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FROM WALLOPS ISLAND TO PROJECT MERCURY 1945-1958: A MEMOIR[+]

Robert R. Gilruth (USA)[++]

INTRODUCTION

The group that created the Mercury concept came largely from the old National Advisory Committee for Aeronautics (NACA). The Missile Range at Wallops Island, operated by the NACA, was a major factor in the development of this group, hence the title of this paper, "From Wallops Island to Mercury." The concept of the Mercury capsule, indeed, the whole plan for putting man in space was remarkable in its elegant simplicity. Yet is was a daring and unconventional approach at the time of its inception, and a subject of considerable controversy. However, the project demonstrated principles that were so sound they were also applied in the design and operation of the Gemini and Apollo flight programs.

The period covered by this memoir extends from the founding of Wallops Island as a missile range in May 1945, through the establishment of the Mercury Project in 1958. It is a period that saw great change, not only in science and technology, but also in world history. With the advent of the space age, the old NACA faded away to become NASA (National Aeronautics and Space Administration). Many of the people who had worked on and developed Wallops Island research projects helped form the nucleus of the Space Task Group, the group that would manage the Mercury Project. They were joined by others from NASA and by specialists from the Army, Navy, and Air Force, and also from industry.

Much of the early work at Wallops Island and in NACA was done in support of the ballistic missile program in the United States. Had it not been for the ballistic missile development effort we would not have had the knowledge of reentry bodies, guidance systems, or other factors such as the launch rockets themselves that were to make possible manned flight in space in such a brief span of time after the space age arrived. The first

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[++]Director, NASA Manned Spacecraft Center (Johnson Space Center), 1961-1972.

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American astronaut orbited the Earth a little over three years after NASA was created. Even so, the Soviets were the first to put man in space. Yuri Gagarin in Vostok 1 would fly nearly a year earlier than John Glenn in Friendship 7.

This memoir is based primarily on my own reflections and recollections of this period. I have also discussed special events at length with Paul Purser, who was my close associate throughout the entire period covered by this memoir, and had recent discussions with Al Eggers and with Max Faget, both of whom played key roles in high-speed aerodynamics and also in satellite design.

In preparing this paper, I have used extensively the History of Wallops Station,[1] written by my former colleague, Joseph A. Shortal, who succeeded me as head of the Pilotless Aircraft Research Division (PARD). That organization operated Wallops Island and conducted the research program carried out there. His excellent history gives the complete story of the development of facilities and techniques used at Wallops Island. In particular, I have used many photographs and figures that Shortal collected from the old files at Langley Field.

THE BIRTH OF THE WALLOPS ISLAND MISSILE RANGE

The Wallops Island Missile Range was created during the final phases of World War II, at a time when the type of warfare and the weapons being used were undergoing rapid change. The flight speeds of aircraft were approaching the speed of sound and problems of aerodynamic shock waves and compressibility effects were assuming very serious proportions. In other areas, the guided missile had emerged as a major weapon of high potential. The V-1 buzz-bomb was harrassing the London area, and the V-2 rocket had just made its appearance. There were other forms of guided missiles in Germany that were playing an increasingly important part in the air war. In the Pacific, the Japanese Kamikaze suicide missile, of dread capability with its human guidance system aboard, was very difficult for us to cope with and caused much havoc with our naval forces. The Allies had put primary effort into the development of superior aircraft, and the appearance of so many guided missiles was a technical surprise. The new jet engines and rockets opened a wide spectrum of weapon possibilities, and the newly created atom bomb made the consequences of all these things seem far more serious.

At this time, the National Advisory Committee for Aeronautics was the principal aeronautical research establishment in the United States. It was responsible for aerodynamic research and for other fields of scientific endeavor dedicated to the improvement of aircraft. However, the high speeds of diving aircraft and supersonic missiles had overwhelmed NACA, for there were no wind tunnels in its laboratories that were capable of model testing for aircraft or missiles at airspeeds that approached close to and exceeded the speed of sound. In fact, wind tunnels of that time had a blind spot that extended through a whole speed region around the speed of sound (Mach number 1) due to a choking of flow in the test section of the tunnel. The NACA had concentrated its work on subsonic aerodynamics and its efforts in the transonic range had been confined to studies of shock wave-boundary-layer interactions during the early onset of compressible flows. The fast pace of events of the war made it essential to proceed much more rapidly with aerodynamic research. New techniques and solutions to problems were urgently required. We also needed to raise our sights and to gear up for a larger part of what obviously was to be a new national effort in weapon research and development.

It was at this time that new facilities for research on the problems of guided missiles and high-speed flight were proposed by the leaders of the NACA with the backing of high officers of the Army and Navy. A detailed account of the wartime work done by the NACA on guided missiles and of events leading to the establishment of the Wallops Island Station is given in Reference 1. Military officials proposed to Congress that a free-flight guided missile range+ be built and operated by the NACA. Geographic surveys were made up and down the East Coast of the United States for a location where such free-flight experiments could take place, yet be convenient to the NACA research center at Langley Field. At that time, a tracking range of about 50 miles southward along the coast was believed to be adequate for research purposes. Wallops Island, a strip of land on the Atlantic Ocean, met this requirement and had unlimited range directly out to sea. It was selected in April 1945.

In May 1945, after Congress had approved this new facility and had appropriated money for buildings and equipment,++ I was relieved of my duties in the Flight Research Division at Langley Field and put in charge of this new work. There was no fanfare about it. Mr. Crowley, the Acting Engineer-in-Charge of the Langley Center, just called me in and told me that I was to manage this new activity and that he had confidence that I was the man to do it. Prior to this time, I had worked in flight research after arriving at Langley Field in 1937. My work involved an extremely close working arrangement with test pilots in establishing handling quality requirements for airplanes.[2] In some of this work, I did a great deal of flying as an engineering observer, and I had developed a keen appreciation for the pilot's side of the man-machine relationships. I learned a great deal from Chief NACA Test Pilot, Melvin N. Gough, in particular. He was very interested in airplane handling qualities and took great pains to educate me in both pilot and airplane characteristics. This background was to be very important later in our work on Project Mercury, and throughout Apollo, in decisions regarding the roles and authority of man in the space capsule.

+They also proposed that NACA build a supersonic wind tunnel which later was authorized for the Ames Laboratory in California.

++Funds in the amount of $4,535,000 were appropriated for the construction of facilities and the first year's operation, Reference 1.

I was also deeply involved at that time in transonic aerodynamic research using (1) free-falling, instrumented bodies dropped from high altitude, and (2) the wing-flow technique,[+] which I had invented to help with the problems of aerodynamics near and through the speed of sound. Some of this work is described in my paper on the development of the wing-flow technique,[3] that covers behavior of wings and controls at sonic speeds and illustrates the importance of thin wing sections for aircraft designed to fly men through the speed of sound. This work was available in time to be considered in the wing selection for the X-1 research airplane. It provided the impetus for selection of the very thin wing (by standards of those days) which first broke the sound barrier in 1947. My supervisor, Floyd Thompson, Chief of Research of the Langley Laboratory, made the decision based on data we had obtained with the wing-flow technique.

While I hated to leave the Flight Research Division, I recognized the opportunity of developing a whole new free-flight research facility, and soon I became completely engrossed in the new job of creating and operating the Wallops Island Missile Range. Wallops Island is a barrier island off the East Coast of the United States. It is a low-lying spit of land separated from the mainland by a salt marsh, and populated by many sea and game birds. Some of the most delicious clams and oysters in the world are to be found in this vicinity. The island itself was populated by miniature wild horses, voracious horse flies, and some of the hungriest mosquitoes I have ever seen. The north end of the island was wooded, while the part we were to utilize in our early operations consisted of sand and sea grass with a few salt water bushes. In the next few months, work went ahead in the design and construction of facilities on the island. All materials of construction had to be barged from the mainland to the island. Boats alone were used for many years to transport men and equipment. In 1959, after the creation of NASA, a causeway was built across the marshes which permitted rapid access by automobile and truck, and greatly increased the work capacity of Wallops Island. Of course, by this time, I was no longer in charge of Wallops Island. Robert L. Krieger had become Director of the Station.

