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ORAL HISTORY INTERVIEW

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Oral history interview with Christopher C. Kraft
[full name of interviewee]

about Mission Control - Worldwide Communications Network,
[main focus of interview]
Management philosophy, personalities.

1963 - NO

Title: 1963 - Flight Operations Dir
[interviewee's current and/or former title and affiliation]
1968 - Dir of Flight Operations

Interview conducted by Robert B. Merrifield - Staff
[interviewer's name/position]

Historian **at** MSC.
[location of interview]

Transcript and tape(s). [for inventory only: # pages 92; # tapes 4]

Masters 4

U.S. Gov't

Education -

Career Path - Flight Research Division - Langley; 1958
~~Space Task Group~~

Topics - early STG Flight Operations - ground support + ~~network for~~ communications; Tracking & Ground Instrumentation Unit (TAGU); Western Elec to buried remote sites w/ IBM computer system; decision for Control center at Cape; poor decision to put computer at Goddard; Philco contract for running network; permanent & mission remote site manning (State Dept help in various countries); Mercury - minimum 3 ground crew: capcom, systems monitor, ~~flight~~ medical monitor; ships (poor communications until satellites); Bigge Program; dev. network & control center procedures; MA-3 abort; NASA reps to DOD programs; NASA-DOD team-work at remote sites; liaison between STG + Atlas launch ~~team~~ program Bob Harrington NASA Hutson to Convair + GE; tracking ship ~~duty~~ problems: crew, controllers, time, accomodations; ship modifications; rocket failure analysis (Redstone + Atlas); 3 Control Center Shifts for Gemini; Goddard competition w/ Air Force on Flight Control; early limitations of teletype communications; evaluation of Barry Graves + Ground Systems Program Office (GSPO); estab of Flight Operations Directorate; Walt Williams management style re vehicle design + Construction; ~~re~~ MSC management shifts; evaluation of Walt Williams + his transfer to NASA Headquarters; Mueller policies;

new control center – IBM won computer contract; training simulator; Charactron display system soon out dated; IBM's Jim Hamlin & 7094 system; Walter LaBrige & Philco location; shift from Cape Control Center to Houston Control Center; process of getting new generation computer; flexible telemetry decommutation system; objections to Mueller's ~~MD~~ Mission Director idea; more tracking stations for lunar flights; attitude sys trouble on MA-4 and MA-5; Scott Carpenter flight + meeting problems

Christopher Kraft Interview

April 19, 1982

Purpose of the interview was twofold: to secure Dr. Kraft's permission to utilize his interview by Bob Merrifield on October 5, 1967; and to obtain his views on the JSC organization structure.

1. Dr. Kraft advised he had no problem with my using the interview by Merrifield. I pointed out several places where I intended to use his words as quotes. He saw no problem with the intended quotes, but suggested that the language be cleaned up.

2. In discussing the organization Dr Kraft stated that the JSC system which used matrix management superimposed upon functional management had worked very well. The program offices know where the fountain of knowledge is, and the technical staff know the communication process. The Directorates offices have two functions, a sort of proving ground for the staff's technical competence and a more or less

home seat for the staff in between programmatic assignments.

Because our system has become traditional ~~and~~, as everyone knows how to work with the system, and as the system has functioned very well, serving us through ~~out~~ the Shuttle program, I would think that we will continue the structure. On the other hand, as we get into the operational phase of Shuttle, the Center may need to change its system.

Dr. Kraft recalls that

3. Dr. Gilruth was very clever in developing the SSC system. He has very received the full credit he deserves as a manager. He was extremely competent. His greatest ability was to recognize the talents that existed and knowing how to bring ~~the~~ these talents out. I learned a great deal from Dr. Gilruth.

4. Dr. Kraft advised that if I have any further questions to please give him a call. He would be interested in receiving a copy of the work when completed.

Kraft

p. 63 - ground ops at Cape

⇒ p. 66/67 Control Center at Houston
~~= KSC~~

p. 72 - KSC & Houston Control Center

p. 54 - relations between Gemini & Kraft
48

Dr. Kraft – April 19, 1982

- class

used to work that way – should continue that way

- communications – when function of knowledge, program offices
Directorates as center

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WHO?

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Interview with Christopher C. Kraft, Jr.
October 5, 1967

Following Sputnik we started to do some work in Flight Research Division at Langley and in particular in Hewitt Phillips' Stability and Control Branch. We had started working on a manned space flight program with three principal areas of specialization: (1) high aspect ratio--skip-type reentry, (2) low aspect ratio--a high-angle attack reentry (both of those configurations looked like airplanes), and (3) the blunt-body drag-type reentry vehicles. In Flight Research we were working on the low L/D high angle attack airplane type reentry, and in particular the stability and control problems associated with this type of design. We also knew that Gilruth had been working hard to get NACA (later NASA) involved in the manned flight program. We knew that a group was going to be formed for this purpose around the summer of 1958. Chuck Mathews said that my name had been mentioned to Gilruth and Faget and he wanted to know if I was interested in working in that program. It took me about a day to make up my mind that was what I wanted to do, and when the Space Task Group was organized I was included.

We moved into the unitary tunnel building. We tried to formalize our organization along functional lines. I was in Flight Operations which Chuck headed. Flight Operations had responsibility for almost everything necessary for ground support of space flight. We worked on communications problems among other things. Howard Kyle was in our group. We set up the requirements for an operations network at the same time that other people in STG were writing

specifications for the spacecraft.

We started by making contacts at Cape Canaveral with people that had experience with launching missiles. We had decided what boosters we wanted to utilize and started laying out a proposed flight test program. We decided to use the Redstone initially to get some space environment and zero g experience for astronauts, and the Little Joe program to study abort problems. I was primarily involved in operating problems. To establish network requirements we started thinking about the problems of orbit determination, how rapidly we could determine the orbit, and if we weren't in orbit at insertion what abort decisions we would have to base our operations on. At that time it was very difficult to determine the orbit very accurately. We usually had to wait a couple of orbits before we really knew that the vehicle was in orbit and then we had to wait almost a matter of days to get an accurate determination of the orbit. We began working on systems which would enable us to make the quick decisions that were vital if we were to insure a safe return of the crew either at insertion or at any time in the flight. I guess on the basis of our lack of knowledge of orbit determination at that time, we felt that we really didn't want to go around just one rev if it would take more than one to determine the characteristics of the orbit and to accurately determine the time of retrofire. We settled on three revolutions. That really formed the basis for the network. We saw that with the inclinations that we wanted to work with, it would be necessary to take advantage of the ranges that had already been developed by the Air Force particularly the down-range capability of the Eastern

Test Range as we thought it would be beneficial both for launch and reentry. We knew that the Australians had a range at Woomera, and if we could get radar data from that part of the world, we could get a much more accurate fix on the orbit. When you can get fixes on two places 180° apart in the orbit, the orbit determination is much easier and much more accurate. So those kind of thoughts were in our mind--number of contacts during the orbit, how often we wanted to be able to talk to the crew and get data back from the spacecraft in order to make the proper decisions, and the inclination. We also wanted to remain within the temperate zone. All those things laid the groundwork for criteria we would use to locate our network sites and determine how many we would need. We considered flight control for an airplane flying across the country. Normally, its pilot will report in about every 15 minutes to one-half hour. We decided that would be a reasonable frequency during an orbital flight. It was fairly obvious that if we had around a 30-35° inclination, we needed a station in Bermuda because the range of the communications systems that we had at Cape Kennedy was not sufficient to be in contact with the spacecraft if it were at very low elevation angles at the time of cutoff. Because of the low elevation at that time we needed a station at Bermuda to start picking up the spacecraft as it came up over the horizon and to give us the capability of doing rapid orbit determination and making the go-no go decision as to whether the spacecraft was in orbit or not. A map of the world will show that there is no land area of any consequence between Africa and Australia in the Indian Ocean, or between Bermuda and the African continent.

If possible, during the first 15-20 minutes of the flight, we wanted to be in continuous contact with the spacecraft, particularly since we thought if we were going to have any problems with the spacecraft system they would probably begin at zero g where we would begin using the systems that would likely be problem prone. We wanted as continuous a contact as we could get in that first part of the orbit. This, then, dictated the need for a couple of ships.

At that time it was fairly obvious that we needed to develop this network and specify operational requirements and the details of the support that we needed--for example the type of radar we were going to use on the spacecraft. The decision was made to use S and C band radar, primarily because if we had an S band and C band beacon, it would give us sufficient accuracy to make an orbit determination. For the sake of redundancy, if we lost one beacon, we still had the S or C band left and the same was true on the ground. We also knew what kind of communications gear we wanted to use on the spacecraft. We wanted to provide both UHF and HF for the same reason--one to be backup to the other. We really didn't know whether we could get reasonable ranges out of UHF. It later turned out that we could get horizon to horizon UHF communications from a given station but at that time we didn't know whether we would be able to count on that. And we didn't know what kind of communications we could get in some parts of the world that we might want to use HF. It later turned out that

that was not the case either, but that was our reasoning at the time. For a telemetry system we decided we were going to use a number of channels of data, and the PCM and D-comm equipment. We started specifying the number of pieces of information we would have to have on the various sytems. This work then formed the basis of a total specification for the network.

It became obvious at that time that we needed help very badly in the Space Task Group since we had very few people who could write a spec and then go through the evaluation of a contractor who would build these network sites. It was necessary to establish where these sites should be and work with the State Department to get them. All in all, it looked like a lot of different agencies and Centers would be required to cooperate if we were to be successful in establishing our network. And that is how TAGIU (Tracking and Ground Instrumentation Unit) evolved. Mathews and Gilruth and I talked this problem over and decided to ask for some help from Floyd Thompson. Since there was some NASA Headquarters involvement in these problems, Buckley, formerly Chief of Langley's Instrument Research Division, and at that particular time he was being transferred to Headquarters, he was invited to join. We invited a group over in IRD headed up by Barry Graves to participate. STG also contributed members. About that time we had obtained some personnel from the AVRO group in Canada. They joined in that effort and we in Flight Operations worked very closely with TAGIU. Together we began writing the detailed specifications for this network. We had a bidder's conference, solicited RFQ's and held an evaluation which the

Western Electric Company won. They were to build our remote site facilities around the world.

Besides the tracking and communications and telemetry there had to be a computer system developed to assimilate the trajectory information, do orbit determination and make real time abort decisions and compute real time retrofire data. To do this work, IBM was chosen as an associate contractor with Western Electric.

One of the decisions that we had to make was where to put the computers. We would have to have a control center under our responsibility. It was part of the network contract, but the design and development of that control center was really the responsibility of the Space Task Group. TAGIU insisted we figure out what we needed then give TAGIU the work and support necessary to run it. Space Electronics, a small company out on the West Coast had had some contact with Gilruth. Its people had come from STL. We got a study contract with that company and they came in and picked our brains as to what we needed in the way of a control center. With their help we formalized our requirements for the type of displays that we wanted, the type of computations we would have to had, the various arrangements and layouts of instrumentation, and the command and control for the spacecraft. Remember at that time we still had to worry about the unmanned flights as well as the manned flights and support of both Redstone and Atlas. We decided, and this is sort of getting ahead of the story a little bit, that we couldn't get the final control center ready to support the orbital Redstone flights because the schedule problem at that time. We decided to put the control center for Redstone

in a trailer as an interim thing, and built that whole trailer system with Western Electric and McDonnell help, but we never used it. The schedule came out such that we had the full blown control center at the Cape ready for the Redstone flights.

Around the time the decision was made to establish the control center at the Cape, another decision was made to build a space flight center at Goddard. We were then a part of that Center. Harry Goett was named as the head of Goddard and construction of Goddard's buildings was underway when we started working on where the computers should be. I say we--I mean Chuck Mathews, myself and people like Tecwyn Roberts, Howard Kyle, and John Mayer who was supervising all the trajectory work being done for us at that time. We wanted that computer system to be a part of the control center we were building at the Cape. Sinee the acquisition of computers was difficult and since this network would support other types of space flight and since we were going to be a part of the Goddard Space Flight Center, it was decided that the computer would be put at Goddard. We fought that decision bitterly but lost. Therefore we had to come up with a whole raft of high speed data lines between the Cape and Goddard and of course high speed data lines from the remote sites to Goddard. We always felt that was a very poor decision because the separation of 1000 miles between the people that were working on the programs versus the people that were using the programs and understanding the requirements for the programs really was a very difficult management problem for us. It formed the basis for our argument of where we wanted the computers when we started building the control center here at Houston. Of course, justification was somewhat easier after Mercury

program really got to going, the Manned Spacecraft Center was formed and it was decided that MSC would be separated from Goddard. Goddard would be the scientific unmanned space flight center and we would be the manned space flight center.

Back in the early days of the Space Task Group we were limited as to the number of people we could hire and the range in capabilities we could afford--that is the right kind of background experience in network operations and ground telemetry systems, radar systems, and general flight control. Not a heck of a lot of people have had flight operations experience. We started looking around for some kind of contract to support our inflight activities. We had a contract with McDonnell to supply us with people to help us understand flight systems, and as I recall we had about three people working in that area. We asked McDonnell if they would be interested in supplying us with some flight controllers--although they probably weren't called flight controllers then. McDonnell came back with a very expensive proposal. At the same time we were looking at other companies in the country and discovered that the Philco Tech Rep organization had a lot of experience in running networks for the Air Force. For the Discover Program, Philco had established a world network that was small compared to the one we were building but indicative of their ability. Philco made a proposal to us which was very reasonable from the standpoint of cost per man. On that basis we selected them to do the job for us. That turned out to be a very good contract as far as NASA was concerned because we got

an awful lot of good people. Philco gave us some of their best people in that line of work and they stayed with us up to the present. We've been very satisfied with the response we've always gotten out of the Philco Company in terms of replacements, increases in the number of personnel we needed for Gemini, and we have kept them on for Apollo. Initially the Philco contract called for 20 people, and I guess it was let around late 1959 or early 1960. A lot of these people later came to work for MSC because they wanted to be more directly involved in the planning of the missions and the detail flight control planning which they were not then really a part of. At Philco they were just spacecraft systems monitors and they wanted to be involved in a lot of the other aspects of the flight, and NASA was happy to have them. We got people like Jim Tomberlin who was an outstanding man in Flight Control and now has been reassigned to the Apollo Program Office where I understand he is continuing to do an outstanding job. Jim Strickland is another man that came to us from Philco. He works in Flight Support Division. Lem DeLuca another transfer has remained more in the systems area than others but has been a great help to us in the early design of the mission control center here in Houston.

