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Interview with J. M. Eggleston
3/14/68

In the spring of 1958, when I got back to Langley Research Center from Princeton, I found that 3 elements ~~at Langley~~ in NACA were designing reentry vehicle concepts. The 3 contenders were Max Faget's back-facing reentry vehicle, Al Eggers' semi-flying forward-facing reentry vehicle, and Hewitt Phillips' (for whom I worked) variable-geometry winged reentry vehicle. The Phillips' vehicle looked very much like a Dynasoar and reentered under the same principles as Max's vehicle. It had sort of a curved bottom and was a high drag device which, when it got to subsonic speeds, would pitch over and actually glide to a landing. We may see more of that vehicle yet. It was then, however, a little too advanced. After many arguments and technical discussions and presentations, Max's vehicle concept won out.

As I say, I was in the branch headed by Hewett Phillips. The assistant branch head was Chuck Matthews and Sig Sjorberg, Chris Kraft, Don Cheatham, Bob Chilton, Harold Johnson, and Porty Brown were among the 16-17 branch members supporting our position in the argument. Even after we lost out in the reentry vehicle competition, many of us still were actively interested, and after the formation of the STG, Chuck Matthews and Chris Kraft went over to the new organization. I did not.

While we were still looking at these 3 types of reentry vehicles, because they applied different kinds of deceleration force on the astronaut, I put together a simulation program to be run at the Johnsville Centrifuge to simulate the deceleration loads. These

three different types of vehicles had the chest to back or what was later referred to as the "eyeballs in" force, for Max's vehicle; the "eyeballs out" force, for Eggers vehicle; and the "eyeballs down" type of force for the Phillips type of vehicle. I think it was in early 1959, that I ran this program, and I used as my subjects a group of pilots: 3 from Air Force, 3 from the Navy, 3 from Ames, and Edwards Flight Research Centers and 2 or 3 from Langley. One of my subjects was a young Maring major named John Glenn. This was the first reentry simulation test that was run full-scale and "closed-loop" in a centrifuge with the pilot actually having control over the simulated spacecraft.

Jack Heberlig was then in a liaison role between STG and Johnsville tests where the Mercury capsule simulation tests were being run "open-loop." Astronauts had not been picked yet; instead, a preprogrammed G profile was run which was calculated on paper and the centrifuge used this G ~~pattern~~ profile. The subjects included doctors, psychologists, etc.

While John Glenn was performing this simulation, which attempted to determine how well a crewman could control his spacecraft under the reentry conditions imposed by each of these vehicles, the announcement appeared in the paper that there was going to be a call for astronauts. I remember John mentioning that he thought he would apply and he did the following week when he returned to Washington where he was attached to a unit.

Doing things like this, which were in support of Mercury and other studies, I worked fairly closely with the Mercury group and

to an even greater degree with the Apollo group because there wasn't too much control and trajectory work left to be done by them for Mercury, and there were many competent people to do it, anyway. Those of us who were in the Flight Mechanics and Flight Dynamics Group at Langley turned our attention more to the next generation spacecraft and worked primarily on the rendezvous techniques that were to be used later in Gemini. ~~Primarily~~ We were also very concerned with lunar rendezvous, and we did a great deal of the early work required to make the decision to go with the lunar orbit rendezvous.

I transferred to STG just after it became MSC, in ~~December 1961~~ January 1962, joining Chuck Matthews' organization. Shortly after the formation of MSC, the Center was reorganized and Max Faget was announced as the Assistant Director for Research and Development. Chuck Mathews was moved from the Operations Division, which he had headed up until then, to Chief of the Spacecraft Research Division. That word "research" was a carryover from our Langley days. In 1962, Hugh Dryden, then Associate Administrator visited Houston, and going around visiting these new organizations, he continually encountered the word "research" in the title of organizations. He said "Look, you are not doing research down here, you are supposed to fly a manned spacecraft - research is done at Langley, Lewis, and Ames--and told them to change those names. So after less than 1 year after we were formed, we had our name changed and Max's organization became Engineering and Development and our division became the Spacecraft Technology Division. Max had 4 divisions at that time, one of which had not been staffed yet. It was called Space Physics Division and in accordance with Dryden's instructions, it was renamed the Space Environment Division. That's

how we lost the name research, science and that sort of thing until NASA made a major policy decision to put science at this Center. That same division later evolved as the Space Sciences Division and later as the Space Sciences and applications Directorate. But it got started in 1962.