I learned many things during those early days of building and operating the Wallops Island range. I was a young man of 31 with little experience except in research matters. I found a whole new world of budgets, land acquisition, hiring, recruiting, and operating with other agencies and companies, as well as with the community around Wallops Island. We had planned all along to retain Langley Field as our research base, using Wallops Island principally as a test site where research missiles and models of various

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[+]The wing-flow technique gave aerodynamic results over the speed range from Mach number of 0.85 to 1.2, the very range which the wind tunnels could not cover. It made use of the fact that the air above the wing of an airplane went quite smoothly and uniformly through the speed of sound. This region of airflow on a P-51 airplane was used to test models of wings and controls. This method and a variation of it in wind tunnels was used extensively for several years.

kinds could be flown without danger to local communities. The models were prepared at Langley and carried over to Wallops Island by air or boat where they were fitted with boosters, internal power, and sustainer rockets. The radars, tracking equipment, and telemeter receivers were located at Wallops Island, but the research work on new telemeters and the instrumentation would be done at Langley Field. This plan was best suited to those times. It gave us the highly sophisticated technical laboratory with all facilities at Langley Field for the research staff, and yet we had the remote spot where the research flight tests could be carried out. This plan continued in effect for many years, and it was only after the advent of the space age and the creation of NASA in 1958 that Wallops Island became the independent NASA Station which it is today.

EARLY GOALS AND ACHIEVEMENTS

Those who first conceived the need for Wallops Island believed that flight trials of guided missiles for the Army and the Navy would be conducted there. However, it was soon evident that the service organizations were going to want their own guided missile ranges. The Navy was to develop such facilities at Point Mugu and at Inyokern, and the Army already was in business at White Sands, New Mexico. The Banana River Test Range at Cape Canaveral later became the guided missile range of the newly formed United States Air Force.

I determined to begin operations at Wallops Island as early as possible so that we would be able to incorporate our operating experience into the design of the permanent facilities. Our operations during the first year were reminiscent of advanced base activities in the war in the Pacific. We used surplus equipment and material, such as landing craft from the United States Navy, pierced steel plank for roads and landing ramps, jeeps, and other kinds of gear that had been developed during the war. Our first launching of a research vehicle occurred in August 1945, only a few months after the start of Wallops. A model of the air-to-air missile, Tiamat,[+] was flown out over the sea in a flight simulating inputs from a guidance system through an autopilot. A view of the Tiamat model on the launch pad during launch preparations is shown in Figure 1. I personally attended all launchings in those days, in charge of launch operations, and made decisions during launch preparations and the final countdown. We operated out of a blockhouse made of sandbags. Our firing leads and cabling to the launch sequencing equipment were strung out over the sand. It took a great deal of enthusiasm and individual initiative to work in the heat, amidst the mosquitoes, horse flies, and with sand in our food. Actually, the Tiamat test was quite successful for what it was intended. However, we could see that the missile was

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[+]Tiamat was officially the Army Air Forces MX-570 project, an air-to-air missile. According to Babylonian mythology, Tiamat was a sea monster; Reference 1.

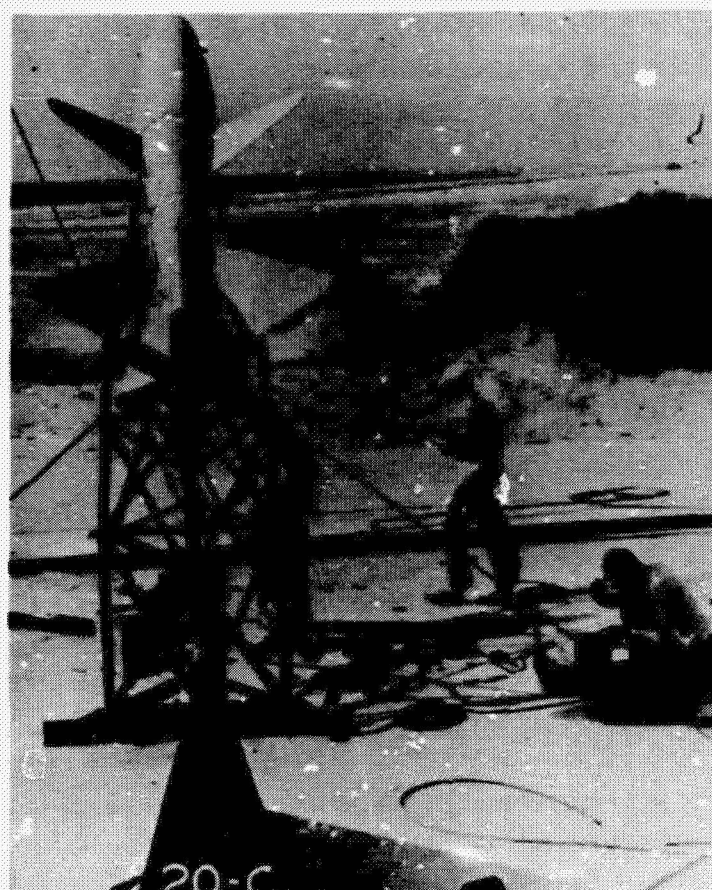

Fig. 1
View of Tiamat Test Vehicle and Booster on
Launching Pad, Wallops Island, July 1945

a very complex and expensive device to construct, instrument, and launch, even if we had had adequate and permanent facilities.

It soon became apparent to me that the original plan, which called for launching a large number of these complex missiles of Army or Navy designs, would take all of our resources and leave little remaining for work on the principal problems of the time, which had to do with transonic and supersonic flight characteristics of new and advanced aerodynamic concepts. From almost the very first, therefore, we began changing the purpose of the Wallops Island range. Although originally conceived as a missile testing site, it became a missile research range. Here we developed otherwise unobtainable basic information on the aerodynamic and structural behavior of wings, bodies, and controls, and on other key items in missile and aircraft design from rocket-powered models in free-flight at supersonic and transonic speeds. In the process we acquired a nucleus of very skillful and creative people in this research with rocket-propelled models. Key people in this very early group and their specialties are listed in Table I.

TABLE I

KEY PEOPLE[+] AND THEIR SPECIALTIES AT

WALLOPS STATION

Paul E. Purser	- General Aerodynamics & Structures
Maxime A. Faget	- Aerodynamics & Propulsion
Paul R. Hill	- Aerodynamics & Propulsion
Joseph G. Thibodaux	- Propulsion & Materials
William J. O'Sullivan	- Propulsion & Aero Sciences
David G. Stone	- Stability & Control
Caldwell C. Johnson	- Design of Flight Models
Edmund C. Buckley Morton J. Stoller Charles A. Taylor	- Overall instrumentation system: radar, telemetry, sequencing, checkout
George B. Graves	- Telemetering
Paul F. Fuhrmeister	- Doppler Radar
Gerald M. Truszynski	- Tracking Radar
Robert A. Gardiner	- Guidance & Stability Equipment
Robert L. Krieger	- Tracking Radar - Operations Management

We devised many specialized techniques for this research over the next several years. We had to seek low cost methods of model construction, instrumentation, and propulsion, to keep within our small budget, and yet cover significant numbers of configurations of interest to designers of new aircraft and missiles. Most of our research models were relatively small, and we used solid rockets almost exclusively for propulsion because

[+]The organization relationships are described in detail in Reference 1. Ray Hooker was my original deputy during the days of building the Range. Buckley became my deputy in January 1948 because of the importance of electronic interface with the research programs. Later Shortal replaced Buckley as my deputy and became head of PARD in 1952 when I took other duties.

they were adequate to give us the speeds that we needed, and were simple and relatively inexpensive. In fact, standard aircraft rockets were available to us free of charge from the Navy. The solid rockets were fitted inside the fuselages of our research models, which were often constructed of wood with metal inserts to have the required aerodynamic shape and the necessary strength and stiffness. An early drag research model in its special launcher is shown in Figure 2. Systematic studies were made of wing and body drag using simple models of this type.