In terms of the people we were going to send to the network--there were really two separate and distinct problems. One problem involved the literal manning of the equipment and its maintenance and operation after it had been built and installed by Western Electric at each of the remote sites around the world. The second problem was our manning of those stations from the standpoint of flight controllers who served at the site only at the time of mission operations. The responsibility for the site maintenance and operation was Bucklye's with Barry Graves as the

lead technical man. That required a great deal of work with the State Department, in particular in places like Africa, Australia and Mexico. There had to be agreements made with those countries. This was no great problem in Australia, where we were building a site at Muchea just north of Perth. We came to a very fine agreement with the Australians. They agreed literally to maintain and operate those stations under a contract to NASA. This was sort of a government to government agreement in that they would supply all of the technical people necessary to operate the station. That has remained the basis of a lot of other work in Australia since then and has been a very fine arrangement for both us and the Australians. They were able to utilize some of their technical people from the Woomera Range at other locations, first at Muchea, and later at other stations around the country. As a matter of fact there are three stations in Australia--two of them deep space stations and one manned space flight station--and except for a few company reps and an occasional NASA technical advisor, the Australians totally run the show. Now in Africa, we had a number of different countries to deal with. We had to get an agreement with Spain because we had to acquire land and convince the Spanish people that we weren't building a missile site. In all of these agreements not only with Spain but with all foreign countries we dealt with we agreed that the information that was obtained in the manned space flight program and particularly the data that was received at that particular site, would be made available to that country on request and they would be kept informed totally of what we were trying to do in the manned space flight program.

That really sealed the bargain I believe and proved to these countries that we were concerned with peaceful uses of space. We then used the military (and still do) to supply medical observers at the remote sites. Most of the foreign countries were somewhat sensitive to the use of military people. That was particularly true in Mexico where there is great Communist influence. There have been a lot of demonstrations around the Guymas station and we have had to use the militia many times even in the last few months. In one of the early Mercury flights, either MA-4 or MA-5 the Mexican government had to call out the militia and sort of ring the Guymas site to prevent some of the local Communists from inflicting damage or putting it out of service. When we sent doctors to Mexico we always insisted that they wear civilian clothes, even though the Mexican government knew that these people were from our military service. We were able to convince Mexican authorities that these officers were there for medical observation and had nothing to do with the military situation. Many papers in Mexico claimed that we were building a missile site in Buymas and although we knew that the responsible element in Mexico and particularly the government, knew better, we had to make sure that we weren't unnecessarily criticized so we were very careful in our dealings with the people in Mexico. We still have a so-called Mexican-U. S. commission for observation of space where we try to keep the Mexican people completely informed of what we're doing and how the programs are going so that they feel they are a part of the overall effort. Of course there aren't a whole lot of people with this type of technical capability in Mexico, so we ended up with sort of

a combination of people in Guymas. We use technical people of Mexican origin when we can get them, and when we can't we make up the difference with Bendix people who are under contract to Goddard. The Bendix people do most of the technical work, while some of the less demanding communications work is done by Mexican nationals. All of the maintenance of the station--janitorial services, power plant operations, etc.--is done by the indigenous people.

In Africa we had a different problem. We built a site at Kano, Nigeria and another at Zanzibar. We made a number of trips to these countries to talk with these people. We invited the Nigerians to come to the U. S., and briefed them on what we were doing in Mercury, took them out to Wallops Island test station, and showed what we were doing. They were dressed in their native garb and had to go at certain hours of the day to have their prayers with the holy water and all that kind of thing, which was an interesting experience while we were at Wallops Island. We didn't have too much trouble in Kano because there some of the officials were highly interested in the education of their own people and they used this to engender support for the establishment of a tracking station. Later we gave up the Kano station in the later phase of the Gemini program because of the unrest that existed in that country. In the last year or so we really didn't need Kano except as a relay station for communications and recording of telemetry.

Zanzibar was an entirely different situation. A great deal of Communist influence existed in Zanzibar and we were forever having trouble. Sometimes we were even reluctant to send our people there because of the many drastic changes in government policy. Near the end

of the Mercury program and before the beginning of Gemini it became fairly obvious that the government was so unstable (in fact they literally asked us to get out through State Department channels), that we did end up moving that station. It was a mobile station and we moved it to Tennarive, Madagascar. Tennarive was used as a station for Gemini and will be a station for Apollo though it is not fully equipped. At present it doesn't have full unified S-band capability but we have hopes that we will have it in time.

In the unmanned and the manned program parts we had several commands we could send to the spacecraft for turning equipment on and off for retrofire. We analyzed the human factors in the spacecraft, the aeromedical aspects of the flight, the communications that we wanted to transmit both to the crew, and the communications necessary from the stations back to the control center at the Cape. We decided that we needed three people to do that job on the ground. We later used a fourth when we ran simulations over the network. We decided we needed three operating personnel. One was the capsule communicator, who would handle the communications with the crew. This capsule communicator really was sort of the flight director of that station. He was the man responsible for making the decisions when the spacecraft was overhead and for carrying out the directions from the control center here and at the Cape. We felt that we needed the second man primarily to monitor the spacecraft systems. He was a Philco Tech Rep Division Contract employee. The third man monitored the aeromedical aspects of the flight--the EKG's, the respiration rate, temperature, etc. of the astronaut. Since aeromedical people were not readily available

in civilian life, we got support from all three military services and the Public Health Service. Doctors were not only permanently assigned to the Space Task Group, but as well we had some that were temporarily assigned for each one of the flights. We had to bring these people in and train them. That's how we got Chuck Berry. Chuck was a flight surgeon at that time and very prominent in Air Force circles. Stan White who was permanently assigned as Aeromedical Chief with the Space Task Group chose a lot of these people and of course he knew Chuck Berry.

As we were utilizing the military services to get the aeromedical people in the program, it presented a training problem for us. We built a replica of a station in the full scale tunnel at Langley to do our training for remote site personnel. We brought these aeromedical people in a number of times before our flights to train them in the use of our consoles and acquaint them with the spacecraft systems that they would be interested in--particularly the environmental control system, the oxygen supply system, all of the instrumentation that the crew wore, and the background of each one of the crew. Of course a great deal of work had to be done with the animals at that time. We got a lot of support from the Army's White Sands Missile Range and the Aeromedical Center at San Antonio.

We also used one of the site personnel for astronaut simulation. We sent the scripts to the remote sites ahead of time along with the tapes we had developed on the trainer. We'd run a full scale version of the flight plan at the Cape in the spacecraft trainer, tape it, and then we'd cut those tapes up into pieces that would be equivalent to flying

over a particular site and we'd write scripts for what the astronaut would be saying over that site. One of the site personnel would be prepared to give that information as if it were coming from the spacecraft. We'd also give them a whole list of questions that they might be asked as to the performance of the spacecraft. That began to get pretty complex with the Gemini ( ) manned flights so we started sending an additional man to the site to act as the astronaut simulator. He aided the other three men in the preparation of that site for the coming flight.

The ships, because we were able to place them in various locations around the world, were very important to the operation. We initially had one in the Atlantic between Bermuda and the Canary Islands, and one in the Indian Ocean. We soon found that we really didn't need the one in the Atlantic and we put it off the coast of Japan when we got to the longer duration flights in both Mercury and Gemini. Also, for these longer duration flights we took the ship out of the Indian Ocean and put it off either the east or west coast of South American because it allowed us to cover that portion of the flight in revs 8 through 12. The Coastal Sentry Quebec located near Okinawa and the Rose Knot Victor off the west coast of South America were really the only two contacts we had on some orbits of the spacecraft. We had a lot of trouble with the ships from the standpoint of communications. We had to use HF communications and they're very susceptible to diurnal effects--going from day to night and night to day. We also had trouble with their location. We had some very long transmissions to make. We had a number of frequencies that we used and we were always switching

frequencies. We never had really good communications until we started using the satellite over the Pacific in early phases of Gemini. Through most of the Gemini Program that satellite greatly improved our communications with the ships. Other problems we had with the ships of course were administrative--that is, trying to maintain good crews on the ships to operate equipment. There was a heck of a turnover. The ships were out for long periods of time especially for Mercury flights when we had a lot of scrubs and the ships ended up being on station for long periods of time and away from port for up to 45-50 days. The ships were very small and offered little opportunity for recreation. One of the aeromedical people we sent out literally built a sailboat on the deck of the ship during the John Glenn flight. The stateroom sleeping quarters for the flight controllers was far from cruise quality. We tried to switch our flight controllers around so that we sent them to Australia one time, then next time we sent them to the ship. On their third assignment we tried to send them where they really liked to go to rather than out on a ship again.

On the John Glenn flight we had put Allan Shephard on one of the ships and we were having a number of holds at the Cape, and it was stationed off the coast of Okinawa. Shephard called me in the middle of the night and wanted to know if the ship could put into Japan. We were having a large number of holds and everyone wanted shore liberty in Japan. He wanted to know how long our hold was going to be. It turned out they didn't have enough time to get to Japan and back and I directed them to remain on station. It so happened that they had allowed the ship to drift, several hundred miles toward Japan and they really had to scurry to get back to their on-site position for

the flight.

Now it always turned out that both in the Indian Ocean and off the coast of Japan we encountered a number of typhoons. Invariably the ships had to avoid those storms and many times were actually caught in them. As I recall, one of the ships out in the Atlantic had to pass through a couple of hurricanes or skirt them. The ship was tossed around a great deal and soon carried an awful lot of sick people.

I think I related before when I was talking about the establishment of the TAGIU group under Graves and Buckley that the responsibility for the design of the control center was totally the Space Task Group's and that was done under direction from Chuck Mathews and me and Fred Matthews, Tec Roberts, and Dennis Fielder. We had a struggle with the Air Force in getting some particular assigned location for that equipment at the Cape. There was a small facility there where we were installing the large antenna that was going to be used for Mercury. We called it Tel 3, Telemetry Station 3, but it was really a large 85' dish that had been built for telemetry for use both by us and the Air Force in launchings. A small building went with that equipment, so we decided that in it would probably be a reasonable place to put the control center. We laid it out and had a lot of argument about who was going to own the building, etc. It was decided that the Air Force would continue to own it and would have some other equipment in there besides the equipment necessary to support the control center. I remember going up to Silverstein to show him a layout of the control center and that was one of the few times I really thought we got a fantastic management decision, in that he looked at the layout and said "It's not big enough, make it twice as big." Was he ever right! We ended up bulging out of that building

very rapidly when we started trying to get all the equipment we needed in there and particularly after we made the decision to put the spacecraft procedures trainer in the building. We really had an awful lot of equipment in that building. We were forever expanding on the communications system and the data system net, and when we got into the Gemini program, it was no where big enough. We had to make a number of additions and modifications in particular for the trainer and the command systems that we were adding for Gemini spacecraft. One of the agreements made with the Air Force was to have the center operated by RCA, under special assignment to NASA, controlled of course by the TAGIU group and eventually by Goddard. That was probably one of the best things that ever happened to us because the people we got were all experienced and highly competent. The person in charge, and I wish I could remember his name, gathered together a group of very strong individuals from the standpoint of radar experience, command experience, telemetry experience, and communications experience. He melded them into a team that was probably one of the most outstanding groups we had throughout NASA both for Mercury and the early phases of Gemini. Particular recognition should be accorded Bill Saunders, Andy Anderson, and a radar guy I really want to mention but can't think of his name right now. They were really a part of the manned space flight team and without their help in developing the procedures, in running the network, and in interfacing with what was then the Atlantic missile range, just couldn't have been done without those guys and they deserve an awful lot of credit in the success of our activities at the Cape. They were as much a part of our flight organization as any of the flight controllers,

and probably in many instances were more important.

Preston was a part of the original team assigned to the Space Task Group. He was sort of a deputy to Chuck Mathews. When we began building the Big Joe spacecraft it became obvious that we needed space at the Cape immediately, rather than waiting until contract people began to come onboard. We had a team of mechanical and electronic engineers to assemble this equipment at the Cape and to check it out. Preston was given that end of the business. He was given the ground checkout equipment and that responsibility in the preparation of the spacecraft at the Cape by Mathews. I was given the flight operations end of the business. We had close relationships with Preston and Scott Simpkinson and a whole team of people was brought down from the Lewis Laboratory to run the operation at the Cape. That's the reason so many of those people down there were previous NACA experience, like Jake Moser, Scott Simpkinson, Mike Wedding, Andy Meyer's brother (I can't remember his first name) and a whole raft of other people from Cleveland. They had been in the flight operations organization at Lewis and that function was sort of being phased out, so almost the whole group was transferred lock, stock, and barrel to the Cape to run the Big Joe Program. They built the instrumentation system for the Big Joe Program, and early established a very good working relationship with them. Such a relationship was necessary to our understanding of the instrumentation not only on Big Joe but later on the Mercury spacecraft.

