U.S. Gov't

Entry Date 5-11-93

Data Base HDOCNDX

Index # INS.0205989

ORAL HISTORY INTERVIEW

DATE OF DOCUMENT [Date of Interview] = 10 - 31 - 67

OFFICE OF PRIME RESPONSIBILITY = JSC

NUMBER ON DOCUMENT = 00

TYPE OF DOCUMENT [Code for Interview] = 1

PROGRAM [3-letter Program Archive code] = INS

AUTHOR [Interviewee's Last Name] = HEBERLIG

LOCATION OF DOCUMENT [Numeric Shelf Address] = 091-2

SUBJECT OF DOCUMENT: [use relevant bold-face introductory terms]

Oral history interview with Jack C. Heberlig
[full name of interviewee]

about Pre-Space Task Groups days to MSC and Engineering & Dev. Div.
[main focus of interview]

Title: 1962 Asst Dir for Research & Development
[interviewee's current and/or former title and affiliation]
1968 Chief, Program Planning & Control Office, Dir of E&D

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

at MSC.
[location of interview]

Transcript and tape(s). [for inventory only: # pages 25; # tapes 1]

Master 1

U.S. Gov't

CONTENTS:

Biographical - [date/place of birth; family background] _____

_____

_____

_____.

Education - _____

_____

_____

_____

_____.

Career Path - July 1957 NACA Langley Research Center Performance Aerodynamics Branch, Pilotless Aircraft Research Division; Nov 1958 – STG (Project Mercury)

_____

_____

_____ (Wallops Island)

Topics - drag re entry configurations; 0 scale model test firings; initial work w/ Faget on manned Satellite concepts; astronaut position; fiberglass mold + centrifuge tests; lithium hydroxide purifiers & oxygen needs; early capsule design for ICBM; early heat shield work; "research authorization" for manned Satellite analysis; estab. of STG; early days of STG; Little Joe R+D prog (Wallops Is); ~~Goddard~~ STG first potential home for STG; addition of Operations people to STG; ~~location~~ Houston decision +; 1962 transfer to Research + Dev Dir (later Eng + Dev); E+D responsibilities: flight programs; facilities management funding authority; split SEDD into Structures + Mechanics and Propulsion + Energy Systems; importance of Space Science Environment work; E+D rel. w/ Advanced Research + Technology & other centers; opinion of MSC facilities; temporary facilities; teamwork

OPTIONAL FORM NO. 10
MAY 1962 EDITION
GSA FPMR (41 CFR) 101-11.6

UNITED STATES GOVERNMENT

Memorandum

TO : EA2/ JACK C. HEBERLIG

DATE: 10/23/67

FROM : ET5/Robert B. Merrifield

SUBJECT: Preparation of a History of Manned Spacecraft Center

As is pointed out in the cover memorandum, I have been commissioned to prepare a history of the Center. There is a large volume of information (memoranda, blue prints, sketches, etc.) available in the official files and, of course, I plan on exploiting it. However, such information is only the bare bones of history; I will also need intimate detail and personal insight from major participants and informed observers. It is especially vital that I have the benefit of the personal recollection of our key personnel who shaped the management philosophy of the Center during its early formative years. It is for this reason that I would like to have the privilege of spending a few hours with you, to help you put together a statement reflecting your knowledge of the Center's history.

If you have no objection, I will plan on using a tape recorder while I am with you, as it is a convenient way of obtaining a lot of information quickly and economically. I fully appreciate the fact that you have been involved in a seemingly infinite number of major activities, all of which are complex and of such significance that they cannot be disregarded in a Center history. At the same time, I recognize that your time is valuable and limited, and will leave to your discretion what you should put into your statement. I am interested in any information you consider to have been important in the establishment, growth, or maturation of the Center, and invite you to feel free to go into whatever depth of detail you feel advisable and within the limits of your available time. There will be no need to be concerned about grammar, structure, or repetition at this point. I will plan on submitting a transcript of this recording to you as soon as I can get it typed; if you wish, you may then amend or add to it.

I am keenly interested in those minor details that will add vividness and vitality to a historical narrative. For example, a key management decision may have been reached in one of those drab, crowded, stuffy conference rooms of the "Dolly Madison House" (rather than "at OMSF"); or the wisecrack or joke that relieved the tension or boredom or weariness of an important meeting; or the unprepossessing appearance of the Carla-battered Clear Lake Site. Although such details may seem trivial, their judicious use will make the difference between dull and interesting reading.