Fig. 2
An Early Drag Research Model in its Special
Launcher at Wallops Island, October 1945

A Doppler radar, TPS-5, obtained from the Army (Figure 3), was used to track the models in coasting flight so that we could measure the drag of configuration without having to put instruments into the model itself. This was possible because the radar was sufficiently precise in its measurement of velocity that one could accurately determine the deceleration of differentiating the velocity-time curve. This measure of deceleration, with the known mass of the model, gave us the drag forces versus Mach number. Radiosondes were used for atmospheric data. A typical velocity-time curve for an early drag test is

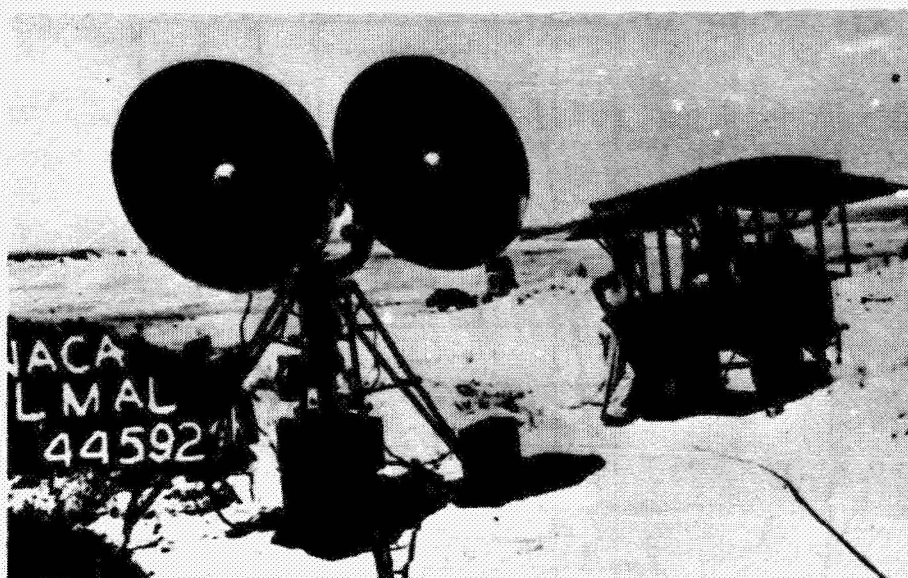

Fig. 3
Early Doppler Radar (AN/TPS-5) Used at Wallops Island
in Initial Aerodynamic Drag Studies

shown in Figure 4. After burnout, the slope of the curve is a measure of the drag. Figure 5 shows the results of systematic study of the effects of sweep-back and aspect ratio on the drag at supersonic and transonic speeds. As time went on, the radars were improved and greater accuracy and range were obtained. Systematic tests were made with larger models and over a greater speed range. Figure 6 shows typical results from systematic tests of various fuselage shapes.

Drag measurements of this type became our most important product during those early years. I recall, however, the first real trial of the technique occurred during a visit of George W. Lewis, Director of Aeronautical Research in Washington, and Jerome C. Hunsaker, Chairman of NACA, both men of great prestige and authority. As luck would have it, the model we launched to show how the technique worked lost its wings due to a structural failure. Hunsaker immediately said, "So the technique is no good; it doesn't work." I recall replying, "No, Dr. Hunsaker, we simply have to learn how to make the wings stronger." This we were able to do, and for the next ten years or so Wallops was to measure the drag characteristics of literally hundreds of new airplane and missile designs, as well as make systematic studies of wing and body drag and of other items.

Data on the effectiveness of various types of controls were provided by another ve simple technique that utilized a polarized signal from a small radio transmitter in the model to measure the rate of roll as the model coasted through the speed range from supersonic to subsonic speeds. The effectiveness of deflected ailerons on winged models

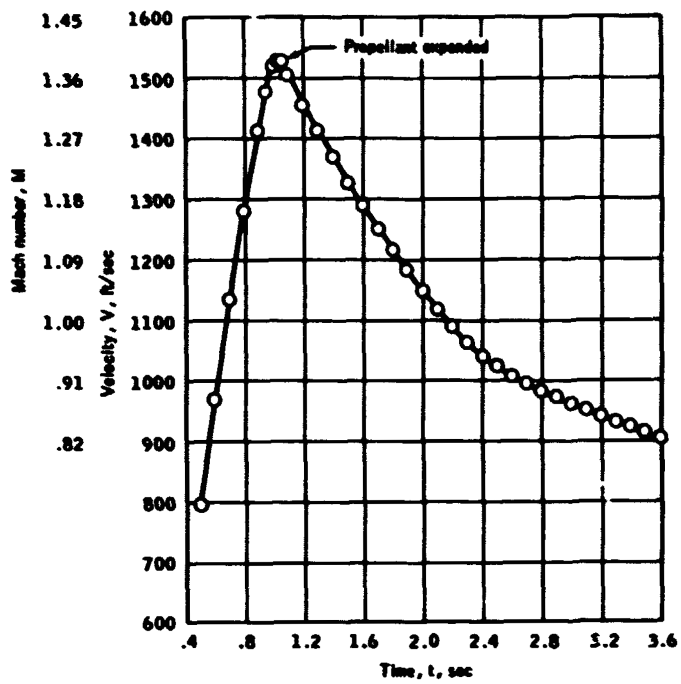

Fig. 4
A Typical Velocity-Time Curve for a Drag Research Model

in producing rate of roll was a direct measure of the aileron power of the design at the various speeds encountered. In this way, we measured the properties of various kinds of ailerons and spoilers on swept and unswept and tapered wings of various aspect ratios. These results found immediate use in the design of missiles and high-speed aircraft. A typical test result is shown in Figure 7, where reversed aileron effectiveness is cured by using a blunt trailing edge on the aileron.

During this early phase of Wallops Island work, the systematic data on the drag and control effectiveness was also used as a yardstick for comparison with other techniques, such as the wing-flow technique in-flight and the transonic bump technique in wind tunnels. Conventional wind tunnels were still unable, because of the choking problems, to provide useful data for aircraft and missile configurations at speeds close to the speed of sound.

As our abilities increased, we were able to provide free-flight data on the wing flutter characteristics and pressure distributions, as well as on dynamic behavior of complex wing-body arrangements. Using our new free-flight techniques, we provided information for the design of the X-1, X-2, and X-3 research airplanes. Model tests of new fighter aircraft were made at Wallops Island while they were still on the drawing board.

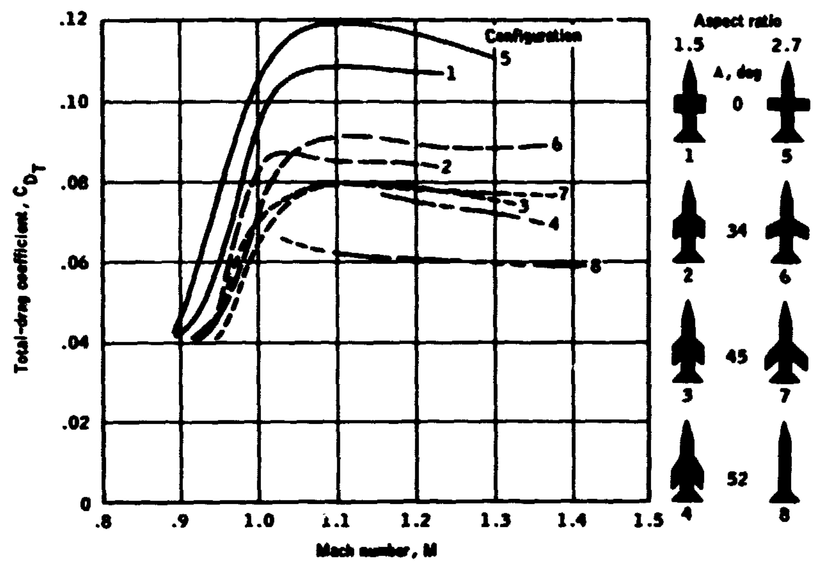

Fig. 5
Effects of Wing Plan Form on Drag From
Early Tests at Wallops Island

The investigations usually concerned drag and stability through the speed of sound. The North American F-100 fighter was tested in this manner, as were many others. In the missile field, models of the Rascal, the Snark, the Sparrow, and the Navaho were flown at Wallops Island, to name only a few.

During these early years of guided missiles and supersonic flight, the ram jet was considered to be an engine of great promise, and a number of ram jet test vehicles were flown at Wallops Island. One such program involved the NACA Lewis Laboratory at Cleveland, Ohio, which had prime responsibility for ram jet engine research. Their program used free-flight vehicles air-launched over the Wallops Island range. The concept was to mount the test vehicle underneath a carrying airplane at the Cleveland airport, and then take off and fly to the vicinity of Wallops Island. Once over the Island and acquired by tracking radars, the airplane would proceed on a drop run with its test vehicle underneath. The ram jet engine would then be activated and the test vehicle launched on a prescribed trajectory. The radars would track its flight while the telemeter receivers at Wallops Island would record the instrument records of the engine operation. This technique proved to be very successful, and many such drop tests were made in the years of 1949 and 1950. Key people in this Lewis Laboratory work were Scott Simpkinson and John Disher, both of whom joined the Mercury Space Task Group in later years.

