with rockets. I of course stayed in the flight research division and worked on things like the F6F and the P51 and the P47 and all those World War II airplanes. And I did a lot of analytical work on gust alleviation. I spent about five years doing that and building a gust alleviated airplane, which worked.

Q. Gust ... how do you spell it?

A. Gust alleviated. Gust alleviated airplane. In other words, it was for the purpose of passenger comfort in flying through rough air. A man named William Hewitt Phillips was the head of the branch I was working in, stability and control branch, and he had some notions about trying to improve the passenger comfort of airplanes, so he took me into that world and I did all the work and he did all the brains of the analytical studies of the best way to go about that. Up 'til that time that had been tried by a lot of people in the world, particularly in France and England. I'm trying to remember the young man's name in France ... René Hirsch. He had designed a gust alleviation system and built a small light airplane and put this system on there. It had not been very successful, but he had a lot of good ideas. The British had done it on a Lancaster bomber and a Polack--a Polish fellow--named Jerzy Zbrozek ... I don't know why I'm giving you all this, but it's interesting.

(297) Q. Yes, it's interesting.

A. .... Jersey ....

Q. Polish names come hard.

A. Yes, Jerzy owski. Not Winkowski ... but Jerzy Zabolzak. He was a very famous man at that time in stability and control and he had some ideas on gust alleviation and they actually redesigned the flaps and control system of I think it was a Lancaster, but I'm not sure. It might have been some other bomber. It was one of those big square bombers. And they took it down in Libya, in Africa, about 1946, 1947, 1948--in that time period--and flew the thing and the first time they flew it, it scared the hell out of the pilots and they vowed they wouldn't fly it again. Zabolzak wrote this in a report. They decided what the problem was and hooked it up backwards, that is, that what they had done was fix the flaps--rather, arranged the flaps--so that they would deflect and counteract the lift due to the gust so that the airplane would not change its lift when flying through a change in angle of attack due to gust, but they had not taken into account--and that was the whole thing I'm coming to here--they had not taken into account the pitch effect that this would have on the airplane, and as a result when they started deflecting these flaps very rapidly due to the changes in the angle of attack, the airplane pitch characteristics were just absolutely terrible. It magnified them, and it scared the hell out of the pilots. Zabolzak figured that out, and hooked it up backwards, and flew it, and it of course increased the normal accelerations that you got, but it smoothed out the pitching, and the pilots liked it. Well, we took all that background, and between most of Phillips' ideas and some of mine, we figured out all the problems, analytically,

(324) that you run into when you're trying to alleviate the gusts--not the gust loads, now, but the gust accelerations--on the wing and the airplane and we figured you not only had to account for the lift changes but you had to account for the stability and control changes, or the pitching characteristics, of the airplane. So, we designed a system on paper that would do that and I took that analytical study and completely redesigned a C45 airplane--a two-engine Beechcraft, with a twin tail--redesigned it aerodynamically, and then got together with a bunch of design engineers at Langley, and mechanically redesigned it and modified the whole airplane, put in a whole new control system and an automatic control system and built it in the shops, and then we tested it on the ground--~~both~~ structurally and we measured its frequency response characteristics--and then flew it. I flew in it many times, and it worked, and it alleviated about 80-85% of the gust accelerations on the airplane.

Q. Was this system still in effect ... ?

A. Well, nobody's every used it. There are many reasons why they haven't, of course. It's probably an expensive system to design, and it would have to be designed into the airplane from the beginning to take into account the changes in loads on the airplane, because it does significantly change the load patterns on the airplane. However, there are a lot of studies being made today, at both Texas A & M and in the Air Force, to still try to do gust alleviation. As a matter of fact, the B1 bomber has a load alleviator on it, which is not too different in principle than the system we designed.

Q. You were all of 23 or 24 at this time?

A. Well, no, let's see. I was older than that. I was about 27 to 32 when we did all that work.

(354) Q. You stayed at Langley, then, until the space program came along?

A. Yes. I was working on the F8U. That's an interesting story. Let me tell you briefly about that. I was working on the F8U airplane, ~~because~~ Chance Vought built that airplane, and the Navy in 1956 -- Crusader, was the first Navy supersonic airplane. They were having a lot of problems with it, not serious problems, but a number of detail flying qualities and aerodynamic control problems with it--so they gave that airplane to NASA and asked us to test it and to give them suggestions on how to fix some of their problems. Those are very interesting, but I won't go into those. It turned out that the man in the Navy Bureau of Aeronautics who held the position of chief of the fighter desk--normally a Navy lieutenant or sometimes a captain, but in that grade period--turned out to be a Marine captain named John Glenn.

Q. No kidding.

A. He and I worked together on the F8U and he and I were working together--he was working for the Navy, of course, and I was the project engineer on the F8U for NACA--when he made the cross-country record in the F8U.

Q. He was one of the few astronauts who was known before he became an astronaut.

A. That's right. That was my first experience with John Glenn, so I knew him well before we got in the space program.

Q. And then of course the crisis came with Sputnik in '57. What do you remember about that period?

A. Lots of things. About that time period, if you've talked to enough people about the Mercury program--if you talked with Chuck Mathews or Bob Gilruth,

(382) in particular, or Max Faget, you find that in the summer previous to the time that the space agency was formed (it was being formed at that time), in the year previous to that and in the spring and summer leading up to that, we at NACA were working on three major techniques for re-entering vehicles. One of these was the blunt-body concept, which was the sort of championed by the Ames Laboratory ... I'm trying to remember the name of the guy who wrote the report ... Al Eggers. Al Eggers wrote a report on the advantages of using the blunt-body concept and turning a lot of the energy into drag. Max Faget sort of championed that configuration with a great deal of support from Gilruth. There were two other concepts: one was called the glide concept, where you came into the atmosphere very slowly, gliding in with a very high aspect ratio wing--and a fellow at Langley named Johnny Becker who was sort of running that part of the program; and then Chuck Mathews and myself and Phillips and a couple of others in the flight research division were designing a delta wing airplane which resembles the present-day shuttle. Those three configurations were sort of in a horse race for what would be chosen as the first manned spaceflight vehicle. I'm sure Gilruth and company, particularly Faget and Mathews, who were working with Gilruth that summer--Gilruth was spending most of his time in Washington working on the program--they were sort of getting NASA (NACA which in a few months was going to be NASA) into a position to be able to accept the challenge of what to do about how to get the first man into space. Obviously we chose the Mercury configuration, which was the blunt-body concept, and that was a very wise move.

Q. Well now, how did H. Julian Allen figure in ... he's generally given credit for the the heat shield ...

(416) A. He was at Ames, and he along with Eggers--see, Allen was sort of in charge of all that aerodynamics work, but Eggers was I guess Allen's help or vice versa, I don't know you'd have to look that up in the report system of NACA which is readily available in a bunch of reports that were written at that time--was at the Ames Laboratory, and that's where that concept came from, sort of the blunt-body concept.

Q. I believe that Eggers contests some of the claims that were made in behalf of Allen.

A. I wouldn't doubt that, because as I recall--and I might be totally wrong about that--if I were going to guess, I think Eggers wrote most of the reports on all the theory behind the ... might have been Eggers and Allen but I don't remember but you could look that up very easily.

Q. I believe that the history of Mercury, "This New Ocean," mentioned that there is some doubt about this but Allen is certainly given the credit, generally speaking.

A. Right.

Q. Well then, how did you get into Mercury?

A. I had worked with Chuck Mathews on a number of different programs in flight research, a lot of interesting programs. I'd worked on the X-1 work; we did a lot of work on the X-1. As a matter of fact, the whole test team from X-1, including Walt Williams--see, Walt Williams was in that branch--came out of that one small branch of people and Chuck and I had worked very closely together on many occasions. We had a very mutual respect for each other. When they got the word that we were going to form a space task group to go into this program, then called by the Air Force the MIS program, or

(442) the man in space program, I guess it was about August of that year, Chuck Mathews approached me and he said, now I know you're a native of this area and probably aren't too interested in leaving this area, but we're about to go into this new program, new space flight program to develop the first manned spacecraft, and we've been looking at the people across Langley and I've recommended your name to Gilruth as being one of the people that I would like to have in the operational aspects of this thing. You ought to give this some consideration and tell me whether you're really interested in doing this job, recognizing that you're probably going to have to leave here eventually, if we do this. And so I thought about it overnight and told him I would do it. Frankly, I was looking for a new challenge at that point in time.

Q. Where did he conceive of your going to from Langley?

A. He didn't have any idea of where we would go. I think that he was smart enough to realize that the space parts of NACA--which was soon to be NASA--would probably require a different location and a different center, the establishment of a different center.

Q. He was anticipating MSC?

A. No, they were anticipating the Goddard space flight center.

Q. Oh, that's right, it was going to Goddard, wasn't it?

A. Yes. The space task group was a part of Goddard, initially. Have you ever talked to Gilruth about that?

Q. No, I haven't gone into those early days yet.

A. You ought to do that because that's a very interesting thing. Harry Goett was brought in from Ames to sort of manage that place and Gilruth was invited

(469) to a lot of staff meetings up there because he was sort of a division of the Goddard space flight center--he never attended one of them. I think Gilruth was aware that this thing was bigger than anyone else thought it was.

Q. I remember this about Goett. Webb told me once he had to fire Goett and I forget just what the reason was. Well, he didn't want to take orders, apparently.