Interview with Jack C. Heberlig
10/31/67

I came to work at NACA's Langley Research Center on June 4, 1957. I was assigned to the Pilotless Aircraft Research Division's Performance Aerodynamics Branch of which Max Faget was branch chief. I was in this Branch up until the establishment of the Space Task Group on November 5, 1958. At this time I was one of the initial 35 individuals who were given the responsibility of implementing the manned satellite project, later to become known as Project Mercury. My work within the Heat Transfer Section of the Performance Aerodynamics Branch related to the analysis of aerodynamic heating and pressure distribution on the afterbody shapes of reentry-type bodies. At this time we were working on drag reentry configurations for the Atlas nosecones, the WS107A Project of the Air Force for its ICBM program. We also did afterbody and leading front face work on Polaris nosecones. It was out of this work that Dr. Faget evolved a concept of using a drag reentry shape in conjunction with the Atlas ~~ICBM~~ LAUNCH VEHICLE as a manned satellite carrier.

My work was very interesting. I participated in test firings of scale models from Wallops Island. One of the firings included separating a 1/6 scale Atlas warhead from an Honest John Nike Nike system. My group leader at the time, Leo Chauvin, had worked on some calculations indicating that the nosecone might not separate from the Honest John Nike Nike third stage AT burnout due to aerodynamic drag. The drag on the nosecone would be greater than the total drag on the nosecone booster combination and therefore it wouldn't separate.

We relayed this information on to the GE contractor who was providing the nose shape. The Air Force and GE were still desirous of having the firing made because of its importance to a fullscale test scheduled at Cape Canaveral a few months later. We made the test and when the nosecone didn't separate properly as predicted by our calculations, I found I had little data to reduce.

It was possibly fortunate for me as an individual, because having some free time, Max Faget put me on some work which led to the initial investigation supporting his concept for a manned satellite. I wasn't fully aware at the time that this was what it was leading to. But within a few weeks it was quite evident that this was a concept for a manned satellite program. This information is recounted in detail in "This New Ocean" where it talks about the drag reentry configuration vs the hypersonic glider or the Ames type lifting body. One of the first jobs that I had to look at was the distribution of weight and volume that this configuration might contain and I investigated the feasibility of a parachute recovery system. I used reports published by the Wright Air Development Center at Dayton, Ohio -- the Air Force Laboratory. I also began working on the interior arrangements and became involved in how we were to put the pilot inside this configuration. The most effective utilization of the drag reentry configuration front face in conjunction with the upright position on the launch pad was to lay the man on his back. At this time, the tolerance to acceleration was about 12 g's in the normal seat position, with the head being up and the feet down and taking

the g loads from chest to back. Putting a man on his back naturally fits the best packaging method for our configuration (having his legs higher than his head, would permit the blood to flow easily to the heart, ~~which~~ AND THE HEART would be able to pump more easily and keep more blood in the head, thereby preventing blackout or tunnel vision). So we set about in the Fall of '57 and Spring of '58 to make some of the support systems with the man lying on his back. At the same time we knew that there would be reentry oscillations and we contoured the seat sround the man's body and his legs in order to keep him from slushing about during reentry. This protected the shape of the chest cavity and prevented the organs from being flattened. They more nearly stayed where they were normally supposed to be. We discussed this problem with our flight crew people at Langley Research Center, and got one of the pilots, Bob Champine, to be a test subject. I talked the shop people into making a mold. Working on the project obtained their enthusiasm, and it wasn't long before we had made a fiberglass mold of one of our own engineers. Later on we made a mold for Bob Champine in the supine position. We subsequently talked to the Navy at Johnsville at the Aviation Medical Acceleration Laboratory. They were quite enthused with the idea because they had been attempting to run higher g loadings on their people in the centrifuge. They were also willing to test the system if we would deliver it to them, so we obtained the structural requirements and the mounting provisions for inside the gondola. We made up a final configuration in the shop, got it mounted in the gondola. Lt Carter C. Collins, a Naval Officer assigned

to the Aviation Medical Acceleration Laboratory, was our test subject. On July 30, he reached a top g of 20.7 and we were on our way.

What this test meant was that we demonstrated that a man could successfully sustain this acceleration, he was aware of what was going on in the cockpit, he was able to function through a 12-14 g level, and after the maximum g, he was able to function successfully. This indicated to us that after reentry acceleration a man would be able to perform pre-and post-landing functions. On July 31 a civil service employee by the name of R. Flannigan Gray also was tested in the same g profile. Prior to making these acceleration runs, Bob Champine successfully completed centrifuge tests through a level of 12 g, and would have gone higher, but he had to leave Johnsville to go to an X15 meeting at Edwards Air Force Base at the Flight Research Center. Champine was a larger man than Lt Carter Collins and Flannigan Gray, and so when we put them in the countoured couch, we had to put some hard foam padding around them. Nevertheless, they were still able to perform adequately. The contoured couch had a fiberglass shell and underneath it was the foam material. We first foamed a manned shape and then we sprayed it with a fiberglass resin. Later on the Mercury capsule couches were all made out of fiberglass material.