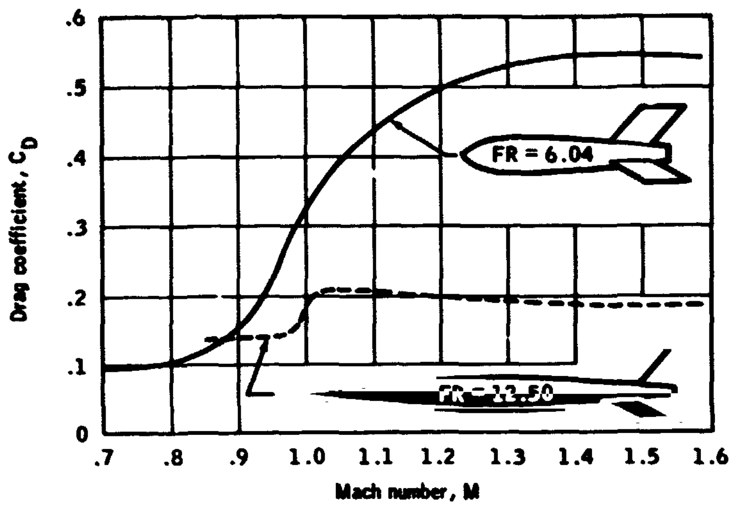

Fig. 6
Effect of Fuselage Shape on Drag From Wallops Island Tests

A second ram jet program involved a Langley group under Paul Hill and Max Faget that specialized in ground-launched test vehicles using ethylene fuel and burners of short combustion length. The configuration consisted of a long central body containing the fuel tank with two ram jet engines mounted as nacelles on either side of the horizontal tail. The first ram jet of this type was flown in March of 1950, achieving a Mach number of 3.02, a new world record. A second flight a few months later achieved a Mach number of 3.2 and the test vehicle coasted on up to a height of over 130,000 feet. Hugh Dryden was greatly impressed at the performance of these small ram jets and took great pains to inform the military services of the success achieved at Wallops Island. However, both the Air Force and Navy were indifferent to this work, and showed little interest in following up on the Langley designs.

After these initial successes, ram jet work gradually lost favor to the increasing importance of the ballistic missiles in the national programs. The Langley work was phased out and the Lewis flight efforts turned to research on reentry bodies. It is of interest that a young engineer, named George M. Low, was working with Scott Simpkinson on reentry bodies air-launched at Wallops Island only a few years before they both joined the Mercury effort for flying man in orbit. The research vehicles flown by the Lewis Lab for measuring reentry body characteristics were able to achieve very large Reynolds numbers at high Mach numbers because an air-launch allowed the test vehicle to penetrate downward into the atmosphere achieving high Mach number in the dense lower atmosphere, whereas the test

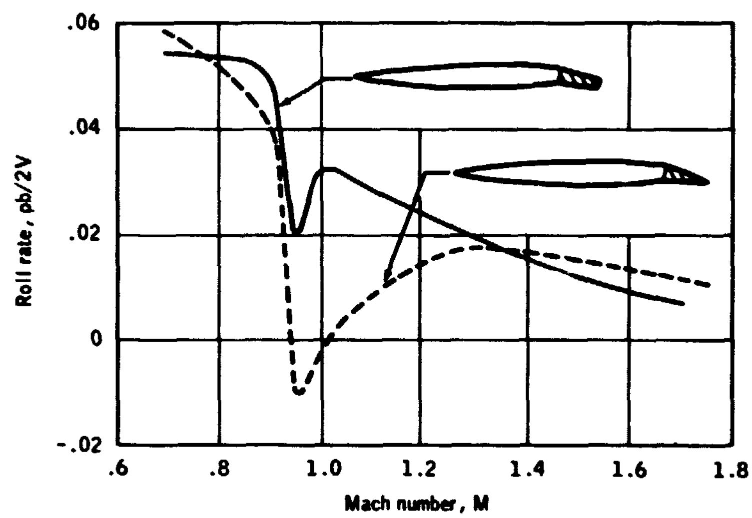

Fig. 7
Effect of Aileron Shape on Rate of Roll at Transonic
Speeds From Early Tests at Wallops Island

vehicles ground-launched from Wallops Island achieved high Mach numbers in the upper atmosphere where the Reynolds numbers were lower. Thus, the Lewis work made a good counterpart for the work done at Langley in contributing to the knowledge of aerodynamic heating on reentry bodies for ballistic missiles.

I cannot leave free-flight techniques without describing the stability and control work using "pulsed controls." In this technique, the elevators, or pitch controls of an aircraft or missile configuration, were moved abruptly up and down in a square wave pattern by a motor drive, and the response of the model in pitch was measured using accelerometers and an angle of attack meter. From these data it was possible to obtain not only the static stability, dC_m but also the lift curve slope, dC_L the neutral point, and the drag due to the lift $\frac{dC_D}{dC_L}$.

A variation of this technique employing "pulse rockets" was also used. Pulse rockets were simply small rockets specially designed to give pitch or yaw disturbances to the model during flight for the same purpose, i.e., stability measurements. A key instrument in all this work was the angle of attack meter. We developed our own device in NACA that consisted of a small delta-wing probe mounted in the undisturbed airflow forward of the nose of the fuselage.

But it was always a problem to obtain enough monetary support to do all the work that needed to be done. In free-flight testing, it was especially difficult to convince the Congressmen and other high officials that work was really worthwhile when the models had to be destroyed in the tests. People would always exclaim: "What a shame to let such a fine model destroy itself in the sea." Recovery efforts would have been much more expensive than the models at that time, and, in spite of the loss of the model with each flight, our costs were highly competitive with the supersonic wind tunnels which had very great initial cost and far less flexibility.

However, American industry, our principal customer, was impressed with the value of our results and they were not bashful in asking for more. A group called the "High Speed Subcommittee," which was led by Eugene Root of the Douglas Company, together with other members of the aircraft and missile industry, supported us in a solid manner. They recommended in a unanimous vote that the Wallops Island work be expanded by a factor of at least three. This recommendation was passed on through their parent Committee to the NACA itself. The NACA budget for the Wallops Island work was increased by a factor of three and this action greatly helped in expanding our work.

During this time of intensive work on new research techniques, I was appointed a member of a group in Washington called the "Planning Consultants to the Committee on Guided Missiles." Karl T. Compton, President of MIT, was Chairman of the parent Committee, the Committee of Guided Missiles, which operated in the Pentagon. It was the job of the Planning Consultants to review the nation's guided missile programs and to make whatever recommendations we felt would make for a stronger and more effective national effort. It was a great opportunity for me as a young engineer, whose specialty was aerodynamics and structures, to work so closely with these people from industry, university, and government, who were skilled in all the disciplines and in management as well.

While I served on this Board (1946-1947) our work covered the early period of the national guided missile program. This was the time when the Lark, the Hermes, the Bumblebee, the Navaho, and the Nike were among the major missile projects in the United States. The atom bomb was still so secret that even people working as Planning Consultants for the Committee on Guided Missiles were not privy to the information on the sizes and weights of these weapons. We made several recommendations to the Guided Missile Committee. The most important one, in my opinion, was that the ballistic missile had great promise as a weapon and that the technical problems in guidance and reentry could be solved. Of course, it was up to the Guided Missile Committee as to how our recommendations would be taken and it was several years before the national program to develop the ballistic missile was instituted.

In looking back over the years following World War II, the data and techniques developed at Wallops Island had an important and quite profound effect on U.S. Aircraft and missile designs. In later years, the Wallops Island group would gain competence and experience in dealing with the problems over almost the entire spectrum of high-speed flight. This background was to be a great help in moving into the space program when the opportunity came.

WALLOPS ISLAND: TRAINING GROUND FOR SPACE

I have beer asked many times: "Why was Wallops Island such a good training ground for the space age?" Several factors were important. First, in a flight test project, the engineer had to consider the whole spectrum of problems. He learned about all the phases of his project even though he might only be seeking a drag curve. The model had to be designed and constructed with a power plant, a launcher, telemeter and instruments, radar tracking, operational crews, and so on. This broad kind of responsibility tends to attract and develop exceptional people. I found this to be true also in flight research with manned aircraft. One had to take a broad view; the detail thinkers tended to gravitate to the wind tunnel or other kinds of specialty work.

The fact that the research models were expendable and therefore had to achieve a very high reliability was also an important factor. For example, the pyrotechnics had to work, the second stage rockets had to ignite, the electronic systems had to survive the vibrations of the launch and the accelerations of the flight. We had to be concerned with structural integrity because the loads imposed on these models were high indeed. We had to be concerned with wing flutter and divergence, and we had to deal with problems of aerodynamic heating. Pressure was on the operational people to acquire the models with radar or the flight would be a failure, and we were operating over the sea, which gave us an intimate knowledge of the recovery environment with which we were to work later in recovery of manned space capsules.