When we started the preparation for operations at Pad 5 with the Redstone it was an interesting experience to go through the development of the countdown procedures. You know you start out on these things with little preparation and little documentation and end up printing

almost every word that you were going to say in the countdown. We went through a similar type thing in the development of the procedures in the control center. This is where we came up with a concept of o operational procedures documents and of course the development of the mission rules rapidly evolved out of that work. One of the flights that we had scheduled for Mercury was MA-3. It was to fly an aborted mission. We planned to achieve less than orbital velocity and after the spacecraft reentry we planned to go through the retrofire procedures and bring the spacecraft down off the Canary Islands. I don't remember all the details of the decision that changed that plan, but we decided that we would go ahead and fly an orbital mission with MA-3. That was really pushing things, because we had had very little experience with our procedures for the network and the control center at the Cape. We had completed a couple of the Redstone flights, but in terms of world-wide operation, communications procedures, gathering of data, and all those kind of things, it was kind of shaky. But we agreed to make MA-3 an orbital flight. Gene Kranz, Paul Havenstein and I set up camp at the Cape. We may also have had "Dutch" von Ehrenfried there too and a couple of other Indians to help us and I recall there was a fellow from Western Electric whose name I can't remember. We started trying to dream up all the procedures that we would have to have for sending messages on the network, the content of those messages, the message headings, a message priority scheme, what we wanted to know about the spacecraft at certain times, what we wanted to know after the pass, after everyone had had a chance to think about what they'd seen, and we started trying to write all that down. We talked with the people there at the Cape handling teletype traffic and communications (Anderson and his people). We really spent a lot of concentrated effort on that

procedural development. Kranz was the guy that carried the ball there. After we finished our work we decided that we should run a training exercise. What we were trying to do was develop the procedures both for the network and the control center, but first we were concentrating on the control center.

We also recognized that there had to be a lot of procedures developed to standardize effort at the remote sites. We had a remote site simulator at Langley but we really didn't have the interfaces that were needed to work with the people at the remote sites. I mean by that the telemetry people, the radar people, the communications men, and M&O supervisors. We really didn't have that worked out very well nor did we have the capability of working that out at Langley. So we decided to go to one of the sites and see if we could work out those procedures. We decided we'd send Paul Havenstein to the Canary Islands because the Canary Islands' station was pretty well along and in pretty good shape from the standpoint of the equipment. We sent Paul and a couple of our flight controllers, I don't recall who they were, and one of the doctors, and a guy named Burton from TAGIU. Everybody came back sure that Burton was out of his mind, including Dr. Berry. They thought that he really needed some kind of psychiatric help, because he turned out to be a real nut. He still works at Goddard I believe. Despite this problem, Paul Havenstein and those guys really did a great job of working out those detailed reporting procedures at the remote sites.

Before he left, Paul was gaining weight and he decided that he would go to the Canarys with Metrecal. Metrecal had just become popular and I

remember his briefcase was full of Metrecal. While he was in the Canaries he lived on Metrecal and Scotch and when he got back he had gained weight.

We went to the Cape quite a bit ahead of the MA-3 mission. We were really fighting to get the network up and we were having a real struggle because we were having a lot of equipment problems. We worked very hard to get up to speed with the simulations we were running at the Cape and then sent our people out early for the MA-3 flight. That was the flight that went straight up off the pad. Its pitch program failed, and the range safety people had to blow it up. I remember when range safety pushed the cutoff they gave the cutoff and destruct signal. We had a lag built in and it was necessary to push the button to get cutoff. The destruct signal couldn't be sent for another three seconds, so that the astronaut would have a chance to get off in the automatic abort system. The automatic abort system was closed loop for MA-3. The battle we had to go through to get Convair to agree to close the loop on the automatic abort system is a story in itself. I had a television picture on my console of the launch vehicle. I saw all these events take place. I heard the range safety guy say he was giving cutoff and then following with destruct. My eye missed the spacecraft coming off of the Atlas and then I saw all the abort sequences taking place on the spacecraft and the whole sequence panel on my console lit up. I didn't know whether we'd just lost signal, or whether we had a bum signal. All I could see was a big pile of smoke and pieces on my TV screen and I didn't know whether the spacecraft had aborted or not. I thought I waited about 30 seconds before I threw the abort switch and that turned out to be 11 seconds when we went back and measured it.

We started getting a signal from the spacecraft within a minute or minute and a half, and we knew the spacecraft was on the chute. We immediately went back and replayed the kinescope of the launch and then saw the spacecraft escape rocket leave the launch vehicle. It was very clear. But nobody had seen it because we just weren't following it that closely. We flight controllers weren't supposed to watch the launch too closely after the liftoff. It was only during liftoff phase that we utilized the abort sensing device to throw our abort switch in the control center.

We took that spacecraft and refurbished it and flew it again on MA-4. On that flight, just prior to retrofire we started having thruster problems. We thought the hydrogen peroxide was freezing in the lines. We put all instruments and some heaters on them. When we flew the next flight, we found the thrusters weren't freezing, they were getting hot. The heat from the thrusters was feeding back down into the lines and it was literally destroying the bed that you need to get the steam reaction of the H_2O_2. We eventually had to add on metal straps to get this heat distributed in the structure.

When we organized the Space Task Group I guess Dryden asked Melvin N. Gough to go to the Cape to be NASA's representative to the Atlantic Missile Range for all NASA programs. Since we were beginning the Manned Space Flight program, it was appropriate to have a full-time representative there. At that time we had only the Vanguard program. Mel Goff took the job and became the NASA rep. I won't go into all the sordid details of that situation, but at the time General Yates

was the commander of the Atlantic Missile Range and Mel Goff and Yates paried with each other on many occasions. Goff was a very difficult guy in dealings with people at that level and he and Yates played all kinds of cat and mouse games with each other. After Silverstein became the head of MSF, Goff became convinced that he was being undercut by Silverstein. Whether that was true or not is not for me to say, but we were trying to work out relationships with the Atlantic Missile Range and it was obvious that we had to have a close working relationship with the DOD. We'd already had some agreements with the Ballistic Missile Division for the use of the Atlas, for support from the Air Force and its principal contractor, then STL, and later Aerospace. We already had some agreements with those people for the use of the Atlas. STL was really the technical integration contractor for the total Atlas system. Convair, GE and others were involved, and they did the guidance equations for the Atlas, etc. We would have need for the DOD on the ranges and for supplying recovery forces. It was obvious that the DOD was needed as it had the naval ships and the airplanes required for search. We used various other DOD forces at the Cape for planning the launch site recovery and these involv ed the Army and Marines, too. So it was fairly obvious that we needed a total DOD agreement. Silverstein at that time was sort of working around Mel, I believe, and it was at this point that Walt Williams was offered the job of Deputy to Gilruth for operations.

Walt had had a great deal of experience dealing with the DOD at Edwards, so he immediately began to deal directly with Yates. I'm sure Goff was pretty unhappy about that, and during the next six

months it was fairly obvious that Goff was unhappy. He left and went to the CAB. Chuck Mathews, then head of the Flight Operations Divis brought Bob Thompson in to work on the recovery effort and began to be the real interface in the recovery operation. He began to work out the details of the various agreements with the DOD with regard to ho we were going to work with the range, and how we needed support on the total network. We needed people as well as facilities because we had no people with network experience. Walt and Yates worked out the type of reimbursement that would be involved--such as how shoul the DOD charge NASA for the use of their facilities at the Cape, a how should they charge for the ships that were used during recover operations. It's my opinion that Williams and Yates worked out s excellent agreements and we wrote a document which embodied all c DOD support.

Yates really befriended NASA, in my opinion, because he ma certain that the DOD would do everything they could to make thi reasonable cost to NASA. He recognized the importance of the n space program and I think the relationship which developed bet Williams and Yates was the foundation on the good relationshi held with DOD up to the present.

It was at that time that Williams conceived the idea th really needed to be a single point of contact within NASA an point of contact within DOD where all this information flow decision process should be carried out. He proposed that t accomplished. He was named as the single point of contact relationships within NASA and Yates then was named the rep for all DOD forces. Not only he was commander of the Atl Range but he was also named as the representative for all

support. Now it was early in that phase right after Williams and Yates had worked out that relationship that General Davis took Yates' place. Although I'm not sure, I think Yates was appointed to that position before Davis came. In any event, along came Davis and Davis took up where Yates had left off in terms of being a real friend to NASA. Through Davis we worked out a large number of useful relationships with the various DOD support organizations. We would tell Davis what we wanted and then Davis would give this request to the CINC LANTIC (Cinclant) Fleet at Norfolk. He had appointed the commander of COM DEZ Flot 4 (ComDesFlot) (Commander of Destroyer Flotila 4) to be the total recovery rep. Bob Thompson worked directly with the Navy at Norfolk to iron out all these details.

We had a similar relationship with what is now known as the Air Rescue Service for contingencies support, that is airplane support deployed on the contingency basis around the world. That required a great deal of travel through Africa and Europe, in particular. The Air Rescue Service broke up the globe into areas. The Pacific area was given to an Admiral in the Pacific, and he looked after our requirements. He was given the total Hawaiian sea frontier area. It required work through the DOD to the Australian forces because in Australia we literally used the Australian airplanes as well as our own for contingency recovery.

The part I was more intimately involved in was the network operation. Davis appointed a DOD staff for manned space flight and we began to work through this staff to create a policy on how we were going to run a total network--a network which encompassed not only the

NASA remote sites and communication system but the Atlantic Missile Range and Western Test Range and White Sands Missile Range as well, because we were using all of these facilities. We worked out the management relationships on a network basis.

The Atlantic Missile Range was going to be the lead range and all of the stations that were under DOD control would be in the system. DOD stations at Patrick, GBI, Antigua, Grand Turk, White Sands and Point Arguello and ship support such as the two ships we were building and which would also be under DOD control would report in through a single DOD rep. We agreed that that was probably the best way to work in terms of management before we actually got into operation. There needed to be some DOD central management of the range which Graves worked through since he was the TABIU group head. When we got to the operational stage, we couldn't stand for that as a means of operational control. We did not want to work through any funnel at that point--we wanted to be able to work directly with each station. The official paperwork would go back and forth through some central facility within DOD, but our testing and buildup to the operation, the countdown and the actual conduct of the operation would have to be directly through the NASA Flight Director. That was a point of great argument and out of the resolution came a fellow named Pete Clements who was appointed to General Davis' staff to work on these network areas. Pete was a very strong force in supporting the NASA position. In later years he wrote a paper which clarified how DOD and NASA management was conducted-to allow NASA to have direct contact with these

various stations and the people at these stations both from an equipment as well as operations point of view in order to get assurance that these things would provide proper support to manned space flight. It was through Pete's effort and through the work of my people, Walt and me that we convinced the DOD that that was the right way to do it. That turned out to be a real good decision and of great benefit to us when we got around to the conduct of the operation. Pete then became the network controller in the Control Center. What we really wanted was a DOD man to run that aspect for us. It provided us with some additional manpower plus the fact that it then gave us the where-withal to get into the DOD stations. We also gave that man the necessary operational control over the NASA sites. Of course, there was some opposition within NASA to having a DOD man run the NASA sites, but Walt Williams and I always felt, and I still feel that it was right and proper to have DOD accept some of these responsibilities because it showed NASA's faith in DOD support and management. It also gave DOD a feeling of being a part of the program. We felt that wherever possible, since the DOD had all these capabilities of depot support for supplies, we should depend on them for logistic support. That has been a good policy. Many people were opposed, particularly Buckley and Graves, but I think our excellent relationship with DOD as a result of that kind of philosophy validated this approach.

Chuck Mathews was an intimate part of the flight operation management team and a key individual in the development of technical policy in the days of Mercury. I literally worked for him. However, having been appointed a Flight Director at that time, and because of our working together on the details of these procedures, I had a closer relation with Walt.

There were a large number of management meetings that had to be held with DOD to get all of these relationships worked out, get the recovery forces trained, and develop equipment necessary for search and location. All of the equipment that had to be put onboard the ships for bringing the spacecraft onboard was the responsibility of Bob Thompson. He did an outstanding job. Through his diplomatic handling of the Navy and the Air Force we were able to do the job as well as we did. In the Carpenter operation where we overshot, and there was great criticism, I think it was Bob Thompson's putting oil on the waters that kept the problems to a minimum. There could have been a great deal of who did what to whom if it hadn't been handled properly. In the network area we have to give the kudos there to Pete Clements because he did a lot to cement the relationships between NASA and DOD. When Pete's time was up at Cape Kennedy, we requested he become a part of MSC here at Houston. That was an excellent decision also because he continued to keep relations between us and the DOD and the network area running on a smooth course as we began to implement the Gemini network and later modify it for Apollo.

When we started working with the Cape on the interface problems between the launch vehicle and the spacecraft, it became obvious that we needed somebody at Cape Kennedy to be liaison between our people at Space Task Group and the Atlas launch vehicle people. We felt like we needed somebody who was capable of getting along well with both. We asked Porter Brown of Langley's Flight Research Division if he was interested in such a job. He was. We brought him over and exposed him to all of the particulars: the prelaunch elements and operations of the Mercury spacecraft, what some of our launch site recovery problems were, some of our launch abort problems, performance of the Atlas, and all of the checkout problems associated with the spacecraft. We briefed him on the operating elements of the total Mercury Program and then sent him off to Florida to work with the Air Force and Convair people at the Cape, and in particular a Colonel Meyers with the Air Force. Originally, it was Colonel Eikle and soon after Meyers came to work for Eikle. Eikle was in charge of the total ballistic missile program at the Cape and Meyers was assigned to the Mercury Atlas. McMad was the chief Convair man at the Cape, and Porter had liaison with him. Tom O'Malley was the test conductor. Porty, because he is a very competent technical man and also very diplomatic and tactful was able to work out a lot of the sticky details of interface between spacecraft and launch vehicle with the Air Force Convair team. I think his contribution in the area of preparation of a manned space vehicle for launch--the countdown preparation, all the details of how you get the testing done on the range, the writing of requirements at the Cape, getting the range to support all of the prelaunch test that you had to go through,

and getting the astronauts worked into the operation on the pad-- in all these areas he made a great contribution.

In like manner, we had been working with DOD and eventually BMD, the Ballistic Missile Division (at that time it was STL and later Aerospace in Los Angeles). A particular individual had to be designated for us to work through on manned spaceflight problems and serve as a point of contact on the West Coast with Convair and GE on modification of the Atlas. Bob Harrington was working as a tech assistant to me at that time and we put him to work in that job. As such he really worked for Williams, because Williams was the single point of contact with DOD and Bob represented him. Bob did a great deal of hard work on the guidance equations for the launch vehicle and in solving all of the associated technical problems. He made a major contribution to the final configuration of the lightweight telemetry system, the variations required to man-rate the launch vehicle, the emergency as-is system, and the abort sensing instrumentation system. The qualification and mods to the Atlas--the changes in the boiloff valve and in the engines, the upgrating of the engines, the lightening of the structure, the lightweight telemetry system-- all of those technical details were looked after by Bob in liaison with the Air Force on the West Coast. His part in the program should not be forgotten. It was Porter's and Bob's attention to the detail and competence in liaison that made all our relationships with the DOD excellent.