A. Goett was one of these cry-baby type of guys, but an extremely capable man. He was always complaining, always thinking that somebody was trying to do something to him. One of those typical individuals, but a very, very bright man and a very capable leader, and a damn good manager. Frankly, I was sorry to see him go out of NASA. But he's right, he was ...

Q. Is he still alive?

A. Oh yes. He's sort of the chief scientist with the Philco-Ford Corporation in Western Development Laboratories in Palo Alto.

Q. Well then, of course the space task group was the Mercury task group. This was your assignment.

A. Right. There were about 25 engineers assigned to the program at that point, in October of 1958.

Q. Which was the date of the founding of NASA?

A. That's right. Just about the same time that they founded NASA, we founded the space task group. The first thing we did was write the specification for the spacecraft, and evaluate that vehicle, that proposal. That was where we spent most of our time. Chuck Mathews and myself and Howard Kyle and John Mayer and Merritt Preston, who came in from Cleveland, they had about 20 guys who came in from Cleveland to join us about three or four

(500) months later--we were sort of the nucleus of establishing the operational requirements for flying this thing. Howard Kyle and myself very rapidly ... just about at/the time the evaluation work was over for the choosing of the contractor, and choosing McDonnell, and I worked on the stability and control and the automatic control and the re-entry characteristics of that part of the evaluation...but very rapidly it was obvious that we needed the so-called worldwide network for supporting the operation and all the radar systems we needed for tracking and obtaining of data during the flight and the need for communications with the crews and the types of data. I had a lot of experience in that, in flight testing airplanes, so I was sort of a natural for that. Howard Kyle came out of the instrument research organization and he knew a lot about radars and instrumentation and communications and antennas and all that ... the intricacies of the electronic communication systems. He and I with Chuck's guidance--because Chuck was then sort of the head of operations, and we didn't have many people to work on the program, but there were a few of us--and we established sort of the basic specifications for this network system and the general numbers of stations we needed around the world and where generally we needed them, based on the trajectory studies that John Mayer was doing on what was the optimum trajectories. As you know, we had already chosen the Atlas vehicle to do it with, so knew the performance we were going to be dealing with and we knew where we were going to launch from, of course, as that was established very early. So, we established sort of the optimum performance trajectories and we talked many times about what the design mission was, and that turned out to be three orbits, and that was primarily based on our ability to

(534) determine the orbit within some reasonable capability so that we'd know where and when to fire the retro-rockets in order to get it down within some reasonable predicted place. All of that formulated the basis of the design of the world network.

A. Oh yes. Q. Q. Well, you were plowing completely new ground in this case./ The Air Force had nothing like this before.

A. Well, that's not totally true. The Air Force did have some unmanned spacecraft that they had started working on. They had the Agena that they were using to put up the Discoverer program. If you remember, the Discoverer program was a recoverable spacecraft for taking pictures of the earth. They had a sort of skeleton network, but their capability was far from real time--that is to say, they had a much delayed system which had many delays in it for determining the orbits and also determining the command and control that they had to do. Of course, we analyzed that from the manned point of view.

Q. But they did have a station, say at Johannesburg, or ...

A. No, they had a station at Kodiak, Alaska, they had one in Vandenberg /AFB/, and a couple of other places which I don't remember.

Q. Well, they were geared for orbits over the poles.

A. That's right. They were going primarily into polar orbit. That's what you would expect when you want to start taking pictures of the earth.

Q. But you had to set up stations in Carnarvon, Australia, or ...

A. No, initially it was at Muchea. Muchea was just north of PERTH. Of course, once you start looking at this stuff and start thinking about orbit determination, the best place to have a station is exactly 180 degrees in the orbit from where you launch, because that's where you want to know what the

(562) apogee or perigee/or what a good hack on what the trajectory is so Australia just turned out to be a natural for that. Of course, you wanted to fly northeast from the Cape because you didn't want to go over Russia, you didn't want to go over Red China. You had these kinds of constraints, and you didn't want to go too far north because of the increased performance requirements of the launch vehicle. So, these sorts of things just sort of dictated a 32 1/2-degree inclination. You wanted to be able to recover in the Atlantic and you wanted to have a recovery capability at the end of each revolution. These kinds of things began to shape the basic requirements of the network, so that was the reason for the choice of the ... As a matter of fact, I wrote a hell of a good speech on that you ought to get. I gave it in 1959 to the Society of Experimental Test Pilots. It was sort of the basic reasoning of why we built the network like we did--sort of the basic design requirements on the basis of the mission we were going to perform. And then how many orbits we were going to fly and why, and that was based on what we thought man's capability was. And did we want to stay up a minimum time for the man's response to zero G, but we also wanted to stay up enough time to be able to establish a reasonable orbit, and figure out what we were doing, and do it right. That turned out to be three orbits.

Q. Well now, how did you know at that time ... I suppose well-known by laws of physics or gravitation or something ... that when you fired a spacecraft down at Cape Canaveral that it would go over the Canary Islands and then dip over Africa and then down south and back up to Guam?

A. That was pretty simple. You just figured the orbit of course is elliptical or basically circular or elliptical and you just atart putting the thing in a plane and you cut that plane across the earth and there it is, you know.

(596) You just see that it cuts this Japanese torture finger thing--if you can get enough orbits--that's just the way it comes out across the earth. You lay that out and lo and behold it's that kind of thing, you know. It's something that none of us had ever done before, but it became readily apparent as ...

Q. You couldn't vary it because ... well, Gordon Cooper flew across the Himalayas.

A. Yes, but only because he went for 27 hours, you see. If you go northeast out of the Cape, and you only go three revs and the rotation of the earth is only just 15 degrees every rev...

Q. Oh, I see. Then ...

A. The earth rotating under the spacecraft. The spacecraft's in orbit in roughly a circle going around the earth and the earth is rotating under it. So eventually, if you let it rotate long enough, that thing just keeps reversing and so it ends up going over the Himalayas if you go for enough orbits.

Q. Isee. I didn't realize that.

A. Our design mission, you see, was just three orbits and that's all we ever designed this thing for. It was only when we began to see that, well, we want to fly more than three revs, that we said, "Okay, now what are we going to do?" That's when we started building ships, as a matter of fact--because then we saw the need for some stations in other locations around the earth. Basically, we said to ourselves, "Well, how often do we want to be in contact with this spacecraft?" And we said, well, when an airplane flies across the United States, it has a checkpoint about every 15 to 20 minutes. And we said, well,

(623) that sounds like a pretty good design criterion, so that's what we'll say. We'll say in the first three revs of this thing we'd like to have a contact a minimum of every 15 to 20 minutes. It was that simple.

Q. Which would mean about six stations.

A. That's right. Therein lay the desire for stations. Now, you can say, well, I want to have one very close to the time of insertion, because I need to determine its orbit rapidly, because if it isn't in orbit I've got to do something about it and get it down and I want to bring it down in the water rather than in Africa. So I've got X number of minutes to make a decision to turn our spacecraft around and bring it down, fire the retro-rockets, and I can play little games there with when I fire the retro-rockets to make its landing point. It was obvious that the best place to put that was Bermuda, and we could aim the trajectory that way. Having one in Bermuda, another 5 or 10 minutes after that is the Canary Islands and that looked like a good place. And then Kano , Africa was another 10 or 15 minutes, and Zanzibar, Africa, was another 10 or 15 minutes. And then the Indian Ocean is a pretty bare place so you want to put one as soon as you can on the West Coast of Australia, and that looks like that ought to be somewhere around Perth, and you don't want to put it in a city, so you put it about 40 miles north in a place called Muchea. We went over and talked with the Australians and they already had a tracking place at Woomera ⟶ so we figured, okay, we'll use that, 'cuz it's already there. And then, okay, the next place we'll probably look at a place over here in the Solomons, and that turned out to be Canton Island. And then it comes up over Hawaii and Hawaii was just too far north to pick up the first

(651) revolutions, so we said we'd better have one off the coast of North America, and what's the best place for that and that's Guaymas, Mexico. In another 15 minutes is Texas and then you get back to the Cape, and there you go.

Q. The sites really were picked out for you anyway.

A. Yes. Just figuring out where the thing was going to cross and started looking at a map and saying well I want one here and I want one here and I want one here. And then we gave these requirements, basic requirements of what I've just described, a lot of these operational mission characteristics that we had figured out in a few months' time, and gave that to the people--we figured that/took a specialized group of people--and told Silverstein about that, he was the boss at that point. We said we need a group of people to do this and so he looked around and said, well, I'll give that to Langley to do. And that's where Buckley got chosen, with a group of people out of that instrument research organization over there, to form a nucleus of people to build it. They wrote the specifications for it and selected the Western Electric Company to build it, under competition. /Pause/

Q. Well now, in the Mercury program, you sent out ...

A. Let me finish ...

Q. Oh, I'm sorry.

A. I want to get at something there. That was the first time I met Dr. Fletcher. Soon after we'd gotten on board the Western Electric people, it was obvious we needed a center of operations and the best place for that was at the launch center, and so we said we want to build a control center at Cape Canaveral. We were the best people to do that--we, the space task

(680) group, as separated from the people who were building the network. They said well, you guys know what you want there, so why don't you get a study contract with somebody to come in and help you design that thing. We chose the Space Electronics Corporation, which was directed by Dr. Fletcher and another fellow. I never had any dealings with him at that time but he was the man who started that company.