The Apollo Program more influenced the Center's institutional program than did Mercury, because it created a subsystem manager arrangement which required E&D to create divisions for each technical area. Before that, the Spacecraft Technology Division contained within it almost the entire capability for doing the development of a spacecraft. This division incorporated the major functions of the Guidance and Control Division, and the Structures Division, as well as most of the design capability in the Center. A fourth area was missions-oriented and looked at the total space vehicle. It was this division group that persuaded Gilruth and Von Braun and later NASA Headquarters that the rendezvous mode was the best of the alternatives.

That first year we were located in the Rich Building. That It was an interesting experience in itself. We stayed in the Rich Building throughout the entire year of 1962. The year was ended by our having finished contract negotiations with GAEC for LEM and definition of what GAEC was to do. In early 1963, Chuck was made Max's deputy. Max moved me over to what was then the Space Environment Division to head up that division. I was Assistant Division Chief over only a handful of people, (less than 12) who didn't fit into any other organization. They were put in here to develop a new capability which was described as space environment definition. There wasn't a division chief for a number of years until after Stoney's division was formed. When Jim Elms

came to the Center, I believe that he was brought in to reorganize at MSC. the Apollo Program. It was under him that the concept of the subsystem managers was developed, and in developing that concept a major change was made in Max's directorate in that the 4 divisions that he then had: the Spacecraft Research Division, the Space Environment Division, SEDD, and Crew Systems Division was reorganized. At that time Barry Graves had a directorate that was almost a duplicate of Max Faget's in certain areas. These two directorates were later combined and the divisions were broken out according to technical orientation. The Structures and Mechanics Division was formed out of part of Aleck's Division and Bob Vale's branch from Spacecraft Technology Division. Guidance and Control was broken out as a separate division, etc., for a total of 8 to 9 divisions. The Space Environment Division which was combined with remnants of the Spacecraft Technology Division and renamed the Advanced Spacecraft Technology Division (the only portion that was left of it was a design and aerodynamics capatility).

I think one of the most important major policy decisions but one that maybe Max Faget sort of regrets now, was his decision back in 1961 to create the Space Physics Division. His thinking on that was that we were going into a new environment where there were a lot of things we didn't know about and they may not be as bad as some people painted them, but we better find out about them. We had run into a little bit of this problem early in the Mercury Program when the people who knew about the

Van Allen belt and radiation waved their arms and cried doomsday, and people who knew about the meteoroid showers did the same. Of course as we found out in Mercury, these weren't really problems. However, he recognized that with the Apollo Program going to the moon, we were going to run into a great deal more problems and some of these environmental problems would become very important. The situation at that time was that we had a number of prominent scientists and each was predicting various characteristics at the moon and in its intermediate environment--such as the solar radiation effects and meteoroids--but rarely did any two of them agree on any details. This disagreement by people who were obviously extremely well-informed and well-educated had to be based on a lack of information. However we recognized we wouldn't get much more information and somebody had to make a decision as to which one of these people were right and to quantitize information so the spacecraft could be designed to land on the moon. This is why Max created a science-oriented division. Unfortunately we didn't have any great scientist who would take the job of attempting to put this scanty and often divergent information together into a usable form. So we gathered together the people who I guess you would call fools, because they were willing to tread where wisemen would not. We had Jerry Modisette who knew nothing about space radiation but had worked in high temperature radiations for PARD and he was willing to give the field of space radiation a try. We had Page Burbank who had worked in optics back in the unitary wind tunnel at Langley