Walt had had a lot of experience at Edwards in dealing with the Air Force and he knew how to work with those people, how they thought, how they reacted, and the way the Air Force worked with their

civilian personnel and contract engineering people (STL and later Aerospace). It was important to understand these facts to appreciate the efforts of those people and make them feel like they were wanted. In fact, I think that although we were very unhappy at times with the Aerospace contribution, the job couldn't have been done without them. Their technical help in making the Atlas a reliable piece of hardware was truly outstanding. We felt like at times that they were overdoing it, but in the end, it turned out to be beneficial to the program. They were as dedicated to getting the job done as we were.

We had, as you would expect, a lot of trouble with the operation of the tracking ships. Initially we had a lot of trouble getting equipment to work because any time you put it onboard a ship you have a maintenance problem. Working in that kind of environment, that is the saltwater environment, and the large vibration problem that exists on a ship and the tremendous electronic interference that you have because you have so many different things on the ship present innumerable difficulties in telemetry, voice communications, and teletype communications. The antennas themselves interfere one with the other. All of those problems were very difficult to overcome, but we were able to overcome them, although voice communications with the ship were poor at best until we got satellite communications. We always had a great amount of noise on the lines making it very difficult to hear the flight control team.

Once we got the technical problems on the ship solved, we had a continuing problem keeping good crews on the ships. This was always a problem because even though they were paid fairly well,

the ship is out of port so much that you have a tremendous turnover and there was always a question in my mind as to whether the people on the ship were adequately trained to handle the job. In some of the later Mercury flights and of course the Gemini flights, the ships were pretty important to us because their location in the world gave us a reasonable contact with the spacecraft.

Another problem involved the flight controllers themselves. Some of the places the ships ended up were very remote like the middle of the Indian Ocean. We had to send these guys off to Africa and get them on the ship well before the network simulations began. They often ended up being on the ship for a very long period. Not only was it bad that we lost so many people for such a period of time, but it was a bad morale problem because of the poor accommodations on the ship. We had to fight tooth and nail to get the flight controllers a reasonable stateroom accommodation and that created disgruntlement among the crew who had to spend all their time on the ship and who felt that the flight controllers should not be treated with kid glooves. Another problem was the per diem allowance on the ships. The civil service personnel had to pay for their meals, etc., and only received a minimum per diem allowance. When we sent Air Force people out on the ships, they got no allowance and paid for their meals out of their own pocket. That was a situation that didn't make much sense to us, and we attempted to work it out with the DOD. We very often used the ship as the Siberia of flight control, and we always rewarded the guys who spent the time on the ship with a good assignment such as Canarvon or Muchea in Australia, Hawaii or the Canary Islands--all placed where people really wanted to go. Even with all their

limitations the ships were a very important part of the network for us, because being mobile we were able to put them in places where we needed them, particularly for the long duration flights. Off South America, either the east or west coast, they gave us a contact during those periods of the orbit where we had very little coverage from the other range elements that we had. We also usually put a ship off Okinawa to give us an adjunct for possible retrofire into recovery areas near the Hawaiian Islands. When we got into Gemini, they were very important to the rendezvous because many times the final rendezvous actually took place over the ship. In the end we really got out of them what we needed, even though the communications were bad.

The ships were actually old victory ships which were cut in two and a piece put in. They were rebuilt from scratch for our use for the Mercury Program and were completely outfitted. They didn't have command capability for Mercury, but it was added for Gemini. They also had telemetry and voice contact with the spacecraft and of course complete consoles and displays just as we had at any other remote site. Each ship was a full fledged remote site. We had just the two ships--the Rose Knot Victor and the Coastal Sentry Quebec. They were modified to support the Gemini Program from FM telemetry to PCM telemetry, and digital command was added. We are using the RKV and the CSQ for the LM-1 flight. One will be off the coast of California and the other off the west coast of Australia to cover the various burns of the descent and ascent engine. Once we complete the unmanned LM flights we will retire them but we will replace them with 5 Apollo ships. One, the Vanguard, we have already used on SA 501. Two others

are similar to the Vanguard in that they are major instrumentation ships, the Redstone and the Mercury. Two more are so-called recovery ships. They have telemetry recording and voice capability onboard--the Huntsville and the Watertown. Both the Huntsville and the Watertown will have C-band radar, but only one has the highly sensitive acquisition device for use while the Apollo spacecraft is in the blackout phase of reentry. Originally the intent was to have flight controllers on them and we did so for 501 and will for 502 but after the unmanned flights, it is our intent to completely remote all the data from those ships just as all the other stations will be remoted. In other words, when we go to the manned aspects of Apollo, we will have only a single representative at each of the stations and his real purpose will be to keep that station informed as to what is going on and give them a good idea what the mission is supposed to be. But from the standpoint of data, command, voice, telemetry, tracking--all that will be remoted to Houston and the world will look to us as it did to the Atlantic Missile Range in Mercury. Data will come to us from all over the world either over cable or by comsatellite. That's the big difference in the Apollo Program. We didn't set out to do it that way. We intended to have flight controllers at the remote sites but with the advent of the communications satellite system which provides us reliable communications, we have been able to completely remote those sites. We can't bring back the full bit stream; rather we bring back selected data. It is almost realtime, as it has very low lag -- 2-6 seconds in telemetry, command, and tracking.

When we first considered the Redstone and Atlas vehicles we

immediately had to face the problem of how we were going to detect failures during the launch phase, particularly when we were within the atmosphere (say below 250,000') while we still had any q on the spacecraft that would result in explosions which would endanger the spacecraft or cause wild gyrations and loss of control of either the Redstone or the Atlas. We were doing this in the period when we had high degree of confidence that the Redstone was going to fly well because it had a good history at least in its last flights. It had a large number of flights --something like 56 flights. But the Atlas during the early phases of Mercury and in 1959 in particular, was having a lot of trouble. By the summer of 1959, the Air Force had had something like 5 or 6 straight failures with this vehicle. These failures were of all types: they lost control, one of the engines would fail at liftoff, they had serious leak problems in either the lox or JPL fuel systems, they were having problems with the pumps rubbing and blowing up and transistor problems, gyro problems, all of the things that would be likely to give us fits about what we were going to do under those circumstances and whether the crews could react to these things.

Initially, Bob Piland and I were given the responsibility within the Space Task Group for working on resolution of this situation. We met with Bob Foster and George Weber, the two lead guys along with Yardley and Faget at McDonnell. I don't remember whether Culbertson was in the act yet from Convair and Jack Keutner, the guy who was representing the Redstone people in the realm of crew safety, at that time. We met at McDonnell and started talking about the various

signals that could be given to the crew and whether the launch vehicle contractors should be involved in design of the display console which tells the crew when they should take certain action. Or perhaps we could put devices in the launch vehicle to detect these failures. We looked at all of the things that cause failure and they were obvious in both vehicles: the pressurization systems, particularly in the Atlas which had a common bulkhead. We were worried about the delta pressure across that bulkhead. We were worried about the angle of attack times the dynamic pressure that we got particularly at max q but there was no direct way of measuring the angle of attack so we ended up using rates. We were worried about fires in the bow tail and considered using a fire detection device, which in airplanes had been historically a bad actor, or fiber optics. We could run a tube down and look at what was going on in the bow tail. We considered what indications we could give to the crew. We started talking about green lights, yellow lights, and red lights -- green to say it was OK, yellow to say maybe there was something wrong, and red that there was definitely something wrong. We rapidly gave that up, as we came to the conclusion that we only wanted to consider catastrophic failures--and those that were absolutely imminent, because if they weren't catastrophic we wanted to get as high as we could before the abort occurred. There were many indications that there were fires in the bowtail of the Atlas all the way to cutoff and still no catastrophic failure. There could be gyro drifts. Since we were using ground control guidance systems, even though the gyros were drifting in the Atlas, the thing could guide you to a normal cutoff and we would rather deal with aborts well up in flight, say above 300,000' than we would

aborts at lower altitudes because of the time involved, the forces on the spacecraft, and the reentry g's that the crew would experience. At that first meeting we did come up with some rather good ground rules. We eliminated the yellow light. As to the sequencing system in the spacecraft and how it was going to detect failures on the spacecraft, and whether in fact we should detect failures in the spacecraft or leave that job to the launch vehicle, we came up with the ground rule that it was probably best to put all this stuff as a special package in both the launch vehicles --the Redstone and the Atlas, rather than have to put an inertial platform, which we didn't have, in the spacecraft to determine attitudes and rates. We established a ground rule that if things were going to happen, and we were going to detect them, we were going to concern ourselves only with things that would occur in several seconds rather than over long periods of time. We could probably detect slowly developing problems through deviations in trajectory rather than onboard measurements. Our philosophy was that we wanted to keep the number of things we were measuring to an absolute minimum because of the complexity of the system and the redundancy that we were going to have to make sure that the system was providing a right signal rather than an unnecessary abort.

Following that meeting we started working individually with the Redstone people. We worked with them first because that was the first vehicle we were going to fly. We came up again with rates and thrust chamber pressures. One of the biggest problems was if we lost thrust very close to the pad. The thing would settle back, which called for a rapid decision. We started looking at thrust chamber

pressure because that was a direct measurement. We also had to consider when we were going to cut off the abort sensing system. It had to be accomplished before the launch vehicle and spacecraft got to places where we knew we were going to have large deviations or the thrust itself was going to decrease, because, in the case of the Redstone, it cut off around some 200,000 feet, we didn't want to abort there so we had to back off some x number of seconds from the intended cutoff and cut out things like chamber pressure and the transients we expected to get when we got shutdown in motion. We came up with what we thought was a pretty good system for the Redstone and then we went out to talk with the Atlas people. We had some very interesting meetings with them. They tried to insist on us giving them some reliability number so they could design the system. We refused to do that and asked them to design the best system they could. We decided on dual parallel redundancy in some cases and serial redundancy in others and we ended up in the Atlas with abort sensing, based on thrust chamber pressure and pressure in the lox and fuel tank. These had to be within certain limits in the delta pressure across the bulkhead and certain rates in pitch and yaw (we found that if you got high rates of roll, the thing could fail structurally so we came up with something like $\pm 20^{\circ}$ in roll). If the spacecraft lost primary electrical power, that was also sensed as a part of the automatic abort sensing system. There was great discussion as to whether systems should be designed in some cases parallel with series redundancy, which

took many hours to decide. We were concerned about the actual values that we were going to have--how far the spacecraft had to be away from the launch vehicle if there was an explosion, and what leadtimes were dictated by the rates at which some of these things could take place, buildup of pitch rate or yaw rate in the launch vehicle, or the rate of decreasing pressure if you had a line break. We considered hard-over signals. That didn't appear to be a problem in the Atlas but it turned out to be a very serious problem in the Gemini launch vehicle (Gemini launch vehicle had ejection seats rather than an escape tower). We wanted to give the crew information from the ground, so we had a command capability in the early stages of the flight to tell the crew to abort or we could in fact abort the spacecraft from the ground ourselves. Then in latter stages of the flight we just used the abort light to tell the crew that they should abort. After getting rid of the tower we considered when the retrorockets should be ignited to get the spacecraft away, or whether they shouldn't be ignited. That got us into the problem of how far it should be away from the vehicle. Near insertion we had to make sure we weren't getting the spacecraft into orbit as opposed to getting down and whether it had to be turned around to get retro velocity. We had to consider what maneuvers had to be performed to make sure recontact didn't occur if retrofired near insertion.

We got good cooperation from all the people involved in that system design. It was probably one of the most rewarding things we did, because there weren't any axes to grind and people were really trying to do the best job they could to make sure that the system was

not unnecessarily complex, that it would work properly, that it would detect only those things that we were concerned about and throw out all the things that had very little probability of occurring. Aerospace was a great help with the Atlas and so were the Convair people. For us Space Task Group people, it was an excellent way to become familiar with the launch vehicle. There was a serious interface problem between the launch vehicle systems and the spacecraft's sequential system and its detection onboard the spacecraft. There were great discussions on whether it should be a hot or cold wire system--that is a system which had a voltage already on it and when that voltage was lost, it aborted. If we lost the interface between the launch vehicle and the spacecraft, we did want to abort. If the launch vehicle fell out from under the spacecraft, or broke off, we wanted to have the escape tower ignite. We eventually came up with what I thought was a good system in both the Redstone and the Atlas. We never had to use it on the Redstone. On the Atlas it worked on MA-3 and proved to be soundly conceived.

One of the other things we were concerned about was range safety. Cutoff and destruct were the two actions the range safety people took in terms of a "sour bird" and we came up with the concept that the shutdown by the range safety people would be detected in either case by our abort sensing system through loss of thrust. When the range safety officer sent the signal to destruct the bird, we wanted

to make sure that the destruct signal was delayed long enough to give us sufficient time to get the spacecraft away from the launch vehicle should a catastrophic failure then result in an explosion. We thought about whether we should do this intrinsically in the electronic design of the system or whether we should do that on a procedural basis. Finally we decided we would put a delay timer in the onboard system in the command receiver. If the range safety people did in fact send a shutdown - manual fuel cutoff (MEFCO it was called) there would be a forced delay of x number of seconds-- that varied between the two vehicles. I think ended up being $2\frac{1}{2}$ to 3 seconds on the Redstone and 3 to 5 seconds on the Atlas. What that meant was that the range safety officer could send his MEFCO signal and even if he pushed both buttons, that is MEFCO and destruct simultaneously, we would first get manual fuel cutoff and there would then be a 3 second delay before the missile could be destructed even though he had sent this signal. In addition to that we got agreements with the range safety people that they would in all cases attempt only to send MEFCO and then wait as long as they could before they would send the destruct signal. In the MA-3 case where the Atlas failed to go through its pitch program and was going straight up, the range safety officer did in fact send cutoff and destruct simultaneously, and our system worked. We had the delay of about 3 seconds between cutoff and destruct and the abort system worked perfectly in getting the

spacecraft away. It worked so well that we were able to use that spacecraft again in the MA-4 flight.