Q. Yes, I've heard him mention the company, but I haven't heard him mention this ...

A. He didn't have much to do with it.

Q. Well, now, in the Mercury program you sent out a certain number of astronauts to these positions to man one of these things.

A. Yes.

Q. ... but not all of them?

A. No, we didn't have that many, and so we decided early--Walt Williams and myself and Chuck--that the guys who know how to do the communications, who are familiar with the spacecraft, and know best how to interpret the instructions from the ground into language that the guy on board will understand--because he's got to understand the systems of the spacecraft and the systems of the ground--we want a communicator in the control center who does that and we'd like to have, in the most appropriate places and the ~~mexk~~ place where we're probably going to end up giving the most instructions, we'd like to have an astronaut. So we chose Guaymas because that's where retro-fire would be apt to take place. It was on the first revolution. Hawaii, if it was in preparation for retro-fire on the second and third revolutions--we had a station in California, by the way, at Vandenberg /AFB/ and we put one there on occasion -- and Australia, because it was half way around and was probably

(712) the place where you were going to give a lot of instructions, too.

Q. I see. You put them then ... when you thought about coming down ...

A. In the strategic ... right. And the place where you could get the most information up to them. And then also Bermuda, because it was apt to be an emergency place where you had to give emergency instructions if the missile had failed to get it into orbit, for some reason.

Q. I see. So then on each Mercury mission you sent out about four astronauts?

A. Yes. Never could get John Glenn to go until after he'd flown. He refused to go. He said, I can't see that that's an important thing for me to do. Never could talk him into going. But of course after he flew he found out how important it was and he did go.

Q. Yes, it's strange now, you know, to read the transcript of these Mercury flights. /It's usually two or three pages for each mission--astronauts talking to the man who's in the spacecraft. Well, then, I think your career in Mercury is pretty well established. /Blank in tape here./ You didn't think Gemini was necessary and you say none of the people at Apollo ...

A. Well, that was probably somewhat of a gross overstatement, but the people in Apollo thought that they were going to bring that spacecraft on a lot faster than they could. I think that Gilruth, with the inputs of Jim Chamberlin and Walt Williams, realized that that probably was not going to be the case, that the technology required to bring the Apollo into being was a little further ahead than the people who were doing the job thought. And he was right. And you put that together with the desire to maintain the program, and you put that together with the lack of operational experience

(748) of the organization in dealing with complex trajectory problems, particularly, such as rendezvous, which we'd obviously decided was a necessary part of the Apollo program, which was a very interesting story you ought to get from Gilruth or Chuck Mathews, who know it very well, about the decision for doing lunar orbit rendezvous. But having made that decision, it was obvious that we needed the operational experience of how to do all those operating problems. So you put all that together and that spell the Gemini program. The best way to get at that was sort of a big Mercury, or just taking the technology that you had from Mercury, and some of the experiences you had from Mercury, both operational and design--mechanical design, spacecraft design--and that spelled a good program. I don't think that many of us realized the large--I say large, but it wasn't large in today's world--the relatively large cost of the Gemini program. I think we thought it could be done for not much more than the Mercury program cost.

Q. Yes, I think Mercury was about $400 million and ... Gemini was about $1½ million.

A. Yes, $1.4 million.

Q. ... which doesn't seem big compared to the A pollo cost.

A. No, but it was a high overrun program because the initial cost estimates were pretty crude and not generally known.

Q. I suppose it would have been possible to get your rendezvous techniques developed in Apollo as well as in ...

A. Oh yes. And that's what the Apollo people would have you believe, and they were right if they could have gotten the Apollo spacecraft flying early enough.

(779) Q. Otherwise, you'd have had a three- or four- or five-year gap.

A. That's right. You see, it probably would have been longer than that because they were trying to take the next generation spacecraft and build not only all these operating characteristics into it but also the capability to go to and from the moon, with the high-speed re-entry requirements and the need for a lifting re-entry, because that's the only way you could survive. And that's what prompted the lifting re-entry in Gemini, of course--to gain some experience with that technique. All of those things added up to too much of a technical jump.

Q. And your astronauts would have been too old to ... they'd have been rusty.

A. Well, the whole operational team needed some place to cut its teeth. That's very true. That's the point I was trying to make. It was primarily one of operating problems, but to some extent also the technology required.

Q. I think that history will prove that Gemini probably was necessary.

A. Yes, I think so.

Q. But the diehards, like

A. I think it's true that the number of flights of Gemini was probably somewhat stretched out, but I think that the amount of investment was relatively small so then it made it worthwhile. You wanted to learn the following things from Gemini: You wanted to learn the operating problems of rendezvous; you wanted to learn the problems of the lifting re-entry--that is, the guidance and control and the computer control problems of the lifting re-entry, as well as the aerodynamic and the heat shield problems; and then the third major objective of Gemini was the long-duration capability. It gave us the capability of determining in the Gemini program what man's

(816) capability in terms of zero G flight was without having to inject that as a limiting part of the lunar mission, and we decided that that should be 14 days since most of the trajectories we were figuring out those days for lunar flights were somewhere between 10 and 14 days. So we said, okay, 14 days is the limit so we'll approach that in Gemini in determining that man can truly survive for that length of time under zero G conditions, and we'll leave the Apollo program with that problem. Then, of course, eventually extra-vehicular activity was added to the Gemini program, because you were going to have to do that on the lunar surface. So, how was man going to perform outside of the spacecraft? So, all of those things were objectives of the Gemini program. One of the other objectives, which we decided to give up, was the land landing. When we got into development problems with putting that paravane on the Gemini program, that ended up being just too much of a development part of the program and we gave it up.

Q. Well, the Russians have never had anything but the land landing. Well, I guess they had one water landing, in the Indian Ocean.

A. Yes, but not manned.

Q. Yes, one of their ...

A. No, that was their lunar spacecraft.

Q. Was it? I thought they had landed once in the ocean, with men.

A. No.

Q. I know they had the ships down there ...

A. Yes, that was for the lunar flights.

Q. You got on through Gemini. Of course your own personal role ... you began to quit being the sole flight director, somewhere in the Gemini program.

(851) A. Yes. That was a tough decision for me to make. We built the ~~Mercury~~ mission control center in Houston. We designed and built that, and that came under my cognizance. We were beginning to plan the details of Apollo, all the operating problems of Apollo. We had gotten to a point in the Gemini program where we needed to start plowing in the experiences of both Mercury and Gemini into the operational planning for Apollo. I think the top management was concerned about me being able to do the management job of all those things that were involved, and still be in a position to have all the knowledge necessary to do the details of flight direction. I mulled that over for a few months, and I think Sig Sjoberg probably had as much to do with that as anybody. He just talked to me about it on several occasions and felt like I really didn't have the time to do both. If I was really going to be a manager, I'd better get out of the flight direc. business. And that was about that simple.

Q. But all the time until Walt Williams left, you reported to him, did you?

A. Yes.

Q. I think I told you that ... well you probably knew this already about Shea's demand before he went to Houston. His first demand was that Williams be got out of Houston, in September '63, or something like that. This was when ...

A. And George Mueller took that upon himself.

Q. I'm told though that Gilruth asked for Shea to come to Houston, probably at the instigation of Jim Elms. Does that jive?

A. Yes. But there were a lot more intricacies involved in Williams' departure from Houston and from manned space flight than just the mere fact

(894) that Joe Shea wanted him out of Houston. And I didn't know that until you just told me. But I think that had little to do with it. I think there were a lot more intricacies involved, and intrigue involved, in that than Joe Shea thought. In truth, Walt Williams was making a power play to get Gilruth's job.

Q. Yes, I saw where they didn't ... I've seen the documents. They didn't get along very well together.

A. Yes. Well, that's not quite true.

Q. Well, not afterwards, anyway.

A. Oh yes. Well, that's to be expected. But Gilruth brought Williams on board.

Q. Oh, did he? You mean from Edwards /AFB/?

A. Yes. He and Silverstein realized that they needed some strong operational inputs. Chuck Mathews was having a real struggle with handling that part of the program, and they needed somebody to deal with the Air Force at the highest levels. That's when Silverstein and Gilruth invited Williams into the program. In truth, Williams and Gilruth got along very well, technically; it was in terms of how things were done and the management of the Manned Spacecraft Center, and those kinds of things, that Williams got it in his head, I guess, that he was a better man for the job than Gilruth. So he set out to do that and immediately--rather, eventually--he became at odds with him on those matters. It was obvious that it was either Gilruth or Williams. It was also obvious who was going to win that battle. Williams just got himself into a position where he couldn't back out of that thing.

(933) Many of us tried to prevail upon him to give up that idea, but we failed. Well, unfortunately, he didn't get along with Mueller very well, either, but Mueller was then talking about having somebody run the operations from the headquarters level. You know all his theories on how to do that. So he tried to get Walt to go up there, and Walt of course knew that that was impossible ... Besides the fact that he didn't want to go up there ... besides the fact that he knew that it couldn't be run from Washington, he knew that that was the beginning of his end, as far as the possibilities of his taking over the manned spaceflight program -- as far as Houston was concerned, anyway. But there were several of us who prevailed upon him to go ahead and take that job. He wanted to quit at that point, and we talked him out of that. We told him if he wanted to go someplace else he ought to go from a position of strength, rather than a position that ... and he did.