During this same period prior to the establishment of NASA and the Space Task Group, we worked on systems requirements for a proposed manned satellite. We worked with the Navy on the development of purifiers such as lithium hydroxide which are used in all of our manned spacecraft vehicles today. Some of our visits took us to the Navy Aircrew Equipment Lab

at Muskin Field, Philadelphia, which was part of the Navy Air Materiel Center. In the summer of 1958, Bob Champine and I talked with people like Jim Correlle who is now with us in our Crew Systems Division. We also talked with Edward L. Hayes who was then the Superintendent of Safety and Survival Systems at the Air Crew Equipment Lab. We talked to them about the respiration characteristics of pilots under stress, how many BTU's an hour their bodies yielded, and how much oxygen flow they needed. All the information we obtained from them we eventually used in developing our first specifications for our Mercury satellite.

We had good support from the people at Langley Research Center. It is interesting to note that there were 2 groups of people: those who were very enthusiastic about studying the manned satellite project and those who, when you wanted to talk with them about manned spacecraft electronics or structures, didn't want to spend any time with you. The latter group thought it was very foolish, and anyone that was pursuing this as part of their career was doing the wrong thing. Some of them even laughed right in our faces, and asked us in a nice way not to bother them.

During this time I did what was known as "boondoggling" a model through the shops. I went to our fabrication shop at Langley which was attached to the PARD Building. I gave ~~Bill~~ Bob Little, the head of the shop, a hand sketch of the capsule that had the radius and the afterbody slopes that we knew would fit on top of an ICBM. The radius of the heat shield was worked out by Max Faget, Garland, Bill Stoney, and other people all of whom are referenced in "This New Ocean," and I talked with people like Caldwell Johnson who was in the Dynamic Model Engineering Section,

and who made some sketches for us on the interior layout of a support system and various types of equipment. We were able to get this model made out of 1" magnesium ~~shield~~ sheeting. The sidewalls and internal capsule volume that enclosed the top of the airtight capsule was made of the same material but not as thick. All of this put together gave us the capability of making moment of inertia analyses and weight distribution analyses. The mockup of this particular piece of hardware which simulated weight, inertia and geometry only, was later used by the LRC landing impact people from the east area model basin who dropped it out in the BACK Bay and later made fullscale tests to determine impact characteristics and impact loading. That is the way we got that going - we kind of bootlegged this model through the shops, as they say, and it later on proved to be a very valuable initial test article.

At the same time I had the east area model shop build some model impact shapes that went along with out configuration. At one time we thought we could drop the heatshield completely and expose a contour shape on the base of the spacecraft which would permit us to land in the water and sustain no more than 12-16 g's maximum on impact. We later on decided that this wasn't feasible but during the latter part of the Mercury Program we did separate the heatshield from the top of the spacecraft exposing a landing bag which was a big air cushion and it was used on a few of the flights. I had the East Area Model Shop make a very small scale model that Dr. Faget now has in his office, and I had them put in the contoured couch in a supine position, distribute certain type of instruments across and atop of the spacecraft to serve as an instrument panel, and show the typical installation of equipment to the left and right side of them and under the man between him and the heatshield.

I had them put typical main and drogue parachute installations on the top. I also had them put in some antennas that might possibly be located at certain places. At this time we did not have the escape tower concept that Dr. Faget worked up as an efficiency measure for weight utilization. The shop people soon became enthused with this model; the more they became enthused, the better work they did. It wasn't long before they completed the model and I took it in to Dr. Faget. He took it to Dr. Gilruth, and Dr. Gilruth showed it to Dr. Dryden. I understand Dr. Dryden used it in the Congressional reviews at the time. You must remember that Dr. Dryden up until this time was not fully in favor of a manned satellite project as a scope of activity for NACA. He felt NACA was an R&D organization and he wanted no part of an operational activity. Later when they talked about having a NASA, he began to change his mind but from where we sat in the organization he still was resisting somewhat that work which we were attempting to accomplish.