A related issue involved the question when the abort sensing system would be utilized. We decided that in the first vehicle the abort sensing system would be a test vehicle. The signal did not go to the spacecraft. The signals were checked as they came across the interface to see if a malfunction did exist and that the signals were properly acted upon in the booster and then properly given to the spacecraft. We decided it would be the one flight where we had it in so-called "open loop". We preferred to fly it closed loop when a man was aboard the spacecraft, because we wanted a guarantee that the interfaces were right, that no vibration problems existed, and that in fact the spacecraft end of the abort sensing system was properly compatible with the launch vehicle system.

We didn't have any problems with that in the Redstone but when we got around to wanting to close the loop on MA-2 we had a great argument with the Air Force and Aerospace and Convair people. They didn't want to close that loop. They were afraid of the system, and I remember Gilruth and I had quite a discussion on this and it was my advice and counsel that we force them to close the loop on the MA-2. He agreed with me. My philosophy was that you had to prove that the system was compatible and was not going to give unnecessary abort before you were willing to start commiting these things to manned flight in particular and to orbital flights as we had planned with the

monkeys. So we prevailed. One thing we compromised on was we allowed them to open up the limits of the system to some extent - that is instead of using say 5^{o}/sec, they could go to 8^{o}/sec. I think they actually doubled the rates, and some of the pressures they decreased the limits. That was brought about because right at staging in the Atlas, we did double the limits for that period of time because of very high transients. As we test flew each one of these vehicles before manned flight we looked at the response of the whole system during these phases to make sure that we didn't have the limit too tight or too loose. We did find some problems, such as delta p across the bulkhead, where we were getting some screwy responses due to g and the initial transients that were in the launch vehicle. We had to put some orifices in that system to smooth or filter out these transients. We changed some of the limits on the tank pressures, although I don't think we ever changed the rates after we had once set them.

Both those systems on the Redstone and the Atlas turned out to be very good. I think we were all very proud of the way in which they worked. They created great numbers of problems, of course, because everybody was always afraid that you were going to have an unnecessary abort with this system and we went through great pains to check out these systems on the pad to prove that we weren't going to get unnecessary aborts. Again, st the Cape we had great cooperation between the people in checking these things out and ourselves who had been intimate with the design.

Very early we could see that the Gemini program because of its long duration flights would required 3 shifts of people working in the Control Center throughout the flight. We also saw the need for a large group of network controllers. We got together with the Air Force and worked out with General Davis the transfer and direct detailing of 5 military people to Houston as well as a direct DOD representative here to represent the DOD. Although the Air Force was hard pressed to get people in thos days, they supplied us with people to do the job and have continued to do so. It has been a very satisfactory arrangement for both MSC and the Air Force in that it gave MSC the proper entre into the range at AMR and the other ranges that the DOD has responsibility for, as well as making DOD people feel like they were an integral part of the network and the manned spaceflight program. We tried to get Goddard to do the same thing: to give us some people to run the network in real time. We had a great struggle over that issue because Goddard had manpower problems, and was reluctant to send good people here to do that job. A great amount of animosity developed between the Air Force and Goddard as a result. Goddard felt that they ran the network and the Air Force got the credit. I don't think any of us here at MSC ever felt that way about it at all. Goddard ended up sending one man to MSC and he turned out to be a very weak individual, unfortunately.

To this day, we still use the Air Force people and we are very satisfied with that situation. Walt Williams and I always felt that whenever we could we would get the DOD to take on tasks which they could do better than the NASA, that was the job we ought to allow

DOD to continue to do.

There was always a great battle about whether the operation of the stations should be done by the NASA contractor or by the Air Force. Walt and I always backed the Air Force and Goddard backed the contractors. This was a bone of contention between us and Goddard for many years and I guess still is, although less and less these days because Goddard does run most of the stations with the exception of the ships.

Our relationships with the DOD were always excellent, as we usually supported the DOD in most of the things it wanted to do as to network preparation and in terms of what stations it controlled as regards to what stations NASA had direct responsibility for. I mentioned previously the excellent support we had gotten from DOD in the control center at the Cape and the many times we got into great argument over that with Goddard. Goddard wanted to take over the Control Center and be totally responsible for running it. I remember we had a direct confrontation on that once and I literally told Goddard that by God if they were going to take it over, they were also going to have to assume responsibility for the failures and assure MSC that there were always going to be well trained people there to run that job. I told them I doubted they could give such a guarantee with their support contractor and they finally agreed with us in the end. After they looked into the real problem they found they were biting off more than they could chew. They found that they couldn't improve on what the DOD (really Pan American) and RCA people were doing for us. I have said quite a bit about the competence of those people previously.

Initially when we were dealing with teletype traffic on the network we were very limited. In some cases we had 100 word teletype lines (that is 100 words per minute) and in some cases (like through Africa) 60 word teletype lines. Initially we didn't have voice to some of the network sites. So our only means of communication to some of those sites were either by telephone which was very expensive and not very good, or the teletype. It wasn't until later in the program that we had voice capability. I think most people have forgotten that we didn't have voice to a lot of the sites, and in fact couldn't hear the astronauts. We had to hear them through the flight controllers that were there.

We had to devise a priority system for administrative and operational traffic over the teletype. Many times this resulted in a great deal of delay not only during the mission but during the preparation and postflight analysis stage because reams of teletype traffic were held up by the limited lines that we had. We had many arguments with the PAO people because they were continuously trying to send out messages as to what was going on in the flight and these were interfering with our operational traffic. As a result of the priority system their information was very slow in getting to the sites. There were only certain times during the day when the network was up (because of the limited shifting capability we had on the network), then there were only about 10-12 hours a day when the total network was up and in some parts of the world the network was up at different times. The ship traffic was very slow because it was HF, and there were only short periods of the day when the HF communications

were good in order to get long messages out. In the operational traffic we had to repeat a number 2 or 3 times in order to make sure that we got the right number in our post-pass messages. All in all it was a very tough situation to deal with in terms of priorities--getting engineering information on systems out to the network as opposed to the other administrative traffic such were represented in people problems and PAO problems. We were forever having to be Solomon and decide which messages could go out and when certain people could use the network. PAO people in particular were always pretty unhappy with us. That was to be expected. As we got into Gemini, we began to use highspeed data lines. This change permitted us to get the traffic out very rapidly. In the latter stage of Gemini, I don't think we ever had any network traffic problems. We still had the priority systems, but because of the automated control systems that were developed by NASA Communications System and run by Goddard for both manned and unmanned programs, we ended up with great communications pipelines going to all the stations in various parts of the world.

In 1963 when Elms came onboard many people felt that the reasons that Flight Operations (which was under my direction) and Graves' organization (which had the responsibility then for the network aspects of the Control Center and control over the monies and the technical direction of the Control Center contracts) had such a great bucking of heads was a personality conflict between Graves and Kraft. That is absolutely untrue. I don't speak for Graves but I think he

probably felt somewhat the same way about me as I felt about him. I have to admit that though I didn't have any personal liking for Graves, I had complete respect for him and never doubted either his or Vavra's technical competence. I had great confidence that the decisions they made in almost every instance were sound, and truthfully I have to say that most of the technical decisions they made concerning the building of the Control Center were damn good ones. Much of the success of the Control Center was due to Graves' and Vavra's attention to detail and great technical capability.

The areas in which we had such strong differences of opinion had to do with management rechniques and responsibility. You have to remember that the original RFP's and the evaluation of these proposals for both the computer facilities and Control Center facilities (which we decided to do separately) were accomplished by the Flight Operations Division. We did all the basic work of design, all the conceptual work (along with the subcontract we had with Philco and IBM), and the total initial concepts were ours. We got lots of help from other organizations in the Center, but irrespective of that, we were the people who wrote the specs and then put it out for evaluation. We picked the people who did the evaluation. At that particular time, based on my analysis of the situation, I went to Walt Williams and suggested that the Center create an organization that had the technical understanding of all these Control Center systems that we were going to build as I felt I lacked this capability in my organization. In retrospect, I was wrong, but that's how I felt about it at that time. I was wrong because of the management tangle that resulted. I told Williams I thought we needed a separate group under

his control. I recognized that he was really Mr. Operations and I was separating this function out of my purview in the level of the management. I was a Division Chief at the time and felt that we needed a group within the MSC similar to that we had when we built the Mercury network. Those people had the technical understanding of telemetry systems, of communication systems, of command systems, of display techniques and systems--including all of the intricacies of the design principles. We needed a group to carry that out and I recommended to Williams that he get Graves to supervise the job. That point--it was we, the Flight Operations Division that recommended that method of management to Walt Williams is very important. He, on our recommendation had been trying previously to get Graves and Vavra to come to the Center because he recognized the Center's need for the technical capabilities of such a man. He asked Graves to take that job and I spent many hours on the phone convincing Graves that he should come to Houston because he didn't want to come. I too, recognized the need for his technical capability within the Center. Graves agreed to come, we set up the Ground Systems Program Office (GSPO) and I gave Graves all of the people that I had within my organization at that time (which amounted to about 20 individuals) who were working in this area. I literally talked those people into going to work for Graves because I thought that he needed that kind of support and those people were better off participating as a

part of his organization rather than mine, and because they had had the background in the Control Center and Graves needed that capability. We set up an organization to sort out the design details, make the contractual arrangements and deal with budgetary problems.

After those contractors had been onboard for a considerable length of time--months--it became painfully obvious that the method of management that Graves and Vavra were applying to them was, in my opinion, gravely wrong. Graves was developing a tremendous amount of distrust that these contractors were doing a proper job and the contractors were very unahppy about the kind of direction they were getting from Graves and his people in terms of the way they were carrying out their job. Graves' organization was giving detailed design direction on how systems should be built, how they should be interfaced, and how they should be tested. Graves took all that responsibility away from the Philco and IBM organization.

I began to get visits, as did the rest of the Center Management, from the IBM Management. IBM was extremely unhappy with the type of management it was receiving from Graves. There were no qualms or arguments with his technical capabilities but the responsibilities and technical decisions the contractors felt should be theirs, and I agreed. These people were being used as warm body contracts to implement the design and development that Graves people were doing. This meant that Philco and IBM really weren't doing the job they had been hired to do. We were paying them to do the design management

of the MSC both in terms of the computers and in terms of the Control Center equipment. We were taking all that responsibility away from them and all they were doing was responding to detailed technical direction of the GSPO. These people were unahppy to the point that they were willing to give up the contract. That is when Graves and Kraft reached the point of complete disagreement. It was there that I had many discussions with Williams, he understood what I was driving at. Unfortunately, he was more concerned then with Williams problems rather than the problems of the Center. I don't say that in a derogatory fashion. If I had been in the same boat, I possibly would also have been preoccupied. Walt knew something needed to be done, and was hopeful that by the time he had gotten his problem straightened out, his power play, if you want to call it that, with the Center, that he would then be able to get this other situation righted. He never got to that point though and that's when Elms got into the picture.

Now you have to remember that Elms came from the Ford Corporate family. He came from Aeronutronics and the Ford Company took over both Philco and Aeronutronics and made them one organization. He knew the Philco people and they began to call him and tell him of their complete dissatisfaction with Graves and his management philosophy. I had had similar visits from IBM as did Gilruth and Elms. Both Gilruth and Elms began to be painfully aware that there was something wrong. On many occasions Elms talked to me about the situation. I presented to him what I thought were the problems. He usually agreed

with me, but he had to be careful and do the right thing by both Graves and Kraft. He was very reluctant to take this thing away from Graves without great deliberation and great study, and wanted to avoid putting Graves in the bad position of appearing to have had something taken away from him because of unsatisfactory performance. I have to say that very carefully because he wasn't failing, but he was just doing the wrong type of management with the type of contracts we had and the type of management policy that had been agreed upon here at MSC.

Elms would call Graves in to his office and he would have a long discussion with Graves. On a number of occasions he would bring Graves and me together in the same room. I very seldom said more than a few words in those meetings because Graves would explain his method of management and the way he wanted to do it, and inevitably Elms would be saying all the things I would have said myself. It was a very peculiar type meeting because Graves felt like I had been going behind his back and stabbing him at every turn of the road to try to put him in a bad light with the management, and nothing was farther from the truth. I had been telling Graves for many months, face to face, how I felt about his approach to this management problem. He didn't believe me in the first place, and in the second place he didn't agree with me in that that was the way to do it. He felt that if he didn't give these people technical direction in every detail MSC was not going to end up with the system it needed to do the job. He was completely sincere. I don't mean

to imply that he was malicious or devious. He was completely convinced that his way was the right way and it was the way we were going to get the best system. I don't think many people argued with him, it was just that we didn't have the manpower within the Center to follow that policy. If we were to follow that management philosophy he needed 10 times as many people as were doing the job. I guess my argument was (and still is with my own people) how did we get so smart to be able to tell industry how to do the job which they were best able to do themselves? I was very concerned that we were going to end up with a system which would give us problems because the contractor had not evaluated it from the standpoint of whether it was going to do the job because Grave's method of management simply wouldn't let them.

After many months of very bad relations between my organization and Graves (we were accused of many things, and I don't know how many were right or wrong) eventually Elms had to become Soloman. He had no choice but to get Graves out of the act. That was when the total responsibility, both technical and budgetary, was given back to the Flight Operations organization, and that was the time that we were made a directorate. We were able to aplit our organization into divisions as it should have been for almost a year and a half. We had become too large and the total responsibilities that we had just couldn't be handled under a division type of management. Elms made Gilruth completely aware of what he was doing and Gilruth was in complete accord. Of course that was a real slap in the face for

Graves and ultimately he left the Center. As you recall he was made a deputy to Faget, a very good move because Faget really needed that kind of technical strength. But when that happened, the job fell to Faget and me to work out the details. Elms had made the decision and he told Max to get it sorted out, and gave him some pretty good direction as to how he wanted it sorted out. Although we had some problems and arguments over who was going to do what, and what people were going to be left to do the job, and what was going to be done with the other people, I think that Max and I were in complete agreement as to how it should be done within the confines of MSC management concepts. It was a very distasteful thing to have to go through for all people concerned because as I said before it got known as a personality conflict rather than a technical conflict which was not true. I am sure to this day that Graves feels that it was a personality problem rather than a technical one. He was just unable to see what was painfully obvious to the rest of us. He had a hell of a lot of unhappy contractors on his hands, and MSC just had to do something about them.