Q. Well, he stayed only about three or four months?

A. Yes.

Q. He got this $50,000 a year lifetime job with ... /Aerospace Corp./. Gittings was getting $125,000. That's a good profitable place to work. I think it's been cut back a lot now. Walt Bonney has retired, by the way. He's in Washington now.

A. I didn't know that. I knew he'd retired, but I didn't know where he was.

Q. Yes, I had lunch with him a couple of months ago. .. still around. I think Walt probably did a lot of harm to the aerospace program and inventing this Life contract.

A. Was that his invention?

(967) Q. Yes, he thought this up. He was a pal of Leo DeOrsey's, you know. As a matter of fact, he called me up. I guess it was DeOrsey who called me up (I was the managing editor of the Saturday Evening Post in '59) saying, "How much would you bid for the astronauts' contract?" And Life eventually bought it, you know, for about $500,000. I have no doubt that the Life contract probably was the incentive behind such capers as the Apollo 15 thing.

A. Well, I agree with you. I think that most people have put the blame on the astronauts themselves for the prima donna position that they found themselves in, and I think NASA itself has to absorb a lot of the blame for it, because I think they put them in a place of reverence or adulation, or whatever you want to call it, and sort of promulgated that situation.

Q. Well, Kennedy came in and announced, you know, through Pierre Salinger, that there'd be no more of this now. But then John Glenn went up to Hyannisport in the summer of '62 and talked Kennedy into ...

A. That's right. I remember that very well.

Q. It probably was a bad thing ... they lived ... I don't know how else you could have solved it. These guys needed more money than they were getting, of course.

A. Why do you say that? I didn't think so. What the Government should have done, and still is remiss in not doing, was to buy them a basic insurance policy which would guarantee their families' future in some failure or some accident, regardless of what it was--whether it be an airplane or otherwise. That would have been the basic way to solve the problem. Of course, that was difficult to do, I guess.

Q. Because of the Vietnam war?

(1,004) A. Well, because of a number of things. Regardless, that's how it should have been done.

Q. Yes. Albert Thomas tried to work out something on that once, but it wasn't ... Well, now we've got you ... we went through Gemini rather fast, but you think there probably were too many Gemini missions?

A. Well, no. I don't say there were too many. I say that we probably could have gotten all that we needed out of the Gemini program without doing as many flights as we did, because we tried a lot of different rendezvous techniques in various forms. We did some EVA's that were ...

Q. But the EVA's didn't really get settled down 'til about the last two missions, did they?

A. That's right. I don't think people really appreciated the detailed planning and scheduling of the man's time when he was outside of the spacecraft to do that. I think that all of us, including people like Chuck Mathews and Gilruth, really believed that EVA is an evil of the space program rather than something which should be done in the space program. I mean by that that doing EVA at zero G as compared to doing EVA on the lunar surface bears no resemblance.

Q. Oh, really? I didn't know that.

A. There's no comparison between the two. The only comparison is the life support system required to keep the man alive in the pressure suit. That technology is the same, and the mobility of the space suit helps. Doing extra-vehicular work outside of the spacecraft should only be used when all else fails, because it's just too damn dangerous. If you can do it otherwise, with manipulators, which is the way we're going to do things now on the shuttle--you know, these arms that have been developed through the years

(1,047) for a number of different things--those kinds of things are just a much better approach to working in space rather than having a man strung out on the end of a string holding a wrench in his hand. You know all these pictures you see of men outside spacecrafts and space stations putting things together. All that is a lot of malarkey. You just ought to do it, if you can do it, in a different way. Now, I don't mean to imply that the EVA's we do today in Apollo to get the film packs off of the CSM ... there's nothing wrong with that. That's fine. It's the best ... They get out there and they've got a job to do and it's easy to do and it's relatively easy to train for it and you make the equipment easy for them to handle. And you can do the same thing in the ATM in getting film there. But, in truth, having man float around like Buck Rogers is still a myth, in my opinion.

Q. But it has such an appeal to the public.

A. I know it. It's terrific. But it's just too damned dangerous--if you don't <u>have</u> to do it.

Q. But you think it is necessary in the CSM?

A. Yes. I mean, there are applications of that that should be used. And there may be applications in the future that require man to get outside the spacecraft under those circumstances.

Q. But didn't we learn anything from the Gemini EVA's that was helpful in developing the space suit for lunar surface work?

A. Not a hell of a lot, no. Well, yes, we learned some things. We learned that we ought to have water cooling instead of air cooling.

Q. Did you start with air cooling?

(1,082) A. Yes, all the EVA's in Gemini were air cooled. You just pumped an inordinate amount of oxygen into the system to cool the body. The body cooled itself under those circumstances by perspiration in its normal process, whereas if you cool the body by radiation--which is what you do with the water cooled suit--it's just a hell of a lot better way to do it. The body never sweats, until you get to the point where the water can't take away all the heat, which happens sometimes on the lunar surface.

Q. I didn't realize that. That's very interesting.

A. But I don't think we necessarily learned that from Gemini. We would have learned that from work inside a vacuum chamber, that that's the best way to do it. As a matter of fact, I guess we had it on the drawing board anyway at that time.

Q. I see. You know, we talked a minute ago about LOR--and I believe we mentioned this the other night---you mentioned once that Chuck Mathews was the one who had convinced von Braun that LOR was the way to go. I've never been able to pin that down. You told that to Don Neff in an interview, page 5, some three years ago. I went to talk with Chuck about that. I had a long talk with him--an hour and a half or two hours--but we never got around to that particular subject. Just how did he convince von Braun?

A. In all the ups and downs Chuck Mathews had in the space world, one of his downs was ... Williams came in and they weren't too compatible, I guess, and Gilruth needed some work done in other places and they decided that they would take Mathews out of the operations business. At that point, he went over and became the head of a division which I think at that point was

(1,131) called the spacecraft design division. That's where all the basic systems planning for the Apollo spacecraft was going on. That was when we first moved to Houston, and that was where the program was going through the machinations of the fact that the spacecraft, to land on the moon, to be a single spacecraft to go from the earth to the moon and come back, was just getting too big, and its "landability" characteristics on the moon were bad. You ended up having to have a rocket on the moon as big as the Redstone--that tall--and in order to have a stage to come back to the earth with, direct, that was just not plausible. And that's where Houbolt from Langley had come up with this so-called bug idea, where you had a very small, very light vehicle that went down just to get the man on the surface. Faget picked that up and Mathews then began working on it, and we began working on the details of the lunar orbit concept, ~~lunar orbit rendezvous concept~~. Gilruth of course was very happy with that idea and he was trying to sell it. ←——I don't really know the details surrounding this meeting, or the preparation for it; you'd have to ask somebody else besides me because I wasn't very intimately involved, I was so deeply involved in Gemini at that point--in Mercury, as a matter of fact. Mathews got together a presentation --put together by a lot of our operations people and a lot of his design people--and had the responsibility of going to Marshall and I think they figured that (and I don't know this; that's why I say you have to get to the surroundings of this meeting) if they could get von Braun to say that it was a good idea, they could carry the day. Mathews had the responsibility of going over to Marshall and briefing von Braun on all of those ideas. I'm

(1,192) sure it was his engineering abilities and his strong technical capabilities that convinced von Braun. That's as I saw it. I might be wrong about that whole situation, but that's as I saw it. There may have been a lot of other things going on under the wing somewhere that I didn't realize, but as I saw it from a distance, that's what I saw happen. I was in on the reviews that Mathews was giving here in Houston before he went over there; a lot of the people who were doing the work for Chuck, were preparing that work, were working for me at that time. So that's the way I saw it.

Q. You probably know this--speaking of Mathews' ups and downs, and he certainly has had them: When Gilruth and Low became convinced after the fire that somebody had to replace Shea, they proposed that Mathews have the job. But hell, he'd just finished Gemini and he'd just moved to Washington, and he said, "I just can't ask my wife to move back to Houston."

A. Oh no, there's just no way.

Q. But think what a difference it might have made in Mathews' career if he had been able to take the job that Low took, and of course Low was led on to greater glory. Poor Chuck, the man who just couldn't take the job that would have made him.

A. Well, his wife has been a problem to his career ever since he's married.

Q. Time after time, things like that have happened to Chuck, apparently. Well, he's got a pretty good job now, hasn't he?

A. Tough job.

Q. Incidentally, to skip around a little bit, George Low mentioned to me one day that he didn't think that Sam Phillips had really flourished in his job until after the fire, that he was always constrained. He didn't say by

(1,247) Mueller--I judged by Mueller.

A. Well, I think that's true, and I think that Sam Phillips himself never asserted himself up until that time--I think because of the strength of Mueller technically, and together with Mueller's position in the organization--Phillips found himself in not a very strong position. I've heard Sam say himself that after that point in time, Webb called him in and told him, look, you've got to run this thing. If I read the tea leaves right--and I might well be wrong here again, because I saw it from afar and I'm probably somewhat prejudiced--I think that Webb probably felt that the best thing to do would be to get Mueller out of that job at that point. But I think that he felt that politically that was the wrong thing to do.

Q. I think that's right.

A. And as a result of that, I think he said: well, I'll do the best I can with this situation and I'll make Phillips my man. After all, Phillips is the program manager for the whole program, so he's the guy I'm going to look to to run the program. Sam told me he told him that at that point. I think that was what decided Sam to say to himself, okay, by god, that's what I've been told to do, and I'll do it the best way I can with the circumstances I've got to live with.