I never will forget one research authorization that we wanted to get approved for Dr. Faget's Branch. A research authorization is something like a work order today. Once executed it permits you to use manhours and shop time on an approved and recognized project. Our research authorization writeups for a manned satellite analysis had been turned down before at Center level. I don't believe Dr. Gilruth was able to get Dr. Reed to approve them because there was nothing approved from Washington in this area of work. We found that Ames had a research authorization approved for studying drag reentry configurations. So we wrote up a research authorization citing this as a reference and we started to process it through channels. I took one in to Mr. Piland who told me I had to give it to Dr Faget. Dr. Faget signed it and said now to get Mr. Shortle,

Chief of PARD, to sign it. I took it in to Mr. Shortle and he said he would gladly sign it, but he didn't feel Dr. Reed would sign it. I took it back up to Dr. Faget and said Shortle has signed it - now where do we go. He said - go get Dr. Reed to sign it. So I left and went over to the Headquarters Building, up to the second floor and I looked for Dr. Reed's office. The door to his secretary's area was closed. I walked on down the hall and I noticed that his door was open and he was sitting behind his desk. There were no papers on his desk. He looked up and saw me and asked if he could help me. I said yes, Dr. Reed, I am Jack Heberlig from PARD and I have a research authorization that I was told to bring over for you to sign. Well, he said, come in. So I went up to his desk, handed it across to him, he looked at it for a few seconds, and said well I'll be able to get to this today. He asked what else there was to do. I said, after you sign, I am supposed to take it back. He said it will be a few minutes before I read it, why don't you go on back and I will call you. So I left and returned to my office. About 10 minutes later a call came into the office to our secretary, Betsy Magin, from Dr. Reed's secretary asking if she knew that a RA was taken in directly to Dr. Reed. He had signed it and given it back to her saying that we should be called to come pick it up. I had to go back and this time face Dr. Reed's secretary, a Miss Bell, who reminded me what the proper procedures were and that a Mr. Rufus House should have gotten it first. Nevertheless, Dr. Reed had signed it and Mr. House said that since he had signed it, it was perfectly in order now and so a little redfaced, I returned to PARD. When I got back to Dr. Faget's branch area, they were having quite a chuckle over it because I was naive enough at that time to go do what needed to be done and I didn't think about the necessary

protocol to be followed. Nevertheless, we had an approved research authorization for $60,000 and we felt that we were in as good a shape as Ames or anyone else as we now had something we could hang our project on.

With the establishment of the Space Task Group, again the process of separating the believers from the nonbelievers began. We had a lot of people at LRC who hesitated to join in the work and analysis and upcoming test planning. We had the preliminary specifications laid out and people assigned to us from the Langley Research Center procurement, budget and staff support areas so that we could speedily organize, shuffle our people around, put them all together in one place which was over in the Unitary Plan Wind Tunnel up on the second floor. We took over some unused office space there, and got a working understanding of who was doing what. We didn't have a formal organization other than an announcement assigning 35 people to the Space Task Group. It was 3-5 months before we had our first skeleton organization and during this time we added a number of people. Nevertheless we had the support of Floyd Thompson, Associate Director of the Langley Research Center, we had a good team in Gilruth and Charles J. Donlan to head up the initial operation. Although Floyd Thompson assigned Charles Donlan to the Space Task Group ~~in May 1961~~, I ~~found~~ LEARNED later (IN MAY 1961) that it was under a gentleman's agreement with Robert Gilruth and that Mr. Donlan was just to be detailed to him to help get him started. In January 1961 or thereabouts, Mr. Thompson put out an announcement saying that Mr. Donlan was returning to the Langley Research Center organization. This upset Dr. Gilruth greatly because he felt he still needed Mr. Donlan for a few more months, and had thought that Floyd Thompson understood that. Mr. Thompson knew that if he wanted

Donlan back he had better get him then because if Donlan ever set foot down in Texas where the new Center was going to be built, he would be gone from Langley for ever. Dr. Gilruth was just madder than Hell that afternoon. I was alone with him when re received the announcement. He said "Tommy said he wouldn't do that yet. I don't know why he didn't talk to me before he signed it. There is nothing I can do about it now that it's signed." Gilruth looked to Charlie Donlan to take care of the Space Task Group as a unit and to plan and execute the resources of the Space Task Group while Dr. Gilruth kept abreast of what was going on at McDonnell, worked with Redstone Arsenal and the Von Braun team with the early Jupiter Redstone Program and kept himself informed on how we would conduct our operations at the Cape.