trying to photograph objects travelling at very high speeds, shock waves, arc jets, and things of that sort. So he didn't feel out of place attempting to get into world of meteoroids which were also objects travelling at great speeds and which were subject to measurement by optics--photography of meteor showers, etc. These were the kind of people we staffed this division with and it was the best we had at the time. They have subsequently developed into very competent people in their areas. We also had another oddball in the sense that he did not fit into any other technical area of the Center. This was John Dornbach. He was a cartographer, who had been assigned by the U.S.A.F. Air Force Aeronautical Chart and Information Center as liaison to MSC to discover what the MSC personnel wanted in the way of maps. John then would convert those needs into cartographic definition so that ACIC could provide maps for Project Mercury. It was apparent that we were going to need this capability for a long time so John was employed by Max. He also was one of a kind at the Center, and ended up in this division in the area of Cartographic ~~Mapping~~ Products and Lunar Surface Analysis. We also hired a few geologists because it was obvious that we had to have some geological interpretation of the lunar surface. About the only geologists we could get at that time were one or two fellows who hadn't finished their PHD, although we were subsequently fortunate enough to get several brand new PHD graduates. One of the first of these was Dr. Ted Foss. They evaluated the lunar surface theories of scientists and recommended to MSC personnel which of the scientists ~~were~~ they believed to be right in what details and which

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of them were believed to be wrong in what details. It was from this total group that a space environment definition evolved which was subsequently used by the Apollo Spacecraft Project Office in the selection of Apollo landing sites. Also this was the group that defined the probable meteoroid and radiation environment which gave us some assurance that the spacecraft structure would serve as a safe protective shell against meteoroid penetration with a high degree of probability and that that same command module shell would provide adequate radiation protection against solar flares. This gave us a great deal of confidence and enabled us to proceed with detailed hardware design and operations procedure definition for the Apollo Program. We subsequently found that our prediction of the probable nature of the lunar surface was pretty good.* We did not expect to find the deep dust that Thomas Gold then predicted. Our predictions of bearing strength have turned out to be very good if the experience of unmanned lunar landings is any criterium.

Having gotten a fairly good handle on the expected space environment and the factors which would affect the lunar landing site, this data was then turned over to Chris Kraft and the mission profile as it is today has been strongly influenced by these considerations.

We next turned our attention to lunar surface experiments. We had already been involved in photographic experiments in Mercury and between Mercury and Gemini we attempted to set up some procedures for handling experiments on Gemini. The reason that the Space Environment Division became involved was that we prepared a number of experiments designed to measure the space environment. In fact, virtually

* We have yet to test our radiation and meteoroid environment definition.

all of the engineering experiments developed within NASA and flown on Gemini were initiated by this division. (There were a number of Air Force experiments flown on Gemini missions that did the same thing). Since nobody else volunteered, we assumed the responsibility for early definition of lunar surface experiments. We developed the concept of what is now called ALSEP (Apollo Lunar Surface Experiment Package), ~~which at that time was a rack with its own telemetry.~~ We decided to use a Radio-active Thermal Generator RTG (?) unit as a power source. A subsystems management arrangement developed for that particular experiment package at that time, but it was not executed until what was subsequently called EXPO (Experiment Program Office) was established in mid-1965. This early work and the early contracts for experiments during 1964 and 1965 were ~~let~~ managed from the Space Environment Division.*

The early work at the Center in what would now be called space science experiments occurred during this period. It was, in a sense ours by default because Headquarters hadn't decided how to go about getting outside investigators into the picture and it was obvious that we weren't going to make our timetable unless the Center started doing some of the early work. We let one contract with Headquarter's blessing and funding, and a lot of our experience was gained in this period. Certainly the spacecraft design which included these experiments was frozen then. The concept of how many pounds and how many cubic feet of space were to be allocated in the command module for the boxes were all fixed prior to the formation of EXPO.

* Two sub systems managers IESD managed the telemetry for ALSEP and PPD managed the RTG power supply source.

