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Entry Date 5-12-93

Data Base HDOCNDX

Index # INS.0206045

ORAL HISTORY INTERVIEW

DATE OF DOCUMENT [Date of Interview] = 03-13-68

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] = MAYER

LOCATION OF DOCUMENT [Numeric Shelf Address] = 091-03

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

Oral history interview with John P. Mayer
[full name of interviewee]

about Dep Dir of Mission Planning & Analysis Office; computer needs and contracts,
[main focus of interview]

Title: 1963- Asst Chief for Mission Planning, Flight Oper Div,
[interviewee's current and/or former title and affiliation]
1968 - Chief, Mission Planning & Analysis Div, Dir of Flight Oper. Dir for Oper

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 14; # tapes 1]

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CONTENTS:

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

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Education - _____

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1947 - Edwards AFB _____.

Career Path - Langley - Flight Research Div, Maneuver Loads Branch; _____

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Topics - International Geophysical Year; early manned Space research; orbital mechanics & lunar trajectories; 1958 concentration on Faget's ballistic shape Spacecraft (became Mercury); Oct 1958 - STG formed; STG divisions: development (Faget), Operation (Mathews), Engineering (Chamberlin); early missions analysis area; Central Center development; Computer processes for Mercury; 1st computer location at Goddard ~~interactive for~~ Central center & Computer to be in same location; source selection board for Computers + specs; separate & Realtime Auxiliary Computing Facility (RTACF) back up for RTCC; move to Houston 1962; Mission design; increased personnel needs; interface w/ MSFC; TRW Contract for software programs; proximity of Contractors important; Apollo demanded bigger faster Computers; IBM 360 selected

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

UNITED STATES GOVERNMENT

Memorandum

TO : FM/John P. Mayer

DATE: March 8, 1968

FROM : AC/Special Assistant to the Director

SUBJECT: Preparation of a History of Manned Spacecraft Center

At the request of Dr. Eugene M. Emme, the NASA Historian, we have agreed to assume responsibility for the preparation of an MSC history. This effort is expected to complement programmatic histories (Projects Mercury, Gemini, and Apollo) which are either in preparation or complete. The MSC history will place primary emphasis on the Center as an institution--its general management philosophy, the evolution of its major organizational elements, growth and modifications of its staff, management of its financial resources and contracts, acquisition of its facilities, and its impact on the economy, culture and society of the community in which it exists.

Dr. Robert B. Merrifield, a professionally trained historian, has been asked to prepare this record of our progress from Langley origins to the present. Since he has been with the Center for over five years, Dr. Merrifield is familiar with many key decisions, events, and trends in the Center's past. However, he will need help from all of us who have been major participants in the life of the Center, particularly in interpreting why and how various forces have influenced the development of the Center as an institution. Your aid and cooperation in this undertaking are vitally important to its successful completion and will be appreciated.

Paul E. Purser

Paul E. Purser

5010-108

OPTIONAL FORM NO. 10
MAY 1962 EDITION
GSA GEN. REG. NO. 27

5010-107

UNITED STATES GOVERNMENT

Memorandum

TO : FM/ John P. Mayer

DATE: March 8, 1968

FROM : BN5/Dr. Robert B. Merrifield

SUBJECT: Preparation of a History of the 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.

Because of your position and long association with the Center, it is quite likely that you are familiar with events where personality clashes, conflicts in judgment or other human failings have played a considerable role. The natural tendency in dealing with such sensitive issues is to avoid them or to gloss over them with generalities. Obviously, any history based on this type of treatment will be bland, innocuous, and superficial. On the other hand, if potentially explosive information were to be incorporated into a history, it would certainly lead to embarrassment or more serious consequences to the Center. As an alternative to these two extremes may I suggest the following: I would like to have your statement to be completely candid; I will consider it to be personal and confidential, and will safeguard it accordingly. After typing your narrative, I will return it to you for verification. At this time, I will ask you to indicate those portions of your statement which you regard as "privileged information." They would never be alluded to in any way in the Center history, and would have the sole purpose of giving me the necessary background information I need to write a factual and objective history.

May I call you in a few days to make arrangements that will be mutually convenient for me to see you?