With the buildup of the Space Task Group there was feeling on our part that we couldn't do the whole job from Langley. We were given some people to work with from Lewis Research Center and with the Silverstein team of George Low and Warren North going into manned spaceflight headquarters (with Silverstein as its head) we had a continuity of old NACA personnel that we knew, that were enthusiastic, and that we could depend upon. We were able to hold a bidders briefing in December on our manned satellite project. Although the Space Task Group was small in number, it had the total support of the Langley Research Center and without this wholehearted support in those early days, we would indeed have had a very difficult time. The bidders briefing, which was held in the Activities Building, came off quite well. We had good response from industry and everyone thought we could do the job. We thought that the job would be a lot simpler than what it turned out to be. Had I personally known it was going to be as

tough as what it was, I am not so sure that I would have been so enthusiastic. But we were able to launch the Big Joe spacecraft within about 11 months after the Space Task Group was organized and this just shows what can be done if one has a small group of people who know what their responsibilities are and who know that they are being depended upon for good quality work.

It didn't take us long to implement a Little Joe R&D flight test program at Wallops Island. We knew Wallops, we knew what could be done with solids, we knew how a vehicle could be built to do a lot of the earlier tests and this proved to be a blessing to the Mercury Project because these early tests did go off well. We hadn't started to think about having to go somewhere else other than Langley but after awhile it became known that the Goddard Spaceflight Center was being organized to consolidate a lot of the unmanned satellite work, which would include the Vanguard team. There were two or three buildings up at the Goddard Spaceflight Center in the construction program to house test operations for the manned satellite project. In fact there was one building on the drawing board in the long range plan there that was called the Manned Satellite Laboratory and this was the office area that we were eventually to take. We didn't think too much about it. We were too busy working on Mercury, getting McDonnell going, and getting programs worked out with the Air Force and the Army on booster utilization, and getting a team of men to the Cape to work on the Big Joe test. It was different than when the move to Houston came. A lot of us felt that we couldn't move until after the project was well under way; it would be 2-4 years before Goddard was built so why worry aoub it -- get on with the job and that's what we did.

We later began to put some time and effort into considering what facilities would be needed to test these new types of payloads. In some cases we cut our teeth in this area in designating the type of facilities we would need at Goddard. Some of the information that we put together for the Greenbelt Center was identical but updated for later use at Houston. This was a valuable exercise to go through initially and then again two years later when the decision was made to come to Houston. The facilities that we have built here at Houston have proved to be extremely valuable in support of the Gemini and Apollo Programs.

Along about this time in the early part of 1959 we started picking up some of our hard core personnel, many of whom are still with us today. These personnel were interested in becoming a part of the manned spacecraft program. The astronaut selection program was indeed a significant undertaking because it became of nationwide interest among the test pilots. When test pilots come from various places to join your program, it creates interest at those locations from where they leave. At Johnsville, when we were doing our contoured couch tests, I first met Neil Armstrong, who was then in the X15 program. Neil at the time was somewhat interested and asked some good questions about Project Mercury but he was only a few months or a year away from flying the X15. He put a lot of time in that program but later on he did become one of our Gemini pilots and very successfully handled that emergency reentry on GT-8.

Now our Center was starting to take shape insofar as we had outlined our construction needs, our organization was more fully developing, the people understood it was a serious program, and we began to grow

from an operations standpoint. Our operations team at the Cape included both flight control and crew training people. They came from Langley Research Center and had worked in the Flight Research Division. There was Chuck Matthews, Johnny Mayer, Chris Kraft, Harold Johnson, and ~~Pete Whitting~~ OTHERS (who had worked in Stability and Control and were still at the Langley Research Center). They began to set down the requirements for conducting a manned satellite operation. This was an extension of the aircraft testing techniques that they were very familiar with. They had helped pioneer handling qualities and characteristics of many aircraft -- fixed wing, helicopters, and the VTOL and STOL work that was then beginning. The team that we had in the Space Task Group became more mature by having more competent people begin working on the total operational aspects of the project. Not only insofar as the design and the engineering and research work that had to be certified, but the systems integration and the effect of launch control and flight control and landing and recovery requirements that had to be considered in the vehicle design.