During the Mercury Program and the initial phases of Gemini, Walt had developed a mode of operation which involved his direct participation in design reviews of manned spacecraft and launch vehicle development. The whole buildup of Atlas eventually fell under his wing. Gilruth let Walt carry that responsibility entirely on his own. Walt wanted a real strong input and some control over the way the vehicles got built. That's where he began to come to ~~lager heads~~ loggerheads with the other MSC elements, and that conflict was further aggravated

when Elms was appointed deputy in the Center. He was brought in to be deputy for Engineering and Development, whereas Walt was going to be the deputy for operations. When that happened, it was fairly obvious to Walt, I guess, that this diminished his responsibility and therefore reduced his authority to control the spacecraft and launch vehicle design. He thought the reliability and quality control organizations ought to be under his wing and that he should be given the authority to continuously criticize the space vehicle design and have this criticism responded to.

I think Walt was probably aiming to establish something like he had at the High Speed Flight Station. The vehicles that were being built, weren't really being built by him, they were being built by NAA. Other organizations such as Langley Research Center had some technical responsibility for ensuring that those vehicles were aerodynamically sound, etc., but in the end, the detailed management of the contracts was his when he was at the High Speed Flight Stations.

In my analysis of this situation I am merely trying to give impressions. I'm not acting as a critic of one mode of operation versus any other. I am just trying to give a picture of what I think was happening. Anyway, as a result of Walt's desires to get his organizational concept accepted, he knew that there should be an expansion of MSC's flight operations in terms of personnel and a separation of functions. We were getting much too large to be handled under a single division concept. Because of his desires to get the MSC organization as a whole changed, he was reluctant to make any changes in the operations organization until he had the larger change accomplished. Therefore we began to suffer in operations

(and he was aware of it), because the expansion of MSC that was taking place was occurring mostly in the Engineering and Development Directorate under Faget. That Directorate was expanding at a relatively high rate of speed both in terms of functions and people. I guess Williams also had an agreement with Gilruth that until we were finished with Mercury, that operations wouldn't expand or reorganize, but after Mercury was over we would. Then instead of the people being shuttled into E&D, the concentration would be on the flight operations end of it. As a result of this kind of thinking on his part, and the organizational struggle he was engaged in, the status of the operations organization was adversely affected. We should have been building up to support Gemini and Apollo. It also affected the status of some of our people, relative to the rest of the Center. It was obvious we needed to split our organization into a number of divisions. I was the only division chief and we really needed other division chiefs because they were actually acting in that capacity. Particularly, people like John Mayer and Bob Thompson, who deserved not only financial remuneration but also Center status and needed that recognition. They weren't getting it because of Walt's reluctance to take those steps. The same is true in regard to my relationship to other Center organizations, because I was only at the division level. Center Management tried to obviate that problem somewhat, because even when we began to have staff meetings locally at the Farnsworth Chambers Building, I was always invited to attend. It was understood that my organization was going to be expanded but it was postponed.

We also really needed the additional manpower capability to properly support the Gemini and Apollo program simultaneously. As a result of our limited resources we weren't able to spend as much time and effort on the Apollo Program as we were spending then on Gemini and I believe that the Apollo concepts and hardware suffered as a result. We weren't able to spend enough time worrying about Apollo operational compatibility. The total mission planning that was being done, the plans for what was necessary to prove the spacecraft, the unmanned aspects of the flight operations in order to manrate the spacecraft, and the test program necessary to accomplish certain things before we were ready to land on the moon, we just simply didn't have the manpower to do. Eventually, both in terms of status and manpower we were able to build to where we could make the right contribution, but it did cost something like a year or two being lost in getting to the position where we should have been.

All of this sounds like criticism of Walt from the standpoint of that it didn't take place. I don't mean it to sound that way because I fully understand how those things happen in management reorganizations. The more Walt pushed this concept of his, the more he became at odds with the MSC management. It became more and more obvious in 1963 that Walt's ideas were not going to carry, that he was falling out of favor not only with MSC Management but the upper management of NASA as well.

About that time Mueller came on board. Mueller had some definite

ideas about operations. He wanted a very strong operations director at the Headquarters level and he wanted Williams to come to Washington and take that job. I guess that was prompted somewhat by the Center's desire (though I didn't realize it at the time), to give up Williams. I suspect they agreed with Mueller and tried to influence Williams to go to Headquarters. At the time Walt wanted to resign because he felt he had no chance to do what he felt was necessary at Houston. We in the operations side of the house were distraught over the possibility of the loss of Williams because of his very capable leadership qualities, and the need for having someone strong within the Center to represent the operations side of the business. I guess that's the concern anybody would have who is of a junior nature and who is to step into his shoes. I guess that was something of my concern of my own future, not in terms of my own status, but in terms of being able to do the things that Williams had done within the Center. I felt very close personally to Walt and had always greatly enjoyed working for him and with him. I had great respect for his management and his decision making capabilities. I was very concerned over his personal future and did everything I could to talk him into taking the job in Washington rather than resigning, because I thought his own future would be better if he went to Washington. I felt that he was the only one that could go to Washington and still run the operations end of the business. He could command the proper respect from Houston and from

the Marshall Spaceflight Center in the overall conduct of the operation sort of as a czar of total Apollo operations.

Apollo had a concept that was entirely different from what we had developed for Mercury and Gemini. In those programs we really looked upon the Air Force as a subcontractor or prime contractor to the Manned Spacecraft Center whereas when we got to the Apollo launch vehicle it was obvious that Marshall had equal billing with MSC. Marshall had as much stature as MSC in that they were supplying the launch vehicle and therefore Walt could very satisfactorily handle this delicate relationship by being at the headquarters level. I thought it was good for him to go up there, and he could continue to maintain direction over our operation elements here at the Center. My final reasoning was that certainly if he felt like the job couldn't be done from up there (and I guess he and I both had some serious doubts as to that because you are only as good as your organization and when the two get separated, then it becomes a very difficult task to try to maintain day-to-day contact with the real problems), and he wanted to leave NASA, certainly his capability and demands for a senior position with some outside organization would be a heck of a lot better by resigning from that level in NASA rather than resigning because of some huff he had had at Houston. I think that proved right, too. I am glad that he decided to follow that course of action.

Once Walt transferred to Washington, it was very obvious that he was continuously unhappy with his position at Headquarters. In retrospect you can see why, because of the policy that Mueller follows.

Mueller just doesn't believe in delegating authority to anybody in his organization. His principal subordinates end up being relatively weak. I think his method of organization prevented Williams from really being effective. I got offered that job later and turned it down for that very reason. I felt I couldn't possibly do a proper job of managing the detailed conduct of any operation for any kind of program from the Headquarters level. You have to be where the people are doing the work, and have the day-to-day problems brought to you so you can give proper guidance and bring influence to bear on the decisions that are being made.

I feel that Williams made a tremendous contribution to the success of the manned spaceflight program. I think he had a superior background in manned flight and airplanes. He understood flight tests extremely well, he understood safety and pilots, which made him very capable of understanding the astronaut problem. He understood piloting problems in terms of how you fly things and what is needed in terms of human factors as well as detailed engineering design to make hardware systems work properly as well as safely. His management capability in dealing with the DOD and his influence in the establishment of managerial concepts between us and the DOD both in the development of the Atlas and in recovery operations and network operations, were evidence of his managerial prowess. I think he was directly responsible for the success we had operationally. I am personally grateful because he gave me the opportunity to work with him and understand his way of doing business. I learned a great deal and my method of operation and management closely parallels his.

We are of different personalities, of course, and that has a lot to do with any kind of management. Certainly I would say that I and the operations area per se learned how to do our job from Walt Williams and I think the country owes him a great deal for the contribution he made. I was very upset over his leaving and I think it took us quite awhile to regain our stature after he left.

Walt had sufficient stature as an engineer and manager that people had great respect for his capabilities. Their response to his direction was bound to be good. Not only NASA people like Von Braun, but the generals of the Air Force as well, were willing to accept his direction and seek out his advice. That's the reason I think he was so good in managing the operational elements of manned spaceflight. He had a lot of background in dealing with military people and had no fear of them. He had high confidence in his own abilities and rightly so, as he was an extremely capable engineer and manager.

We all have our faults -- no one individual is perfect. You could say some things about Walt's faults, but I think that would be unfair. Though I know he had them, I was completely willing to accept them because of his contrasting strengths in other areas. A lot of people disliked Williams, and from the time I came to work for NACA people used to complain about his method of management and his grossness in handling situations. But I never had that trouble with him. We developed a mutual respect for each other and therefore I found him to be a very reasonable man to work for. As long as you were doing the job right, Walt was always there to support you and to make sure you got the tools you needed. I felt it was a great blow to to our program when we lost him.

Somewhere back in 1960-1961 we started looking into the things we had to do for both the Gemini and Apollo Programs and the future of manned spaceflight in terms of the ground control operation. It became obvious that the Control Center that we had at the Cape was inadequate. We were using the same philosophy in the design and construction of that Control Center as we had for the Mercury spacecraft--that is, we would use off-the-shelf equipment and wouldn't try to use any sophisticated methods of displaying data and computation techniques for resolving systems data in real time. In around the beginning of 1961 we started an in-house study to look at the particular requirements for Gemini and Apollo, but we also had in mind trying to make sure that our requirements were more generic in nature. We wanted something that would be useful for programs beyond Apollo. Dennis Fielder, Tec Roberts, and John Hodge and his organization worked on this assignment. We decided we would have a competition and put out an RFP for preliminary design and specification of what the Control Center ought to be. We got a large response to that RFP and in the selection that followed Philco was chosen. This was the Philco Company out of Palo Alto, California, known as Western Development Laboratory. We started working with those people and made a number of visits to Palo Alto to impress upon them that we were not trying to get back our own words on paper as we had in the case of Space Electronics Company when we tried to set down the specifications for the design of the Control Center at the Cape.

What happened there was we just got our own thoughts put down on paper and that's what ended up being a spec. We didn't want that to happen--we wanted some ingenuity applied by the Philco Company.

We also recognized that a large element of this new control center would be the computers. We would need a different communications interface with the outside world because of the data rates we were talking about, and we would need different handling of the data aimed more toward computerizing the display capability. We wanted that to be an integral part of the Control Center we were designing. We decided there ought to be a separate RFP.

One of the major differences in the Mercury and Gemini Program was the use of a much more complex command system not only from the standpoint of the spacecraft but also from the standpoint of command to the Agena. It required a complex digital command system which had to be computerized as well, or had to have the use of building command loads done in a computer. Such a new command system should be a part of a new Control Center. We felt by going out on a separate computer contract, since it was a fairly large contract, we would prefer to have more direct MSC control.

We went out with an RFP and had that evaluation completed considerably before the evaluation for the Control Center per se. We had been working with IBM and knew their capabilities, but we felt it only fair to industry to put this out on a general RFP rather than going sole source to IBM. IBM had a leg up because they had

been involved in the Mercury Program and they knew a great deal about the manned spaceflight program. They also had a lot of the programs already developed which could be utilized for the Gemini Program, in particular. Thus, it was not unexpected when IBM won the contract. They won it on the basis of not only capability but on the basis of cost. That was one of the first things we did when we got to Houston in the fall of 1962, was to evaluate that contract. We got the spec together for the rest of the Control Center--all the display and command and communications systems, the development of the console layouts and our concept on the need for having support staff rooms came out of the study that we had with Philco. It was obvious that we were going to have to have large numbers of people involved in systems analysis as well as trajectory analysis of Gemini Program and the Apollo Program. This was where we came up with the concept of having the systems specialist, trajectory specialist, and network specialist an integral part of the Control Center. These were housed in separate so-called support staff rooms which were really separate control rooms for various systems areas as well as backup for the mission operations control room. We had been through the analysis of our needs with Philco--the layout of the consoles, the number of consoles we needed, the types of large screen displays we wanted, plot board vs the projection-type displays, and the advisability of using television for display. We had a fairly complete spec when we went out with an RFP.

One of the biggest problems we had in development of the Control Center at the Cape was the fact that the computers were located at Goddard while the Control Center was at Cape Kennedy. That separation was a difficult management problem as well as a technical problem. We insisted that the computers had to be an integral part of the control system, and in fact there was no other way to build it since the display system was as much a part of the computer system as was the trajectory computations. We went out with the RFP and Philco won that competition. During the winter and spring of 1963 we got Philco onboard.

In the meantime we went out with an architectural bid to provide the building to house the Control Center. In late 1961-1962 the question arose as to where the Control Center should be located. At that time, many of us had formed the opinion that it was important to be at the Cape during the latter phases of checkout of the space vehicle, since there was a large amount of interface between the spacecraft and the launch vehicle. The people in the Mercury Program had spent a great deal of time at the Cape and it was not the best of worlds from the standpoint of separation of the people from their place of work. However, there was an opposing argument to the effect that the people involved in the operations should have some say about the design of the spacecraft (particularly its instrumentation system), the various aspects of the failure modes of the spacecraft systems, the type of control that could be exercised both by the ground and the crews, and the types of systems analysis (which required an intimate association between the operations people and the program office and systems engineering people). We really didn't want to separate those operations

people from the design elements because we felt we would end up with a vehicle that wasn't operationally sound, if we didn't have that kind of intercourse between operations and program office people. We talked this problem over many times with Walt Williams, Bob Gilruth and the program office people. I think we all arrived at the conclusion that we should put it in Houston.. Many of us at that time had some second thoughts about the soundness of that decision because of our past experience in working with the checkout people at the Cape. However, in retrospect there is no question but what we made the right decision. The intercourse between the operations people and the design engineers is extremely important.