Robert B. Merrifield

Interview with John P. Mayer
3/13/68

I started with what was the beginning of MSC in 1958. In that year there was some work going on at NACA's Langley Center on an unmanned vehicle, about the same time there was talk about a Dyna Soar manned vehicle. After Sputnik, in October 1957, I worked in Mel Goff's Flight Research Division in the Maneuver Loads Branch under Henry Pearson. Another of the branches of this Division was the Guidance and Control Branch under Hewitt Phillips, and this was where Chuck Matthews and Chris Kraft, Sig Sjoberg, Johnson, Faber, and Kuhnel all came from.

From 1947 on I had worked at Edward AFB. Originally I was on the X-1 project and later worked on the research airplanes. When I returned to Langley I began working on a statistical program for flight research on determining loads in flight of AF airplanes. A number of us in that office had been interested in space research for several years. In fact, about 1955, one of the fellows in the office made a $50 bet that we would land a man on the moon within 20 years. That would have been about 1975. Looks like he might win his bet. It was mainly from a science fiction standpoint that we were interested. About the time this geophysical year came up, and especially right after the Russians orbited their first satellite, Pearson simply told a bunch of us that we were going to work on space research. I guess if his bosses at Langley had known they would probably have fired him. That's what happened--we essentially quit on our own within

a branch and started working on space-related things. At first we simply studied all the evidence we could find and put together lectures which we gave to ourselves and these lectures later turned into a textbook. It was the first spaceflight textbook that ever came out. Unfortunately we never published it commercially. It was published in NASA, and I guess the Agency has given out thousands of copies. If we had published it right then it would have beaten all the others by about a year.

I worked on orbital mechanics and lunar trajectory. A lot of people worked on lunar trajectories before then. In fact, the Rand Corporation had done a considerable amount of work on lunar trajectories in 1956-1957. The Russians had also been working on them for some time. One of their reports was useful in deriving trajectories for Apollo. Last year I met the author of this Russian report in Yugoslavia. While we were doing this work, two parallel conceptual designs for manned spacecraft were being investigated at the same time. Max Faget was working on a ballistic shape and Chuck Matthews was working on a vehicle that could land on a 5000' runway. Around June or July 1958, it was decided to concentrate all effort on the ballistic shape concept, and some of the Langley people who had been investigating the feasibility of a manned satellite were moved over ~~to the East side of the field,~~ to the unitary wind tunnel building. I was one of that group, which at that time numbered about 17 people. We started working

on the specifications for the Mercury spacecraft. Originally I was assigned to work on loads, but there wasn't enough work being done in orbital mechanics and as my background and experience was strongest in this area, I began to devote more of my time to this area. The only thing that had been done in the orbital mechanics area up to this time was some reentry calculations.

When the STG was formed in October of that year I was given a trajectory analysis group which consisted of myself. Ron ~~Cohen Kevitch,~~ KOLIEN KVICH (?) and Bob Chilton of STG both did some work in that area as did ~~also a guy by the name of~~ Jim Bula at PARD. A guy at Ames, Jack NIELSON wrote a basic report on orbital mechanics about 1958, which was used as a basis for a lot of our programs and in fact still is. Around the early part of 1959, the first flight mechanics group was formed in the STG, flight mechanics being really orbital mechanics, and I was named to head up this group. Its basic function was to do trajectory analysis. I had around 4-5 people. We moved to the old administration building in the East Area and the building in back of it.

There were then two divisions; one under Max Faget and one under Chuck Matthews; later a third was added. Max headed up the development division, Chuck the operational division and shortly after that Chamberlin was given the engineering division. I was in Faget's Division, which seemed to be a logical location for my function. However, Chuck Matthews convinced Gilruth that the mission analysis activities were ~~trajectory~~ OPERATIONALLY oriented and ought to be in the operations area. I didn't like this idea initially, but since I knew that probably Gilruth and Matthews knew better than I, I went along with it. I figured Max would have to

develop some capability in this area. There were about 10 people in the Mission Analysis Branch when it was started. Our division now is a direct descendent of that particular branch. Since then we have had essentially the same function and organization. The job has just gotten bigger and bigger.

From that time on we started to grow fairly rapidly but even by the end of the Mercury Program I doubt whether there were very many more than about 40 people, and probably 20 people did the trajectory analysis and mission design for the Mercury Program. We also had another important job, and that was Control Center development and realtime control of flights. At that time the design of network and the control center was assigned to Hartley Soulé and a group of people in the Instrument Research Division at LRC. They actually controlled the development of the control center computing center and what has developed into the manned space flight network. Under Soulé Barry Graves headed the IRD group up and under him Paul Vavra was the head of the area dealing with hardware, and electronics systems. Gene Davis originally headed up the computer area ~~and after~~ but he left fairly soon. Jim Donegan and Bill Tindall took over. It was actually under Davis' direction that the work on computer and orbit determination was done. Donegan is now at Goddard where he went to keep the Mercury Program running after we came down here. Bill Tindall came with us as my deputy.