We were constantly understaffed; at the time of Al Shepherd's first suborbital flight. We had less people in the Space Task Group than in some of our MSC directorates today. But operationally, and in design areas things were moving on at a very acceptable rate. We weren't meeting the schedule that we originally set out, but this is understandable in retrospect. When the decision came that we would have our own Center to consolidate all of our prefessional staff, we all felt it was a good and

proper decision. We felt that we could have done it at Langley in the West ~~Side~~ AREA just as well, however. I personally felt that there was enough acreage at Langley to do it. Nevertheless it was going to be a large activity and there was concern that if we would stay at Langley the fundamental role of the Langley Research Center within the Agency would be diluted. I do believe the decision to relocate was a proper one and I also thought at the time it was proper that we relocate somewhere different than at Greenbelt, Maryland where unmanned satellite programs would be the major interest. The move to Houston, I think, strengthened us. The plan was made to build a Center, the decisions were laid down on what the Center would do, and it was well understood by ourselves what we needed at the Center. When we laid down our test facility requirements, our housing requirements and our logistical support requirements, these were quite acceptable to the Agency and the administrator put a price tag of $60 million on the initial construction program. Those of us who had worked on the information that went to Goddard, and who updated the information on what we needed here at the Manned Spacecraft Center knew that we couldn't build it for $60 million. Nevertheless, we knew that funding of facilities construction in later fiscal years would be possible, and we could wholeheartedly enter into the c reation of this new Center.

I think the transfer of the personnel from Langley to Houston was done in an excellent manner. Everything was done to give the employees affected a maximum amount of information on the Houston area. Arrangements were made so that our employees and their wives could ride a ~~chartered~~ ON A SPACE AVAILABLE BASIS aircraft to the Houston area, look for homes, evaluate schools, and

consider prospective contractors who might be employed to build their homes. Every effort was made to make it as practical as possible within government regulations, and it was just handled very well.

The reception in Houston was of course phenomenal. I think the real pioneers were those people that came down in November and December of 1961, particularly those families who used Christmas vacations in December of 1961 to make the move. Some of them were on the road during the Christmas holidays. It reminded me of evaluating the Mercury proposals in December 1958. We worked through the Christmas holidays. Many of us took Christmas morning off but came in during the afternoon to continue working on our technical evaluations. So once again, 3 years later our people were asked once more to make personal sacrifices, and a lot of them did. They were able to move their families during Christmas vacation and some did not miss a day of school. They got their children in school the first week of January, 1500 miles away, they lived in new or temporary housing or in apartments, the employees went to work the next day, many of them on the Apollo Program, as that was the first contingent that arrived in Houston. While they were at work their wives went out to find a contractor, to get building plans, and locate a lot on which to build a house. This has been the whole history of the Manned Spacecraft Center -- initiative and drive by individuals who more often than not put their work before their personal comfort and even ahead of their families, in an effort to get the job done. They say when the going gets tough, tough men get tougher, and in many instances this was certainly true.

Once we got to Houston things began moving fast. We increased our personnel strength by a couple of thousand people in one year. We were in temporary facilities downtown, with one division here, two divisions there. We still had test programs to execute, contractor specifications to get finalized for the Apollo Program and we were already beginning to look at the Gemini Program. It is interesting to note that the Apollo Program was approved before the Gemini Program, and I felt at the time that a lot of people thought we ought to have been farther along in the Mercury Program. We had not yet made our orbital flight and approving Apollo at that early date may have been somewhat premature. Nevertheless, President Kennedy did elect to approve and kick off the Apollo Program in May of 1961 right after Al Shepherd's suborbital flight.

In June 1964, after my arrival in Houston, I transferred out of the Director's office and into the new Research and Development Directorate which was established March 5, 1962. This directorate was headed up by Max Faget, my old boss before going to the Directors' Office as Technical Assistant for two years. The organization was rapidly expanding in numbers of people and it was time that the core of the Flight Systems Division that Max Faget was Chief of, be expanded to a directorate in order to take care of the broad base technical problems remaining in Mercury and beginning in Apollo and Gemini. This new Research and Development Directorate was organized into four divisions; the Systems Evaluation and Development Division under Aleck Bond, the Spacecraft Research Division headed by Chuck Matthews, the Space Physics Division with Dr. Faget acting chief and John Eggleston, Assistant Chief, and the Life Sciences Division which was headed up by Lt Col Stanley White, M.D.

who was on detail from the Air Force. On October 3, the designation Research and Development Directorate was changed to Engineering and Development Directorate. This was primarily done at the request of Dr. Hugh Dryden who felt that research at the Manned Spacecraft Center should not be done. Many of the senior staff at MSC did not agree with this concept as many of the problems that we had to solve to make our manned spacecraft operational had not been the object of research activities. This in turn made our job more difficult because on some problems we had to do the basic research, the applied research, and the engineering and development all at the same time. At that time some of the electronics systems personnel were taken out of E&D and put into a new organization called the Information and Control Systems Directorate. G. Barry Graves was put in charge. Mr. Graves had been the man at Langley Research Center most responsible for establishing the Mercury tracking network and instrumentation requirements. Although this directorate was created October 3, 1962, about a year later, November 1, 1963, it was recombined with the E&D Directorate with Dr. Faget as director and Barry Graves as his Deputy. A small part of Graves' Ground Operational Support Systems Project Office (the GOSS Office), was turned over to Mr. Kraft, Director of Flight Operations.