Q. Yes, Webb at that point was taking everything away from Mueller, including the contracting ability. Strange thing. I asked Webb one day, I said, "If Miller was so bad there, why didn't you get rid of Mueller?" He said, "But I needed him."

A. Yes, I think he did. I think that Mueller was a right reasonably strong individual with the Congress at that point. People like Teague and the

(1,300) Senate space committee had great respect for Mueller, and rightly so. Look, you can't deny that Mueller is a tremendously capable man.

Q. No.

A. I think that everybody would say that. I've never dealt with a more capable man, in terms of his technical capability. It was his managerial capability, his lack of perception of people, or his lack of talent in drawing the best out of people--he didn't know quite how to do that.

Q. Do you agree that if it hadn't been for Mueller's pushing all-up testing, we probably wouldn't have gotten to the moon by 1970?

A. Gee whiz, that's a tough question to answer. We didn't do all-up testing. Everybody says we did, but we didn't.

Q. Well, you didn't do it here in Houston, but you did in Huntsville, anyway. I mean, you didn't fly Saturn V after Saturn V after Saturn V.

A. No, but we flew Saturn IB's and we flew a dummy ...

Q. You flew three, two unmanned and one manned.

A. Yes, okay. But I think that was the result of the fact that the Saturn V came along faster than the spacecraft in terms of catching up with it as a result of the fire. That was an interim thing that happened. If we hadn't had the fire, we would have launched a whole flock of Saturn IB's with spacecraft on them. And they were all planned. There are a whole lot of stories behind that. But we fired the first Saturn V ... was launched full of sand, for god's sake, if you recall.

Q. Yes, in November of '67.

A. Yes, and we had the unmanned vehicles--you can call them all-up if you wanted, but they really weren't. I think the progressive test that we ran

(1,364) in Apollo--you can say that that was all-up, but it really wasn't. I think it was a step-by-step approach which was pushed very, very strongly by Houston and opposed by Mueller and sold by Phillips. /END OF TAPE ONE, BEGINNING OF TAPE TWO/ I would have to say this about Mueller : I think that his pushing of the schedule probably had a great deal to do with the progress that the program made. I'd have to admit to that, but I'm not sure that that was the right way. I'd say that that was a way, and it came out okay. There are some things that I wouldn't say to even you about my feelings about some other matters there relative to his pushing the schedule.

Q. Well, he knocked out the E mission. Of course, he wanted to knock out F, too.

A. He didn't knock out the E mission. We knocked out the E mission because we did the C prime (C') mission and there was no longer any need to do the E mission. We accomplished all that we had set out to do with the E mission, with the exception of carrying the lunar module into a lunar environment. So, that's not true. He didn't knock out the E mission. The E mission got replanned because of the fact that we did the C' mission.

Q. Yes, that's right. But I thought it was his insistence that ...

A. Hell, no.

Q. It wasn't?

A. No, sir.

Q. I hadn't realized that.

A. Now, he fought boldly to prevent the Apollo 10 mission from being flown. He didn't want that flown at all, but we prevailed there with Phillips too.

(019) Q. And it was a good thing we did, didn't we?

A. Hell, if I were to go back and plan the damn thing, I don't see how we could've done it any better or any quicker, in terms of the steps we took and the things we learned from each one of those steps. You'd never get Mueller to agree with that and I would have to say I would respect his judgment, but I would admit that I was probably one of the planners of all that jazz back there at Houston. So I'm parochial, I'm biased. I guess we'll always have a difference of opinion on that. I know his opinion was that the C' mission was a deterrent to the program instead of a step forward. I think that to me says he never did understand the operating problems of a lunar flight, and he didn't. I don't think George Mueller ever really understood what operations was all about. To this day I don't think he understands flight operations. I think that he thought that the things that we did in mission control were entirely overdone. I think he feels like that was a luxury of the program that need not be there, I really do.

Q. I remember your saying once that if he'd had his way, he'd have landed a man on the moon with the first Saturn V shot.

A. That's right.

Q. I've never understood his obsession for worrying about it. The more missions you fly, the more dangerous it is.

A. That's right. That was his argument. And if you'll recall--I've told you about this before, I'm sure a lot of people have--we went through that reliability chart that he had us all prepared ...

Q. No, you didn't tell me.

(041) A. He gave us all a ... we went into the Management Council meeting one day and had this whole chart made out of the various missions and the various parts of the program and he wanted us to write down ... he was trying to prove that what we were attempting to do--a step-by-step thing--was the wrong thing to do from the standpoint of reliability and probability. He gave everybody in the room that day, on the Management Council and surrounding it, this thing and said, well, now, I want you to send me these things in a week. He got them all back and I think he burned them all, because he found out he was wrong.

Q. What date would that have been?

A. It was the summer of '68.

Q. Just before Apollo 7.

A. Yes.

Q. After Apollo 6, which wasn't a resounding success. That seems to me to have been a very bold decision, to fly Apollo 8 without repeating an unmanned flight after Apollo 6. However, this concerns mostly Huntsville, more than Houston.

A. I agree with that. That was a very bold step--to fly a man on the third Saturn V, after the problems they'd had on the second Saturn V flight. I agree with you completely. That was an extremely bold step.

Q. I don't blame Sue Borman for worrying about Frank flying another one after that.

A. No, I don't either. But I guess I felt like that was somebody else's decision to make, and I think George Low did, too.

(063) Q. By the way, in that connection, luck has played a big part in this program, hasn't it?

A. I think you make luck.

Q. Well, there are some things though that you can't say you make. For instance, if we had ...

A. I don't disagree with you, but I think that you make it, too.

Q. Well, suppose on Apollo 8 they would have the same trouble we had with the oxygen tanks on Apollo 13. You're sunk, aren't you? ... Might just as well have happened on 8 as on 13?

A. No.

Q. Why not?

A. Because the failure was the fallacy of man, not the fallacy of the system. There wasn't a damn thing wrong with the goddamn cells, or the cryogenic system. What we did following the Apollo 13 was a goddamn waste of money.

Q. How do you mean? I don't understand. You mean putting the third tank in?

A. No. The reason for the malfunction that occurred in Apollo 13 was because the system had been treated improperly during their preparation phase at the Cape. There was nothing wrong with the basic design of that system.

Q. Well, now, I've always heard that the same thing might have happened on any mission.

A. No, sir. That is false.

Q. Is that in the report they finally ...

A. I don't know whether it's in the report or not, but if it isn't I'll show you where it's wrong.

(079) Q. What was peculiar to Apollo 13?

A. What happened is this: In the heater system of the tanks, there is a a thermostat, which, when the heat of the heater gets to a certain point, it causes a certain voltage and amperage characteristic, which has a thermostat on it which automatically shuts it off. So, the heater, like any thermostat, is going on and off in order to maintain the limits of the power that can be absorbed by the system. And it still has enough heat to provide the heat required to maintain the vaporization of the liquid oxygen in the tank, for flight. On the pad, because of the one G characteristics of the thing--as opposed to the zero G characteristics--and the desire to get the liquid oxygen out of the tank more rapidly (that is, when you fill it, you still want to get it out), you go through this process where you're continuously heating the element. In this particular tank, there was probably a vapor lock, or liquid lock, that resulted in the abnormal amount of heat having to be put into that tank. When they did it the second time, they thought that that was what was happening, and so they continuously carried on with this thing. Well, it turned out that the thermostat that controls this thing was designed for 28 to 32 volts and two or three amps. And they hit this goddamn thing with 65 volts and two amps and it burned the damn points on that thermostat closed. So, instead of the thermostat acting as they thought it was acting--and it was complicated by this leak problem and this lock problem--they didn't realize that the damn thermostat had burned closed and they were continuously dumping the 65 volts and the two and a half amps into that heater. And all that heat burned the damn insulation--the teflon insulation--off the wires.

(109) Q. Well, now, I thought that the same conditions obtained on all the flights.

A. No, sir.

Q. Well, I'll be damned. This is the first I've heard that.

A. There was absolutely nothing wrong with that goddamned design, and the work we went through was a bunch of eyewash. Now, I don't say it didn't make it better, in terms of its mechanical design, but from the standpoint of its operational use, it made it a hell of a lot worse because we hadn't had any experience with the tanks.

Q. And then of course we installed a third tank.

A. Yes. We had thousands of hours of experience with that tank, and it was good and it was well designed and there was nothing wrong with it. And I say that emphatically.

Q. Beech has been given hell time after time for it.

A. And unfairly so. You can take my argument anyplace you want, including Mr. Cortright. Bob. Reller forgot the telephone!

Q. Well, I don't remember any of this being in any of the reports on the Apollo 13 mission.

A. Well, you're wrong. You go read them.

Q. But this was the only one where they allowed the 65 ~~amps~~ ?volts? ...

A. Yes, sir. It was a goof.

Q. Well, whose goof was it? I mean who's responsible?

A. It was just the fact that the guy didn't realize that he had the thing set at 65 ~~amps~~ ?volts?. You have a variable reostat and you can set it on the power supply and you can crank this thing all the way from X number of volts to Y number of volts. Now, this is all in retrospect. They found

(128) this out--they didn't of course know this before they went.