Before we had a Control Center the original concept of Langley was to track the vehicle and determine the orbit from it. We decided the computer had to do a lot more than simply determine what the orbit was. It had to compute the things for people on the ground on what was going on up in orbit so that they could make a lot of go - no go decisions all the way through the orbit. About the first thing I came

up with was a go - no go criteria for going into orbit. This was the V-B gama plot. In fact I guess I invented the word go - no go. In our branch we got together with Jim Donegan and Bill Tindall and decided we belonged together even though we were organizationally separate, so we worked as if we were in the same organization. We all worked as a completely cooperative team in Mercury even though we were STG and they were at Langley. I guess the reason why we got along well was that Jim Donegan and I sat across the desk from each other for 10 years before I went into the STG and were good friends. The relationships between other groups working with systems and hardware seemed to be characterized by a considerable amount of friction. For example, a lot of antagonism existed between Barry Graves' people and Walt Williams' people in STG, particularly those under Chuck Matthews and Chris Kraft.

In any event Donegan's people and mine developed computer processes that were needed for Mercury. We started out with a 704 and went to a 709 and finally had the first 7094 used in the Mercury Program. This computer was in Washington and the Control Center was at the Cape. This was a mistake, as the logical place for those computers was in the Control Center, but in those days parochial interests had undue influence on who should do what in the space business. The Control Center was put at the Cape because it was believed it had to be there, but it was decided that the computing data would be remoted from Washington to the Cape. This decision was arrived at about the time Goddard Space Flight came into being and before Goddard had completed its building program. At that time MSC was supposed to be part of Goddard, we had already been reassigned to Goddard, and

we all expected that one day we'd be moving to Goddard. This was true up until about 1960-1961. NASA wanted the computers at Goddard because somebody felt the Air Force might get control of them if they were at the Cape. The computers were originally located in the IBM Bldg on Pennsylvania Avenue in Washington. Later they were moved into an unfinished building at Goddard's Greenbelt facility. These computers were installed in a building when they were still putting cinder blocks and bricks up. There were canvas walls and mud everywhere and it was the biggest mess you ever saw. And at the same time the building was being built, we were running those computers and fighting the dust.

Despite the fact that the decision on the location of the computers was politically motivated and a mistake, it turned out to be a blessing in disguise as it enabled us to demonstrate the feasibility of remoting high speed data over telephone lines. It proved that control centers especially in long space missions can be any place in the country and that's one reason we now have the control center in Houston and not at the Cape. We had already proved you could use long lines easily to remote data and besides when you are on the moon, what's the difference between Washington, Houston, and the Cape? The important thing is to have the Control Center and the computing center at the same location.

Before we moved to Houston, Chuck Matthews wrote a letter which said the operations division ought to be at the Cape. I'm very happy that it isn't for I like to live here. In our business, which is designing missions, I think it's more important that we be close to the other people in the Center where the spacecraft is being designed and

the Program Office, than being close to launch site. I think the decision to move us to Houston was the right decision. It was right to have the computers in the control center but it was also right to have the control center and our people near the people designing the spacecraft. Actually, I think the whole center would have operated better down at the Cape, and for that matter, if we could put the whole NASA at the Cape, it would be advantageous.

As we went along in Project Mercury it soon became evident that this business of running back and forth between the computers and computer contractors in Washington, and the Control Center at the Cape was an extremely bad situation and difficult to work with. A lot of our people spent half their time commuting. When I say our people, I am talking of both the IRD, Donegan's people, and my people who worked together as one group. It was an awful job implementing things from 200 miles away. It simply isn't feasible to instruct someone to write requirements and call when they're ready.

When we began preparations for Gemini, and the move began to Houston, all our people were insistent that the computers, computing, and the Control Center be in the same building or next door to each other. Again politics got involved, as Goddard was well established and it had done the computing job on Mercury and wanted to continue doing it for Gemini and Apollo. This did not come to pass, fortunately, and we got the computers down here where they belonged. When we began planning for computer service in the control center, we put out bids on the basis that the company awarded the contract had to be located in Houston.