We were using as everyday tools items that would be vital to space projects. This activity thus became a great training ground for the young people coming up. We learned the necessity of keeping our designs extremely simple and attacking a problem in steps. During the first two or three years, I insisted that we use no more than 4-to-6 channels of telemetry in a given model, and that we build our programs to minimize the effect of a loss by not involving too much effort in any one flight. Wherever possible we used the backup systems that were to become important in spacecraft design. However, the extreme weight sensitivity of these early rocket models made redundancy difficult to achieve except, of course, for very small, light items such as the electrical squibs used for igniting rockets and activating other devices.

NEW HORIZONS

In the fall of 1947, the X-1 research airplane, piloted by Major Charles Yeager, became the first manned airplane to break the sound barrier. Thanks to the results from the Wallops Island rocket models and earlier transonic research, there were practically no surprises. It was a great milestone of flight. Later that same year, John Stack and his associates at Langley Field built the first truly transonic wind tunnel. It was neither open nor closed, but had a slotted throat which permitted the flow to go quite smoothly through the speed of sound in the test section without the choking phenomena of the past. This type of wind tunnel was to be further refined and applied to wind tunnels all over the world.

Our work at Wallops Island began changing. There was less incentive for transonic testing with the improved wind tunnel capability, and our interests were increasing in higher Mach number problems. In 1952 Bob Woods, the Chief of Airplane Design for the Bell Aircraft Company, proposed a new research airplane, the X-15, for flying man to the very edge of space. I remember well his proposal, and equally well the people even in those times who said, "Why fly man? Why not fly an instrument?" My reply was: "Man will fly in space someday, so why not start doing it as soon as possible." This was the first time I heard the theme I would hear so often in later years reflecting the conflict between manned and unmanned flight. The X-15 became a successful research airplane, but the blunt space capsules in both the United States and Russia became the first true space vehicles.

In the guided missile field, heat transfer was becoming very important. Heat transfer research lent itself well to rocket model techniques at Wallops Island. Very accurate and useful measurements were made in free-flight using the skin of the missile as a calorimeter. This work started with research on slender bodies of revolution and in later years proceeded to concentrate on reentry bodies having blunt faces. Figure 8 shows a research missile designed for boundary-layer and heat transfer work at high Reynolds numbers at supersonic speeds. The model was designated the RM-10 and used for correlating flight and wind tunnel results to show the effects of Reynolds number and tunnel turbulence at various Mach numbers.

My own career was changing along with the changing research picture. I became an Assistant Director of the Langley Center, and my responsibilities broadened to include the Structures and Aerodynamic Loads Divisions in addition to the free-flight testing at Wallops Island. During the next few years we worked very hard to develop structures and techniques for high-speed flight and high temperatures. The work at Wallops Island went to higher and higher speeds using multistage solid rockets. In order to reach the high Mach numbers required, powerful new solid rockets were acquired and staging techniques reached a high degree of sophistication. In the years just prior to Sputnik, five-stage

Fig. 8
Research Missile RM-10 Designed for Boundary-Layer and Heat Transfer
Research at High Reynolds Number and Supersonic Speeds

rockets were used to achieve airspeeds greater than Mach 15.0. A five-stage rocket designed to measure heat transfer on a blunt body is shown on the launch pad in Figure 9. The know-how acquired in designing multi-stage solid rockets was applied in later years in the development of the multistage rocket called the "Scout." The Scout was to launch satellites into Earth orbit from Wallops Island as well as from other locations. It is still frequently used.

At Langley Field, meantime, we were also creating new kinds of ground facilities such as arc jets, pebble-bed heaters (using alumina, zirconia, and thoria), and combustion jets of various sorts. Paul Purser worked with me on special assignment to create many of these new high-temperature facilities. Most of these were pilot models of ideas that were to be pursued further in the future, but they were also very productive of data on heat transfer and ablative materials for ballistic missile designs at that time. Figures 10 and 11 show the design of a zirconia pebble-bed heated jet which is still in service.

Fig. 9
Five-Stage Heat Transfer Test Vehicle Launched
December 1956, Wallops Island

Supersonic air jets with stagnation temperatures of 4000° were used for material testing. A larger facility of this type was later built at the Ames Research Center in California.

EVOLUTION OF THE MERCURY CONCEPT

The launch of Sputnik 1 on October 4, 1957, had a great effect on the thinking of all of us in the NACA. I can recall watching the sunlight reflecting off of the Sputnik 1 carrier rocket as it passed over my home on the Chesapeake Bay in Virginia. It put a new sense of value and urgency on the things we had been doing. When one month later the dog, Laika, was placed in orbit in Sputnik 2, I was sure that the Soviets were planning for man-in-space.

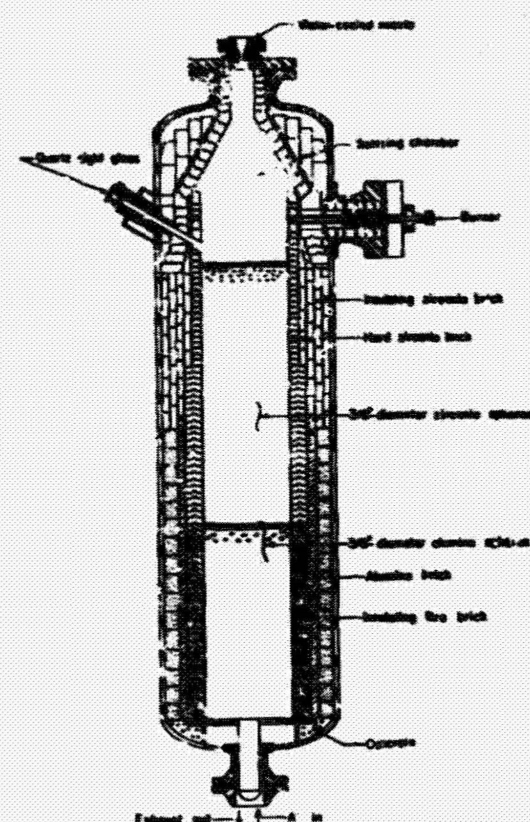

Fig. 10
Diagram of Ceramic-Heated Tunnel at Langley Field, 1957

At that time, many of us started thinking intensively about manned satellites of one kind or another. It seemed to me that the United States would surely compete with the Soviet Union in space research. Prior to this, many of our research people had been studying manned hypersonic gliders. In fact, there was a high-priority research project sponsored by the Air Force for studying just such a vehicle. This work later became the basis for the Dyna-Soar Project that continued until the end of 1963 before it was cancelled. Another hypersonic glider, the X-15 research airplane already mentioned, was under development. It was designed for a maximum speed of about a Mach number of 7. According to histories of this time period,[4,5] work was being done on manned satellites even before Sputnik. I do not recall having contact with this work except for that of Harvey Allen of the Ames Research Center. He was the first, to my recollection, to propose a blunt body

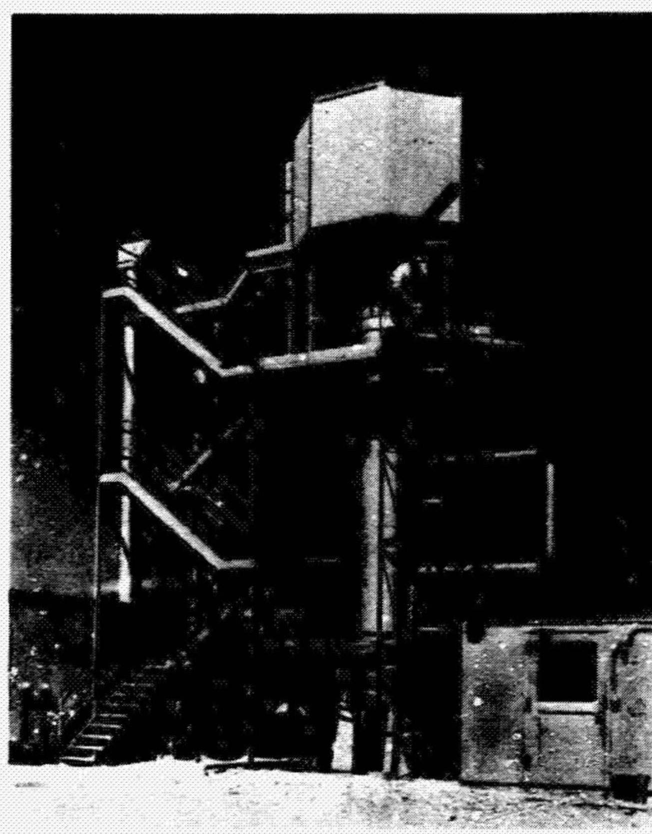

Fig. 11
Photograph of Ceramic-Heated Tunnel at Langley Field, 1957

for manned satellites. He suggested a sphere to enclose the man and said, "You just throw it," meaning, of course, launching it into orbit with a rocket.