Q. You mean he didn't know it was set for 65?

A. No, he didn't realize that that was what he was doing to the system, but we went back and went through all the jazz and we found out that was the case, and we duplicated the goddamn thing.

Q. You mean, if the 65 volts had been there on Apollo 8 or Apollo 10, the same thing would have happened?

A. No, because there was another malfunction in that particular tank--I don't remember the exact details of it--but it caused a lock in the exit from the tank. It gave the appearance of not getting enough heat into the tank, whereas it just wasn't coming out because it couldn't get out. If you go back and look into the records of the Apollo 13 review, you'll find all that.

Q. I must read it again, because I really never studied the reports, but I have a file this thick ... .

A. Yes, Gilruth and I did everything we could to prevent that from happening.

Q. You mean, in this same oxygen tank?

A. Hell, yes.

Q. In what way? Tell me more.

A. We didn't want the design changed.

Q. And it was changed just before Apollo 13.

A. No. I'm talking about once the Apollo 13 happened, and we realized what had happened, we didn't want that thing changed.

Q. Yes, I remember. Paine insisted on the third tank. Nobody else wanted the third tank.

(149) A. Well, there was nothing wrong with putting a third tank in, because the third tank was put in for other reasons. It was put in because we'd taken the fans out and we were afraid that because of the so-called stratifications you get in the tank, there could be a certain demand on the tank--particularly during an EVA, or if you got a high demand on the tank where those tanks couldn't supply enough gaseous oxygen. That was the reason the third tank was put in...because of the demand problem once you took the fans out.

Q. I see. Well, you told me something I didn't know before. I'm sure in all the public reports about this and all the congressional testimony ... remember they were called up to Congress to testify on Apollo 13? I'm sure that none of that came out at that time.

A. Well, I think it came out, but I don't think it came out as strongly as Chris Kraft would tell you.

Q. I appreciate Chris Kraft's putting it honestly and bluntly.

A. Well, George Low knows how I feel about that thing.

Q. Does he agree with you?

A. I don't know whether he does or not. You know, you can argue that there were other reasons why you'd do it, other than technical, and who am I to judge that. You might have to say you had to do something to show that you'd fixed the tank, and that's probably true.

Q. That's why I think the third tank was probably ...

A. No, the third tank was put in for good reason, for a good technical reason, once you had decided to change the design of the tank.

Q. But you were saying that it wasn't necessary to take the fans out.

A. Well, it wasn't necessary to do anything in the re-design of that tank.

(173) Q. Therefore, it wasn't necessary to have three tanks.

A. That's true.

Q. That's very interesting. By the way, I see I have a note here to myself ... It used to be possible to get senior staff meeting minutes, but apparently the historical people here aren't getting them anymore. Do you know anything about that?

A. We still write them, but they don't mean anything.

Q. How's that?

A. Because all that George Abbey does is tell the ... just give a brief description of the subjects discussed and not the content of the discussion.

Q. In the Apollo spacecraft chronology, which hasn't been printed, they have this interesting note for May 1, 1964--that Dr. Shea reported that fast egress from the CSM by the crew on the pad is not required, which led to an awful lot of complications, didn't it? Because fast egress was what you really needed on the pad. That's in the senior staff ...

A. If you had a fire, it did.

Q. That's in the senior staff meeting minutes of May 1, 1964. Well, how about getting the old copies? Can ...

A. Oh. I didn't really fully understand what you meant. Getting the copies of that I guess would be possible. I don't know where you'd find that, but that would probably be in Paul Purser's files.

Q. Yes, but Purser has been gone for some time.

A. Yes, but they're probably available somewhere.

Q. Who would know? Abbey? Would he know about that?

A. Well, he might not know about it, but he could find out where it is.

(194) Q. Of course, he's not going to be here tomorrow.

A. You might ask Phoncile Devore. She was Purser's secretary. Do you remember her?

Q. That's a fascinating name. I've never heard ...

A. Phoncile Devore. /Kraft spelled it out./

Q. I don't believe it.

A. Wait a minute--I'll give you her telephone number. ...

Q. That is a ...

/Tape went blank for a few seconds./

A. ... were all operational arguments. You know, I argued with him about the damn unmanned landing on the moon, for hours--all of us did, in operations. He had that whole thing going at Grumman and we finally just had to give it up because it's so damn expensive.

Q. You mean, he wanted to have an unmanned landing--Shea did--before the ... ?

A. Yes.

Q. I don't have anything on that. I must look that up.

A. Mueller did, too. Compare that to his all-up theory and his ability ← to land on the moon first. I tried to get him to cancel the other unmanned flights that we had.

Q. You mean Apollo 4 and 5?

A. Yes.

Q. You wanted to fly the first one with a man?

A. No, I wanted to fly those two as ballistic flights rather than putting them up like we did, because they were too complicated.

Q. You mean not even in orbit?

(218) A. Yes. You know, I didn't have any objection to putting something in the air that would come down by itself, but trying to build automatic systems that did everything the man was supposed to do in that vehicle was an extremely difficult thing to do. All the experience we'd had up to that time said it was very unlikely that you could do it.

Q. I didn't realize that. Well, no, that doesn't sound very all-up.

A. Well, we finally talked him out of that because they were getting so much money involved in the thing that they couldn't afford it.

Q. You want to take a glance at these answers from LBJ that I told you about? I brought them along thinking that you might like to... . They don't tell much, actually. As I say, I could get a lot more out of him in conversation, but perhaps you might want to glance at that. The questions are much longer than the answers. I have an idea they prepared these and he glanced at them and said, allright. Although it does indicate once or twice that he had some input of his own. If you ask him, he's a pretty frank fellow in most cases. Sometimes he gets mad at some of the questions you ask of him, LBJ does. But you were saying you didn't think we ought to have the unmanned flights at all: Apollo 4, on November of '67 ...

A. No, I didn't think we ought to have as complicated missions as Apollo 4 and 5 were. Because they required the spacecraft to be put into orbit and then to go through several trajectories to fire its engines to get into higher orbits and then go through all kinds of lifting re-entries in order to get it down to a landing point.

Q. And what was it you would have had the ballistic flights for?

A. To do the same thing, except that I would have shaped them entirely

(251) different, and done them in a much more simple fashion so that we have gotten the spacecraft back regardless of whether its systems functioned properly. It would have just come in as a ballistic projectile.

Q. Well, would you have used those full-sized Apollo 5's for just a ballistic missile.

A. Sure. It was just depending upon how you shaped the trajectory to do it with. Now, I was wrong there. It worked. I'd have been right if it hadn't.

Q. If it hadn't, we wouldn't be on the moon yet.

A. No, I don't agree with that. I think we would've pressed on.

Q. All that* doesn't give me very much, but I guess it's better than nothing. At least I can tag Lyndon B. Johnson's name on it. Do you notice how he avoided that Jackie Kennedy answer? Well, he couldn't have done that, you see, if I'd been able to ask him. Had you ever heard that story?

A. No, not until you told me about it.

Q. I told you the other night about it. /Pause, during which there was the sound of turning pages./ What are you laughing at?

A. He gave you no answer at all, that time, about Rice University and the center for research, and all that.

Q. No, it takes a little more pressure than I was able to exert through the mails. Well, as I say, it's better than nothing at all. Who knows, I may come down here someday when he isn't hauled off to the hospital. /Pause, during which there was the sound of turning pages./ I saw Gene Emme,

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*referring back again to the LBJ answers.

(312) the NASA historian, he was here yesterday. He's been trying to get somebody to see Johnson for four years now. I finally told him that I had an appointment with Johnson but it didn't work out. He hasn't seen anybody for an historical interview at all. He's had these programs with Walter Cronkite. ← Well, are you game for a few more questions? I'm keeping you up awfully late.

A. Okay, sure.

Q. Apollo 13. Who did you say called you when you were in the shower?

A. George Abbey.

Q. You told me once it was Kranz.

A. No.

Q. Sure it was Abbey?

A. Positive it was Abbey. Kranz was so busy at that point in time that there was no way he would have called me. Kranz talked to me when I arrived in the control center ... very quickly.

Q. Yes, that's logical. The other night I asked you about Friday, the ninth of August, 1968 and the C' decision came up. All of Low's records show that at nine o'clock you met with Gilruth and Slayton and so forth. Nine o'clock was the time I had an appointment with you, and I always write it down in my notebooks, you know ... what time it is. Have you

A. No. I don't remember what time of day it was. I remember what time of day it was when we made the Gemini decision--to fly a Gemini 7/6--but I don't remember what time of day it was when we decided that.

(335) Q. Well, of course George had talked to you a few days before about the possibility of doing such a thing as the lunar orbit for Apollo 8.

A. Right.

Q. You gave me a list once of the people you told about it. It's a list of about six people. ... I have it written down.

A. Yes. That's right. Jim Stokes, Lyn Dunseith, Sig Sjoberg, Gene Kranz, I don't know who else.

Q. I see Dunseith has been promoted to Bill Tindall's deputy, hasn't he? He's a bright fellow, isn't he?

A. Fabulous fellow.

Q. Here's a note I have from a conversation with you on the twenty-third of October, 1968: "With Mercury we went more to automatic. On Gemini we went too far on manual control. On Apollo we made it too manual, then back down. Now, we don't have enough automatically." This is just after Apollo 7. "The balance is too far toward manual control, particularly in operation of the computer. It puts an awful burden on the crew. They drove it that way. They were as much to blame as anybody."