I came to Houston in June, I reported for duty down here on June 4, and immediately began working with Dr. Faget on the engineering and programmatic management of the new R&D Directorate. We had to get our

test activities going in the interim facilities at Houston. We had to develop an extensive flight instrumentation program for Apollo R&D boilerplate flight and aircraft drop tests. We had to specify and analyze a new environment of flight regimes. Apollo required longer stay times in space and at that time we did not have detailed definitive specifications for design, manufacturing, and checkout. I can recall the great effort that Dick Johnston and Ted Hays put forth to turn the Lane Wells Building into a Life Systems Laboratory to do test work on pressure suits and on environmental control systems, and to derive biomedical information. They did a remarkable job in maintaining the continuity of the workloads despite the move of some 1500 miles from the Langley Research Center.

My work at the time was to insure that the new Engineering and Development Directorate had both a strong mainstream program and a strong supporting development and research program. I worked with people at NASA Headquarters in establishing funding authorities upon which alternate or new systems work could be accomplished and other programmatic authority could be established. When we wanted to undertake a new systems development activity there was really no adequate mechanism for getting it approved, and so working with the people in the OMSF, mainly people under George Low, we established certain funding authority in areas like spacecraft supporting technology, propulsion supporting technology, aerospace medicine, and other systems tasks, and we were then able to move on and develop hardware.

This has been extremely beneficial to the Center and it later made it possible for MSC to have the capability for a GT-4 spacewalk such as was accomplished by Ed White. We developed the necessary technology for spacesuit insulation, for the handheld maneuvering gun, for new engineering simulation technology, new ground test procedures, new measuring devices, and many other innovations which later became a vital part of the MSC programmatic activities or permanent facilities. We had a flight program to maintain, we had interim facilities to make use of and to modify in order to make them auitable for the work we had to do, and at the same time we had to give our attention to building our permanent facilities at the Clear Lake Site. We indeed had a terrific workload during the first several years in Houston because of these ~~overlying~~ OVERLAPPING vital responsibilities.

As our Directorate matured during 1963, and our staff increased, we were able to take the old Systems and Evaluation and Development Division and divide it into the Structures and Mechanics Division and the Propulsion and Energy Systems Division. To do this, we took some PROPULSION people from the Spacecraft Research Division, which later was redesignated the Spacecraft Technology Division. In November 1, 1963, the Guidance and Control Division was established because its responsibilities were expanding rapidly with the Apollo Program. Out of the Spacecraft Technology Division we took half of the people and created the Advanced Spacecraft Technology Division. At this time we put our Space Environment Division under this new ASTD Division because we were unable to find a suitable division chief. Space Science

at the Manned Spacecraft Center had not become of age, although the workload was there. The responsibility of the Center and the significance of its work in this area wasn't really recognized or well understood by NASA Headquarters or by the outside scientific community. As of today, I am sure that the fact that we have the Science and Applications Directorate speaks for itself as to the importance of science in the manned spaceflight program. Max Faget has always been a strong proponent of the necessity of having good scientific support capability within MSC and as early as 1962 he established the group out of which was to come many of the elements of our Science and Applications Directorate. He did this at a sacrifice to his own Engineering and Development capability by assigning to the science area positions which were needed in propulsion, environmental control systems, or G&N work.

The Guidance and Control Division's workloads in terms of dollar funding for the Apollo G&N Systems is greater than the whole Mercury Program. So it can be understood why we kept creating new divisions. It has been our philosophy that we organize around the workloads. The workload in manned spaceflight is a continuous one from one program to another. That is to say you have an environmental control system for life support in the various missions, and you have guidance and control systems in the various programs. E&D has thus become a very strong and stable organization. As the Gemini Program phased out and the Apollo Program came on we were affected very little. In fact as an organization, we hardly noticed, organizationally, the phaseout of

the Mercury Program except for some activity within our Crew Systems Division, and even there we simply moved their Mercury team over into Gemini and Apollo work. The program offices such as Mercury and Gemini, when they were phased out, of course created management problems which had to be dealt with in a special way. But this method of organization within E&D has been one which has insured the continuity of our hardware program, the transfer of knowledge, and the high quality of work which we are expected to provide as a technical arm of the Center. I don't think the stability that E&D has given to the Center during its early years of formation and now during the hardcore Apollo Program is generally recognized, but following the Apollo accident it was evident that the E&D Directorate was indeed a mature and very well organized directorate. The special and heavy workloads which had to be accomplished during the board investigation of the Apollo accident were executed by many people within E&D under a minimum of direction.