In the fall of 1957 we held a meeting to discuss "Round 3" at the Ames Research Center in California. "Round 3" was a term for the next step for research aircraft beyond the X-15. As it happened, the Soviets launched Sputnik the week before the meeting. The impact of the Soviet achievement on the meeting was the realization that orbital flight was a legitimate national goal. I could not attend the meeting, and Purser and Faget represented the Langley and Wallops group. They came away disenchanted with hypersonic gliders and convinced that a blunt body-type of manned capsule should be the next step in manned flight. Al Eggers of Ames had proposed a compact-type of glider, now termed a "lifting body," shaped like "half-a-baked potato." This was a clever design and one which would have inherent advantages in lower reentry g-forces.

In March 1958, at another conference held at the Ames Research Center, Max Faget presented a paper+ called "Preliminary Studies of Manned Satellites - Wingless Configuration: Non-Lifting." This was a very significant paper. It put forward most of the key items that we would use in conducting the Mercury Project. It showed that a simple, nonlifting satellite vehicle of proper design could follow a ballistic path in reentering the atmosphere without experiencing heating rates or accelerations that would be dangerous to man. It showed further that retrorockets of modest performance were adequate to bring the capsule down from orbital speed and altitude to a reentry into the atmosphere. It also described the use of parachutes for final descent and small attitude jets for controlling the capsule in orbit, during retrofire, and reentry. This paper concluded: "As far as reentry and recovery are concerned, the state of the art is sufficiently advanced so that it is possible to proceed confidently with a manned satellite project based upon the ballistic reentry type of vehicle."

Because of its great simplicity, the nonlifting, ballistic-type of vehicle was the front runner of all proposed manned satellites, in my judgement. But there were many variations of this and other concepts under study by both government and industry groups. The choice involved considerations of weight, launch vehicle, reentry body design, and, to be honest, gut feelings. Some people felt that man-in-space was only a stunt. The ballistic approach, in particular, was under fire since it was such a radical departure from the airplane. It was called by its opponents "the man in the can," and the pilot was termed only a "medical specimen." Others thought it just too undignified a way to fly. Even Hugh Dryden, who at that time was Director of NACA, labeled the ballistic capsule proposal of the Army's "Project Adam" the same as "shooting a lady out of a cannon." When we proposed a similar ballistic phase in the Mercury Project a year or so later, he approved it since it was by then a buildup phase to orbital flight in the proof-testing of the spacecraft.

Various design concepts under study by U.S. industry at that time are shown in Figure 12. These configurations were compared and discussed at a conference held at Weight Field in January 1958. Weights of satellites varied from 1,000 to 18,000 pounds, and a wide variety of launch vehicle combinations were proposed. Even the X-15 was in the running. Its backers thought they could "doctor it up" somehow to get it through the reentry heat phase and thereby make it an orbital vehicle. Arthur Kantrowitz of AVCO had a very interesting concept which would deploy a very large metal parachute in orbit to cause enough drag or braking action to cause reentry. During reentry, the metal parachute would actually become white hot from the aerodynamic heating. Nevertheless, the majority

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+Benj[illegible] J. Garland and James J. Buglia collaborated with Faget in preparing this paper.

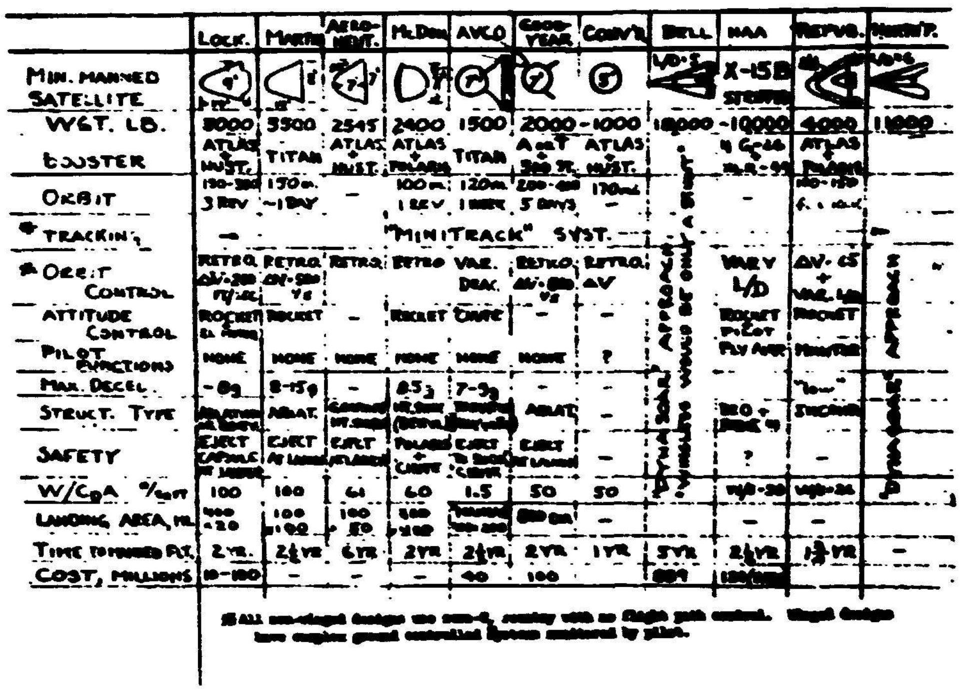

Fig. 12
Manned Satellites Under Study by U.S. Industry, January 1958

of the aerospace companies felt that some sort of ballistic shape should be used. Cost estimates varied from 40 million dollars to about 900 million.+

One concept of considerable interest, not shown in Figure 12, was proposed by Charles Mathews of Langley. It was the forerunner of the space shuttle designs of today. He proposed a circular-winged craft that would reenter at a very high angle of attack. In this mode of flight the heating rate would be greatly reduced and confined largely to the lower surface of the wing. Following the reentry heating phase, it would pitch over and fly to a landing like a conventional airplane. All these and other concepts were interesting and no doubt could have been made to work in due time. However, the most advanced ballistic missile at that time, the Atlas, could be expected to lift only about 2,000++

+It is of interest that the actual cost of the Mercury Project was about 400 million dollars. This cost covered everything, including the construction of the worldwide tracking range and the steaming time of the U.S. Navy for recovery purposes.

++The lift capability of the Atlas grew to more than 2,500 pounds by the time Mercury was ready to fly.

pounds into orbit. It seemed obvious to our group, therefore, that only the most simple ballistic capsule could be used if manned space flight were to be accomplished in the next few years.

During this period, I spent more and more time in Washington working as an assistant to Hugh Dryden, the Director of the NACA. There was a great deal of interest in space flight, and plans were going ahead at various levels in Government for the creating of a national space agency. Testimony was being given to the Congress by eminent people in science and technology on what they thought should be done in order to help make the United States competitive with the Soviet Union in this new space age.

THE HOT SUMMER OF 1958

In the early summer of 1958, a number of us from Langley Field and other laboratories of NACA went to work in Washington full-time to help Dryden and other members of his staff put together a plan and a budget for the new space agency that seemed certain to be created by the Congress that year. About 20 of us made our headquarters in one large room on the sixth floor of the old NACA building. There were about 10 telephones in the room, and we worked together and with others in Washington and around the country to create a plan and a budget. By this time, Abe Silverstein of the Lewis Research Center had been transferred to Washington, and Dryden placed him in charge of planning the entire space flight program.

Abe Silverstein and Hugh Dryden assigned to me to manage the man-in-space program during that hot summer of 1958, several months before NASA was created. I put together a plan that I hoped would be acceptable, not only to the people in NACA, but to the Advanced Research Projects Agency (ARPA), and, of course, to the President's Scientific Advisors. In order to do this, I collected a select group of people from Langley Field and from the Lewis Laboratory to form a sort of task force. Members of this group included Max Faget, Paul Purser, Chuck Mathews, and Charley Zimmerman of the Langley Lab, Andre Meyer, Scott Simpkinson, and Merritt Preston of the Lewis Lab, and many others on an "as needed" basis. George Low and Warren North of Lewis were brought in toward the end of the summer to help with the final plan, as was Charles Donlan of Langley.