The formation of EXPO was a major policy decision at the Center. We had been trying somewhat unsuccessfully to retrofit experiments into spacecraft not designed for them, namely Mercury and Gemini. This entailed working through the project or program office here in the Center and working with the contractor. Norm Foster had been involved in the Gemini Program Office and Bill Armstrong in Flight Crew Operations, and we had been involved in the technical side of it. Thus, three Center elements were trying to find a way to get the job done. The working relationships were not clear cut. So between Mercury and Gemini we set up a separate independent experiments group. Bill Armstrong was pulled out of the Flight Crew Support Division and put into Stoney's Division along with several other people and they were supposed to work under me. Functionally it didn't quite work out that way, but it at least put two of the groups under the same roof. Norm Foster stayed on in the Gemini Program Office and served as our interface there. After one or two of the Gemini flights EXPO was formed which pulled together all of these elements including Norm Foster, into one central office which handled experiment integration and management under the direction of Bob Piland. Since Bob came from a project office, he knew how to work with them and knew how to work with contractors through them.

That was mid-1965. I left the picture for a year at that point. That picture didn't change too much while I was gone, except that in early 1966, the decision was made to break out what was later

designated the Space Sciences Division as a separate entity and to put it ~~too~~ under Bob Piland. So he headed up both EXPO and Space Sciences Division. There was a very good reason for this from a management point of view, ~~as~~ The scientists who developed the experiments and the engineers who integrated them into the spacecraft needed to work closely together in order to work effectively. Putting them under one functional head helped; however, they were remotely located from each other and the communications that we hoped for never really materialized. That fundamental principle is still observed in what is now the Science and Applications Directorate in that half the elements are engineer-, implementation-, and hardware-oriented and the other half of the directorate is scientifically oriented. The intent is to maintain a close working relationship between these two groups, both of which are necessary to experiment operation.

In 1962 a proposal had been made for a building to handle the space physics groups here at the Center. It was turned down in that particular C of F budget. In 1963, we again submitted it (that would have been the 1964 C of F budget), and after a number of close calls it was approved. Building 31 it was to be, and it was very hard to define what function it was going to serve. I think it was called "Lunar and Earth Sciences" -- some such glamorous name which tied it to Apollo. In it we planned to do research in support of Apollo such as on radiation and meteoroids. Today, that is exactly what it is used for. It is fairly close to what we said we needed.

This was a highly specialized building. A meteoroid gun was built into it. It also contained a number of geological laboratories,

radiation ~~sources~~ instrument labs, specialized optical devices, and a seismic pier. We had extremely rough sledding convincing people that these were necessary. Science in the Manned Spacecraft Center in those days was almost a dirty word and it took a great deal of perseverance on the part of Max and myself who believed in it to get it approved. We made a major impact on the Center by proposing that ~~this~~ a facility (LRL) was going to be required in order to handle the samples that were going to come back from the lunar surface. This concept was first proposed by Dr. Elbert King and Don Flory both of whom were working for Dr. Foss at the time. The two of them proposed that when the samples were brought back from the lunar surface it would be necessary to have some place to handle them. We were not aware of quarantine requirements at that time, and we were proposing it purely on a scientific basis. Those lunar rocks would be extremely valuable and some early analysis would have to be done on them. We couldn't split them up and send them all over the country because each one was unique unto itself and we didn't want to destroy it by breaking it into pieces. I thought the proposal had a great deal of merit and recommended it to Max. Although he had some reservations as to how large this facility should be, Max supported the idea and put Jim McLane to work on trying to scope this thing to the right size. I should mention that what we were talking about at that time was about 1/4 of what we are talking about now and even then it was thought to be too big. We presented the proposal to Dr. Gilruth

and I regret to say he thought we were a bunch of nuts the first time. Max simply couldn't understand why Dr. Gilruth couldn't see it. But we decided it was important enough that we would try again, so he had Jim McLane change the size of it. We shrunk it a little bit and we again tried to show the justification.

At about this time something occurred that helped us. I think it was about at that time that the issue of quarantine became important. The National Academy of Sciences had recommended--but the report hadn't been released yet--that the astronauts be quarantined for 1 month. I was informed of this while at headquarters attending a meeting on this receiving laboratory. When I returned to the Center I asked for a meeting with the assistant directors and Dr. Gilruth and reported this fact. Their reactions were uniformly one of incredulity--nobody could do that--surely they must be jesting! Then their comments became derisive--the scientists could recommend that but it was completely impractical. I pointed out that they had some teeth in their proposal as the Public Health Service had been given the responsibility by the President for insuring this. The Public Health Service had indicated it would probably exercise this right in this case, and if they decided to do so, we would probably have to work out a reasonable compromise with them.