Chris was chairman of the source selection board for the computers and I was deputy chairman. Since Chris was busy flying missions during much of this period, I got the whole job dumped on me to write the specifications for the computing center and computing contract. We ended up with IBM as our contractor in Houston. IBM happened to be the same one who furnished computers for Mercury, which made it easier for us, although I remember the standing of bidders was a lot closer than anybody expected.

Back at Langley our mission analysis branch not only had control of the realtime computing and mission analysis but the center computer facilities as well. We were about the only ones that used them and when we first got the computing facilities at Langley they were under my direction. Stan Cohn and John Shoesmith, who worked for me, were in charge of the computer at Langley. Later when we decided it should be a Center facility, it was put directly under Hjornevik, which is probably where it still should be. In utilizing computers on Mercury, we weren't handicapped by having a central computing facility. We could get computer time as we needed it, and essentially it was a computer for our use. Later, when more organizations began demanding time we no longer had this advantage.

Once we started flying missions we found that the things that we put in the RTCC were not always complete. We didn't get everything in and either flight controllers developed needs for other data which we simply couldn't get into the RTCC because of the time requirements or because we didn't want to put it in there. However, we could use our own computer (the Langley 7094) offline, and as an auxiliary computer. This was where the concept of what we now call RTACF - Realtime Auxiliary

Computing Facility (in those days it was called the ACR--Auxiliary Computer Room)-- came into being. In the beginning people didn't trust the RTCC completely. We decided we'd pull the other computer online and use engineering programs that could be changed easily to support the people at the Cape and give them answers. We originally got data by people reading, for example, position and velocity vector back over the telephone. The position and velocity vector is time plus 6 quantities, and 6 quantities are usually 8 numbers apiece, so that's 48 numbers somebody had to read over the telephone and then punch into cards and make computations for it. Then we would read our answers over the telephone to the flight controllers. Gradually this developed to the concept we now have in which we still have a RTACF which is a separate computer - a 7094 here in Houston. Earlier I mentioned in Mercury we had a 7094, but it was a 7090 to begin with. Now we get our vectors automatically from the RTCC and we have display devices that we can show data immediately to the flight controllers without reading numbers over the telephone.

We kept growing in size and moved to Houston between January and July 1962, into the old Houston Petroleum Center. Out of about 40 people that we had at Langley when we left, all came to Houston except two or three. Shortly after arriving in Houston, we reorganized the division creating two assistant division chiefs who were John Hodges and me. Later on in 1962, they formed our areas into divisions. We have been the same since.

Our functions were first of all to design missions from a trajectory standpoint, and develop realtime computer programs. We supported the operation as experts behind the scene for the flight control team. We

remained in this capacity until about 2 years ago. Around December 1965-1966, the onboard program development for the Apollo spacecraft, which had been under the control of Flight Control Division, was moved into our Division. At the same time the onboard implementation computing program and the direction of the MIT contractor was given to us. Max Faget and Chris Kraft agreed that we would do all the trajectory development and mission design work for the whole center. A trajectory branch in G&C under Jack Funk was moved to our division, and part of that branch went into the onboard program development, and a part of the branch under Jack Funk became our advanced mission design branch.

In 1967, Pete Clements, head of the Flight Support Division, went back to the Air Force and Bill Tindall was offered that job but he wanted to stay in mission design, so Lyn Dunseith, head of our realtime program development branch, was offered and agreed to become chief of the Flight Support Division. Thus it became logical to move the implementation of the computer programs into the Flight Support Division. He took his branch with him which implemented the computer program. Our division still kept the responsibility for the mathematical formulation and the mission logic that goes into both the RTCC and the onboard computer program, but the fellows who manage the contract are with Lyn in the Flight Support Division.

After 1962, it became obvious that the Apollo Program was several orders of magnitude more complicated than the Mercury and Gemini Programs and that we were going to need a lot more people. In my opinion, part of this growth was due to the growth of NASA itself. We had so many more people, especially in Washington, who could ask us more questions for which we had to get answers -- whether their questions were valid or not.

I sometimes think if the NASA had half as many people, it might be better off.