In 1964 when Barry Graves' organization came back into the E&D organization and we were able to establish the Information Systems Division under Mr. Vavra, a much larger workload was assumed by the Center. It included the automatic checkout equipment for the Apollo Program, the ACE work. This came to us from the Florida Operations Office on January 6, 1965, along about the time that the Cape Operation became part of the Kennedy Spaceflight Center.

We had a lot of trials and tribulations during our early months here in Houston because of the added duties that we had, but we certainly found out during this time who our strong team members were and to the great extent that they could be depended upon to accomplish their roles. We took great satisfaction in knowing that developing within the directorate were strong autonomous divisions who could deal effectively with complex technical problems and who were becoming skilled in dealing with the many interfaces that were inherent in the huge Apollo Program. The engineering management and total programmatic operation of Apollo is no doubt the most complex job, certainly the interfaces seem to be more troublesome than the hardware itself. Some of the men that we have working in the directorate have had to help build the techniques of management and operation in order to keep the system alive and keep it vital so that work could progress from month to month and not stand still.

We were successful in establishing shortly after coming to Houston a stronger program relationship with the office of Advanced Research and Technology. However, we were only able to accomplish about 50% of the workload each year that they wanted to program to us, and this has been true clear through FY67, but our organization has been very helpful to all of NASA. I think we have one of the strongest technical directorates in the Agency. Our people have established the rapport with New NASA organizations such as Marshall and JPL and have maintained their technical rapport with the older research centers. They also have been very effective in influencing the research centers in accomplishing

that work which will be needed to support manned spaceflight missions of the future. In particular, we have maintained a strong working relationship with the Langley Research Center. At times in the Apollo Program, Langley was executing over 300 ~~tasks~~ MAN YEARS OF EFFORT on behalf of MSC technical requirements. The vast majority of these requirements came from the E&D Directorate. So we have effectively extended our engineering capabilities by getting the cooperation and support from the rest of the NASA team. This has greatly enhanced our capability of meeting our milestones and in attaining the level of confidence and assurance that we need prior to proceeding with flight test programs.

In establishing our facilities here at the Clear Lake Site I feel we did a good job, because facilities that are here at the end of 1967 are mainstream to the Apollo Program. The ground test activity has to be accomplished or the next Apollo mission can't fly, and the one after that can't fly and these tests are being conducted and are providing the type of flight assurance data that is needed prior to commitment to an active flight program. The fact that we are able to do this today means that between 1961 and 1963 our people understood the mission requirements and had the competence to designate the type of facilities that have been built with the funding available. This fact is often overlooked after the successful completion of a program.

As an organization, I feel that the E&D Directorate exercises an unusual degree of influence over many elements of the Center. The manner in which we conduct our business to a great extent influences the

manner in which the Center conducts its business. Our strong interfaces with the Administration Directorate and the other technical directorates in executing the workload of the Center has indeed worked well. No doubt this is because we have always been a lean organization. We have always had more work to do than staff to do it, and we have been trained to look for and accomplish that work which is of significance -- control work -- work that has to be done before the total job can proceed. Under Dr. Faget's guidance we have been able to maintain the broad base competence that manned spaceflight requires and in so doing we were able to foster the Science and Applications Directorate and the Medical Research and Operations Directorate to a great extent.

There were many times, in the Farnsworth Chambers Building where we were housed after we came to Houston, that we wondered how we were going to get there from here. After being through the Mercury Program, we became more educated as to the complexity of conducting manned spaceflight and the very very important aspects of team operation. A lot of our people came from research-oriented work where they worked as individuals on a problem. In the Mercury Program they were successful in adapting themselves to a workload that not only required individual responsibility in house and applied research but which also required teamwork in development, flight test, and in flight operations. This team has matured and I don't know of anyone who feels today that manned spaceflights through the Apollo landing is not going to be a reality in the very near future. This team also has developed concepts for

future missions, such as a large space station in earth orbit. This particular mission is one that Dr. Gilruth, Faget, and many others have thought about and talked about even as early as the initial Mercury flights. With the tremendous launch capability of the Saturn V vehicle it is the opinion of these men that we should NOW pursue this misson goal.