A. I still stand on that.

Q. Did I quote you accurately?

A. Yes, sir.

Q. How would you make Apollo ...

A. As a matter of fact, we rebuilt a lot of those programs after that, to make them more automatic, particularly the rendezvous programs and some of the tracking tasks.

(358) Q. Could the rendezvous, after the lift-off from the moon, be done completely automatically, or do you need the human being?

A. No, but almost it could be, yes. The problem you would have is in getting the engine set up to burn properly and the arming of the various electrical circuits. And then the final braking phase, in the present design, could not be done automatically, because that's done by the human being actually using his throttle to fire the rockets for braking. And that's done by a sort of semi-manual, semi-automatic means.

Q. Well, the Russians had to do a rendezvous in order to get back their unmanned spacecraft, didn't they? with the lunar samples that they brought back?

A. No, that's just a direct launch back to the ~~moon~~ earth. That's the reason they can land only in certain parts of the moon, in order to get it back to earth. Limited performance, and therefore they can go to only very limited places on the surface of the moon.

Q. I didn't realize that. As I told you earlier tonight, I think, there is a report this thick on our analysis of their lunar samples that they'd given us.

A. We've got a report that thick on our analysis of their samples. It's embarrassing, because we know so much more about their samples than they do.

Q. Well, you must send them a copy of the report, don't you?

A. Oh, sure.

Q. What do you know about the assignment of Alan Shepard to Apollo 13 and Deke's being overruled and his reassignment to Apollo 14? Deke told me yesterday he was overruled ... he said by Mueller, it wasn't by Gilruth, he said. I didn't realize that. What was Mueller's reason for it, do you know?

(393) A. He said that it was too rapid to train him for the mission. He felt he needed more time to be trained and Deke went away and thought about that for awhile, and found out that Mueller was right.

Q. That's very interesting. This is an example of the Shepard luck, wasn't it? He wasn't on the one that had the bad oxygen tanks. What does he do? Does he have anything to do now, Shepard?

A. He's head of the astronaut office. Makes money, I guess.

Q. Done very well. The market's gone sour on him recently.

A. Most of his investments land.

Q. In land? It hasn't gone down lately. Now, in this same category of automaticity versus manual control, you said: "Particularly in the operation of the computer, it puts an awful burden on the crew." But has a lot of that been eliminated since?

A. Yes. We redesigned a lot of the programs in the computer, and made them function without interference or the need for the astronaut to tell the computer to proceed. But it still requires a great deal of manual intervention which we didn't have the time to re-do.

Q. You remember on Apollo 11, this young fellow, Steve Bowles, kept telling them to ignore the computer, that they were going to be allright?

A. Yep. Steve Bales.

Q. Bales. That's right. You see nothing since then to change you mind on that, that he was as good as he was said to be at that time?

A. Yes, sir. I was very proud of that because I forced that program to be in the computer, to allow our introspection into what was going on in the computer. That was a result of our experience on Gemini, where it was damn

(426) good that we had the capability to see certain things going on in the computer.

Q. You mean to the point that you could ignore the computer?

A. To the point that you could ignore the fault signal that the computer was giving you, because you could tell that, although it was true that the computer was overloading, it was getting the tasks done that were required to do the proper guidance during the descent.

Q. And that's where he got his training in such things? I mean, he learned the lessons from Gemini?

A. No. It was the lessons from Gemini that told us we should put those kinds of things on the telemetry which would allow us to have an introspection into what is going on inside the computer as it does its computations--so that you can see enough of its performances, enough of its tasks, to evaluate that it's doing its job properly. It wasn't able to do some tasks, but those tasks are prioritized in the computer and enough of the tasks were still getting done in each two-second time period of the computer base, that allowed the proper computations to be made for guiding the spacecraft to its landing point and performing the proper steerage of the engine.

Q. Then you said, "They drove it that way. They were as much to blame as anybody." Meaning the crew. Then you mentioned the poor computer design ... and the pilots helped to make it poor.

A. Again, I was still talking about the need for pilot intervention. It wasn't poor computer design, it was poor computer program design. There's a very subtle difference there. As a matter of fact, that had a great deal

(455) to do with the management problem we had with MIT: there were just too many people telling them what to do. When George Low took over the program office and we started getting into troubles in that thing ... Bill Tindall had given those guys all the management tools and all the management direction and all of the innovations that they needed from us to get the problems solved, but they still had so many different people telling them what to do that they didn't know which way to go. That was the one piece of new business that I brought to the thing when I took over the direction of the MIT work. Like, by god, from now on, I tell you what to do; you don't listen to anybody and unless it comes out of my mouth, or is signed by me--preferably signed by me--you don't do it. It was that simple.

Q. What date was that, that you had this showdown with MIT? Before Apollo 7, or after?

A. It was well before Apollo 7, but I don't remember. Bill Tindall can probably tell you.

Q. There was a lot of trouble with MIT through the years, wasn't there?

A. Most of the trouble we had with MIT was an MSC problem. The management of the whole thing, from MSC's point of view, was extremely poor. It was disjointed and it didn't have the right program objectives in mind. MIT was too responsive to the astronaut inputs and they weren't argued with enough by MIT or by the MSC people. That was only part of it, though. I mean, we just had a lot of people who were going along with MIT. One of the other problems was that they were continuously trying to fix and improve and perfect the program rather than solidifying it and getting on with getting

(487) the programs developed to the point where they were useful--and then fix them as you had the time to fix them. That was the kind of management that I brought to the thing.

Q. They didn't realize that the better is the enemy of the good.

A. Precisely.

Q. That's not an old Russian proverb, by the way. Did I ever tell you that? I looked it up. I had the Time magazine research people in New York look it up, and the Library of Congress people. It's from Voltaire.

A. Oh, it is? That's interesting.

Q. I wrote a note to Frutkin the other day, saying that ... and I know somebody said this slogan is emblazoned on the front of the Academy of Sciences in Moscow. And the only picture the Time, Inc. people had was one taken in 1935, of the Academy of Sciences. And all it said was "Academy of Sciences" up at the top. I asked Frutkin if he remembered seeing it lately. You don't remember seeing a slogan like that above the Academy of Sciences building, do you?

A. No, but I haven't been in all the Academy of Sciences buildings.

Q. At the time of the Apollo 9 mission we talked about ... when Schweickart got sick. Apparently, if he'd gotten violently ill when he was doing his EVA, he really would have been in trouble, because the PLSS can't handle solids ... if he'd started vomiting inside his ...

A. I think the biggest problem there is that he could have choked to death very easily under those circumstances, and he'd also probably have been blinded by the refuse in the face mask. I suspect there was enough pressure to blow that stuff out of the way, in terms of the air coming into the

(517) pressure suit. In the case of the Apollo backpack, which he didn't use in that flight, did he? I don't think he used the backpack in that flight.

Q. I'm not sure. I don't think so.

A. I don't think so. That was tested by throwing a can of vegetable soup into it.

Q. About the same consistency.

A. To make sure it wouldn't fail under those circumstances.

Q. And when they tested it, it didn't fail?

A. No.

Q. Well, looking back on it now, wasn't it taking quite a chance to assign him to this flight in the first place, knowing he had a history of seasickness?

A. No, I don't think so. Deke argued that point very strongly. He knew all that stuff about it, but he thought that if he was a pilot and he'd gone through all kinds of training, that although he may have been more susceptible than other astronauts, he was still not very susceptible to air sickness. He thought that--and he talked this over with the doctors, I'm sure--he was still as good as anybody who could fly. That turned out not to be the case, but I think that most of the doctors would agree that the reason he got sick was that he thought about it so damn much, and he just talked himself into it psychologically.

Q. Nonetheless, he did get sick, no matter how he arrived at it.

A. That's right, but I think my feeling on the matter has softened a great deal with time.

Q. You mean you are tolerant of his having made the mission, whereas you weren't before?

(509) A.. That's right, and I think I'm a lot more tolerant of Deke's evaluation of that situation.

Q. Although he didn't accomplish all he was supposed to.

A. No.

Q. It does cost about $400 million. How far can you strain your tolerance? ... I wonder if these on-board tapes have ever been declassified? Last I heard six months ago, they were being declassified. You know, all the cussing and swearing ... saying bad words that the little girls won't type up. Have you ever noticed that in the D.S.E.A. ...?

A. Oh, yes. They clean it up.

Q. They have some special word for "fuck." I've forgotten what it is ... it's not "frig," it's some polite word they always type in the tapes that are dumped, you know. Oddly enough, Pete Conrad, for whom more fears were held than anyone else, was fairly circumspect.

A. Right. He worked at it.

Q. But the Apollo 10 crew--my God. You read the /transcript/ and it's all purple and blue. Here's something you said after the Apollo 11 crew: "Sjoberg and I knew we were going to land 20,000 feet downrange, but we let the others talk us out of it." Remember saying that?

A. Yes, I don't remember the details of that, but that was an interpretation of the data that we had in empirically extrapolating the data that we had from one orbit to the next. We felt like they were probably doing it

/At this point, tape 2 was shifted to another machine whose numbering gadget was out or order; thus, henceforth no numbers will appear in parenthesis in the top left margin of each page/

wrong and we were right. I don't remember the details of that particular issue.

Q. And then you went on to say,"Not one major event that took place was reported by the crew. Apollo 11 told us it had happened after we asked."