Our interface with the Marshall Space Flight Center was much more involved in the Apollo Program than in Mercury or Gemini. When you are designing the spacecraft and a launch vehicle at the same time, you have demanding requirements for both to be satisfied and we had a much bigger interface with MSFC. We grew to something on the order of 200 people, but it was obvious that we would need over double this number to do the job. We decided the only way to do it was to hire a contractor to assist us. This is why we have a contract with TRW. While we were still at Langley we needed some work done on an orbital debris study, and we decided to contract for this. We talked with Jack Nielson who had left Ames and had a company of his own by this time. We intended to give the contract to him on a sole source basis but couldn't, and ended up by having to go out on bid. Instead of this fellow winning, TRW put in the best proposal and they won the contract. As the Gemini Program development began, we were completely wrapped up with Mercury operational studies, so the TRW contract was expanded to cover some rendezvous studies for Gemini. When we got to Houston, it became obvious that TRW would need a lot more people to support the Apollo Program as well as the rest of Gemini. We expanded the contract to cover this effort. We insisted the people had to be located in Houston, because as we approach the operational phase, the turnaround time gets more and more critical. We are now reaching the peak of activity on the TRW contract, and have recently added some AAP support requirements.

The TRW contract was entirely our responsibility. After a while Joe Shea decided he wanted TRW to do some work in the systems area for ASPO. We expanded contract to cover the Apollo work. Later E&D also asked to be covered and they too are using the same contract. As a result of a little orbital debris study back in 1961, we now have close to ~~900~~ 700 people working here in Houston for TRW.

Incidentally, the TRW contract was one of the first incentive contracts for software programs. I was dead set against it at first, as I couldn't see how we could spell things out definitively. However, we were allowed to use the concept of grading, and essentially gave the contractor a report card periodially. It has worked fabulously. I think we get three times more for our money now than we did when it was a cost plus contract. However, probably half the improvement is due to the fact they are in Houston. There's nothing like having contractors working close to you. We are learning this fact in running a computer program contract at MIT. It's awfully hard to work efficiently when the contractor is located several thousand miles away. I think sooner or later we will have a software contractor on site, just as is the case with our ground software contractor.

After we had operated the Houston Control Center and computing system for awhile using the 7094 computers and the requirements for Apollo came along, it became apparent from our Gemini experience that the 7094 computers would not be adequate for the Apollo missions. There were two reasons. One was that the 7094's were expected to be too slow and the other was that they were believed to be too small, that is they would not have sufficient storage space. About 1963-1964, I was

appointed as chairman of a committee which would investigate the need for new computers in the Control center. The committee consisted of people from this center, people from Goddard, the Washington office, and representatives from ~~BellCom.~~ Bellcomm. We made a study which lasted a couple of months. In terms of Apollo requirements, it appeared that we would need a storage capacity of over 1 million words. Under the Mercury Program, the comparable requirement was for 30,000 words and on the Gemini Program it was initially 300,000 words and later 700,000 words. We estimated all the computing we would have to do and the calculations we would have to make, and it looked like it might take two to three times the Gemini capacity.

In terms of speed, it was felt we would have to optimize the mission in realtime and we estimated we would require a computer at least three times as fast as the IBM 7094 (II). Our predictions were relatively accurate. We need somewhat more storage than we guessed-- about 1,500,000,000 words but the speed that we felt we needed is less than expected. Our knowledge in computing and optimizing lunar trajectories, has improved so much, and we made such big breakthroughs in terms of computing speed that what we used to take 5 days to do, we can do in about 5 minutes today. The speed we need now is probably on the order of 3 times and not much more than that.

We had a schedule to meet which showed a lunar landing mission in February 1968, with the other missions strung out ahead of that. It looked like it was going to be extremely difficult to design a computer, get it operational, tested, and checked out in time to meet the schedule and at the same time keep supplying Gemini and Apollo requirements. We examined all the computers available, among which

were an IBM 360 Model 75, and also a 360 Model 92. CDC had the CDC 6600, and 3600 which could be converted to a 3800 later on. Burroughs had a 5500, GE a 735. It appeared that there were only two computers capable of meeting our needs--the IBM 360 or the CDC computer. It appeared to us that the CDC 6600 was behind the IBM 360 in development and the 3600 seemed not fast enough for us. It looked like the 360 was the only computer that would fit our needs and be operational in time for the mission. This was about the time that IBM became a dirty name in computers. If we wanted to buy any other computer in the country except IBM, I think we could have gone sole source and gotten it. Since IBM had such a big share of the market, people were inclined to question any extension of their share. We were thoroughly questioned by NASA Headquarters on this. They decided to bring all the computer manufacturers together, explain the problem, show them the requirements, and ask them whether they could meet our operational requirement dates with their computers. All these companies were brought in and CDC was the only one who really thought they could. Finally, after some relatively high level discussions in Washington with CDC, they too were convinced that they couldn't hack the requirements.