Also back in the late 1961-early 1962 time period we began to look at the scheduling of the Gemini and the Apollo Programs. Back then there was a great overlap between Gemini and Apollo, because we were originally going to fly unmanned and manned vehicles on the Saturn I. It became obvious to us that one Control Center was going to be insufficient to support both programs. We had a number of discussions with Williams and Gilruth again on the need for two control rooms to support the programs. In retrospect that was an outstanding decision because we later did begin to have the overlap between Gemini and Apollo which forced us to use two control rooms for checkout of the systems in the Control Center, checkout on the pad, and the actual overlap in flight. In retrospect we would probably have been better off if we had built four rather than two. But I doubt that we'd ever been able to convince anyone at that time that that was the right thing to do.

One of the other things which I haven't mentioned is the simulation facilities. Simulation was something very difficult to sell back in the Mercury Program as an integral part of a control center. The Control Center actually got built without the simulator being a part of it. It was added on at our insistance after we were finally able to sell management on the idea that simulation was an important aspect of training and necessary not only for the flight crews but for the flight controllers as well. We built this closed loop simulation capability on as an extra to the Mercury Program and actually interfaced it with a procedures trainer.

When we got to Gemini and Apollo the training simulator of course was an accepted concept. We built the simulation facility in as an integral part of this Control Center design which was also an excellent decision. It turned out later that we failed to allow sufficient space to provide both Gemini and Apollo training within the 4 walls of Bldg 30, so we ended up using a warehouse, Bldg 422, for that and the computers necessary to provide the modelling of the various equipment that we were using in flight, particularly the Agena. We did not have a trainer which could simulate the Agena vehicle and we had to build a mathematical model to simulate the performance of the systems, the telemetry that was coming out of those systems, and also the command system. The command system was a very important aspect of not only the simulation but the detailed training of the crews who had the command and control of this completely unmanned vehicle.

It was fairly obvious that the Control Center would not be ready to support the early phases of the Gemini Program and therefore we were going to have to make some modifications to the Control Center at the Cape to support the non-rendezvous aspects of Gemini. We knew the computer facilities that we had at Goddard. We also knew that control from the Cape would be impossible because of the complexity of the rendezvous aspects of Gemini. But we thought we could fly the early unmanned and the first manned flights of Gemini from the Cape. At the same time we were building the Control Center here at Houston we had to modify the Control Center at the Cape to support that phase of the Gemini Program. At the time it seemed probably that we might have to support some of the early Apollo tests from the Cape Control Center. Getting the Control Center built and equipment installed and checked out as a working system is a real tribute to all the people who had anything to do with it. The Philco and IBM companies had a major role. Philco actually established an interim site where they set up the 7094's. The company started developing the software in one of the buildings off the Gulf Freeway way before we had the control center finished. They were well ahead of the game by the time we moved into the Control Center.

The GSPO did a fantastic job of getting the Control Center built in the time available. The technical problems that they faced in development of that overall system were horrible. They had to make a number of compromises in the display system and the command system areas. For instance in the display system they went to the charactron (?)

system which probably was not pushing the state of the art at all. These other digital television techniques could have been developed at that time, but we chose to go with the charactron system because it was an off-the-shelf system and one which we were fairly certain we could make work. In retrospect I think that was a good decision. Whether we would have been able to develop the digital television system in time seemed questionable. We wish we had it now but certainly the charactron system proved completely adequate for the Gemini and the early phases of the Apollo Program. We wish we now had the digital system for the more complex parts of Apollo. But the charactron will suffice. Another thing they did was to use a completely wired command system rather than go to a computerized command system. Again a digital system would have been more appropriate and more flexible and would have put us ahead in Apollo, but I think the tasks facing us were too momentous to take on computer software development at that time. There were a lot of compromises made, but I think that all in all they ended up giving us a system which had tremendous capability. In terms of more detail in the systems and the Control Center and how they were arrived at and some of the development of the requirements and the interfaces between what was then the Flight Operations Division and GSPO and the rest of the contractors in the outside world, I think you are better off discussing that with people like Dennis Fielder and John Hodge, Dick Hoover, Tech Roberts.

IBM's Jim Hamlin was extremely prominent in the development of the IBM system to support Mercury and was named IBM's technical manager

of the Houston operations. Hamlin was probably the outstanding contributor from the industrial side of the picture in the Mercury Program and he certainly was the man responsible for the development of the 7094 system which we used to support Gemini. I can't praise him too highly. He was really an outstanding individual. The Houston Philco operation was put under Walter LaBirge and certainly he and his people along with IBM deserve a great deal of credit for getting MCC systems. They had a lot of very complex technical problems, they had a lot of new hardware to develop and getting it here on the schedule, and that they did was really outstanding.

One of the interesting decisions we had to make in the spring of 1965 was when to switch from the use of the Cape Control Center to the Houston Control Center. We did some real soul searching about that. We realized the complexity of utilizing this Control Center for the first time and the whole development of all the new procedures, utilizing particularly the display system and the computer system that we had. We decided we would monitor the Gemini II flight from Houston to bring our systems there up for the first time. That took a lot of doing, but we were able to accomplish it and we did have the display system and the whole Control Center monitoring what was going on during the flight. It worked fairly well. Having seen that come to pass and being well satisfied with the capability, we decided that for Gemini III, we would develop all the procedures and literally in a simultaneous fashion act as if we were controlling the flight from Houston while actually controlling it from the Cape. We set up all the teams necessary so we could get all the procedures developed. That also came off extremely well, but because of the

complexities in the upcoming rendezvous flight, we wanted a minimum of two flights under our belt before we began the use of Houston. Gemini 4 was not a very complex mission. Gemini 5 was a much longer duration mission and we were going to do some practice rendezvous with the rendezvous pod. That would give us a reasonable buildup to the very complex rendezvous mission of Gemini 6 and the use of the Agena as well. We could have flown Gemini 4 and 5 from the Cape but we could not have flown Gemini 6 from the Cape. We wanted to get into the Control Center and get used to it, develop all our procedures with Gemini 4 and 5, since we had had this good experience on Gemini 3.

We had one hell of a time convincing George Mueller that that was what we should do. We brought him down here and talked to him. I had not trouble convincing Gilruth. Gilruth felt like if we felt it was possible then we could certainly do the job. He always had a great amount of confidence in the flight control team being able to do what it said it could do and if that was what we wanted to do, then he supported it. But we couldn't convince George Mueller and it took us almost 1½ months to convince him. We had to staff both the control center at the Cape and at Houston. That split operation was beginning to get to us because we didn't have the manpower. If you recall, Gemini 3 required only one team of people since it was a 3 orbit flight, but Gemini 4 required a 3-shift operation. We needed the full capability of our flight control team here in Houston or at the Cape, we wouldn't afford to run both simultaneously. We came to a point where we either had to fish or cut bait and we finally convinced George Mueller that Houston was the right place.

We decided that we would utilize the Control Center at the Cape for the launch phase of Gemini 4. We developed the launch and launch abort software in the Goddard computers and we set up a cadre of people, as I recall it was Glenn ~~Tuney~~ Lunney, to be the backup flight director at the Cape, but then made the decision to fly the thing from Houston if we could. It turned out of course that we did and it performed extremely well.

Getting into Houston and getting that under our belt before we had to worry about the rendezvous was probably one of the best things we did from the standpoint of flight control operations. It gave us a real leg up. There were a whole raft of things that happen when you get into a new facility that you really can't plan for and think out. The whole procedural development in this Center was a tremendous job and people like Gene Kranz and John Hodge were certainly the leaders in that sort of development.

The whole scheme of rendezvous, program development, etc., was certainly a tremendous effort on the part of everybody concerned: the Mission Planning and Analysis Division developed concepts together with the Flight Control Flight Dynamics people and IBM. Although that is more technical than institutional and of no particular interest to you, there is an aspect of the development phase in support of the Apollo Program which should be discussed. That was the trauma that we faced when we considered the increased requirements we would have as we moved from the Gemini Program to the Apollo Program. We began to study the problem. John Mayer and his people reviewed the capabilities of the mod II of the 7094.

The size of the computer programs that we had for Gemini would not begin to compare with what would be necessary to do the Apollo Program and particularly the lunar landing, and it became painfully obvious to us that we were going to have to go to the new breed of computer. The 7094 was really a scientific computer and not a computer developed for real time use whereas the new computers that were being developed by IBM, CDC, GE, and Univac were specifically real time computers. This second generation computer was designed with that in mind as well as scientific use; these were high speed input-output computers.

Another major management decision had to be made. First, we had to convince ourselves that we needed a new generation computer; then we had to convince our own management. Then we had to convince George Mueller and OMSF, and then we had to convince Webb (a long drawn out process) that we should to to the new generation computer. In addition to that, it was fairly obvious to us that it should be IBM's computer. IBM was the only computer manufacturer in the country who had the software capability, and it was probably the only company in the country who could meet our schedule for delivery of both hardware and software in time to support the Apollo Program.

Complicating the picture was the fact that this contract would be a big plum to the successful computer manufacturer. There was great reluctance on Webb's part to accept our recommendation to give this contract to IBM on a sole source basis because he was surely

going to get a great amount of flak from the other manufacturers. We felt certain that if we had to go through a competition and IBM didn't win it that our chances of meeting the Apollo schedule were impossible. I think people like John Mayer and Lyn Dunseth are better equipped to talk to you about that.

The sequence of events were like this: we convinced Seamans and Webb that we were right in terms of schedule. We appointed a committee made up of computer experts from government establishments- from Langley, Marshall, and Ames, and convinced Seamans and Webb on a technical basis that we were right. I don't think we ever had much argument that the software was still going to have to remain an IBM contract because in face we had a contract with IBM that ran through the Apollo Program so I doubt even legally we could have gotten anybody else to do the software besides IBM.
But on the hardware basis, Webb decided his method of approach would be to call in the computer manufacturers who had computers in the development phase and ask them if they could meet the schedules for the Apollo Program. We had already determined when we would have to have the computers, when they would have to be installed, when they would have to be fully operational in terms of all the Control Center systems they were going to have to support, and when the software would be required. After lengthy discussion with Webb, the competing manufacturers, the Headquarters people, and ourselves, it became obvious that nobody outside of IBM could meet the schedules

with the possible exception of CDC, and all other competitors agreed to abstain from competition. CDC still felt that it was possible for them to provide the hardware and software in time.

We felt that CDC didn't understand the total magnitude of the job that they faced. With Webb's approval, we decided that we would take another month and further brief CDC on the details of the job that had to be done. We had CDC people come to the Center for about a 2-week period and went through the entire details of what we had to do. They finally threw in the towel and admitted that they had small chance of meeting the schedules. They would go along with the decision to let IBM have the contract, although it was obvious that they would have liked to have had it. They agreed to go along with the NASA decision to go sole source to IBM. That was a traumatic experience for everybody.

At the same time, when we started developing the new digital command system for Apollo it was obvious that the digital command system we had for Gemini was not flexible enough. We were continuously having to change it. It was an inflexible system and we needed a new generation of computer also in the communication processing area to handle data. We needed a more modern type of computerized communications system. That's when we began to conceive the system called C-Cats--Command communications and telemetry system. Telemetry was also a part of this thing. We had been using d-comm stations and it was fairly obvious that because of the flexibility needed in the Apollo Program that we needed a more flexibile telemetry

decommutation system--one which could be easily changed. This again became a computerized system almost by definition. So we then had to start developing C-cats. We developed it almost as an in-house design along with Philco engineering support and under contract to the Univac organization. On the basis of schedule, software, familiarity with the systems, and the need to provide that system for the Apollo schedule, we went from 2 Univac 490's, which were acting as comm processors, to the 494 for the total C-cat system. That required a lot more detail and technical discussion which I am not capable of doing. People like Pete Clements, Jim Satterfield, and Lyn Dunsuth ought to be involved in that kind of discussion if you are interested.

MSC, after the Mission Director concept got started had been strongly opposed to the way it was carried out. When the Operations Director concept began we had good reasons for establishing it within MSC and with contacts outside MSC. The operations director was a part of MSC. We established that for a number of reasons. One reason was that interfacing with the Department of Defense as we were, we felt there should be a single point of contact within manned spaceflight operations for all inputs to and from DOD for support of any kind--financial, technical, or managerial. In addition, it has always been fairly obvious when you are running an operation and you get in the conduct of the flight that you have to have a military type of organization where there is one man totally in control. That works very well when you have a single organization within NASA that has the operational responsibility and the responsibility for running all aspects of the job and it is really the best way to do it.

When we got to talking about this problem with George Mueller, we told him we liked this concept and tried to tell him that that individual ought to be in the Manned Spacecraft Center. When we got to the point of running the future programs, where there was more than one Center involved (there were three Centers involved in the Apollo Program and two during Gemini, he felt that the location of this function should be at Headquarters and the single point of contact ought to be at Headquarters. We never have agreed with either philosophy because all of the work that is done with the DOD within OMSF is done through MSC. We have all the operational responsibilities except launch, and the minor role that Marshall plays in the operation, and which even there is carried out through us. So we have felt all along, that we should have the total operational responsibility. We have always had great arguments about that with Mueller and when Williams left Headquarters, he came up with a concept of having more than one of those guys because he thought they ought to work on different missions. He came up with a triumpherate concept. He wanted to have 3 people. He had thought of people like Hewy Eikel, who was initially in the Atlas Program for us at the Cape, and then went to work for TRW. One of the others was Bob Payne from the Cape who was also with TRW and later somebody else. Anyhow he wanted to get 3 guys to come to work for NASA and take on this mission director job at the Headquarters level. The whole concept has been very objectionable to us, and got more so as time went on, in terms of what their total responsibility was to be. The concept was that certain parts of the various organizations within OMSF Centers

would report directly to the mission director both prelaunch and during the flight rather than through the normal Center channels. That's been our major objection. For instance a flight director, a medical director, a flight crew director, and a recovery director all reported separately to the mission director leaving out any of the organizational lines of authority that we have within the Center operations organization and further leaving out the Center Director. We wrote a large number of critiques on this subject, in which we expressed our objections. We also commented directly to Headquarters on some of the directives they have sent us from time to time regarding mission director responsibility. To my knowledge, I don't believe we have ever received a reply to any of our comments.