A. Okay. Go back and listen to the ...

Q. Tell me a little more about it.

A. Well, what I mean by that is that they were very non-talkative and they never described things as they happened--like separation or disconnect to the docking system or when they fired the engine--those kinds of events that were significant to us on the ground to know that proper sequences were taking place. They just never bothered to announce those on the air.

Q. You just assumed, and then judged from the telemetry that this was true?

A. Yes.

Q. Have the crews improved since then in that regard?

A. Some of them have and some of them haven't. That's always been the case with pilots, though. Some talk a lot, and some talk a lot and say nothing, and some say nothing.

Q. Here's where you talk about the computer software. You took it over in October '67? Well, how did they report before then? Who did MIT report to?

A. They reported to the guidance and control division at MSC.

Q. And they had all the time, since they started the contract?

A. Yes.

Q. Of course, they had the first contract, before the Apollo spacecraft.

A. That's right.

Q. You said, "MIT wasn't being told what to do by NASA." Which is what you just said.

A. That's right.

Q. Then you added, "White and Schweickart were telling MIT how to design software." You mean, they were just taking the word of an astronaut how to do it, rather than reporting to ...

A. ... rather than having the engineers tell them how it ought to be done.

Q. ... through channels. That's very interesting.

A. Ed White and I had many, many arguments in the cafeteria at lunch time over how that should be done. He never agreed with me.

Q. Well, did old Dr. Draper ... Well, this is NASA's fault, mostly? It wasn't MIT's fault, you say?

A. Well, MIT had never really worked with the astronauts before and I think they didn't realize the kind of inputs they were apt to get from the astronauts. In the first place, they were not getting consensus opinions, and in the second place, they were not getting opinions that ought to be listened to, that they really ought to come from other places within the organization. Most people get overly impressed with astronauts. I always said that when you get into arguments in engineering discussions, one astronaut is usually worth about 10 engineers, in terms of how he gets listened to. It's very difficult to argue against astronauts, and they don't even realize sometimes that they're in that position.

Q. Yes, and sometimes they grow to believe that they're infallible. I've seen that happen.

A. You bet.

/tape goes blank for a few seconds/

Q. Yes, you know the only time Shea has been down here was for McDivitt's flight, the Apollo 9 flight, since he left?

A. No, that's not right.

Q. I thought he said it was the only one ... he might have said it was the one he especially came ...

A. No, he came down here for the Apollo 11 flight, I'm pretty sure.

Q. Well, I saw him here during one of them ... I'm not sure. I guess I saw him during /Apollo/ 11, actually. He had on his red socks.

A. I believe he was here for the Apollo 11 flight.

Q. Well, he said McDivitt had invited him to come down. That's where I first met him, during Apollo 9 at Houston. You know, he refused to talk to me for three years.

A. He told me that.

Q. He finally came through. Well ...

A. Think you've had enough?

Q. Almost. If you'll bear with me 10 more minutes, I'll be through.

A. Okay.

Q. Once we talked about the things that were indispensable in getting to the moon, the technical factors that were indispensable. We mentioned the mastering of liquid hydrogen, which had to be done, which apparently the Russians haven't done yet? We have no evidence that they have?

A. No.

Q. Fuel cells. ... Had to have fuel cells, which is what this Time cover story concerns ... Now, you mentioned once a certain computer that

you had to have. I don't have that accurately. What computer?

A. We had to have two different kinds of computer. We had to have one on board the spacecraft, and that's what MIT developed. That was developed on the precept that we needed a memory that would not be affected by electrical pulses or interference, and that was the reason we used the hard-wired memory, as opposed to a memory which could be erased easily and re-set up.

Q. You mean like my machine here, where you can erase everything that's on the tape?

A. Yes, but it's somewhat different in that a computer has a whole bunch of magnetic cores which get set by reading in a tape which then sets these things which sets the memory for doing a certain process. NASA built that as a hard-wired memory which would not be affected by these electrical interferences, and that was probably a good idea.

Q. Incidentally, had as sophisticated a computer as this ever been built in such small size before?

A. Yes. There were other computers of equivalent size, maybe even smaller, that were that sophisticated and that large at that time period, or that were being built at that time period. Now, the other* thing was the need for the 360/75 type of computer, or what is called the third generation computer--rather, the second generation computer--and the reason for that was the need for what was called a high speed input output computer, or IO computer,..

Q. IO means input output?

A. Right. The 70 74. I've forgotten the damn number ... whatever the number of the computer that IBM was putting out--was building--at that time that

*referring back to second line from the top of this page.

we used for Mercury and the first parts of the 70-something series. Anyway, that required a lot of external equipment to allow you to get in and out of the computer rapidly, in order to get at this high speed input output function. Its internal computations were also relatively slow. We made some computations early in the trajectory studies for Apollo and found that if we used the 70, or 94 or whatever the name was of that computer...we already knew it could do the Gemini job in the time period we needed to get an answer which we felt was reasonable--that is, we could run an end-to-end process of a rendezvous in about a minute's time. Then, when we started trying to do that with the lunar trajectories, we found that that took somewhere between five and six to eight minutes, and that was, as an process, not satisfactory. It was just too long to go through that, because if you found it was wrong and you wanted to do it over again, by the time you got a decision out of the computer, it may be too late to do anything about it in terms of direction to the crew. That's when we decided we needed the next generation of computer, which was already being built, of course, by a number of companies, the 360 series by IBM, the 1108 series by Univac, and the 6600 series by CDC, all of which were capable of doing that job.

Q. Did the space program accelerate the computer development?

A. Oh, yes, no question about it. The space program was the thing that demanded the high speed input output computer. I think they would have eventually gotten around to it, but certainly the space program were the people who were demanding that kind of processing.

Q. How many calculations per minute did you need, or per hour, or some fantastic figures that I've heard?

A. I really don't know what the computation time is. Someone better than I would have to answer that. The thing you have to remember is the size of the program. The best rundown of that is: in Mercury we started out with a 32,000-word memory and we went to 64,000 by the end of the program. And then we went from there to Gemini. The first Gemini program we wrote was a non-rendezvous program with about 350,000 words. The rendezvous program was about 1,250,000 words. The first Apollo program required--for earth orbit--about 2,500,000 words and the program we ended up going to the moon with was about 5,500,000 words. So that shows you the size of the programs that we were dealing with.

Q. Of course, not to be overlooked is the financial support you were giving computer companies all this time.

A. Oh, yes. And the biggest problem they were having with those class of computers was the development of the software. They thought they could take the 70/94--I guess it was--and just do some minor adjustments and modifications to that program and use it to write the software programs for the 75. But that turned out not to be the case at all and they required almost a complete rewrite of the programs. We really got into trouble with that, and had it not been for the tragedy of the Apollo fire, we would have never been ready to meet ~~this~~ spacecraft. flight schedule It was only that interim period that allowed us to develop the software. That really got the whole software out of trouble, because they were bringing those computers on and nobody could use the things, because they didn't have the software to work with them. That was true of all three of those computers that I just mentioned. All three of those companies were having trouble getting their

software to work.

Q. What all does software comprehend? I know that software means the programming of the computer.

A. It's the technique of taking a mathematical equation and the logic of how you go through the mathematics, or the questions you normally ask in a logic diagram--if this has happened, you go down this branch and if it has not happened you go down this branch, and so on--that's what computer programs mean to me, and it's a technique for transmitting that into a system which the computer can ... you can put that into language which the computer can understand. As the machines become more and more complex, that becomes more and more complex. It's just the process of transmitting English language into machine language, it's called. Now, there are all kinds of processes that that is called, like ~~full-train~~ FORTRAN is the one that is most used and that's a technique for doing that. It required a whole lot of modifications to all the knowledge we had at that time of how to get that stuff in there, and that's about as much as I know about it. I don't know how to program today's computers. When I was a working engineer 16 or 17 years ago, I knew how to write computer programs, but I don't anymore. The whole process has changed.

Q. Who's the greatest computer expert in MSC?

A. Lyn Dunseith. Lynwood Dunseith, without question. He not only understands computer programming, but he understands computer systems--what makes computer systems work. I guess the second guy would be Jim Stokes.

Q. I asked Dunseith how much money he had invested in this building 30 over there, and he came up with an answer: $99 million. He wouldn't make it $100 million, you know. Did you ever hear the story about the how the figure

came out about 5,000,000 parts in the Saturn V?

A. No.

Q. Well, Mueller demanded a figure, one day--right away he had to have a figure, he was making a speech, and he wanted to know how many parts were in the whole stack. Somebody said, well, hell, we don't know, so we might as well give him a figure. So they gave him the figure 5,000,000, and it's been used ever since as gospel. So I was told by some of the people who contrived it all. Well, Dr. Kraft, I have kept you up for a long, long time.

A. No, I've enjoyed talking with you.

Q. I appreciate your kindness and tolerance. And I hope you have a great tour as director of MSC.

A. /Chuckling...-/ Well, maybe Senator Anderson will fire me. I should be so lucky.

/At this point there was some host/guest chit-chat not related to the interview./

Q. Well, it's been a very enlightening evening and I do thank you.

A. Okay. There are a million paths we could go down, you know, when we get started ...

Q. I know, there are a lot of angles to this whole thing. Sometimes I wish I just had something or other ... which has no path except the straight path.

A. No you don't.

Q. Little bit about ....