After this meeting we gave Dr. Gilruth a second presentation on this facility and tried to shape it more to a form that would appeal to him. He gave sort of a tacit approval to including it in the budget. Shortly thereafter the environmental quarantine conditions were

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imposed on the Apollo mission, and the size of the building grew greatly. It generated a certain amount of support by virtue of the fact that a quarantine was necessary.

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This building evolved at a series of subsequent meetings with Harry Hess of Princeton, who had been appointed by Mr. Webb to decide if the building really should be built, and if it should be at Houston or at some of the other Centers, and roughly what size it should be.

I particularly remember the meeting with Harry Hess because there were several forces in play at the time that we recognized as competition but didn't know exactly how to handle them. One was an individual from USGS on assignment at NASA Headquarters, a geochemist by the name of Dr. Ed CHAO Chou. He was making a very strong effort from within NASA to have the lunar receiving laboratory assigned to the USGS, preferably at Flagstaff where the geological interests would operate it. He was extremely dangerous to our objectives because although he was a USGS man he seemed to speak as though he were working for NASA. Also Goddard Space Flight Center had suddenly decided that maybe such a facility should be located at Goddard along with other "data bank" facilities they wanted. They made an initial attempt to be considered for having the lunar receiving laboratory put there however, it never was an extremely strong pitch. Every university in the country also wanted to have it near them, and they jointly questioned the wisdom of locating it at the Manned Spacecraft Center. However, it would have been a real dog fight for any one university to have gotten it, as they couldn't get agreement among themselves.

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I think the deciding factor was the fact that the crew was going to have to be in quarantine. Obviously if the crew was in quarantine, it would be more desirable to have these facilities in one place, and it made sense for MSC to have it. We insisted we needed the information from these rocks quickly in order to prepare for the next mission which would probably be coming down the line within 6 months. We didn't have time to go out and spread this stuff around the country and still get the feedback that we required for the next mission.

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Although I was familiar with all of the experiments, I guess the one I remember the best is the one that was developed first for Gemini. This was a device developed by Jerry Modisette which was designed to measure the static discharge buildup on a spacecraft. I think it was designated as ~~E1, or T-1~~ MSC-1, which was the category we used at that time. When an airplane flies through air at even a moderate speed it builds up a static charge - like a condenser - and all of a sudden it is discharged. This can be very disturbing to passengers, as well as ~~very~~ dangerous. For this reason if you look on most airplanes you will see little cords or discharge lines off the trailing edge of the wings. These little cords are designed to get rid of this static charge by discharging it into the wake rather than causing arcing on the airplane itself. The same principle arose during the Mercury days when the very high velocity arc jets were developed. In putting models into these jets it was noticed in a number of areas, large discharges occurred across them, particularly in a low vacuum condition, because that was the condition we were

concerned with in space. We weren't too concerned with atmospheric discharges although incidentally, Marshall was--they wanted to put a similar device on the launch vehicle to see if it occurred during launch. It was suspected as the cause of the loss of several unmanned spacecraft in this time period. An arc, if it occurred between the two stages as they separated, or within a few fractions of a second after they separated could be extremely dangerous. Suppose a spacecraft in orbit builds up a charge and then a rendezvous is attempted--with two spacecraft very close together with different charges on them, a tremendous discharge could occur just before docking which might destroy all onboard systems. Because the problem was extremely complex in an analytical sense, it was almost impossible to handle it analytically with any assurance; therefore at the request of the Gemini office and funded by the Gemini office, we designed and had built a device for measuring static charge buildup and discharge on a spacecraft. It told us that we were not going to have a major problem in rendezvous in space because if any charge built up on the spacecraft, it leaked off extremely rapidly in space. It was not retained as in a condenser. Since there would not be any massive amount of charge strength buildup on the spacecraft, the rendezvous would be safe.

We had a number of other experiments, of course. The radiation dosimeters and various radiation spectrometers being developed for Apollo were tested as experiments on the Gemini spacecraft by this group of people in the Radiation and Fields Branch, now part of the Space Physics Division.