During those humid summer days we came up with all of the basic principles of Project Mercury. The capsule would have a blunt face and a conically-shaped afterbody. It would be pressurized with a breathing atmosphere for the astronaut. The first real design to be put on paper was the work of Caldwell C. Johnson of the Langley and Wallops Island design groups. This original design is shown in Figure 13. Working closely with Max Faget and others at Langley, Caldwell Johnson continued to create successful spacecraft designs over the years. He helped create the designs for Apollo soon after doing those for Mercury. The escape tower was conceived by Max Faget, Andre Meyer, and some of their

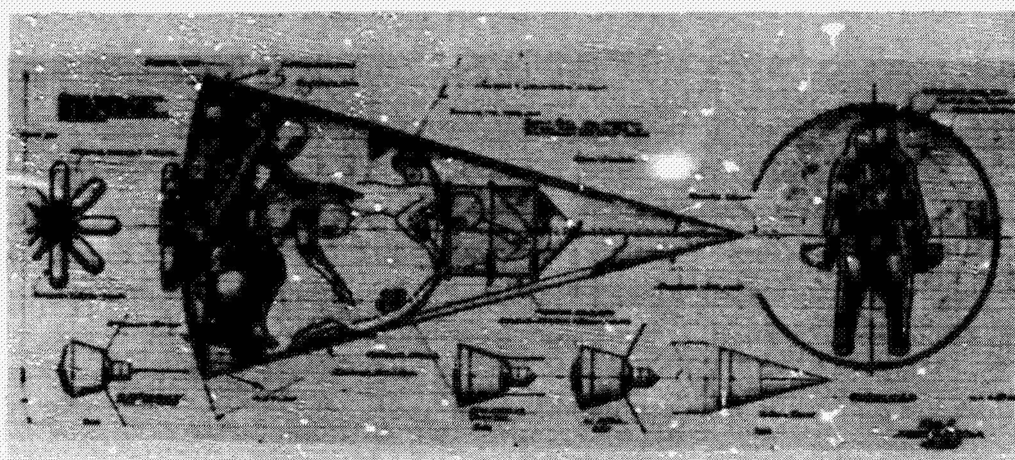

Fig. 13
Early Version of Mercury Capsule, Summer 1958

associates that same summer. The contour couch, another of Max Faget's ideas, that protected the astronaut against the high g-forces during launch and reentry, was the last key development in our plan that made Mercury possible. It was tested at the Johnsville centrifuge also that summer and proved effective for g-loads of over 20-g.

The launch vehicles proposed were the Atlas for the orbital phase of flight, with a Redstone used for early test flights of the capsule with man aboard. The Little Joe launching rocket, which was a new solid rocket cluster design proposed by Purser and Faget, was planned for use at Wallops Island · a means of testing the capsule configuration to gain experience with this type of flight vehicle before proceeding to the more expensive and difficult phases in the latter part of the program at Cape Canaveral.

The program did not yet have a name, but shortly after "go-a-head," Silverstein suggested we call it Project Mercury. This proved to be an excellent name and one we became very fond of. The basic principles on which we built the Mercury all emphasized simplicity. We wanted to use the simplest and most reliable approach—one with a minimum of new developments and using a progressive buildup of tests. It was implicit with this approach that we use the drag-type reentry vehicle, an existing ICBM booster, a retro-rocket to initiate descent from orbit, a parachute system for final approach and landing, and an escape system to permit the capsule away from a malfunctioning launch rocket. That was the way we went forward.

During July 1958, I made the presentation of this concept to the Scientific Advisor to the President, James Killian, and to the members of the President's Scientific Advisory Board. There were a number of members present who listened carefully to the presentation, among them: Edwin Land, George Kistiakowsky, and Killian himself. Herbert York, head of the ARPA Technical Staff, was also present. However, some of these gentlemen were not at all enthusiastic about our plan to put man into space. In fact, I was concerned at that time that they would not recommend that the program go forward at all. At one time in the hearings, Kistiakowsky remarked with great displeasure that our plan "would provide the most expensive funeral a man has ever had." However, when we left the hearings, Dryden was very pleased. He thought we had done well and he believed that the program would go forward.

We also appeared before Congress in presenting this program. The hearings held before the Select Committee on Aeronautics and Space Exploration on August 1, 1958, were the first disclosure to the public of how we planned to put man in space. I am impressed in going back over these hearings how very closely we followed what we said we would do. We said we would use the Atlas rocket, a special capsule with a blunt heat shield, and parachutes for landing at sea. All these things worked out very much as we had proposed. I was surprised at the public interest in our testimony. Crowds of people jammed into the small hearing room to look and listen during the presentations.

GO AHEAD - SPACE TASK GROUP

On July 29, 1958, The National Space Act was signed by President Eisenhower. The National Aeronautics and Space Administration was created and came into actual operation on October 1, 1958. T. Keith Glennan was appointed by the President to be our first Administrator. Hugh Dryden, the Director of the old NACA, was named the Deputy Administrator. NASA had been in business but a few days when we presented to Glennan and Roy Johnson, head of the Advanced Research Projects Agency of the Department of Defense, our plan for putting man in space. The presentations were the work of the Joint NASA-ARPA Panel for Manned Space Flight.+ Within two hours we had approval and a "go-ahead." Glennan advised me to get on with the project, go back to Langley Field, and put together a group to manage it.

The group, however, was not to report to the Center at Langley, but directly to Abe Silverstein in Washington Headquarters, who had now officially been made head of space projects in the new NASA. The Mercury presentation was made at the original NASA headquarters in the old Dolley Madison House in Washington, which had been fitted out for executive offices and conference rooms. Among those with me at the presentation were Donlan, Faget, and Mathews from Langley, Low and North of Lewis, Sam Batdorf of ARPA and, of course, Silverstein and Dryden. Paul Purser was not present as he was down at Redstone

+Members of the Committee included: Robert Gilruth (Chairman), NASA, S. B. Batdorf, ARPA, A. J. Eggers, NASA (September 24), Max Faget, NASA, George Low, NASA, Warren North, NASA, Walter Williams, NASA (September 24-30) and Robertson Youngquist, ARPA (September 24).

Arsenal already starting negotiations for the Redstone boosters for the program. George Low was to have gone to Langley Field with me to help work on the project; however, Silverstein found out within a few weeks that he needed Low in Washington. Therefore, Low remained there as assistant to Silverstein in charge of the Manned Satellite Program, at the Washington level, until Mercury was finished and Apollo was well underway. He then joined us at the new Manned Spacecraft Center in Houston.

We returned to Langley immediately after the Glennan "go-ahead." We realized that we had been given a job of tremendous difficulty and responsibility. I had no staff and only verbal orders to return to Langley Field and "get on with the job." Floyd Thompson, my boss prior to this new assignment, was of great help in getting the effort going. When I asked him how I could get men transferred from the research center at Langley Field to my new Space Task Group, he suggested that a simple memorandum to him (stating that I had been authorized by the Administrator to draft people from the Langley Center) would allow me to name those whom I wanted. This is exactly the way we went forward. The historic memorandum is reproduced in the Appendix. A good share of the leadership in the U.S. space program eventually came from this group.

In those first early weeks we prepared a specification for the Mercury capsule that went out to industry with a request for their proposals within the next two or three months. As a matter of fact, the entire time span from project "go-ahead" in October 1958, through the request for proposal, bidders briefings, source selection activity, and placing of the contract, all occurred before the middle of January, less than four months later. This kind of performance could only occur in a young organization that had not yet solidified all of its functions and prerogatives.

During this same period of time we established an arrangement with the Ballistic Missile Division of the Air Force for the procurement of the Atlas launch vehicles and for launch services. We also worked out a plan with General Medaris and Wernher von Braun of the Army Ballistic Missile Agency for the Redstone launch vehicles, and we started work in our own staff for a design and specification for the Little Joe rocket to be used in tests at Wallops Island. We gave to the Lewis Laboratory, now a NASA installation, the job of creating a full-scale Mercury model spacecraft for an unmanned flight at an early date to establish levels of heat transfer and stability in a full-scale free-flight test on an Atlas booster at Cape Cana[illegible]. Scott Simpkinson of Lewis was the key man in this project. He and his group, working w[illegible] others at Langley under Jack Kinzler, created a spacecraft called "Big Joe," which was the first major step in proving the capsule design. Simpkinson and his people did the lower part of the capsule, the instrumentation, control system, and the heat shield, while Kinzler's group did the upper heat shield and the parachute deck. The project started in December 1958 and the spacecraft flew successfully in September 1959!