As a result, we have been at odds with OMSF over this whole concept and therefore admittedly have not supported it too well. Also many other things are involved such as establishment of the initial requirement of the network, and the detailed day-to-day conduct of business with Goddard and the DOD. OMSF has tried to make the mission director a part of its management chain, which since we have the total responsibility for that job, we don't see why it is necessary.

Over about the last $2\frac{1}{2}$ years we have had lots of battles over this issue. The more recent one was when both Kennedy and ourselves got together and tried to combat this effort because Debus feels exactly the way we do about it. He saw no need for a mission director. We both agree that he has the responsibility for launch and we have the responsibility for flight and the interfaces between the two are

pretty clearly defined. The responsibilities that each has in the area during launch and during flight are very well understood between the two of us and I don't think there is any conflict there. I have always contended that during the flight operation there is no need, as Debus thinks, for a mission director. However, prelaunch and during the buildup to the operation, I do think there is a need for someone at Headquarters to be responsible for a given mission. I think that one man is sufficient to do that job, but I think the operational details and decisions that have to be made, of a necessity, have to be made by the Center that has that responsibility. Nobody can be in Washington and keep up with the day-to-day decisions and handing down of policy with regard to the operations and then expect those people when it comes the day of flight to walk away from it. It just doesn't work and if the guy is really going to be director of the operation he has to participate in it day-by-day. You just can't do it from Headquarters level.

In the differences of opinion and the technical problems that develop between the Centers, to make sure that the people of Headquarters level understand the operating problems, there needs for somebody to represent the operating problem at that level. I think there is a need for an operations organization in Headquarters to be responsible for this kind of administrative problem, but they should not participate at all as far as I am concerned in the detailed conduct of the operation.

We made these points again recently with Mueller, we told him that if he was going to have an operations man he ought to work for the particular program director. OMSF has agreed to this arrangement, and has now assigned a mission director as a deputy to the program director. I think I finally got agreement from Sam Phillips that this whole business of chopping certain pieces of an organization and putting it under the control of the mission director at certain points of time is wrong. However, I've never seen this admission on paper from him. Maybe someday we will. I think he agrees with that principal. Whether he can ever convince Mueller I seriously doubt. But he and I at least have had some working agreement. OMSF is forever sending us all kinds of directives which we have great differences of opinion over and don't want to do because we think it is usurping our management responsibilities here at the Center. Gilruth has always been opposed to it. All in all it has been a real thorn in our sides and I am sure a thorn in the side of Headquarters ever since this concept came up. I don't think either we or Headquarters is ever going to get our way. That is to say I don't think it will ever get to the point where we are completely satisfied, nor do I think it will ever get to the point where Headquarters is satisfied unless possible if Mueller were to leave the organization and we could get some different arbitrator as to what should be done up there. He has been pretty adamant about his concepts in that field. I don't know where he became such an operations expert but nevertheless he sets himself up to be one.

I think it was unfortunate that we at MSC felt like there had to be an operations director after Walt left because I think if we had thought very long we would have realized that without somebody like Walt Williams in the job that the concept really doesn't work too well. I am convinced that operationally, we don't need one. Debus has the responsibility for conducting the launch and we don't contest that at all. We are perfectly happy to have him run those parts of it as long as we are satisfied that we have our inputs on the status of the spacecraft and the status of flight control facilities and he has never questioned that. We have never had any real problems with him on recovery operations. I think he recognizes our responsibility there. Those are the areas we would expect conflict and I think he would always be agreeable to whatever we wanted to do and to take our inputs. Then once the thing lifts off the pad, he has always been very happy to wash his hands of it and only make inputs when asked. I think that's only right and proper because I think that's where our job lies.

When we started looking at the lunar flight plan it became obvious that a lot of the points for injecting from early orbit to translunar flight were going to take place over the southern part of Africa and southern Atlantic or over the Pacific after passing Australia. If we wanted to have tracking or needed to have tracking during that period we were going to have to provide some kind of capability to do that--either ships or airplanes or both. We also considered the need for tracking during reentry. In our early studies we hand't progressed sufficiently far enough along in the program

to know if we would need to have continuous tracking during the trans-lunar injection burn. If we did we were going to have to have a tremendous number of ships in order to provide that kind of contact. We arrived at a compromise and picked a time of 7 minutes after injection where a go - no go decision would be made as to whether we were truly on a proper trajectory to the moon. This decision meant we would have a requirement for two ships. If we were to look at the launch azimuth at Cape Kennedy and the cutoff range of the Saturn V at orbit insertion, the coverage that we can get from Bermuda and the coverage we can get from Antigua, we needed a ship there to cover that orbital insertion capability. So we ended up with a third ship to cover that variable launch azimuth for lunar launches. It looked like it was going to take 20 or 30 ships to get continuous coverage and that was prohibitive in cost. What we wanted to make sure of was to have the recording of data and voice during that period, and if possible real time voice during that time period so we could be cognizant of what was going on. This could be done with airplanes. We could cover the injection area with 4-8 airplanes depending on the type of airplanes and the number of launch opportunities. We were considering 3 revolutions around the earth from which we could launch to the moon on any one of those three. We found that the checkout requirements on both the launch vehicle and the spacecraft required several hours in earth orbit. In order to cover the total injection

capability we ended up with 8 KC-135's. We modified them to accommodate S-band communications, voice, telemetry, and a record capability. The plane has the capability of flying to one of our remote sites and dumping the data. This gave us the capability of assuring ourselves that we had the data recorded should some catastrophy take place during that burn. We would have voice relay by going from HF source of communication to the airplane up to the spacecraft and then having it relayed out of the airplane much the same as we have done during reentry on both the Mercury and Gemini.

When we began to get enough experience with lunar trajectories and tracking it developed that unless we have a very gross malfunction, where the S-IVB completely turns around and burns retrograde, that we really didn't have a time critical problem. If worse comes to worse we could probably do without the two injection ships. We will probably end up using them in a somewhat different concept. It has always been my belief that the 2 ships we had in Mercury and Gemini allowed us a great amount of flexibility in our mission planning, as we were able to put them anyplace in the world we wanted to support some critical maneuver or phase of the mission where we needed communication data. The three Apollo ships and the eight airplanes are really going to allow us a great deal of operational flexibility in future lunar flights. The three ships have S-band and C-band tracking capability. Our first experience with them has been pretty good and a lot better than most of us had hoped for. They have been worth the cost and effort although they did end up costing a lot of money and were one hell of a management problem for Goddard to build. A tremendous number of organizations got in

the act -- the Navy, several factions within the Air Force, OTDA, and Goddard and us. The complexities of the contract itself were a nightmare. The Navy built the ships and General Dynamics did all the installation of equipment. The computers have been a very tough management problem for Goddard.

333-4

Continuation of Chris Kraft Interview

To intelligently discuss the MA-5 flight, it is necessary to go back and look at the MA-4. In that flight we had a problem right near the end of the first rev. That was a one orbit flight, and the control of spacecraft attitudes began to be sluggish and the limit cycle on the attitude system appeared to be going awry. Something was happening in the thrusters. We didn't have much instrumentation to indicate what was happening in the lines or the filters or directly with the hydrogen peroxide system, and there was speculation that we were getting slush in the lines. This was thought to be the reason for poor firing of some of the thrusters.

We put some instrumentation on MA-5 to give us a look at this problem. We attempted to solve the problem in advance by adding filters and other things to the lines. Trouble began for MA-5 at about the same point in time as it had in MA-4. This was intended to be a three orbit flight with the monkey onboard and it was to approximate the anticipated flight procedure we were going to have for the first manned flight. We began to have the same attitude control problem again. It turned out that the H_2O_2 wasn't getting cold, it was getting hot. We didn't know exactly what was heppening, but we were again beginning to lose control and we were using an excessive amount of fuel because of limit cycling. By this time, we were coming up over Carnarvon and it looked like the situation was worsening. When we came up over Hawaii, it looked worse. We still hadn't made a decision as to whether to abort the mission or try one more rev. There isn't much separation between Hawaii and California and we had to make up our minds by the time we got to California. We had about 20-30

seconds prior to initiation of the retrofire sequence at California to make a decision. Don Arabian was the systems engineer on the console and I was really pressuring him for a decision. I had also talked to Arnie Aldrich, the CapComm at California.

It is interesting to note that just before the Hawaii station made contact with the spacecraft, the Cape lost all communications with Hawaii, and it was about 5 minutes before communications were restored. It seems that a bulldozer had severed our communications with the West Coast and Hawaii somewhere near Tucson, and they had to be quickly rerouted. That was a lesson for us in terms of diversification of our comm lines for Mercury.

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After pressuring an answer from Arabian, I decided that if the system had not righted itself and begun to operate satisfactorily by the time we got to California that we would in fact retrofire. We had 20 seconds to make that decision. We told Arnie Aldrich what we wanted and then listened to him describe the situation of the RCS system as the spacecraft passed over California. It had not improved, and in fact was getting worse. I told him we wanted a retrofire in that 20 second period, and counted him down to make sure he had the same time of retrofire that we had in the control center at the Cape. He pushed the button. We had a very satisfactory reentry and it came down about where we had planned. Postflight analysis showed we were in fact right, that we would have probably run out of propellant on the third rev and would have been very fortunate indeed to have been able to get around for another rev and still have enough H_2O_2 left for reentry.

Later analysis showed that the problem was the nickel cadmium beds that we were using as reactants for the hydrogen peroxide. Heat was destroying the bed and pieces of it were getting into the valve thruster seats and causing them to stick open as well as getting the wrong thrust. We later put some heatsinks, some brass straps as I recall, into that area to drain off the heat.

In regard to Carpenter's flight, I think that whole story and the situation that existed, preflight and postflight, is one which is probably better left untold. The Mercury spacecraft had two entirely separate control systems--one, an electrical, so called automatic, control system which gave rate control as well as fly-by-wire direct attitude control, and the other was a back-up mechanical system which had a separate propellant and separate thrusters. I'll give the story as we saw it from the ground. In the first rev (again this was a 3 orbit flight, and almost a repeat of the John Gleen flight), Carpenter used an excessive amount of automatic control fuel. We told him to stop using the automatic fuel and conserve propellant. On the next rev he used up almost all of the mechanical system propellant and at the end of that rev we told him to stop using any propellant at all, and conserve the propellant, an instruction which he little heeded. By the time we began to prepare for retrofire over Hawaii, he was very low on fuel.

Now when he came up over Hawaii and we told him to go to retrofire attitude, we had a very hard time getting him to do so.

He had just discovered that when he beat on the side of the spacecraft he could see some of the same glowing particles that John Glenn had seen. He became so enthralled with that discovery that he was ignoring the pre-retrofire procedure. Jim Prim was the CapComm at Hawaii and he gave Carpenter hell. As a matter of fact, I fully expected him to cuss Carpenter out over the line, but he didn't. He contained himself very well and told Scott to get on with the task. Then when Scott tried to get into attitude for retrofire, he found that the automatic system was not working. It was displaced some 45^{o} from the normal control. (It turned out later that he knew that already but had not informed the ground.) Our first knowledge of that problem came when he tried to go to retrofire attitude at Hawaii. As he approached the California station he was almost out of fuel in both systems. He had been fighting this thing and had gone finally to manual control. Al Shepherd the CapCom in California was on that particular flight and through Al Shepherd's guidance Carpenter got himself in hand and went through the steps leading to retrofire. His retrofire attitude was very poor because he was running out of propellant and trying to fight the cg offset of the retrorockets. He ended up at the conclusion of retrofire failing to achieve sufficient reentry velocity. He had plenty to get down, but he overshot his planned landing area by about 250 miles. Following retrofire, he had practically no propellant, and he had a time getting the spacecraft in the right attitude for reentry. The reentry was a very hairy one on the ground for us because we weren't

sure whether he had been able to maintain attitude control.

This was the point where we got in trouble with the press. The press thought that he had lost control and that he would burn up in reentering. Because he was 250 miles long, the communications we did receive from him on voice and telemetry showed very peculiar things happening. That later proved to be the problem of trying to transmit and read the EKG simultaneously. The doctors were alarmed over the possibility that something had gone wrong. It appeared as if screwy things were happening to the EKG, whereas in fact he was not having any physical problems at all. We had C-band tracking all the way through the blackout, as was normal in the Mercury spacecraft, and we knew that the spacecraft was safe. After blackout we had received some brief bursts of telemetry, and had continued to track with C-band and knew precisely where the spacecraft was. Shorty told the news media that we had continuous C-band track contact with the vehicle. But the people in the outside world didn't realize that C-band track meant we were receiving electronic signals from the spacecraft and that we knew that it had properly survived the reentry. It took airplanes about an hour beyond splashdown to get a signal from the spacecraft and to talk with Carpenter to assure everybody that it was OK. In the meantime, after we went into blackout, the outside world had given up hope of Carpenter ever being recovered. Such people as Walter Cronkite were really beside themselves, whereas inside the control center everything was calm. We knew he had survived properly, that the odds were very very good that the parachute system had worked normally, and we weren't too concerned about it at all. But we got into trouble with the press

as a result of a lack of information flow out of the control center.

It turned out from postflight analysis and from listening to Carpenter's report that the first time over Africa he had noticed this offset between the horizon scanners and the attitude gyros and to quote Carpenter - "I knew I had a problem but I thought it would fix itself by the end of the flight and I realize now that was wishful thinking." He apparently was never in an attitude control mode that would have allowed us on the ground to determine that he was in trouble when he was over any of our sites. The control system had to be in a particular mode for us to be able to determine what the attitude reference was relative to the horizon scanner. We were completely oblivious to the problem and Carpenter himself never reported it. So we were unable to give him any help until he got over Hawaii and then in my opinion, he was damn fortunate that he had enough propellant left and he had some people on the ground with the proper presence of mind to talk him through the situation.