All of our people worked holidays, evenings, and weekends. We even worked on New Years Day that year, but we did take off New Years Eve. Those were days of the most intensive and dedicated work by a group of people that I have ever experienced. None of us will forget it. We were making tests of escape rockets over on the beach at Wallops Island; testing parachutes in full-scale drops from helicopters, and measuring water impact loads on capsule configurations at Langley Field.

In our early organization, I was the Director of the Space Task Group with Charles Donlan the Associate Director. Paul Purser served as my Special Assistant, Max Faget headed the Flight Systems Division, Charles Mathews the Operations Division and Charles Zimmerman, who came from the Langley Stability Division, became head of a group on Reliability and Quality Assurance. Our new contracts were handled by Sherwood Butler. We obtained physicians and flight surgeons with aero-medical training on loan from both the Air Force and the Navy, as well as some psychologists who were to help us in writing the selection procedures for the astronauts. Dr. Randolph Lovelace of the Lovelace Clinic in Alburquerque, New Mexico, agreed to head an Advisory Committee on space medical problems. He was a great help to us in dealing with the medical community in the early days of space.

The events and accomplishments of the Mercury Project have been documented many times. I will not try to cover them all here again. But there are a few memories and anecdotes that are interesting and have never been told before. One such memory has to do with the term "astronaut." I remember very well using this term to describe the men to be selected as flight crew members with Dryden. The question came up as to whether we should call them "astronauts" or "cosmonauts." Dryden was of the opinion that "cosmonaut" would probably be more accurate because astro, of course, applies to the stars, and we really were beginning to probe only the nearby cosmos. However, the way it turned out, everyone we talked to seemed to prefer "astronaut," and this was the name that stuck. That is fortunate, I believe, because now when we say "astronaut," we know we mean Americans, and when we say "cosmonaut," we know we mean Russians.

Initially our specialists in crew selection proposed choosing space pilots from people who had dangerous professions, such as race car drivers, mountain climbers, scuba divers, as well as test pilots. Few people realized then the degree of skill, knowledge, and training an astronaut would need. When President Eisenhower decided that astronauts would be chosen from a military test pilot pool, we breathed a sigh of relief. This was one of the best decisions in the program. It made it quite simple and logical to delegate flight control and command functions to the pilot of the satellite.

We returned again to Wallops Island in the Mercury Program to launch Mercury-type capsules with the Little Joe rocket (Figure 14). We started first with a research program just to get more flight experience with the new space capsule configurations. We had to be sure there were no serious performance and operational problems that we had simply not

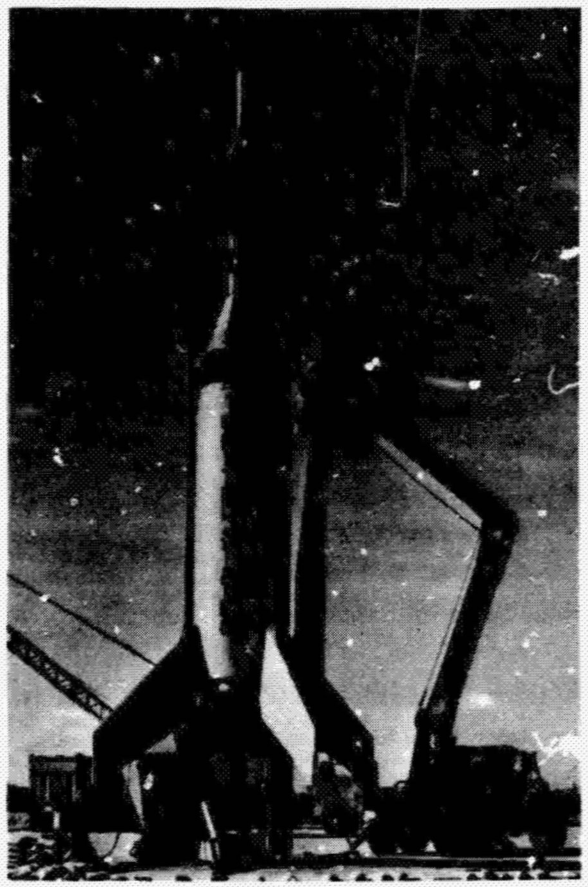

Fig. 14
Production Mercury Capsule and Little Joe Booster
Rocket in Background, Wallops Island 1961

thought of in such a new and radical type of flight vehicle. We also went back to Wallops in April 1961, before the first Vostok and Mercury manned flights, to test the McDonnell capsule at high dynamic pressures with the Little Joe rocket. The purpose of these tests was to ensure that the escape tower and the spacecraft would function properly in the abort mode at high dynamic pressures. These tests proved highly successful by uncovering some deficiencies in the limit switch system that sensed separation which, if they had not been corrected, might have caused unnecessary aborts in the flight tests that followed at Cape Canaveral.

Our Space Task Group was growing. We were hiring not only from Langley, but from Lewis and from other agencies of the Government, and from industry as well. Those days must have been particularly difficult for Langley and its Director, Floyd Thompson, because there was such a need for good people that we could not help but continue to recruit from the Langley Research Center. Floyd Thompson was very wise in the way he handled this. He told me one day, "Bob, I don't mind letting you have as many good people from Langley as you need, but from now on I am going to insist that for each man you want to take, you must also take one that I want you to take." So this is the way it happened. From that day forward, whenever we took a new man that we had recruited, we also took a man the Langley was eager to transfer.

During those early days we had technical reviews of the progress about every two months. We either journeyed to Washington with our charts and models to discuss with Administrator Glennan and his staff our progress and problems, or he would come to Langley Field with Hugh Dryden, Abe Silverstein, and others from Headquarters. These reviews were very good indeed because they set up milestones against which we could measure our progress. We were able to flag our problems for the Administrator, and we had plenty of them.

One of my problems in those days was getting authority from Washington to buildup an adequate staff. This was a project of an entirely different dimension than anything the NACA had ever done before, and even with my best efforts, I still had only a hundred or so people for a project which was growing in complexity and spending many millions of dollars a year. We had to cover many fronts, not only in the manufacturing area and the launch vehicle area, but also in the operations area as well. We had to develop a worldwide tracking net, and work out recovery operations with the U.S. Navy and Air Force. All in all, we had many, many tasks, and for a time I got very little sympathy from my superiors in Washington when I approached them with the need for more men. Fortunately, however, they were ultimately convinced, and we received the Civil Service billets we needed from the newly created Goddard Space Flight Center.

We selected astronauts in April 1959; we flew the Big Joe capsule, our first full-scale reentry test launched by an Atlas, in September 1959. In July we suffered a major setback when our first Atlas-Mercury production vehicle failed structurally under launch loads. These problems were not cleared up until the flight of Mercury-Atlas II in February 1961. In December 1960, we had our first successful flight test of a production capsule launched by the Redstone at Cape Canaveral. Things were beginning to fall into place, and we worked hard to accomplish the first manned flight in 1961.

THE END OF THE BEGINNING

Our first flight in 1961 launched the chimpanzee named "Ham." Ham was a friendly little fellow who received great play in the newspapers. His flight was highly successful from an aeromedical point of view. He was able to function perfectly during the period of weightlessness. He did all his chores and withstood not only the normal launch accelerations, but also a 20-g thrust of the escape tower which occurred because a Redstone timer was improperly set and had not deactivated the abort circuit at the time of main-stage burnout. This was to give the capsule an added push on its way to a new distance record for a Redstone and a landing point far away from the recovery ships.

After the flight of Ham and other tests on Redstone, Atlas, and Little Joe rockets, we were ready for manned flight. On May 5, 1961, Alan Shepard became our first man into space. Later that month, the decision was made to establish a goal of landing Americans on the Moon and returning them safely to Earth before the end of the decade. President Kennedy made that decision on the advice of Lyndon B. Johnson, the Vice President, James Webb, the new NASA Administrator, and with the unanimous support of the Congress of the United States. This truly marked the end of the beginning. John Glenn would fly successfully into orbit in February 1962, to be followed by Scott Carpenter, Wally Schirra, and Gordon Cooper—Cooper making the longest flight of the Mercury Program in May 1963.

The years from "Wallops Island to Mercury" saw the science of flight progress from the sonic barrier to manned satellites capable of orbiting the Earth. In looking back over those years, I think we were extremely fortunate to have developed the people and the capability for manned spacecraft design and operations. We were fortunate also to be there when needed, and even more fortunate to have been given the opportunity to participate in such an important and exciting part of world history.

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